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The Incidence of Affirmative Action: Evidence from Quotas in Private Schools in India

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Abstract

This paper studies the effects of India's main school-integration policy—a 25 percent quota in private schools for disadvantaged students, whose fees are reimbursed by the state—on direct beneficiaries. Combining survey and administrative data from the state of Chhattisgarh, with lottery-based allocation of seats in oversubscribed schools, we show that receiving a quota seat makes students more likely to attend a private school (by 24 percentage points). However, within eligible caste groups, quota applicants are drawn disproportionately from more-educated and economically better-off households and over three-quarters of the applicants who were not allotted a quota seat also attended a private school as fee-paying students. Consequently, we estimate that ~ 70 percent of the total expenditure on each quota seat is inframarginal to school choice. The policy delivers clear gains for direct beneficiaries but is unlikely to affect school integration without broadening the pool of applicants.

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1 Introduction

Social and economic stratification across schools is a concern in many countries. While legislated segregation, such as in apartheid South Africa or during the Jim Crow era in the US, is rare, *de facto* segregation often arises through selective admissions, sorting of households across neighborhoods, or the differential ability to pay school fees.[1] Government interventions to reduce segregation — such as busing, admission quotas, or targeted vouchers — are often controversial, and their effectiveness is subject to considerable scrutiny.

In India, one major route for stratification is access to fee-charging private schools, which account for almost one-third of primary school enrollment in rural areas and one-half in urban areas (Pratham, 2019; Kingdon, 2020). These schools vary widely in their amenities, fee levels, and quality, and access to them is determined by parents' ability to pay. To address concerns about education stratification, the Right to Education (RTE) Act, enacted by the Indian Parliament in 2009, imposed a quota of 25% of the incoming cohort in all private schools for students from disadvantaged economic and caste backgrounds. The government pays the tuition fees for children enrolled under this quota (up to a specified cap) and schools are not allowed to select which students they admit. This policy was controversial at the time of enactment, prompting litigation up to the Supreme Court, and several state governments have chosen to not implement it despite a legal mandate to do so. Despite its salience and scale — the program covered ∼4 million students in 2018/19 — evidence on its effectiveness in improving educational opportunity remains very limited.

We study the effects of this policy in Chhattisgarh, a state of ∼29 million people which has implemented this policy consistently since 2010. Our primary focus, reflecting the intent of the policy, is to investigate whether quota seats allow students from disadvantaged groups to attend schools that they could not have accessed otherwise. A secondary focus is to study effects of receiving a quota seat on educational inputs and student learning and, specifically, on insuring students against educational losses at a time of school closures and income shocks. These questions are central for assessing the effectiveness of this program; further, as we describe below in detail, our results have important implications for the design of affirmative action programs, the targeting of social policy more generally, and the combination of public funding with private management in education.

[1] A vast literature examines racial and socioeconomic segregation in US schools, including broad trends over time in racial segregation and the effects of desegregation initiatives (including countervailing responses by parents). See, for example, Clotfelter (2011); Cascio et al. (2010); Billings et al. (2014); Lutz (2011); Baum-Snow and Lutz (2011); Reber (2011); Reardon and Owens (2014). *De facto* segregation, in relation to both public and private schooling, is more broadly salient: examples include the UK (Jenkins et al., 2008), Chile (Hsieh and Urquiola, 2006), and Scandinavia (Söderström and Uusitalo, 2010).

In Chhattisgarh, RTE quota seats are assigned through a centralized mechanism with lottery-based allocation of seats in oversubscribed schools. We collect data on outcomes by surveying parents and children, and leverage lotteries for identification as in Abdulkadiroğlu et al. (2017), to estimate the causal effects of receiving a quota seat.

Our first result focuses on the extensive margin of private school enrollment. Being allotted an RTE quota seat increases the probability of attending any private school by 24 percentage points (p-value < 0.001). This modest effect is *not* due to a lack of take-up — over 99% of students with an RTE seat do enroll in private schools — but because over 75% of applicants who were not assigned an RTE seat attend a private school anyway (as fee-paying students). Much of this effect is concentrated in the two preschool grades (Nursery and Kindergarten), shifting students from home care to preschool.[2] By Grade 1, when enrollment is compulsory and near-universal, the extensive margin effect is only 12 percentage points (p-value < 0.001); 88% of the control group enrolls in private schools anyway.

Next, we study whether being offered an RTE seat changes the characteristics of the school a child attends. Securing a quota seat does not result in children attending schools with higher enrollment, lower pupil-teacher ratios, or better infrastructure. Most strikingly for a desegregation initiative targeted on caste, the schools attended by applicants who received an RTE seat do not differ in the caste composition of the student body from those attended by unsuccessful applicants.[3] Quota recipients are, however, 11 percentage points (p-value 0.01), over a base of 51%, more likely to attend English-medium schools — an important characteristic differentiating private schools with potentially large labor market returns (Azam et al., 2013). Schools attended by quota recipients are also more expensive and rank higher in parents' applications. Combined with modest extensive margin effects, this finding suggests that some students use the quota seat to upgrade within the private sector.

Third, we explore the effectiveness of fiscal spending on this program. We focus on the proportion of expenditure that is inframarginal for educational choices. On average, Grade 1 students with an RTE seat attend schools that are INR 2,645 ($\sim$USD 35) more expensive (p-value < 0.001). Since this treatment effect is only about 47% of the average fee reimbursed by the state much of the public expenditure on the program is effectively a cash transfer to households.[4] The true cost of the program is even higher since government

[2] The quota covers Nursery and Kindergarten classes, common in private primary schools.

[3] Since the quota only affects up to 25% of the student body (the school determines the other 75%), substantial variation in schools' caste composition remains, even within neighborhoods.

[4] The expenditure is completely inframarginal for students who would have attended the same school without an RTE seat. For other treated students, the inframarginal portion equals the difference between fees reimbursed by the state and what they would have paid (potentially in a different school) without the policy.

reimbursements are capped at INR 7,000 (and 30% of schools charge higher fees than the cap). Valuing a quota seat at the full price paid by non-quota students, the full cost of the policy rises to INR 8,785 per child on average; about 70% of this sum is inframarginal.

The high proportion of inframarginal spending seems to be driven by regressive selection into the applications. Comparisons with representative data suggest that applicants are drawn disproportionately from more-educated and economically better-off households within eligible groups; they are also much more likely to enroll in private schools than the average eligible child (77% vs 27%). This regressivity is unlikely to be explained by low demand but rather by various barriers to applying, such as low information about the policy and the complexity of the application process.[5] Thus, the effectiveness of fiscal spending may be greatly improved by addressing this regressive selection into applying.[6]

Our results thus far reflect enrollment choices made by households in July 2019. Much of the subsequent study period, however, coincided unexpectedly with the COVID-19 pandemic. As in much of India, primary schools in Chhattisgarh were closed in March 2020 and have not re-opened since. Schools were asked to adapt by providing remote instructional support, often through mobile phones, but the extent of such provision has varied widely across schools and students (Pratham, 2020). This disruption does not affect the interpretation of our previous results, which reflect choices made well in advance of these school closures. However, it precludes us from estimating effects of a quota seat on other student outcomes (e.g., learning) in a "business-as-usual" setting. For completeness, we investigate the effects on student outcomes during the pandemic; however, these should be interpreted as estimating whether the publicly funded entitlement through the quota seat helped insure students against (some of) the pandemic's adverse effects on educational outcomes.

We first study the enrollment decisions for the 2020–2021 school year, which were made in the summer of 2020 (amidst school closures and a harsh lockdown). We focus on students who applied for Grade 1 admissions in 2019, and had already completed their transition to primary schooling. Enrollment for students without an RTE seat fell by 4 percentage points in any school and by 8.8 percentage points in private schools (compared to a base of 97% and 88% in 2019 respectively). An RTE seat partially mitigated these effects: Students

[5]As a lower-bound of expected demand, official data report 64,834 Scheduled Caste students (eligible for an RTE seat in Chhattisgarh) enrolled in private schools in Grades 1–3 in 2018. Yet, there were only 12,464 valid applications from Scheduled Caste applicants across Nursery, Kindergarten, and Grade 1.

[6]Even among current applicants, we find greater extensive margin effects for households with less-educated parents and in rural areas. This pattern is similar to that documented for charter schools in Boston, which have larger effects for disadvantaged students but are in higher demand among more advantaged students (Walters, 2018). Similarly, reflecting larger extensive margin effects, the share of inframarginal spending is also lower at the preschool level (where enrollment is not compulsory).

allocated a quota seat were only 1.5 and 1.9 percentage points less likely to enroll in any school and in a private school, respectively, in 2020–2021.

We further collected information about the educational inputs children received and their learning outcomes. Information on inputs was elicited from parents using phone-based surveys between November 2020 and January 2021, while student learning was measured directly using a phone-based learning assessment developed and psychometrically validated by the research team. Quota recipients were more likely to report receiving remote educational content, including video and audio lectures. Treatment effect estimates suggest gains of .19σ (p-value < 0.001) on foundational numeracy and language skills in Hindi and English for quota recipients. These gains vary from .27σ (p-value 0.05) for students in Grade 1 in 2019–20 to .14σ (p-value 0.04) for those in Nursery, although we lack the statistical precision to reject equality of estimates. We find no evidence of heterogeneous effects across cognitive domains or student characteristics.

These gains were achieved at a time of significant disruption and non-standard (remote) instruction. As such, it is difficult to benchmark them against comparable interventions since the study period is not representative of typical implementation scenarios. Compared to effect sizes in business-as-usual settings, these standardized effect sizes compare favorably to those achieved in most field experiments targeting learning outcomes in developing countries, including those focused particularly on public-private partnerships in education.[7] Further, the program was as cost effective in raising test scores as several promising interventions with similarly aged students in India.[8]

Our results complement four main areas of research. First, we advance the literature on affirmative action in education by providing new evidence on one of the largest such schemes in the world: at full scale, the RTE quotas would directly benefit $\sim$ 16 million students annually. Studying the effects of a similar quota on wealthy students in elite schools in Delhi, Rao (2019) shows that wealthier students exposed to poorer peers were more pro-social and generous with, at most, a modest negative effect on test scores. We explore a different question — the effects on direct beneficiaries — in the full range of private schools in a more impoverished state. The only other study we are aware of that focuses on this question is Damera (2017), who uses a

[7]For example, Muralidharan and Sundararaman (2015) document ITT effects of $\sim$0.13σ after four years of receiving a generous school voucher and Romero et al. (2020) show ITT gains of $\sim$0.16σ in language three years after transferring government schools to private management. The median effect size across randomized control trials in developing countries is 0.1σ (Evans and Yuan, 2020).

[8]This includes home visits for preschool-aged children (Andrew et al., 2020), adding staff to public preschools (Ganimian et al., 2021), and vouchers to attend private schools (Muralidharan and Sundararaman, 2015). It is less cost effective than group-based early childhood education sessions (Grantham-McGregor et al., 2020) or, for older students, interventions focused on Teaching at the Right Level (Banerjee et al., 2007).

similar strategy in Karnataka but does not examine effects at the preschool stage (which is the primary margin of impact in our setting).

Second, we contribute to the literature studying how selection in take-up *within* eligible groups may change the actual incidence of policies. In the United States, the take-up of targeted programs is often sharply constrained by a lack of information and by the complexity and costs associated with the application process (see, e.g., Currie (2004); Bettinger et al. (2012); Bhargava and Manoli (2015); Deshpande and Li (2019); Finkelstein and Notowidigdo (2019)). We find, similarly, that application rates for the quota seats are low and regressive within the eligible group. Our results, in contrast to an influential literature in development economics in which self-targeting and ordeals *improve* progressivity (see, e.g., Ravallion (1991), Besley and Coate (1992), and Alatas et al. (2016)), suggest that reducing barriers to application may be essential for improving opportunities for disadvantaged children. Similar issues are likely to be relevant for affirmative action policies more generally, including quotas in higher education and employment in India that have been shown previously to have positive effects (Bagde et al., 2016; Khanna, 2020).

Third, we add to work that studies approaches to combine public funding with private education provision. Past work in this area includes voucher systems (Epple et al., 2017), charter schools (Cohodes and Parham, 2021), and other forms of public-private partnerships (Patrinos et al., 2009; Aslam et al., 2017). By providing a school-specific endowment that pays tuition fees for students in private schools, the RTE quota seats combine many elements of these programs, albeit with several differences in how they are implemented. Unlike most targeted voucher programs, the endowment is school specific (students cannot move their entitlement from one school to another); schools cannot select whether to participate; nor can schools compete to increase the number of voucher students above their 25% cap.[9] The RTE policy is also differentiated from charter schools because only a portion of the student body is subject to non-selective admissions and no tuition fees.

Fourth, and finally, our study directly relates to research on the effects of pandemic-induced school closures and policy responses to "learning loss" (e.g., Bandiera et al. (2020), Angrist et al. (2020b), Carlana and La Ferrara (2021)). Since we evaluate a national policy, with scaled-up implementation, these estimates form a natural benchmark for the many such ongoing studies in developing countries focusing on primary school-aged children. Our results also illustrate how access to in-kind, publicly-funded welfare entitlements in developing countries may serve to insure vulnerable households against severe shocks (also see Gadenne et al. (2021), Gehrke (2019) and Singh et al. (2014) for complementary insights).

[9]These differences are important because the effects of school vouchers are often sensitive to design choices around eligibility, selection, reimbursement, and competition (see Epple et al. (2017)).

2 Study design

2.1 Background

2.1.1 Private school quotas in the Right to Education Act

The Right to Education (RTE) Act is one of India's furthest-reaching educational reforms. Enacted in 2009 by the national Parliament, it sets the regulatory framework for organizing the entire school system, including public and private schools. It makes free and compulsory education from 6–14 years a fundamental right.

We focus on Clause 12(1)(c) of the act, which established the 25% quota in private schools. This provision was motivated by concerns that the rapid growth of fee-charging private schools led to segregated schools and classrooms, and impeded access to high-quality schooling for students from disadvantaged backgrounds. Guidelines issued for implementing the act stress "the need for moving towards composite classrooms with children from diverse backgrounds, rather than homogeneous and exclusivist schools", which echoes desegregation reforms elsewhere.

The clause requires fee-charging private schools to "admit at least 25% of the strength of class I, children belonging to weaker section and children belonging to disadvantaged group from the neighborhood and provide them free and compulsory education till completion of elementary education. Further, where the school admits children at pre-primary level, such admissions will be made at that level". "Weaker section" in the law typically refers to income-poor households, and "disadvantaged groups" to castes and tribal groups that have historically been discriminated against. The government reimburses private schools for tuition fees and other expenditures on students admitted through this quota at notified levels.

This provision has been contentious and was litigated up to the Supreme Court, which affirmed its constitutionality in 2012. However, as with many desegregation policies elsewhere, notably *Brown vs. Board of Education* in the US, universal adoption did not immediately follow the ruling. Individual states in India retain substantial power to decide in whether (and how) to implement the quotas, such as defining the rules for reimbursement and the precise composition of eligible groups. Thus, adoption has been partial and staggered across states; the policy remains unimplemented in several states. In 2018–19, $\sim$4 million students were enrolled in an RTE quota seat,; full national implementation would cover an estimated $\sim$16 million children annually (Indus Action, 2019).

2.1.2 Quotas in Chhattisgarh: context and lottery design

Our study is based in Chhattisgarh state, which had a population of ∼29.4 million in 2020 and has historically been disadvantaged across several development indicators. In 2011, ∼40% of the population was estimated to be below the poverty line (compared to ∼22% nationally). In 2019, the national government ranked the state 21 (out of 25 major states) in its achievements of the UN's Sustainable Development Goals.[10]

Chhattisgarh has implemented the RTE-mandated quota since 2010. Children are eligible for an RTE seat if they are aged 3–7, and meet one of the two following criteria: i) their guardian's annual income must be less than INR 200,000 (∼USD 2,667) *or* ii) belong to a Scheduled Castes (SC), Scheduled Tribes (ST), or Other Backward Classes (OBC).[11] The government reimburses school fees for students admitted under the quota up to a cap of INR 7,000, and provides student grants for books and uniforms. Schools cannot charge top-up fees (even if the school fees exceed the cap for reimbursements).

In 2019, the state moved from decentralized school-level applications to a centralized online application system. Data from this system forms the basis for our sampling frame. The allocation mechanism for quota seats operates in the following steps:

1. Parents rank as many private schools in their catchment area as they want, in their order of preference. The grade to which a child applies (Nursery, Kindergarten, or Grade 1) is determined by their age.[12] Each school-grade combination is treated as a different allocation throughout the lottery process.
2. All students are assigned to their first-preference school if it is not over-subscribed. No priority is given to students with enrolled siblings, living nearby, or otherwise.
3. Students whose first-preference school is over-subscribed enter a lottery (separate for each grade). Each child is in only one school-grade lottery per round.
4. Schools with filled quotas and allocated students are removed.
5. Steps 2–3 are repeated for unassigned students, treating the next school in their preference list that is not full as their "first preference", until either all students are assigned, all schools are filled, or there is no possible match.

[10] Although still at a low level, Chhattisgarh's development indicators have rapidly improved. It experienced one of the country's fastest improvements in the Multidimensional Poverty Index between 2005 and 2015 in both absolute and relative terms (Alkire et al., 2021).

[11] The Constitution of India recognizes the terms Scheduled Castes and Scheduled Tribes. Likewise, Other Backward Classes are formally known in the Constitution as socially and educationally backward classes (SEBC). The Constitution provides positive discrimination for these groups and citizens can apply for a caste certificate to access these benefits.

[12] Applicants to Nursery are 3–4 years old, those applying for Kindergarten (KG) are 4–5 years old, and for Class 1 are 5–6.5 years old.

The lottery-based allocation in Step 3 is central to our empirical strategy, which primarily compares lottery-winning students to lottery-losing students in oversubscribed schools.[13] In practice, local officials also seem to attempt to pair unmatched individuals to available seats (potentially in schools that parents did not initially rank) after the lottery process. We treat this scenario as non-compliance and rely only on the lottery-based variation for identification.

2.2 Empirical strategy

First, we use the following specification to estimate the intent-to-treat (ITT) effect of being assigned a lottery seat:

$$Y_i = \alpha Z_i + \sum_x \gamma_x d_i(x) + v_i, \tag{1}$$

where Y_i indicates the outcome for child i and Z_i indicates winning the lottery for an RTE seat in a private school. This offer (Z_i) is randomly assigned conditional on applicants' ranking of schools but not unconditionally. Therefore, we condition on a vector of dummy variables $d_i(x)$ to account for the application choices of each student i ("randomization strata" or risk sets). Our coefficient of interest, α, is the ITT effect of being offered an RTE seat through the lottery.

Our preferred specifications adopt Abdulkadiroğlu et al. (2017)'s approach to controlling for applicant risk sets. We condition on a vector of narrow bins (of 0.001 probability each) of being assigned to a private school. We computed these probabilities by running 10,000 simulations of the assignment mechanism given the applicants' preferences. For each simulation, we recorded the school each student was assigned to. We then estimated, across all simulations, each child's probability of being assigned to a private school. The identifying assumption is that the offer of an RTE seat is conditionally exogenous after controlling for these narrow bins of the probability of an offer. For transparency and robustness, we also present estimates conditioning on the full set of preferences in Appendix B.[14]

[13] This algorithm satisfies the "Equal Treatment of Equals" (ETE) property, which is a pre-requisite for the Abdulkadiroğlu et al. (2017) procedure, although it is not strategy-proof. See also Borusyak and Hull (2020), who discuss the broad class of applications that feature exogenous assignment shocks in some subgroups, where groups have varying risk sets; centralized admissions mechanisms offer one such setting.

[14] This latter strategy provides conditional ignorability by only comparing students who applied to the same schools in the same order. It is inefficient, as limiting comparisons to exact matches discards much of the available variation. Our results are similar in magnitude and statistical significance across both procedures. In addition, all our results are substantively similar if we use wider bins of 0.01.

Ex-post, some lottery losers may be assigned RTE seats in schools that still have space. Since the policy variable is offering an RTE seat, we estimate, and focus on, the local average treatment effect (LATE) of being allocated an RTE seat.[15] We estimate the LATE by instrumenting an RTE seat assignment with winning the lottery. Specifically, we estimate the following equations via two-stage least-squares:

$$T_i = \beta Z_i + \sum_x \gamma_x d_i(x) + u_i, \tag{2}$$

$$Y_i = \delta \widehat{T}_i + \sum_x \gamma_x d_i(x) + \varepsilon_i, \tag{3}$$

where T_i indicates being assigned an RTE seat, and everything else is as in Equation 1. Here, δ is the effect of securing an RTE seat (through any means) on the outcome.

Further, we will compare the LATE estimate to the control compliers mean — the mean outcomes for compliers who lose the lottery (and therefore do not get an RTE seat through other means). This is the relevant comparison, as it is the counterfactual outcome for compliers (over which the LATE is estimated). To do so, we follow Imbens and Rubin (1997) and Abadie (2003) (and specifically Abdulkadiroğlu et al. (2018)'s implementation of Lemma 2.1 in Abadie (2002)). Intuitively, the mean outcome for those without an RTE seat is a weighted combination of the mean outcome for never-takers and for compliers who lost the lottery; the weights correspond to the probability of these subpopulations in the entire population, which we can infer from the data. Since we can also infer the mean outcome for never-takers by studying those who won the lottery but do not have an RTE seat, we can back out the mean outcome for compliers who lost the lottery.

Specifically, let $Y_i(1)$ and $Y_i(0)$ denote the potential outcome for individual i as a function of whether they were allotted an RTE seat. Let $T_i(1)$ and $T_i(0)$ denote the potential treatment (being allotted an RTE seat), as a function of the outcome of the lottery (Z_i). The mean value of $g(Y_i)$ for compliers who lose the lottery is:

$$\mathbb{E}[g(Y_i(0))|T_i(1) > T_i(0)] = \frac{\mathbb{E}[g(Y_i)(1-T_i)|Z_i = 1] - \mathbb{E}[g(Y_i)(1-T_i)|Z_i = 0]}{\mathbb{E}[1 - T_i|Z_i = 1] - \mathbb{E}[1 - T_i|Z_i = 0]} \tag{4}$$

Setting $g(Y_i) = Y_i$ we obtain the average control outcome for compliers (i.e., $\mathbb{E}[Y_i(0)|T_i(1) > T_i(0)]$). This quantity can be estimated via two-stage least-squares by regressing the interaction of the outcome (Y_i) with an indicator for not being

[15] Nearly everyone ($\sim$ 95%) who is offered a seat, takes it. Thus, in practice there is little difference between estimating the LATE of being offered a seat, and enrolling in an RTE seat.

assigned an RTE seat ($1 - T_i$) on an indicator for not being assigned an RTE seat, using the outcome of the lottery as an instrument.[16]

2.3 Data

We use data from three sources: (i) application data provided by parents in 2019; (ii) two rounds of survey data collected by the research team to study outcomes; and (iii) administrative data on school characteristics. We describe each of these sources below.

2.3.1 Application data

We obtained data for all eligible applications submitted in 2019 through the online allocation system to implement the RTE in Chhattisgarh. The data has parents' rankings over schools, the assigned school (if any), and limited household characteristics, including their phone number. Parents applied to schools in March–April 2019 and were notified of the school assignment in May. The school year began in mid-June. Figure 1 illustrates the timeline of RTE and data-collection-related events.

In 2019, valid applications were received from 54,676 eligible students, 6,830 of whom were not matched (see Table A.1; Panel A). Nearly half (48%) of the applicants were female and 56% live in a rural area. More than 50% of applicants have only one school on their preference list, and 92% have at most three preferences.

For ∼69% of applicants, the allocation system does not provide variation in whether they are assigned to a private school.[17] Our primary data collection focused on the remainder of the sample (N=16,703), for which we have some identifying variation on the extensive margin. One-third of these students were left unallotted (see Table A.1, Panel B). This subsample has a similar proportion of girls and number of schools applied for as the full sample that includes all applicants. However, the subsample is more urban (since urban areas are more likely to have oversubscribed schools) and, relatedly, has a lower proportion of Scheduled Castes and Scheduled Tribes. There are 5,863 schools in the lottery, each with

[16] Analogously, the mean value of $g(Y_i)$ for compliers who win the lottery is:

$$\mathbb{E}[g(Y_i(1))|T_i(1) > T_i(0)] = \frac{\mathbb{E}[g(Y_i)T_i|Z_i = 1] - \mathbb{E}[g(Y_i)T_i|Z_i = 0]}{\mathbb{E}[T_i|Z_i = 1] - \mathbb{E}[T_i|Z_i = 0]},$$

which can also be obtained via two-stage least-squares by regressing the interacting the outcome (Y_i) with an indicator for being assigned an RTE seat (T_i) on an indicator for being assigned an RTE seat, using the lottery outcome as an instrument.

[17] Given applicants' preference ordering, applications by other parents, and the number of seats available in each school/grade, these applicants are allocated to *some* private school with certainty (even if the private school they end up in is stochastic).

roughly 10 seats available on average, but with 15 students applying for a seat.[18] Schools are more likely to have seats available in Nursery than in Grade 1 (see Table A.1, Panel C). Table A.1 provides further details on the characteristics of applicants and schools, and Table A.2 explores how application behavior varies by household characteristics (see Table A.2).

2.3.2 Primary data from phone surveys

We conducted two rounds of phone-based surveys to collect primary data on schooling choices, educational inputs, and learning outcomes from treated and untreated students. We randomized the order in which we called households in both survey rounds.

First, between August and September of 2020, we attempted to call all individuals with an ex-ante probability of less than one of being allotted a private school quota seat (see Table A.1, Panel B) using the phone numbers provided by parents on their applications. We collected information about which school the applicant eventually enrolled in for the 2019–20 and 2020–21 school years, along with basic school characteristics (e.g., medium of instruction and fee level) and household characteristics (parental education and occupation). We made up to five attempts to reach each household and completed interviews with about 45% of the targeted households.

Between November 2020 and January 2021, we attempted to recontact all households interviewed in the first phone survey and completed interviews with 59% of them. This second round focused on two main areas. First, due to state-wide school closures caused by the COVID-19 pandemic, we collected information from parents on the level and type of academic support students received from schools, family members, or paid extra tuition.

Second, we conducted a phone-based learning assessment for students. During the survey, enumerators engage children in a conversation in which questions designed to capture foundational numeracy and language skills in preschool and primary school-age children were embedded. These questions were adapted from tests we had previously administered to preschool-age children in other states and was validated through extensive pilot testing before being administered in this sample. We estimate a two-parameter item response theory (IRT) model to obtain a proxy for students' ability.[19] Appendix C describes the test and the analyses undertaken to validate its content: the test items correlate well with each other (Cronbach's alpha $>$ 0.9); the test scores are (as expected) higher for

[18]Figure A.1 provides the full distribution of the number of applications schools receive. The average (median) school is ranked in 15 (10) applications. If we focused on parents' top choices, the average (median) school is ranked first in 9.3 (7) applications.

[19]We use a two-parameter logistic (2PL) model since our assessment did not feature any multiple-choice questions. Our use of IRT scores follows modern practice in international assessments. It is also increasingly common in studies in developing countries that rely on researcher-developed student assessments (e.g. Das and Zajonc (2010), Bau and Das (2020) Muralidharan et al. (2019), Singh (2020) and Mbiti et al. (2019a,b)).

older children and for those from economically better-off households; and there is a good empirical fit to estimated Item Characteristic Curves from the IRT model and no evidence of differential item functioning across grades.[20]

2.3.3 Administrative data on school characteristics

Finally, we use the U-DISE (Unified District Information System for Education) database, an annual census of all recognized (public and private) schools in the country.[21] The U-DISE dataset contains information on school enrollment, infrastructure, fees, and teachers. We use data from the 2017–2018 school year, the most recent for which data were available at the time of writing.

2.4 Validity of the research design

2.4.1 Balance

We test for balance of observed characteristics in the applicant data and both phone surveys. Table 1 reports the results using our preferred specification, which conditions on bins of the probability of being offered a private school seat as in Abdulkadiroğlu et al. (2017), for all three samples. Table B.1 presents the results conditioning instead on the full vector of unique preference lists. Conditional on strata fixed effects, we cannot reject the equality of mean characteristics across lottery winners and losers in any sample.

2.4.2 Attrition

Attrition is moderately unbalanced across lottery winners and losers: conditioning on the lotteries, we are slightly more likely — by 2.1 percentage points (over a base of 45%) in the first round and by 2.9 percentage points (over a base of 26%) in the second round — to reach students who were offered a seat than those who were not (see last row in Table 1). Survey non-response is driven by being provided inaccurate phone numbers or failing to obtain a response even after five attempts. Attrition is higher for households in rural areas and those belonging to Scheduled Castes and Scheduled Tribes (see Table A.3). We investigate the sensitivity of our results to using low differential-attrition strata and Lee (2009) bounds.[22] Given the modest differences in attrition, our main findings are robust to these corrections.

[20]To the best of our knowledge, our study is the first to demonstrate the viability of phone-based learning assessments in preschool-age children in low-income settings. For a similar demonstration for older students in East Africa and Sierra Leone, see Angrist et al. (2020a).

[21]The U-DISE dataset does not include unrecognized private schools — schools that are operating without license or authorization from the government (Kingdon, 2020). This is not relevant in our setting since, by necessity, the policy only applies to recognized private schools.

[22]We follow Engberg et al. (2014)'s approach to construct bounds, under a monotonicity assumption of the attrition process, for continuous outcomes. For binary outcomes, we implement Lee (2009) style trimming within each stratum.

2.4.3 Non-compliance / First stage

We verify that winning the first lottery corresponds to an offer of a free seat. Nearly all lottery winners reported having been allotted a seat (∼94%) in the phone survey, but so do about 18% of lottery losers (Table 1). As mentioned above, non-compliance among lottery losers (i.e., "always-takers") is expected, since local authorities attempt to fill vacant seats after the lottery-based allocation (the data we use) with unmatched parents. There is some heterogeneity across grades (see Table A.4): compliance decreases from Nursery to Grade 1. As mentioned above, we focus on the LATE of being allocated an RTE seat, using the outcome of the lottery as an instrument.

2.5 External validity

While our empirical strategy provides internally valid estimates for students with an ex-ante probability of less than one of being allotted a private school quota seat, it does not allow us to draw inferences about the treatment effects for students who are always assigned to a private school. To compare our core sample to the overall sample of applicants, we also collected data from a random sample of 1,203 students who are always assigned to private schools. Of these 1,203 students, 462 answered our phone survey. We use these data to discuss the external validity of our results in Section 3.2.2, when discussing self-selection into applying and the incidence of policy benefits.

3 Results

3.1 Effects on enrollment decisions

Receiving a free seat may allow some quota-eligible students, who may not be able to secure admission or pay fees, to enroll in schools they could not attend otherwise. This potential shift in enrollment choices is the primary channel of (potential) impact for the RTE quota seats, and the guiding mechanism that motivates the policy. These changes may operate on both the extensive margin, moving students into private schools (from no schooling or public schools), and the intensive margin, changing which private school they attend. Our first focus is therefore to estimate policy-induced shifts on both margins.

3.1.1 Extensive margin of (private) school enrollment

The 4–6 age group, when students apply for RTE quotas, is a period of transitioning into primary schooling from either preschool or non-enrollment. Unlike primary schooling, which is mandatory from 6 years of age, preschool enrollment is neither universal nor compulsory. Guidelines for the enrollment age are often loosely applied.

Therefore, children in this age group may be enrolled in a government childcare center or the pre-primary section of a private school, or enrolled in Grade 1 in either a government or private primary school, or not be enrolled in any preschool/school. Thus, a movement into the private sector can be induced on multiple margins. We collapse these possibilities into three states — (a) enrolled in a private preschool or school, (b) enrolled in a government school, and (c) not enrolled — and study the effects of being offered an RTE seat on each of these margins separately.[23] Here we study enrollment choices for the 2019–20 academic year, which were made before the COVID-19 pandemic.[24]

We note four main results. First, nearly all applicants who were assigned an RTE seat were enrolled in private schools in 2019–20. However, this translates to around a 24 -percentage-point (p-value < 0.001) increase in the probability of private school enrollment, as over three-quarters of compliers who did *not* receive an RTE place were also enrolled in private schools (Table 2, Columns 1–4).[25] Thus, the pool of applicants seems to disproportionately consist of students who would have attended private school anyway. For comparison, the administrative data indicate that only 27% of the state's Scheduled Caste students in Grades 1–3 attend a private school, which is much lower than the control complier mean in our sample.

Second, applicants assigned an RTE seat were 18 percentage points (p-value < 0.001) more likely to be enrolled in *any* school in 2019–20 from a base of 83% among the compliers (Table 2, Columns 1–4).

Third, the extensive margin effect is concentrated in the two preschool grades (Nursery and Kindergarten) that precede formal schooling, shifting students from home care to (private) preschool. Applicants to Nursery who are assigned an RTE seat were 25 percentage points more likely to be enrolled in any school and 28 percentage points more likely to be enrolled in a private school in 2019-20; in Kindergarten, this declines to 16 and 22 percentage points, respectively; in Grade 1, this declines further to 2.8 and 12 percentage points. This finding suggests that the steady-state effect of being allotted an RTE seat is likely to be only around a 12 -percentage-point increase in the probability of attending private school (the estimated effect in Grade 1 in 2019–20, when nearly all children were enrolled in school).

[23]We do not distinguish between non-enrolled and government daycare centers (called *anganwadis*), because the latter provide very little early childhood stimulation in practice. Nor do we distinguish between pre-primary and primary grades in private schools, since they exist in the same schools and kindergarten (preschool) classes serve as feeder grades into primary schooling (Singh, 2014).

[24]While data on these choices was collected in August-September 2020 (i.e., after the 2020–21 school year enrollment choices were finalized), we study enrollment choices for the 2020–21 school academic year in Section 4.1, since these were affected by the COVID-19 pandemic.

[25]Throughout this section, and in what follows, we discuss LATE estimates as the principal parameters of interest. We present the ITT estimates only for transparency and do not emphasize them in the text.

These results are robust to using only strata with no attrition, to focusing on strata with low differential attrition, and to Lee (2009) bounds correcting for differential attrition (see Table A.5).

Finally, we study heterogeneity in the effects by gender, maternal education, and caste group (see Table 3). In all groups, applicants who were allotted a seat were nearly universally enrolled in private schools. Thus, receiving an RTE offer through lottery eliminates the gap between the more- and less-advantaged subgroups. Specifically, the probability of private school enrollment is lower for children whose mother has less than a high school education and for those from Scheduled Castes; thus, these groups have higher treatment effects that compensate for this initial disadvantage. We do not find that the probability of private school enrollment differs between boys and girls in our sample, and therefore detect no heterogeneity in treatment effects. For all subgroups, the absolute treatment effect on private school enrollment is still modest in Grade 1, the steady-state effect.

3.1.2 Characteristics of the schools attended

Modest effects on the extensive margin may be consistent with larger effects on the intensive margin. That is, a quota seat may change which school a child enrolls in within the private sector. We therefore examine whether receiving a quota seat changes the characteristics of the schools that students attend.

Table 4 presents the results for a vector of characteristics that persist in the short run. We focus on applicants to Grade 1 — for which enrollment in formal schooling is near universal — to avoid confounding the effects on school characteristics with those on the extensive margin on school enrollment. In this sample, applicants assigned an RTE seat are 11 percentage points (p-value 0.01) more likely to attend English-medium schools (from a base of 51%). This increase is significant because English-medium instruction is perceived to have large labor market returns (Azam et al., 2013).

As a caste-based desegregation initiative, the quota seems ineffective: the average child allocated a seat is not exposed to a different socio-economic mix of peers (as measured by the proportion of students from Scheduled Castes and Tribes) than they would be without an RTE seat. This is also true if we explore heterogeneity by caste group. Scheduled Caste students allotted a seat do not attend schools with a different proportion of Scheduled Caste students. Likewise, Scheduled Tribe students allotted a seat do not attend schools with a different proportion of Scheduled Tribe students. Table A.9 provides more details. Finally, there are no discernible differences in the schools children who receive an RTE seat attend in terms of infrastructure, size (enrollment), or pupil–teacher ratios.[26]

3.1.3 Do lottery winners attend more expensive schools?

An alternative approach to summarizing the extent to which a quota seat changes the schools children attend is to study whether quota students attend more expensive schools (and how much more expensive they are). In this context, school fees are the unsubsidized market price paid by non-quota students (taken from administrative data).

The median private school in our sample charges INR 5,650 per year (∼USD 75). The distribution of private school fees varies from INR 2,100 (∼USD 28) at the 5th percentile to INR 18,000 (∼USD 240) at the 95th percentile. Public schooling and non-enrollment are both free options (i.e., have a market price of zero).

The schooling choices of applicants allotted an RTE seat have a market price that is INR 4,274 (p-value < 0.001) higher, on average, over a base of INR 5,263 (see Panel A - Table 5, Column 1). This treatment effect reflects both extensive margin shifts from zero-fee options (public schools and non-enrollment) to private schooling and movements within the private sector. The effect falls from Nursery to Kindergarten/Grade 1 as more applicants without an RTE seat move from non-enrollment to fee-charging private schools. Among applicants to Grade 1, when nearly all children are enrolled in schooling, the effect on market price is INR 2,645 (p-value < 0.001), over a base of INR 6,012.

Further, we further decompose the total effect on market price (Y_i) into its constituent parts:

$$
\begin{aligned}
\mathbb{E}\left(Y_{1i} - Y_{0i}\right) \quad = \quad & \underbrace{\underbrace{\left[P\left(Y_i > 0 | T_i = 1\right) - P\left(Y_i > 0 | T_i = 0\right)\right]}_{\text{(Participatory effect)}} \times \underbrace{\mathbb{E}\left(Y_i | Y_i > 0, T_i = 1\right)}_{\text{(Average fee (winners))}}}_{\text{Extensive margin effect}} + \\
& \underbrace{\underbrace{\left[\mathbb{E}\left(Y_i | Y_i > 0, T_i = 1\right) - \mathbb{E}\left(Y_i | Y_i > 0, T_i = 0\right)\right]}_{\text{(Conditional-on-positives effect)}} \times \underbrace{P\left(Y_i > 0 | T_i = 0\right)}_{\text{(\% losers in private school)}}}_{\text{Intensive margin effect}}
\end{aligned}
$$

This decomposition provides an accounting benchmark for the relative importance of the

[26] These results are not mechanical. On average private schools have a lower proportion of students from Scheduled Castes and Tribes as public schools, even within the same pincode. Shuffling Scheduled Castes and Tribes students from public to private schools (to use all available RTE seats) would eliminate the current gap (of almost 20 percentage points) in the proportion of students from these groups across public and private schools (see Table A.18). Other schools characteristics (medium of instruction, enrollment, number of teachers, and facilities) are also different between public and private schools, even within the same pincode (see Table A.19).

extensive and intensive margin effects (see Panel B - Table 5).[27] In the early grades, where the offer of a quota seat induces children to move from non-enrollment to private pre-schools, the extensive margin accounts for three-quarters of the total effect. By Grade 1, most students are already in school, and the extensive margin is much less important, in both absolute and relative terms, and accounts for about half of the total effect. The conditional-on-positives effect, which quantifies the upgrade by applicants who would have attended private schools anyway, is INR 1,373 and is similar across the sample in different grades. In Grade 1, the conditional-on-positives effect of INR 1,516 is only about 18% of the average market price in schools attended by lottery winners. This suggests that much of the spending on quota seats is likely to be irrelevant or inframarginal to school choice, which we discuss in more detail in Section 3.2.

3.1.4 Do quota seats lead to more-preferred schools?

The implicit motivation behind the policy is that, due to the constraints posed by selective admissions criteria and fees, students from quota-eligible groups cannot attend schools that they would otherwise prefer to enroll in. The extent to which this is true depends on the fraction of applicants who would, in the absence of a quota seat, attend the same school as fee-paying students.

We investigate this directly by examining treatment effects on the probability of enrolling in the parents' top-ranked school on the RTE application.[28] Our specification is analogous to that used to estimate the intent-to-treat in Tables 2 and 3, except that the treatment (lottery-based RTE offer) is specific to the top-choice school rather than any school. We do this to avoid violations of monotonicity that are implied in using an offer of *any* seat through the RTE lottery, which would lead to difficulties in interpretation (Heckman et al., 2006).[29]

We find that 30% of students who did not receive a lottery-based offer of a seat at their top-choice school are nonetheless enrolled in their top choice; this figure rises to

[27]The decomposition should not be interpreted causally. While the participatory effect is well identified, the average fee of the winners and the conditional-on-positives effect both condition on private school enrollment, a post-treatment outcome (see Angrist (2001)).

[28]As mentioned above, the mechanism used in Chhattisgarh to assign RTE seats is not strategy proof. However, strategic considerations are unlikely to play an important role in practice, because the allocation rule was never mentioned in documents available to the public beyond stipulating that allocations will be lottery based; it was also the first time that centralized admissions allocation decisions were made in the state. Thus, we expect parents' first-choice school to reasonably reflect their true preferences in this setting.

[29]For instance, although the offer of a free quota seat in their top-choice school makes a student more likely to attend that school, an offer for their second-choice school may make her *less* likely to enroll in the top-choice school as a fee-paying student. We report evidence of such cross-partial effects when regressing enrollment in the top-choice school on a vector of offers at top/second/third schools (Table A.8). We do not attempt to estimate local average treatment effects for the full sample, as we do not have an endogenous measure of whether a child was offered a seat (outside the lottery) in their top-choice school: although we asked parents whether they had been offered an RTE seat, we did not ask which school it was for.

83% for students who were offered an RTE place, a treatment effect of 53 percentage points (Table 6, Column 1). In Grade 1, our steady-state sample, these numbers are 35% and 79%, respectively (a treatment effect of 44 percentage points). In the sample of control students who are enrolled, and were not allotted any RTE seat, ∼39% of students nonetheless attend their top-choice school in Grade 1. Thus, even among students who were allotted a seat at their top-choice school, the offer only induces changes in enrollment decisions for 50–60% of them (Table 6, Column 4). Overall, quota seats are inframarginal to the choices of a substantial share of recipients.

3.2 Fiscal cost and incidence of RTE quota seats

Section 3.1 established the causal treatment effects of receiving a quota seat on students' enrollment choices. A fuller assessment of the policy's effectiveness must consider at least two additional questions. First, how effective is the fiscal spending on this program at achieving its targets? Second, and relatedly, to what extent does the policy succeed in targeting the households in greatest need of support?

3.2.1 Inframarginality of program spending

First we consider the share of public spending on the program that is inframarginal to schooling decisions. This focus reflects our interpretation of the primary intent of the policy, which is to provide better schooling options for disadvantaged students. We focus on Grade 1 applicants, after enrollment transitions are complete, since these provide the closest estimates of the fiscal cost of the program through elementary schooling (since the entitlement of the quota seat is up until Grade 8). As seen in Section 3.1.4, the expenditure is completely irrelevant for school choice for the ∼30% of students who would attend their top-choice school even without an RTE seat. For many other students, some of the expenditure is still inframarginal — the difference between the fees reimbursed by the state and what they would have paid in school fees (potentially in a different school) in the absence of the policy.

Our benchmark here is the causal effect of receiving a quota seat on the market price of the schooling option (i.e., the average economic value of the improvement in educational options received by beneficiaries). This sum, which is INR 2,645 in Grade 1, represents the lowest mean value of a top-up voucher required for parents to choose, in the absence of selective admissions, the same options as they avail in the quota regime. This thought experiment takes the pool of applicants, their preferences, and the availability of seats as given.[30] This estimate, first reported in Table 6, is repeated in Table 7 for convenience.

[30] We ignore income effects from the transfer (treating them as small in relation to annual household budgets). This exercise also disregards the welfare effects of the inframarginal portion of the expenditure,

We compare the policy's average cost to the government to this benchmark. This sum is given by the fees charged by the allotted school up to a maximum of INR 7,000 (Table 7, Panel B). In Grade 1, this sum averages INR 5,621.[31] Thus, in Grade 1, approximately half of the reimbursed amount is inframarginal to school choice.

However, the total cost of the program must take into account private schools' contributions — 30% of schools charge a higher fee than the reimbursement cap of INR 7,000. The total cost is identical to estimating reimbursements in the absence of the capped limit of INR 7,000. This sum is INR 8,785 on average, which is ∼3.3 times the incremental educational expenditure on fees received by the beneficiaries. The difference between the "full cost" and the reimbursed value of the RTE seat effectively represents a tax on high-fee private schools (imposed by the cap). This effective tax may partly explain the strong opposition to this policy by elite private schools in many states across the country. The value of the tax is similar to the net incremental value in school fees that students gain.

In summary, a substantial portion of the average cost of the quota — about half of the reimbursed amount and three-quarters of the total cost — is inframarginal to school choices.

3.2.2 Incidence of policy benefits

Our second exercise explores the incidence of policy benefits *within* the groups eligible for the quota and in the population overall.[32] Our primary concern here is selection into the pool of applicants within quota-eligible groups, which we quantify by comparing applicants to population-level representative sources.

First, we use official U-DISE data on enrollment in each recognized school in the state broken down by caste to compute the share of students belonging to Scheduled Castes and Scheduled Tribes enrolled in private schools in Grades 1–3. Among Scheduled Caste students in Grades 1–3, 27% attend a private school, which is much lower than the control mean of 77%. This comparison suggests substantial positive selection in the pool of applicants.

Further, we compare applicants to other households in Chhattisgarh using the National Family Health Survey (NFHS) from 2015–2016, which is representative at the state level

which is effectively a cash transfer, as these are outside the policy objectives. In this, we follow the long literature on the impact of educational vouchers and other inputs in multiple settings (Epple et al., 2017).

[31] In practice, this is computed by running the identical regression as shown in Panel A for the benchmark with a dependent variable for reimbursements, which is defined as zero for all untreated students and equals the actual school fee or 7,000 INR (whichever is lower) for all treated students.

[32] This reflects the motivation behind the policy's design and affirmative action in India more generally. Quotas for Scheduled Castes and Tribes aim to address the legacy of a long history of discrimination, not just current economic disadvantage. Even so, publicly funded access to (expensive) private goods is likely to be most important for poorer households within these groups.

(see Column 6 of Table 8). The data we collected on applicants who are always assigned to private schools allow us to compare the average applicant to the average eligible household in the NFHS survey.[33] We restrict the NFHS sample to households with children aged 4–7 and present estimates for the overall population and individual caste groups. We focus on two margins. The first is asset ownership, which we summarize with an index based on a Principal Component Analysis (see Table A.15 for detailed asset information). The second margin is maternal and paternal education, which we summarize as whether the parents have above primary education or not (see Tables A.16 and A.17 for detailed parental education information). Overall, the average applicant lives in a household with more assets (e.g., a television and a refrigerator) and more educated parents than the average child in the state (without conditioning on eligibility for an RTE seat). Applicants are also better off within each caste group.

These tables also indicate that our main sample, of students for whom we have lottery-based identifying variation, is moderately better off than the sample of students who are effectively guaranteed a private school allocation. This likely reflects the moderate over-representation of urban areas in our sample (which have more oversubscribed schools). Nonetheless, the socioeconomic characteristics of these subsamples of applicants are much closer to each other than to the full population in the respective caste groups.

Overall, these analyses suggest that the *de facto* incidence of the policy benefits is regressive within quota-eligible groups due to selection into who applies for quota seats.

4 Policy effects during the COVID-19 pandemic

The latter period of our study unexpectedly coincided with the COVID-19 pandemic. The exceptional nature of the pandemic precludes us from studying the business-as-usual effects of quota seats on learning inputs and outcomes. However, it also provides a unique opportunity to study a different question — whether the RTE quota, by providing a publicly funded entitlement, helped insure students against (some of) the negative educational consequences of this disruption.

4.1 Effects on enrollment

The onset of the COVID-19 pandemic in March 2020 in India was accompanied by a strict national lockdown. This phase of the pandemic, which was characterized by severe income and unemployment shocks to households, also coincided with the period in which parents

[33] We account for non-response in our survey by predicting the likelihood that applicants will answer the survey using household characteristics, and then re-weight the data using inverse probability weights.

made their enrollment decisions for the subsequent school year.[34] Thus, our first set of investigations focus on the effects of the COVID-19 shock on enrollment, and the extent to which quota seats may have insulated beneficiaries.

For control students who originally applied to Grade 1, and thus should have transitioned to Grade 2 in 2020, enrollment in any school dropped by 4 percentage points (p-value 0.005) in 2020–2021. Enrollment in private schools dropped by 8.8 percentage points (p-value < 0.001) during this period. These declines are likely due to parents' reduced ability and willingness to pay school fees. An RTE seat partially mitigates these effects: students allocated a quota seat were only 1.5 and 1.9 percentage points less likely to enroll in any school and in a private school, respectively, in 2020–2021 (see Table 9).

4.2 Effects on educational inputs

Schools stopped in-person instruction in March 2020 and did not resume before the end of the 2020–2021 academic year. As in other countries, schools and education systems have tried to adapt by providing remote instructional support. The extent of this support, often provided through mobile phones, has varied widely across schools and students (Pratham, 2020). By enabling access to more expensive schools (which may have provided better support for remote learning), and by ensuring enrollment despite income shocks, an RTE seat may have ensured a greater degree of educational support during the pandemic. We investigate this possibility in Table 10 using parents' reports of educational inputs.

Among applicants who were not allotted an RTE seat, nearly all parents reported that schools were closed, 40% reported some academic support was provided by the school, 27% reported video lectures, 13% reported audio lectures, 82% reported some instructional support provided by household members and 14% reported having hired a private tutor. These estimates are in a similar range as state-wide estimates in Pratham (2020), in which $\sim$37% of primary school students reported receiving any instructional material in a reference week, the vast majority of which was obtained through WhatsApp and phone calls. Since both our surveys and those reported in Pratham (2020) implicitly condition on owning mobile phones, these estimates are likely to represent an upper bound on the extent of remote support during school closures for students in Chhattisgarh.

Parents of applicants assigned an RTE seat report moderately more support, with treatment effects of 13 percentage points (p-value < 0.001) for some academic support, 22 percentage points (p-value < 0.001) for video lectures and 13 percentage points

[34] A large survey across six large states, funded by UK Aid and the World Bank, documented that, between March and July 2020, weekly earnings fell by 43% for those employed, the share of unemployed rose from 17% to 40% of all respondents, and per capita consumption fell from INR 2,409 to INR 1,700 (Pinto et al., 2020).

(p-value $<$ 0.001) for audio lectures. These treatment effects are concentrated in Nursery and Kindergarten but are close to zero (and statistically insignificant) in Grade 1. This difference arises from lower support reported by applicants who were not allotted an RTE seat in the Nursery/Kindergarten sample, and is consistent with the main channel of effect being the extensive margin of enrollment.

The proportion of parents reporting home educational activities and private tutoring, which may have been used to compensate for school closures and differential levels of support being provided by schools, does not differ across the groups.

4.3 Effects on learning outcomes

Finally, we investigate the treatment effects on learning outcomes measured using phone-based learning assessments. Reflecting our test design, the aggregate score from our assessment may be interpreted as a composite measure of foundational literacy and numeracy.

Applicants who were allotted an RTE seat have test scores that are .19σ higher (Table 11; p-value $<$ 0.001).[35] Following Abadie (2002) and Abdulkadiroğlu et al. (2018), we estimate the cumulative distribution of test scores for treated and untreated compliers and find that receiving an RTE seat shifts the full distribution of achievement rightwards in all grades (Figure 2). The treatment effects appear to be larger in the Kindergarten and Grade 1 samples (at .25σ and .27σ, respectively) than for students in Nursery (at .14σ). However, although these individual effects are all statistically distinguishable from zero at the 5% level, we cannot reject the equality of estimates across the grade-level samples (p-value .51).[36] We also find no significant evidence of heterogeneity by student age, gender, caste, or location (see Table A.11).

Although standardized effect sizes are commonly used to study learning gains in both high-income and developing countries, the magnitude of these treatment effects may be sensitive to dispersion in different samples, students' age, the specific tests administered, and the scoring procedures used (Singh, 2015a,b). We use two procedures to provide a more interpretable metric. First, we disaggregate our tests, by subject, into the specific competencies being tested. We find consistent evidence of positive treatment effects in both

[35]The results using the pooled sample are robust to correcting for the modest differential attrition by considering only low-attrition strata and to Lee (2009) bounds (see Table A.12).

[36]One possibility that could account for lower treatment effects in the Nursery sample is that remote instruction is less productive for very young students. In addition, the treatment effects observed for Grade 1 may reflect higher-quality instruction being transmitted by the schools attended by RTE quota students (since we found no evidence of a greater likelihood of receiving remote instruction in Grade 1).

math and language: these effects average roughly 4–8 percentage points (across individual competencies), over a base of around 50% correct for most competencies (on average).[37]

Second, we express the treatment effect relative to the inter-group differences in this setting. We estimate a progression of $.32\sigma$ for each grade by regressing test scores on grade, district, gender, caste, an asset index, and rurality among lottery losers (see Table A.13).[38] The aggregate treatment effect is roughly 59% of the progress across grades in the control group or 115% of the urban–rural gap.

Our estimates of the aggregate effect compare favorably with other related interventions implemented at scale in similar settings. For instance, in other Indian states, Muralidharan and Sundararaman (2015) find that school vouchers have little aggregate impact on the likelihood of attending private schools after 2 years (and a local average treatment effect effect of 0.23σ after 4 years), while Ganimian et al. (2021) report aggregate treatment effects of 0.29σ after 18 months of doubling personnel at public childcare centers.[39] Globally, Evans and Yuan (2020) review 156 randomized control trials in developing countries and report a median effect size of 0.1σ (0.05σ for large trials with N>5,000). Our effect sizes are roughly at the 80th percentile of all trials in their review and the 90th percentile for large trials.

In summary, quota seats generated substantial learning gains in this sample. While our results are specific to a period of school disruptions, they indicate that the policy served as a substantial safety net for students with quota seats during the COVID-19 school closures.

4.4 Cost effectiveness

We now benchmark the program's cost effectiveness against other interventions, focusing on the Grade 1 sample for this analysis.

The full cost of the policy, including the tax on expensive private schools, is on average INR 8,785 per RTE seat per year in Grade 1. Thus, we see aggregate treatment effects of 0.27σ for a cost of $\sim$ USD 175 for 18 months of treatment, i.e. $\sim 0.15\sigma$ per USD 100. These estimated treatment effects on learning, and therefore their cost effectiveness, should be interpreted in light of having occurred during the COVID-19 pandemic. Our estimates may therefore serve to benchmark the cost effectiveness of

[37] We separate the language results for Hindi, the local language, and English given the specific focus of private schools in teaching English. We included only a few questions in English, focusing mainly on basic tasks, the alphabet, and vocabulary. Appendix C details the full assessment.

[38] If we instead estimate the progression by age, students progress $.31\sigma$ each year. These estimates are similar in magnitude to per-year gains reported in other samples in India and other developing countries with tests catering to a broad range of ability and scored using IRT models (Singh, 2020; Das et al., 2020).

[39] In similar settings, Andrabi et al. (2011) estimate gains of $\sim 0.25\sigma$ from attending private schools in Pakistan, while Romero et al. (2020) report gains of 0.18σ after 1 year of attending public–private partnership schools in Liberia.

numerous programs focused on home-based learning during the pandemic (evaluations of which are expected to be available in the future).

Still, a natural further comparison is with other programs for school-age children in similar settings. During our study period, the quota seats displayed similar cost effectiveness as some of the most promising initiatives in India that have been evaluated with children in preschool and school-entry age under business-as-usual settings. For instance, the policy appears more effective than a contract worker added to public preschool centers, where Ganimian et al. (2021) find effect sizes of 0.29σ at a cost of \$200, or school vouchers in Andhra Pradesh, where Muralidharan and Sundararaman (2015) report (insignificant) gains of 0.016 SD after two years at an average cost of \$45 per child per year, or the home visit program evaluated by Andrew et al. (2020) with treatment effects of 0.19–0.22σ after 18 months at a cost of \$135 per child per year.[40] It is, however, less effective than the group-based early childhood education sessions program evaluated in Grantham-McGregor et al. (2020), which generated effects of $\sim 0.3\sigma$ after two years at an average cost of \$38 per child per year. Ganimian et al. (2021) estimate a present discounted value of INR 124,425 from a 0.29σ increase in test scores. Our treatment effect (0.27σ) is of a similar magnitude, achieved at a cost of ~INR 13,177 over 18 months, providing a rough cost–benefit ratio of 9.44.

The RTE program appears to be more effective than many "default" inputs in education systems beyond India that have been scaled up, including reductions in class sizes using regular civil service teachers, school grants, or unconditional increases in teacher salaries. However, it is substantially less cost effective than targeted pedagogical interventions such as evaluations in primary schooling, which aim to target instruction at appropriate standards for learners (Kremer et al., 2013; Glewwe and Muralidharan, 2016).

5 Conclusions

The RTE quotas are the main policy vehicle used to address educational segregation in Indian primary schools, of which private schools form a substantial share. Private schools are considered more desirable by parents, have demonstrated evidence of positive effects on achievement, and may affect lifetime income and opportunities. In this paper, we have evaluated whether the policy, as implemented in Chhattisgarh, delivers on that promise.

Our results paint a complex picture. Conditioning on the set of applicants, the policy delivers large gains to applicants who receive a free place. Obtaining a seat allows some quota-eligible students to attend preschool and others to attend schools

[40] For both Andrew et al. (2020) and the related evaluation in Grantham-McGregor et al. (2020), we report the scores on language, which is the domain closest to the elements in our tests.

they would not have been able to afford. During the education disruptions caused by the COVID-19 pandemic, quota seats also translated to more educational inputs and better test scores, providing a safety net that fulfils a similar insurance role as other national publicly funded programs in India.[41] The magnitude of the test score gains, and their cost effectiveness, compare well with several interventions targeting preschool-age students, delivered here as part of a nationally scaled-up policy.[42] Based on these metrics, the policy appears to be a success.

Yet, this success is qualified. A free quota seat has a substantial monetary cost: in our sample, the average value of the transfer (the market price) was approximately INR 8,785 per child per year in Grade 1. The entitlement of a quota seat lasts for up to 10 years (two years of preschool and up until Grade 8). Our estimates suggest that approximately 70% of this cost is inframarginal to education choices. The quota is used primarily by households that would send their children to private schools anyway; at least 30% of lottery losers send their child to the *same* school even without a free place.[43] We find meaningful extensive margin effects only at the pre-primary stage, where the quota moves some students from home care/daycare to formal preschool; this stage is not, however, the primary focus of the policy, nor does it account for the bulk of its costs.

The policy, thus, largely acts as a transfer for beneficiaries without achieving the goal of changing the composition of classrooms. The inframarginality of public spending seems to be driven by regressive selection into applications within eligible groups. Since RTE quota seats are undersubscribed in all states (Indus Action, 2019), this inframarginality is likely to be as or more severe in other states.[44] Although perfect targeting is infeasible, this inframarginality would be lower if applicants were more representative of the population of quota-eligible groups. We therefore interpret our results as suggesting that RTE quotas may have substantial *potential* to improve access to private schools for disadvantaged students, but this would require substantially broadening the pool of applicants.

[41]See, for example, Gehrke (2019) on the National Rural Employment Guarantee Scheme, Singh et al. (2014) on the national school feeding program, and Gadenne et al. (2021) on the Public Distribution System for subsidized food.

[42]Treatment effects, in both rich and poor countries for a wide range of interventions, have a well-documented tendency to decline in large government-led implementations compared to researcher-led studies (e.g., Bold et al. (2018); Vivalt (2020); DellaVigna and Linos (2020); Araujo et al. (2021)).

[43]A useful comparison for our results is the evaluation of the PACES voucher scheme by Angrist et al. (2002) in Colombia. Like us, they find modest treatment effects on the extensive margin of private school enrollment ($\sim$15%) accompanied by similar gains of 0.2σ on standardized tests. The PACES program, however, required applicants to have sought and secured admission to a private school before applying. While the RTE quota did not feature this requirement, a similar selection seems to have occurred *de facto*, subverting the explicit policy goal of expanding access to private schools for disadvantaged groups.

[44]Damera (2017) documents the low-extensive-margin effects of winning quota lotteries in Karnataka, which would be consistent with regressive selection into applying.

In contrast to many large public reforms in this setting (see, e.g., Muralidharan and Singh (2020)), undersubscription and inframarginality do not reflect partial policy implementation: Chhattisgarh, as an early (and consistent) implementer of the policy, exhibits above-average implementation of the RTE quota with most design features emphasized by experts.[45] Undersubscription is also unlikely to be explained by low demand: more students from eligible groups attend private schools as fee-paying students than apply for free seats. More generally, private schools continue to cater to a large market of customers willing to pay full price, and parents continue to perceive private schools as attractive options. This contrasts with many development programs in which even substantial price discounts are insufficient to induce take-up by households (see, e.g., Mobarak et al. (2012); Cole et al. (2013)).

Instead, we hypothesize that undersubscription is probably explained by a lack of awareness of the policy, the complexity of the application process, burdensome documentation requirements, and further barriers (such as geographical restrictions) that prevent eligible households from accessing these benefits that have considerable monetary value. Some private schools may also actively dissuade applications under the quota or treat students admitted under the program differently from fee-paying students in the same classroom. These barriers have received little attention at the national and state levels and represent a persistent policy blindspot. In all of these respects, the puzzle of low take-up resembles similar patterns of participation in social programs in the US (see e.g., Currie (2004), Bhargava and Manoli (2015), and Deshpande and Li (2019)). If burdensome applications deter poorer applicants in this setting, there are potentially high returns for interventions that simplify the information and admission procedures or provide application assistance (see, e.g., Bettinger et al. (2012) and Dynarski et al. (2021) in the context of higher education in the US).

RTE quotas in private schools could be a substantial vehicle for social mobility if patterns of take-up can be improved. Future work should explore how this may be achieved. It should also evaluate the policy effects on the social integration of quota-admitted students in classrooms, learning outcomes in "normal" periods of in-classroom instruction, and longer-term outcomes on test scores as well as non-cognitive outcomes and effects on the labor market.

[45] For instance, Muralidharan (2014) recommended creating a comprehensive school list with notified fee levels, including the costs of uniforms and textbooks, eliciting preference orderings from parents, and centralized lottery-based allocation. The policy in Chhattisgarh incorporates all of these features.

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Figures

Figure 1: Timeline

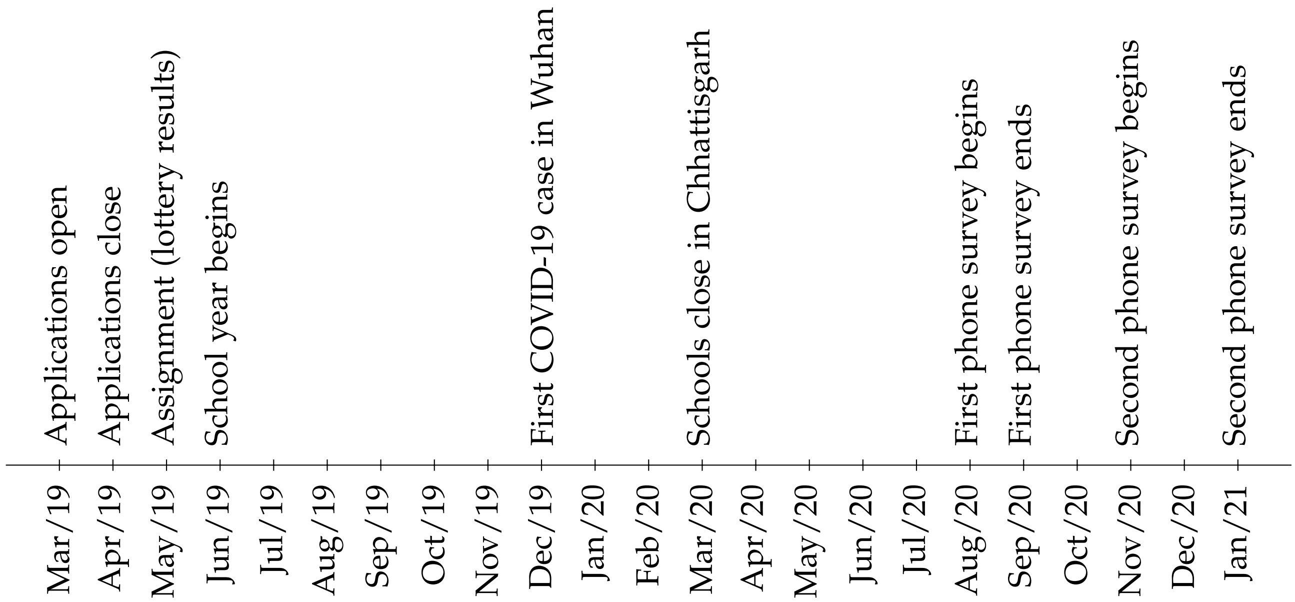
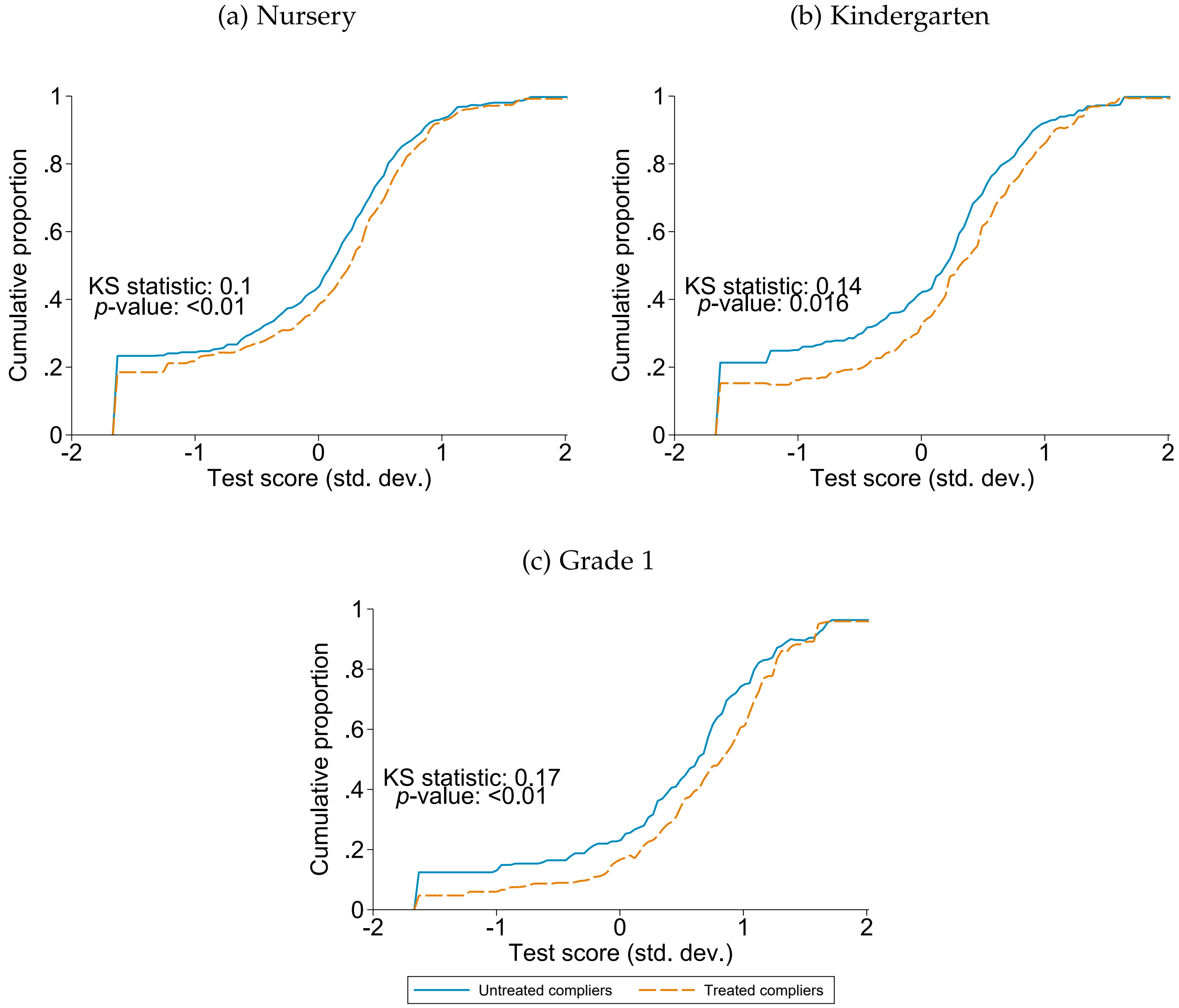


Figure 2: Test score distribution



Note: This figure presents cumulative distribution of the test score distributions for lottery compliers following Abadie (2002) (and the implementation of Abdulkadiroğlu et al. (2018)). Treated cumulative distribution functions (CDFs) are estimated using 2SLS regressions of the interaction of a kernel density function and an RTE seat indicator on the RTE seat indicator, instrumented by a random offer indicator and controlling for risk set dummies. Untreated densities are estimated by replacing the RTE seat indicator with a "no RTE seat" indicator in the 2SLS procedure. All models use a Gaussian kernel and Silverman (2018)'s rule of thumb bandwidth. Kolmogórov-Smirnov (KS) statistics are maximum differences in complier CDFs. p-values are estimated via bootstrap.

Tables

Table 1: Balance across lottery winners and losers

	Admin data		Phone survey #1		Phone survey #2	
	Control mean	Treatment differential	Control mean	Treatment differential	Control mean	Treatment differential
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.48	0.00	0.48	0.01	0.48	0.02
	(0.50)	(0.01)	(0.50)	(0.02)	(0.50)	(0.02)
	[5,388]	[11,024]	[2,371]	[4,682]	[1,326]	[2,702]
Age (Jan 1st, 2019)	4.06	-0.01	3.99	-0.02*	3.97	-0.01
	(0.94)	(0.01)	(0.93)	(0.01)	(0.91)	(0.01)
	[5,388]	[11,024]	[2,371]	[4,682]	[1,326]	[2,702]
Scheduled Caste	0.17	-0.00	0.16	0.01	0.15	0.04**
	(0.38)	(0.01)	(0.36)	(0.01)	(0.35)	(0.02)
	[5,388]	[11,024]	[2,371]	[4,682]	[1,326]	[2,702]
Scheduled Tribe	0.16	-0.00	0.12	-0.00	0.10	-0.00
	(0.37)	(0.01)	(0.32)	(0.01)	(0.30)	(0.01)
	[5,388]	[11,024]	[2,371]	[4,682]	[1,326]	[2,702]
Other Backward Class	0.54	-0.00	0.57	-0.01	0.60	-0.02
	(0.50)	(0.01)	(0.49)	(0.01)	(0.49)	(0.02)
	[5,388]	[11,024]	[2,371]	[4,682]	[1,326]	[2,702]
Rural	0.37	0.00	0.30	0.01	0.29	-0.00
	(0.48)	(0.01)	(0.46)	(0.01)	(0.45)	(0.01)
	[5,388]	[11,024]	[2,371]	[4,682]	[1,326]	[2,702]
Surveyed			0.45	0.02**	0.26	0.03***
			(0.50)	(0.01)	(0.44)	(0.01)
			[5,388]	[11,024]	[5,388]	[11,024]
Allocated a seat			0.18	0.76***	0.18	0.77***
			(0.39)	(0.01)	(0.38)	(0.01)
			[2,315]	[4,644]	[1,305]	[2,686]

Notes: Odd columns report the control (lottery losers) mean, standard deviation of the mean (in parentheses), and number of observations in the control group (in square brackets). Even columns report the treatment effect (difference between lottery winners and losers), the standard error of the effect (in parentheses), and number of observations in the treatment group (in square brackets). Columns 1–2 focus on the full sample. The p-value of the null hypothesis that the differences across all the observable applicant characteristics (Column 2) are jointly zero is .81. Columns 3–4 focus on those who completed the first phone survey. The p-value of the null hypothesis that the differences across all the observable applicant characteristics (Column 4) are jointly zero is .5. Columns 5–6 focus on those who answered our second phone survey. The p-value of the null hypothesis that the differences across all the observable applicant characteristics (Column 6) are jointly zero is .31. All differences control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 2: Effect on the extensive margin of enrollment

	Any school				Private school			
	Control mean (1)	ITT (2)	CCM (3)	LATE (4)	Control mean (5)	ITT (6)	CCM (7)	LATE (8)
All	0.85	0.14***	0.83	0.18***	0.80	0.18***	0.77	0.24***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
		[7,053]		[6,959]		[6,891]		[6,816]
Nursery	0.80	0.19***	0.77	0.25***	0.77	0.21***	0.73	0.28***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
		[3,782]		[3,737]		[3,685]		[3,648]
KG	0.88	0.11***	0.86	0.16***	0.81	0.17***	0.79	0.22***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
		[1,874]		[1,848]		[1,835]		[1,816]
Grade 1	0.97	0.02**	0.97	0.03**	0.90	0.08***	0.88	0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
		[1,397]		[1,374]		[1,371]		[1,352]

Notes: Columns 1 and 5 report the control (lottery losers) mean and the standard error of the mean (in parentheses). Columns 2 and 6 list the itent-to-treat (ITT) effect (difference between lottery winners and losers), the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). Columns 3 and 7 report the control complier mean (CCM) — the mean outcomes for lottery loser compliers — and the standard error of the CCM (in parentheses). Columns 4 and 8 list the local average treatment effect (LATE) of being assigned an RTE seat (instrumented by winning the lottery), the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). All differences control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 3: Heterogeneity on school enrollment LATE

	Any school (19-20)		Private school (19-20)	
	All	Grd 1	All	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Heterogeneity by gender				
Allocated a seat	.17***	.034*	.23***	.13***
	(.015)	(.019)	(.017)	(.034)
Allocated a seat × Female	.021	-.012	.013	-.018
	(.02)	(.019)	(.023)	(.042)
Female	-.012	.02	-.0062	.023
	(.018)	(.018)	(.021)	(.035)
N. of obs.	6,959	1,374	6,816	1,352
CCM	.83	.97	.77	.88
Panel B: Heterogeneity by parental education				
Allocated a seat	.19***	.031**	.25***	.12***
	(.012)	(.015)	(.014)	(.027)
Allocated a seat × Mother HS	-.067**	-.033**	-.1***	-.098*
	(.027)	(.016)	(.031)	(.058)
Mother HS	.062**	.026**	.09***	.077*
	(.025)	(.012)	(.028)	(.04)
N. of obs.	6,773	1,340	6,640	1,318
CCM	.83	.97	.77	.89
Panel C: Heterogeneity by caste				
Allocated a seat	.19***	.038	.22***	.15***
	(.025)	(.026)	(.029)	(.051)
Allocated a seat × OBC	-.02	-.021	-.001	-.09*
	(.027)	(.023)	(.031)	(.051)
Allocated a seat × ST	.013	-.022	.031	-.051
	(.04)	(.029)	(.047)	(.081)
Allocated a seat × SC	.027	.029	.088**	.13
	(.037)	(.055)	(.042)	(.084)
Other Backward Class (OBC)	.011	.018	-.0027	.059
	(.025)	(.021)	(.028)	(.042)
Scheduled Tribe (ST)	-.018	.015	-.034	.029
	(.037)	(.022)	(.043)	(.068)
Scheduled Caste (SC)	-.04	-.035	-.092**	-.12
	(.034)	(.046)	(.039)	(.073)
N. of obs.	6,959	1,374	6,816	1,352
CCM	.83	.97	.77	.88

Notes: This table presents the local average treatment effect (LATE) of being assigned an RTE seat (instrumented by winning the lottery). CCM stands for control complier mean — the mean outcomes for lottery losers compliers. The outcomes in Columns 1–2 relate to whether the child was enrolled in any school in 2019–2020 (=1). The outcomes in Columns 3–4 indicate whether the child was enrolled in a private school in 2019–2020 (=1). Mother HS indicates whether the mother completed high school. Columns 1 and 3 use the full sample, while Columns 2 and 4 use only Grade 1 students. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Table A.6 provides the intent-to-treat (ITT) effect of winning a lottery seat. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 4: Effect on the likelihood of attending different schools

	English medium	% students ST & SC	Facility index	Enrollment	Teachers	PTR
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITT						
Lottery seat	.076**	-1.1	.045	17	.04	2.2
	(.031)	(1.4)	(.047)	(26)	(.58)	(1.7)
N. of obs.	1,344	972	974	920	943	902
Control mean	0.57	27.80	0.74	410.21	13.40	29.88
Control mean | enrolled	0.58	28.78	0.76	426.10	13.88	31.04
% Enrolled (Control)	97.79	96.60	96.60	96.27	96.53	96.27
Panel B: LATE						
Allocated a seat	.11***	-1.3	.052	18	-.055	3.1
	(.042)	(1.9)	(.064)	(35)	(.77)	(2.3)
N. of obs.	1,324	961	963	909	932	891
CCM	0.50	28.53	0.73	416.81	13.82	28.28
CCM | enrolled	0.52	29.74	0.75	438.22	14.48	29.66
% Enrolled (CCM)	97.37	96.08	96.09	95.82	95.85	95.86

Notes: Panel A presents the intent-to-treat (ITT) effects of winning a seat through the lottery on different characteristics of the school the child is enrolled in. The sample is restricted to students applying for seats in Grade 1. Panel B presents the local average treatment effect (LATE) of being allocated an RTE (instrumenting with the outcome of the lottery) on different characteristics of the school the child is enrolled in. CCM denotes the mean outcomes for lottery loser compliers. In Column 1, the outcome is whether the child attends an English medium schools or not. In Column 2, the outcome is the percentage of enrollment taken by Scheduled Castes and Tribes in the school the child attends. In Column 3, the outcome is a principal component analysis (PCA) facility index based on whether the school has computer assisted learning, a homeroom, electricity, a library, a playground, a solid building, a boundary wall, functioning toilets, and solid classrooms. In Columns 4-6 the outcomes are enrollment, number of teachers, and the pupil-teacher ratio (PTR). All columns control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 5: Effect on fees

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Causal effect (LATE)				
Allocated an RTE seat	4,274***	5,803***	2,639***	2,645***
	(302)	(454)	(517)	(562)
CCM	5,263	5,443	4,486	6,012
CCM in private	7,869	9,582	6,087	7,047
% out of school (CCM)	17	23	14	2.6
% in public (CCM)	5.1	3	6.9	8.9
N. of obs.	5,136	2,621	1,481	1,034
Panel B: Decomposition (non-causal)				
Participatory effect (%)	35	46	28	16
Average fee (winners)	9,242	10,950	7,370	8,564
Extensive margin	3,220	5,027	2,036	1,341
Conditional-on-positive effect	1,373	1,368	1,283	1,516
% private school (CCM)	67	56	74	85
Intensive margin	915	765	948	1,284
Total effect	4,135	5,792	2,984	2,625
N. of obs.	5,107	2,606	1,473	1,028

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the local average treatment effects (LATE) of being allocated an RTE (instrumenting with the outcome of the lottery) on the market price of the school a child attends. Table A.7 presents the intent-to-treat (ITT) effect of winning a lottery seat. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). CCM denotes the mean outcomes for lottery loser compliers. Panel B presents a non-causal decomposition of the effect among the extensive and intensive margins. The total effect in Panel B may be different from the effect in Panel A as the sample is slightly different, requiring information on both fees and private school enrollment. Likewise, the participatory effect is different from the effect presented in Table 2 due to sample differences. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 6: Effect on enrollment in top-choice school

	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Lottery seat at first choice	.53***	.6***	.42***	.43***
	(.012)	(.015)	(.024)	(.03)
N. of obs.	7,076	3,782	1,876	1,418
Control mean	0.30	0.25	0.37	0.35
Control mean $\mid$ enrolled	0.34	0.30	0.42	0.35
Control mean $\mid$ enrolled & no RTE seat	0.40	0.39	0.44	0.39
% Enrolled (Control)	87.77	83.10	88.72	97.94
% RTE seat (Control)	28.93	32.13	24.61	26.14

Notes: This table presents the intent-to-treat (ITT) effects of winning a seat in the first-choice school through the lottery on the likelihood of enrolling in this preferred school. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 7: Government expenditure

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Market price				
Allocated an RTE seat	4,274***	5,803***	2,639***	2,645***
	(302)	(454)	(517)	(562)
CCM	5,263	5,443	4,486	6,012
CCM in private	7,869	9,582	6,087	7,047
% out of school (CCM)	17	23	14	2.6
% in public (CCM)	5.1	3	6.9	8.9
N. of obs.	5,136	2,621	1,481	1,034
Panel B: Reimbursed fee				
Allocated a seat	5,941***	6,496***	4,956***	5,621***
	(54)	(61)	(112)	(131)
N. of obs.	5,895	3,068	1,624	1,203
Panel C: Non-limit reimbursed fee				
Allocated a seat	9,740***	11,270***	7,076***	8,785***
	(215)	(277)	(453)	(435)
N. of obs.	5,895	3,068	1,624	1,203

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the local average treatment effects (LATE) of being allocated an RTE (instrumenting with the outcome of the lottery) on the market price of the school a child attends. Panel B presents the LATE of being allocated an RTE (instrumenting with the outcome of the lottery) on the reimbursed fee (set to zero for children without an RTE seat). Panel C presents the LATE of being allocated an RTE (instrumenting with the outcome of the lottery) on the hypothetical reimbursed fee in the absence of the maximum reimbursement limit (set to zero for children without an RTE seat). All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). CCM denotes the mean outcomes for lottery loser compliers. Table A.14 presents the intent-to-treat (ITT) estimates of winning a lottery seat. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 8: Differences between applicants and average households in Chhattisgarh

	Stochastic Applicants	Deterministic Applicants	All Applicants	NFHS	(2)-(1)	(4)-(3)	(4)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full sample							
Asset index	1.98	1.88	1.90	0.15	-0.10***	-1.75***	-1.83***
Mother education: above primary	0.81	0.74	0.76	0.46	-0.07***	-0.30***	-0.35***
Father education: above primary	0.87	0.85	0.86	0.63	-0.02	-0.23***	-0.24***
Panel B: Scheduled Caste							
Asset index	1.95	1.69	1.74	0.26	-0.26***	-1.48***	-1.69***
Mother education: above primary	0.80	0.78	0.79	0.49	-0.02	-0.29***	-0.31***
Father education: above primary	0.89	0.82	0.83	0.68	-0.07**	-0.15***	-0.21***
Panel C: Scheduled Tribe							
Asset index	1.83	1.84	1.84	-0.77	0.01	-2.61***	-2.60***
Mother education: above primary	0.72	0.66	0.67	0.31	-0.06	-0.36***	-0.41***
Father education: above primary	0.80	0.90	0.88	0.48	0.10***	-0.40***	-0.32***
Panel D: Other Backward Class							
Asset index	2.03	2.00	2.01	0.55	-0.03	-1.46***	-1.48***
Mother education: above primary	0.83	0.75	0.77	0.52	-0.08***	-0.26***	-0.31***
Father education: above primary	0.88	0.84	0.86	0.68	-0.04**	-0.18***	-0.20***

Notes: This table shows the prevalence of different characteristics for applicant households in our main sample (Column 1), a sample of applicants with no variation in the schools they are assigned to (Column 2), all applicants (a weighted average of Columns 1 and 2, in Column 3), and households in the NFHS sample (Column 4). It also shows the difference between the samples and whether this difference is statistically significant (Columns 5–7). Panel A uses the entire sample, Panel B focuses on Scheduled Caste households, Panel C focuses on Scheduled Tribe households, and Panel D on Other Backward Caste households. We re-weight our sample to account for differential non-response by household characteristics. We estimate the probability of responding to our survey using a linear probability model that accounts for the household district, caste, and the child's age and gender. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 9: Effect on school enrollment during the COVID-19 pandemic

	Any school	Private school
	(1)	(2)
Panel A: ITT		
Lottery seat	.019**	.072***
	(.0094)	(.018)
COVID	-.035***	-.073***
	(.011)	(.014)
Lottery seat × COVID	.017	.049***
	(.012)	(.015)
N. of obs.	3,060	2,926
Control mean (2019-2020)	0.977	0.906
COVID + COVID × Lottery seat	-0.018	-0.024
p-value(H_0:COVID+COVID×Lottery seat= 0)	.00045	.000099
Panel B: LATE		
Allocated an RTE seat	.026**	.1***
	(.013)	(.025)
COVID	-.04***	-.088***
	(.014)	(.018)
Allocated an RTE seat × COVID	.025	.069***
	(.017)	(.021)
N. of obs.	3,018	2,892
CCM (2019-2020)	0.974	0.883
COVID + COVID × Allocated an RTE seat	-0.015	-0.019
p-value(H_0:COVID+COVID×Allocated an RTE seat= 0)	.0062	.0052

Notes: This table estimates difference-in-differences models of the effect of an RTE place before and after the COVID-19 pandemic began. The data includes outcomes for the 2019–2020 academic year (pre COVID-19) and for the 2020–2021 academic year (post COVID-19). Panel A contains the intent-to-treat (ITT) effect of winning a lottery seat. Panel B includes the local average treatment effect (LATE) of being allocated a seat (instrumented by the outcome of the lottery). COVID is a dummy equal to 1 for the 2020–2021 academic year. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table 10: Effect on instructional inputs

	All		Nursery		Kindergarten		Grade 1	
	CCM	LATE	CCM	LATE	CCM	LATE	CCM	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is open	0.06	0.00	0.05	0.00	0.04	0.05*	0.11	-0.05
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)
		[3,960]		[2,159]		[1,065]		[736]
School: academic support	0.40	0.13***	0.38	0.17***	0.36	0.13**	0.50	-0.00
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.07)
		[3,863]		[2,116]		[1,034]		[713]
Lectures last month	0.37	0.23***	0.36	0.27***	0.29	0.27***	0.52	0.03
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.05)	(0.05)	(0.06)
		[3,812]		[2,089]		[1,023]		[700]
Video lectures	0.27	0.22***	0.25	0.28***	0.22	0.23***	0.42	-0.01
	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.05)	(0.07)
		[3,106]		[1,689]		[879]		[538]
Audio lectures	0.13	0.13***	0.13	0.15***	0.08	0.16***	0.20	0.01
	(0.01)	(0.03)	(0.02)	(0.04)	(0.02)	(0.04)	(0.04)	(0.07)
		[2,218]		[1,155]		[691]		[372]
HH: academic support	0.82	-0.00	0.83	-0.01	0.81	0.00	0.79	0.00
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)
		[3,873]		[2,123]		[1,035]		[715]
Tutor: academic support	0.14	-0.00	0.12	0.01	0.18	-0.08**	0.12	0.07
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.05)
		[3,877]		[2,127]		[1,033]		[717]

Notes: Odd columns contain the control complier mean (CCM) — the mean outcomes for lottery loser compliers — and the standard error of the CCM (in parentheses). Even columns report the local average treatment effect (LATE) of being assigned an RTE seat (instrumented by winning the lottery), the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). Columns 1–2 focus on the full sample, Columns 3–4 on Nursery students, Columns 5–6 on Kindergarten (KG) students, and Columns 7–8 on Grade 1 students. All differences control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

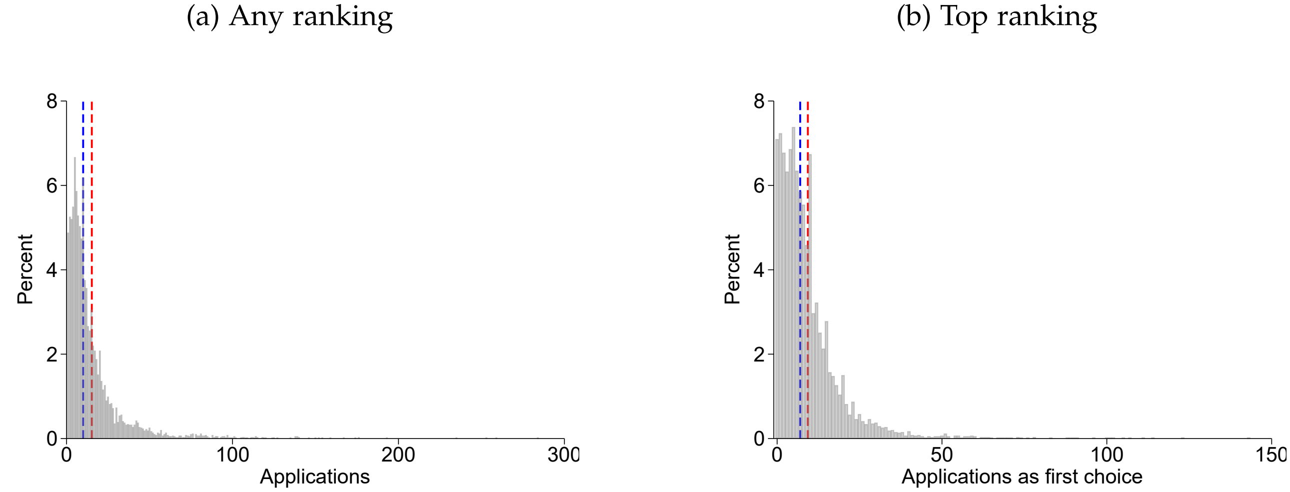
Table 11: Effect on test scores

	All		Nursery		Kindergarten		Grade 1	
	CCM	LATE	CCM	LATE	CCM	LATE	CCM	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall								
IRT score	-0.05	0.19***	-0.19	0.14**	-0.13	0.25**	0.39	0.27**
	(0.03)	(0.05)	(0.05)	(0.07)	(0.07)	(0.10)	(0.09)	(0.13)
% correct	47.65	6.45***	43.08	4.93**	45.52	8.46**	62.41	8.32*
	(1.10)	(1.68)	(1.57)	(2.16)	(2.35)	(3.37)	(2.96)	(4.24)
Panel B: Numeracy								
Counting (%)	68.20	6.40***	64.35	4.52	66.58	9.90**	79.83	7.15
	(1.40)	(2.14)	(2.12)	(2.87)	(2.99)	(4.25)	(3.44)	(4.84)
Number comparison (%)	47.86	7.13***	43.12	5.63*	46.39	10.20**	61.58	7.22
	(1.45)	(2.24)	(2.11)	(2.92)	(3.04)	(4.48)	(3.74)	(5.55)
Addition (%)	45.30	4.65**	38.78	2.57	44.53	8.15*	66.34	6.05
	(1.45)	(2.22)	(2.09)	(2.88)	(3.12)	(4.48)	(3.76)	(5.45)
Subtraction (%)	45.07	7.17***	39.04	4.21	44.33	8.87**	60.55	14.33***
	(1.39)	(2.16)	(2.01)	(2.80)	(2.99)	(4.38)	(3.66)	(5.20)
% math	51.49	6.42***	45.79	4.61**	50.67	9.07**	67.73	8.24*
	(1.19)	(1.81)	(1.70)	(2.33)	(2.59)	(3.69)	(3.13)	(4.51)
Panel C: Hindi								
Letters (%)	35.80	6.59***	32.75	5.21**	33.98	8.10**	47.30	8.81*
	(1.24)	(1.96)	(1.79)	(2.52)	(2.58)	(3.84)	(3.51)	(5.16)
Vocabulary (%)	59.94	6.46***	55.97	3.67	57.73	10.31**	72.93	9.66*
	(1.46)	(2.24)	(2.19)	(2.95)	(3.25)	(4.61)	(3.51)	(5.06)
Sentences (%)	49.08	8.18***	43.40	6.22**	47.11	10.85**	64.14	10.49*
	(1.47)	(2.28)	(2.10)	(2.96)	(3.17)	(4.62)	(3.95)	(5.62)
Listening (%)	50.41	6.09***	47.26	3.79	46.82	10.78**	62.55	6.33
	(1.37)	(2.15)	(2.00)	(2.82)	(2.93)	(4.23)	(3.62)	(5.28)
% Hindi	47.36	6.81***	43.50	4.78**	45.03	9.80***	60.13	8.82**
	(1.15)	(1.77)	(1.66)	(2.30)	(2.45)	(3.55)	(3.08)	(4.40)
Panel D: English								
Vocabulary (%)	29.50	5.75***	26.04	6.04***	24.71	4.29	45.09	7.09
	(1.07)	(1.64)	(1.47)	(2.08)	(2.08)	(3.15)	(3.51)	(4.68)
% English	40.51	5.88***	36.90	5.85**	36.11	4.85	55.90	7.58
	(1.16)	(1.79)	(1.65)	(2.33)	(2.35)	(3.46)	(3.38)	(4.64)
Number of obs	3,991		2,176		1,077		738	

Notes: Odd columns report the control complier mean (CCM) — the mean outcomes for lottery loser compliers — and the standard error of the CCM (in parentheses). Even columns contain the local average treatment effect (LATE of being assigned an RTE seat (instrumented by winning the lottery) and the standard error of the effect (in parentheses). Columns 1–2 focus on the full sample, Columns 3–4 on Nursery students, Columns 5–6 on Kindergarten (KG) students, and Columns 7–8 on Grade 1 students. All differences control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Table A.10 presents the intent-to-treat (ITT) effect of winning a lottery seat. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

A Additional tables and figures

Figure A.1: Applications per school

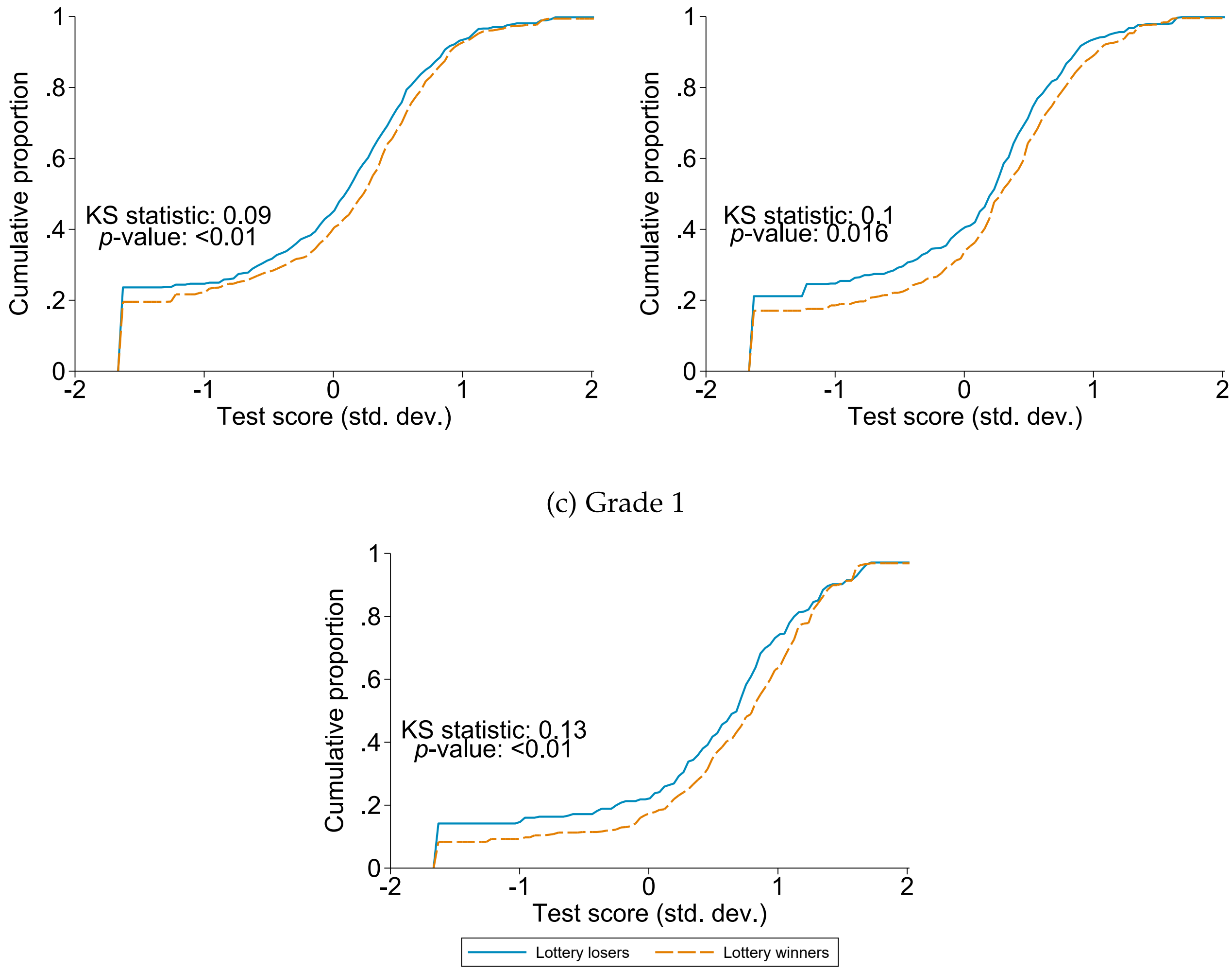


Note: The left panel (Figure A.1a) presents the distribution of the number of times a school is ranked by parents in their preference list. The average (median) school is ranked in 15 (10) applications. The right panel (Figure A.1b) presents the distribution of the number of times a school is ranked first by parents in their preference list. The average (median) school is ranked first in 9.3 (7) applications.

Figure A.2: Test score distribution

(a) Nursery (b) Kindergarten

(c) Grade 1



Note: This figure presents the cumulative distribution of the test score distribution by lottery outcome following Abadie (2002) (and the implementation of Abdulkadiroğlu et al. (2018)). Treated CDFs are estimated using regressions of the interaction of a kernel density function and a lottery winning indicator on the lottery winning indicator and control for risk set dummies. Untreated densities are estimated by replacing the indicator with a lottery losing indicator. All models use a Gaussian kernel and Silverman (2018)'s rule of thumb bandwidth. KS statistics are maximum differences in CDFs. p-values are estimated via bootstrap.

Table A.1: Summary statistics

	Mean (1)	Median (2)	Std. Dev. (3)	Min (4)	Max (5)	N. of obs. (6)
Panel A: All applicants						
Unalloted	0.12	0.00	0.33	0	1	54,676
Rural	0.56	1.00	0.50	0	1	54,676
Age (Jan 1st, 2019)	4.16	3.97	0.96	2.8	6.3	54,676
Female	0.48	0.00	0.50	0	1	54,676
Scheduled Caste	0.23	0.00	0.42	0	1	54,676
Scheduled Tribe	0.21	0.00	0.40	0	1	54,676
Other Backward Class	0.47	0.00	0.50	0	1	54,676
No. of preferences	1.63	1.00	1.35	1	26	54,676
Panel B: Applicants in phone survey						
Unalloted	0.33	0.00	0.47	0	1	16,703
Rural	0.43	0.00	0.49	0	1	16,703
Age (Jan 1st, 2019)	4.03	3.79	0.94	2.8	6.3	16,703
Female	0.48	0.00	0.50	0	1	16,703
Scheduled Caste	0.18	0.00	0.39	0	1	16,703
Scheduled Tribe	0.18	0.00	0.39	0	1	16,703
Other Backward Class	0.52	1.00	0.50	0	1	16,703
No. of preferences	1.63	1.00	1.21	1	12	16,703
Panel C: Schools						
Seats	10.07	9.14	6.37	1	80	5,863
No. applicants	15.21	10.00	19.01	1	284	5,863
Has nursery seats	0.52	1.00	0.50	0	1	5,863
Has KG seats	0.35	0.00	0.48	0	1	5,863
Has Grade 1 seats	0.47	0.00	0.50	0	1	5,863
Hindi medium	0.52	1.00	0.50	0	1	5,863
English medium	0.41	0.00	0.49	0	1	5,863

Notes: This table presents summary statistics for all lottery applicants (Panel A) and for applicants we attempted to contact during our phone survey (Panel B). Further, we present summary statistics from schools in the lottery (Panel C).

Table A.2: Application behavior by household characteristics

	(1)	(2)	(3)	(4)	(5)
Panel A: Applies to more than one school					
Age (Jan 1st, 2019)	-.02***	-.031***	-.03***	-.029**	-.029**
	(.0032)	(.01)	(.01)	(.013)	(.013)
Female	-.0012	-.0069	-.0073	-.019	-.019
	(.0029)	(.0092)	(.0092)	(.013)	(.013)
Scheduled Caste	-.018**	-.013	-.012	-.038	-.039
	(.0091)	(.023)	(.023)	(.03)	(.03)
Scheduled Tribe	-.022**	.0071	.0098	.012	.012
	(.0093)	(.022)	(.022)	(.03)	(.031)
Other Backward Class	-.015*	.0043	.0053	-.017	-.017
	(.0082)	(.018)	(.018)	(.023)	(.023)
Mother: Education>Primary			.025*	.024	.025
			(.014)	(.02)	(.02)
Asset Index					-.00092
					(.0045)
Outcome mean	.29	.39	.39	.41	.41
N. of obs.	53,679	6,625	6,625	3,574	3,574
Panel B: Market price of first choice					
Age (Jan 1st, 2019)	46	-51	-30	64	62
	(157)	(756)	(760)	(1,058)	(1,055)
Female	-55	-75	-92	-608	-604
	(135)	(237)	(239)	(594)	(588)
Scheduled Caste	-1,132	-1,707	-1,664	-202	-218
	(702)	(1,484)	(1,472)	(575)	(570)
Scheduled Tribe	-1,645*	-4,497	-4,379	-4,927	-4,945
	(865)	(3,406)	(3,370)	(4,041)	(4,084)
Other Backward Class	-1,286	-2,988	-2,950	-2,098	-2,102
	(840)	(2,441)	(2,429)	(1,601)	(1,611)
Mother: Education>Primary			968**	1,432*	1,456*
			(392)	(751)	(807)
Asset Index					-49
					(142)
Outcome mean	8,265	11,147	11,147	11,998	11,998
N. of obs.	45,221	5,739	5,739	3,093	3,093
Sample	Admin	Phone 1	Phone 1	Phone 2	Phone 2

Notes: Fee information comes from administrative data. All regressions control for habitation (school cluster households are allowed to apply to) fixed effects. That is, regressions control for the supply of schools available to parents. Panel A has as the outcome whether more than one school was ranked in the application. Panel B contains the market price of the first choice. Column 1 contains the full set of applicants. Columns 2 and 3 restrict the sample to those who answered our first phone survey (when we asked about parental education). Columns 4 and 5 restrict the sample to our second phone survey (when we asked about assets). Standard errors are clustered at the habitation level. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.3: Attrition by child characteristics

	Survey #1	Survey #2
	(1)	(2)
Female	.0034	.0053
	(.0078)	(.0069)
Age (Jan 1st, 2019)	-.013	-.006
	(.012)	(.01)
Scheduled Caste	-.047***	-.027*
	(.016)	(.014)
Scheduled Tribe	-.12***	-.084***
	(.016)	(.014)
Other Backward Class	-.018	.0044
	(.014)	(.012)
Rural	-.066***	-.069***
	(.012)	(.011)
N. of obs.	16,412	16,412
Outcome mean	.44	.26

Notes: The outcome is whether we were able to conduct the interview (=1). All columns control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.4: Compliance

	RTE seat			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Allocated a seat	.76***	.77***	.75***	.73***
	(.01)	(.013)	(.021)	(.025)
N. of obs.	6,959	3,737	1,848	1,374
Control mean	0.18	0.18	0.17	0.19

Notes: This table presents the effect of winning a lottery seat on being allotted an RTE seat. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.5: Effect on the extensive margin of enrollment, controlling for the probability of being assigned to a private school: Lee bounds and stratas with low attrition

	Strata without attrition		Low attrition strata			Lee bounds	
	ITT	LATE	Differential attrition	ITT	LATE	ITT	
						LB	UB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All grades							
Private school (19-20)	0.19***	0.24***	0.02	0.17***	0.23***	0.08	0.28
	(0.05)	(0.07)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
	[211]	[207]	[10,086]	[4,244]	[4,204]	[3,748]	[3,748]
Any school (19-20)	0.14***	0.19***	0.02	0.14***	0.18***	0.07	0.21
	(0.05)	(0.06)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
	[140]	[139]	[10,086]	[4,345]	[4,291]	[3,810]	[3,810]
Panel B: Nursery							
Private school (19-20)	0.26***	0.33***	0.01	0.22***	0.28***	0.12	0.33
	(0.08)	(0.11)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
	[81]	[81]	[4,888]	[2,318]	[2,300]	[2,121]	[2,121]
Any school (19-20)	0.21***	0.28***	0.01	0.19***	0.25***	0.10	0.29
	(0.07)	(0.10)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
	[82]	[81]	[4,888]	[2,381]	[2,357]	[2,161]	[2,161]
Panel C: Kindergarten							
Private school (19-20)	0.13	0.15	0.01	0.15***	0.20***	0.05	0.32
	(0.11)	(0.13)	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)
	[17]	[17]	[3,047]	[1,220]	[1,209]	[957]	[957]
Any school (19-20)	0.13	0.15	0.01	0.11***	0.14***	0.05	0.22
	(0.11)	(0.13)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
	[17]	[17]	[3,047]	[1,244]	[1,229]	[971]	[971]
Panel D: Grade 1							
Private school (19-20)	0.07	0.11	0.02	0.08***	0.12***	0.03	0.13
	(0.07)	(0.10)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
	[39]	[39]	[2,151]	[750]	[743]	[670]	[670]
Any school (19-20)	0.02	0.03	0.02	0.03**	0.04**	0.01	0.02
	(0.03)	(0.05)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
	[41]	[41]	[2,151]	[768]	[757]	[678]	[678]

Notes: Columns 1–2 display the results restricting the sample to strata without attrition. Column 1 shows the intention-to-treat (ITT) effect of winning the lottery, and Column 2 the local average treatment effect (LATE) of being assigned an RTE seat (instrumented with winning the lottery). Columns 3–5 report the results after dropping the 25% of the strata with the most differential attrition. Column 3 shows the results of differential attrition, Column 4 the ITT effect, and Column 5 the LATE of being assigned an RTE seat. Columns 6–7 show Lee (2009) style bounds — Column 6 has the lower bound (LB), while Column 7 has the upper bound for (UB) — for the ITT effect of winning the lottery. Standard errors are in parentheses. The number of observations in the treatment effects estimates is in square brackets. All treatment estimates control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.6: Heterogeneity on school enrollment ITT, controlling for the probability of being assigned to a private school

	Any school (19–20)		Private school (19–20)	
	All	Grd 1	All	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Heterogeneity by gender				
Lottery seat	.13***	.023*	.17***	.087***
	(.011)	(.013)	(.013)	(.023)
Female	-.0043	.015	.0021	.015
	(.015)	(.013)	(.017)	(.027)
Lottery seat × Female	.014	-.0062	.0057	-.008
	(.015)	(.013)	(.018)	(.03)
N. of obs.	7,053	1,397	6,891	1,371
Control mean	.86	.98	.81	.91
Panel B: Heterogeneity by parental education				
Lottery seat	.15***	.022**	.19***	.087***
	(.0093)	(.011)	(.011)	(.019)
Mother HS	.051**	.019**	.072***	.056*
	(.02)	(.009)	(.022)	(.029)
Lottery seat × Mother HS	-.053**	-.024**	-.08***	-.071*
	(.021)	(.011)	(.024)	(.04)
N. of obs.	6,854	1,361	6,710	1,337
Control mean	.86	.98	.81	.91
Panel C: Heterogeneity by caste				
Lottery seat	.15***	.024	.17***	.097***
	(.02)	(.016)	(.023)	(.032)
Other Backward Class (OBC)	.011	.012	.0048	.036
	(.021)	(.015)	(.024)	(.031)
Scheduled Tribe (ST)	-.0083	.0086	-.02	.016
	(.029)	(.016)	(.034)	(.05)
Scheduled Caste (SC)	-.03	-.033	-.069**	-.11*
	(.028)	(.036)	(.032)	(.056)
Lottery seat × OBC	-.02	-.012	-.01	-.054
	(.022)	(.015)	(.025)	(.034)
Lottery seat × ST	-.0027	-.013	.0069	-.032
	(.03)	(.019)	(.035)	(.055)
Lottery seat × SC	.013	.025	.057*	.11*
	(.028)	(.039)	(.033)	(.059)
N. of obs.	7,053	1,397	6,891	1,371
Control mean	.86	.98	.81	.91

Notes: This tables presents the intent-to-treat (ITT) estimates of being assigned a seat by winning the lottery. The outcome in Columns 1–2 is whether the child was enrolled in any school in 2019–2020 (=1). The outcome in Columns 3–4 is whether the child was enrolled in a private school in 2019–2020 (=1). Mother HS indicates whether the mother completed high school. Columns 1 and 3 use the full sample, while Columns 2 and 4 use only Grade 1 students. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.7: Intent-to-treat effect of winning the lottery on fees

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Causal effect (ITT)				
Lottery seat	3,406***	4,817***	2,023***	1,980***
	(237)	(366)	(396)	(424)
Control mean	5,454	5,415	4,707	6,627
Control mean in private	7,766	9,155	6,265	7,749
% out of school (control)	22	36	16	3.4
% in public (control)	7.3	4.6	8.8	11
N. of obs.	5,184	2,639	1,499	1,046
Panel B: Decomposition (non-causal)				
Participatory effect (%)	27	38	21	12
Average fee (winners)	8,877	10,546	7,096	8,230
Extensive margin	2,435	3,956	1,487	967
Conditional-on-positive effect	1,111	1,182	999	1,135
% private school (control)	70	60	76	85
Intensive margin	776	709	759	970
Total effect	3,210	4,665	2,245	1,937
N. of obs.	5,154	2,625	1,490	1,039

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the intent-to-treat (ITT) effects of winning a seat through the lottery on the market price of the school a child attends. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Panel B presents a non-causal decomposition of the effect among the extensive and intensive margins. The total effect in Panel B may be different from that in Panel A as the sample is slightly different, requiring information on both fees and private school enrollment. Likewise, the participatory effect is different from the effect presented in Table 2 due to sample differences. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.8: Effect of winning different lottery seats on enrollment in the top-choice school

	(1)	(2)	(3)	(4)	(5)	(6)
Won lottery	.41***					
	(.013)					
Won seat in first choice		.53***	.65***	.63***	.67***	.65***
		(.012)	(.019)	(.021)	(.029)	(.031)
Won seat in second choice				-.13***	-.15***	-.16***
				(.03)	(.041)	(.043)
Won seat in third choice						-.15***
						(.047)
N. of obs.	7,076	7,076	2,477	2,477	1,146	1,146
Sample	Full	Full	≥ 2 choices	≥ 2 choices	≥ 3 choices	≥ 3 choices

Notes: This table presents the effect of winning different lottery seats on the likelihood of enrolling in the top-choice school. All columns control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.9: Effect on the diversity of the student body

	% SC (1)	% ST (2)	% SC+ST (3)
Panel A: ITT			
Lottery seat	.64	-.3	.34
	(1.9)	(1.5)	(2.4)
Scheduled Tribe	-2.8	18***	15***
	(2)	(4.2)	(4.1)
Scheduled Caste	7.4***	-2.9	4.6
	(2.3)	(1.8)	(2.8)
Other Backward Class	1	1.1	2.2
	(1.7)	(1.5)	(2.3)
Lottery seat × Scheduled Tribe	.53	-2.3	-1.8
	(2.3)	(4.7)	(4.6)
Lottery seat × Scheduled Caste	.95	.72	1.7
	(2.6)	(2.3)	(3.2)
Lottery seat × Other Backward Class	-1.1	-2.1	-3.2
	(1.9)	(2)	(2.7)
N. of obs.	972	972	972
Control mean	12.54	15.26	27.80
Control mean | enrolled	12.98	15.80	28.78
% Enrolled (Control)	96.60	96.60	96.60
Panel B: LATE			
Allocated a seat	.98	.04	1
	(2.6)	(2.1)	(3.4)
Allocated a seat × Scheduled Caste	1.2	.77	2
	(3.6)	(3.1)	(4.5)
Allocated a seat × Scheduled Tribe	1.1	-5.4	-4.3
	(3.3)	(6.9)	(6.7)
Allocated a seat × Other Backward Class	-1.3	-3.2	-4.5
	(2.6)	(2.7)	(3.7)
Scheduled Caste	7.2**	-2.7	4.5
	(3)	(2.3)	(3.6)
Scheduled Tribe	-3.3	20***	17***
	(2.8)	(5.7)	(5.5)
Other Backward Class	1.2	2	3.2
	(2.2)	(1.9)	(3)
N. of obs.	961	961	961
CCM	12.52	16.01	28.53
CCM | enrolled	13.07	16.67	29.74
% Enrolled (CCM)	96.08	96.08	96.08

Notes: Panel A presents the intent-to-treat (ITT) effects of winning a seat through the lottery on the proportion of students from Scheduled Castes (SC) and Scheduled Tribes (ST). Panel B presents the local average treatment effect (LATE) of being allocated an RTE (instrumenting with the outcome of the lottery) on the proportion of students from SC and ST. CCM denotes the mean outcomes for lottery loser compliers. All columns control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.10: Intent-to-treat effect of winning the lottery on test scores

	All		Nursery		Kindergarten		Grade 1	
	Control mean	ITT	Control mean	ITT	Control mean	ITT	Control mean	ITT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IRT score	-0.05	0.15***	-0.19	0.12**	-0.13	0.18**	0.39	0.20**
	(0.03)	(0.04)	(0.04)	(0.05)	(0.06)	(0.07)	(0.08)	(0.10)
% correct	47.91	5.08***	43.09	4.21**	45.86	6.04**	63.08	6.25**
	(0.94)	(1.28)	(1.34)	(1.70)	(1.91)	(2.46)	(2.44)	(3.17)

Notes: Odd columns report the control mean and the standard error of the control mean (in parentheses). Even columns include the intent-to-treat (ITT) effect of winning an RTE seat through the lottery, the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). Columns 1–2 focus on the full sample, Columns 3–4 focus on Nursery students, Columns 5–6 on Kindergarten (KG) students, and Columns 7–8 on Grade 1 students. All differences control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.11: Heterogeneity in learning outcomes, by child characteristics

	(1) Female	(2) Age	(3) SC	(4) ST	(5) OBC	(6) Rural
Panel A: ITT						
Lottery seat	.13**	.096	.15***	.16***	.1*	.17***
	(.052)	(.17)	(.043)	(.041)	(.058)	(.047)
Covariate	.022	.23***	.053	-.046	-.1*	-.17**
	(.058)	(.057)	(.085)	(.092)	(.06)	(.082)
Lottery seat × Covariate	.033	.014	-.026	-.12	.071	-.054
	(.071)	(.044)	(.1)	(.11)	(.073)	(.083)
N. of obs.	4,028	4,028	4,028	4,028	4,028	4,028
Panel B: LATE						
Allocated a seat	.17***	.077	.19***	.2***	.13*	.21***
	(.066)	(.23)	(.056)	(.054)	(.076)	(.061)
Allocated a seat × Covariate	.036	.029	-.037	-.13	.091	-.05
	(.091)	(.059)	(.13)	(.15)	(.095)	(.11)
Covariate	.021	.22***	.067	-.043	-.12	-.17*
	(.073)	(.064)	(.11)	(.12)	(.076)	(.099)
N. of obs.	3,991	3,991	3,991	3,991	3,991	3,991

Notes: The outcome is the child's IRT score. Panel A presents the intent-to-treat (ITT) effects of winning a seat through the lottery. Panel B presents the local average treatment effect (LATE) of being allocated an RTE (instrumenting with the outcome of the lottery). CCM denotes the mean outcomes for lottery loser compliers. Each column displays the interaction of a different covariate with the treatment variable. All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.12: Learning outcomes controlling for the probability of being assigned to a private school: Lee bounds and stratas with low attrition

	Strata without attrition		Low attrition strata			Lee bounds	
	ITT	LATE	Differential attrition	ITT	LATE	ITT	
						LB	UB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.36	0.46	0.01	0.13***	0.18***	-0.00	0.43
	(0.37)	(0.49)	(0.01)	(0.05)	(0.06)	(0.04)	(0.03)
	[64]	[64]	[8,550]	[2,124]	[2,107]	[4,028]	[4,028]
Nursery	0.06	0.09	0.01	0.07	0.08	-0.02	0.44
	(0.60)	(0.83)	(0.01)	(0.06)	(0.08)	(0.05)	(0.05)
	[27]	[27]	[3,752]	[1,077]	[1,071]	[2,196]	[2,196]
KG	1.06	1.29	0.00	0.13	0.18	0.14	0.46
	(0.61)	(0.87)	(0.00)	(0.08)	(0.12)	(0.07)	(0.06)
	[16]	[16]	[2,990]	[692]	[685]	[1,088]	[1,088]
Grade 1	0.02	0.02	0.04	0.35***	0.47***	-0.08	0.37
	(0.65)	(0.79)	(0.04)	(0.11)	(0.15)	(0.10)	(0.09)
	[21]	[21]	[1,807]	[357]	[353]	[744]	[744]

Notes: Columns 1–2 display the results of restricting the sample to strata without attrition. Column 1 shows the intention-to-treat (ITT) effect of winning the lottery, and Column 2 the local average treatment effect (LATE) of being assigned an RTE seat (instrumented with winning the lottery). Columns 3–5 show the results after dropping the 25% of the strata with the most differential attrition. Column 3 shows the results of the differential attrition, Column 4 the ITT effect, and Column 5 reports the LATE of being assigned an RTE place. Columns 6–7 show Lee (2009) style bounds — Column 6 has the lower bound (LB), while Column 7 has the upper bound for (UB) — for the ITT effect of winning the lottery. Standard errors are in parentheses. The number of observations in the treatment effects estimates is in square brackets. All treatment estimates control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.13: Progression in test scores by grade and age

	IRT score	
	(1)	(2)
Class	.32***	
	(.033)	
Age (Jan 1st, 2019)		.31***
		(.028)
Rural	-.16**	-.16**
	(.067)	(.066)
Female	-.00066	.0037
	(.051)	(.051)
Scheduled Caste	-.096	-.1
	(.098)	(.097)
Scheduled Tribe	-.12	-.13
	(.11)	(.11)
Other Backward Class	-.17**	-.16**
	(.074)	(.073)
Asset Index	.081***	.083***
	(.017)	(.017)
N. of obs.	1,380	1,380

Notes: This table presents the results of regressing IRT scores on grade (or age), district, gender, caste, an asset index, and rurality among lottery losers. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.14: Intent-to-treat effect on government expenditure

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Market price				
Lottery seat	3,406***	4,817***	2,023***	1,980***
	(237)	(366)	(396)	(424)
Control mean	5,454	5,415	4,707	6,627
Control mean in private	7,766	9,155	6,265	7,749
% out of school (control)	22	36	16	3.4
% in public (control)	7.3	4.6	8.8	11
N. of obs.	5,184	2,639	1,499	1,046
Panel B: Reimbursed fee				
Lottery seat	4,841***	5,551***	3,802***	4,328***
	(67)	(83)	(123)	(164)
N. of obs.	5,944	3,088	1,641	1,215
Panel C: Non-limit reimbursed fee				
Lottery seat	7,961***	9,665***	5,447***	6,761***
	(188)	(252)	(367)	(381)
N. of obs.	5,944	3,088	1,641	1,215

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the intent to treat (ITT) effects of being allocated an RTE through the lottery on the market price of the school a child attends. Panel B presents the ITT effects of being allocated an RTE through the lottery on the reimbursed fee (set to zero for children without an RTE seat). Panel C presents the ITT effects of being allocated an RTE through the lottery on the hypothetical reimbursed fee in the absence of the maximum reimbursement limit (set to zero for children without an RTE seat). All regressions control for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.15: Differences in assets between applicants and average households in Chhattisgarh

	Stochastic Applicants	Deterministic Applicants	All Applicants	NFHS	(2)-(1)	(4)-(3)	(4)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full sample							
number of rooms used for sleeping	2.83	2.76	2.78	1.94	-0.07	-0.84***	-0.89***
table	0.52	0.40	0.43	0.35	-0.11***	-0.08***	-0.17***
cot or bed	0.91	0.91	0.91	0.93	-0.00	0.02**	0.02***
chair	0.81	0.77	0.78	0.69	-0.04***	-0.09***	-0.11***
has electricity	0.99	0.99	0.99	0.96	-0.00	-0.03***	-0.03***
electric fan	0.93	0.89	0.90	0.73	-0.03***	-0.17***	-0.19***
has television	0.72	0.58	0.62	0.66	-0.14***	0.04***	-0.06***
has refrigerator	0.25	0.19	0.20	0.16	-0.06***	-0.04***	-0.09***
has bicycle	0.69	0.78	0.76	0.75	0.10***	-0.01	0.06***
has motorcycle/scooter	0.52	0.41	0.44	0.39	-0.11***	-0.05***	-0.13***
Panel B: Scheduled Caste							
number of rooms used for sleeping	2.67	2.30	2.38	1.83	-0.37***	-0.55***	-0.84***
table	0.48	0.31	0.34	0.30	-0.17***	-0.04	-0.17***
cot or bed	0.90	0.83	0.85	0.95	-0.07**	0.11***	0.05***
chair	0.76	0.74	0.74	0.69	-0.02	-0.05	-0.07***
has electricity	0.99	0.98	0.98	0.98	-0.01	-0.00	-0.01*
electric fan	0.93	0.87	0.88	0.79	-0.06**	-0.09***	-0.14***
has television	0.72	0.53	0.57	0.77	-0.19***	0.19***	0.04*
has refrigerator	0.24	0.16	0.18	0.12	-0.08**	-0.06*	-0.12***
has bicycle	0.70	0.76	0.75	0.72	0.06*	-0.02	0.03
has motorcycle/scooter	0.46	0.35	0.37	0.39	-0.10**	0.02	-0.06**
Panel C: Scheduled Tribe							
number of rooms used for sleeping	2.96	3.13	3.10	1.92	0.17	-1.17***	-1.04***
table	0.48	0.43	0.44	0.23	-0.05	-0.21***	-0.25***
cot or bed	0.87	0.97	0.95	0.87	0.10***	-0.09***	-0.01
chair	0.80	0.72	0.74	0.55	-0.07	-0.18***	-0.24***
has electricity	0.99	0.99	0.99	0.91	0.00	-0.08***	-0.08***
electric fan	0.80	0.80	0.80	0.50	-0.01	-0.30***	-0.30***
has television	0.61	0.41	0.45	0.42	-0.20***	-0.03	-0.18***
has refrigerator	0.15	0.21	0.20	0.06	0.06	-0.14***	-0.09***
has bicycle	0.68	0.84	0.81	0.78	0.17***	-0.03*	0.10***
has motorcycle/scooter	0.53	0.43	0.45	0.25	-0.10*	-0.20***	-0.28***
Panel D: Other Backward Class							
number of rooms used for sleeping	2.88	2.83	2.84	1.96	-0.05	-0.89***	-0.92***
table	0.53	0.40	0.44	0.40	-0.12***	-0.04*	-0.13***
cot or bed	0.93	0.92	0.92	0.96	-0.01	0.04***	0.04***
chair	0.82	0.81	0.81	0.76	-0.01	-0.05***	-0.06***
has electricity	1.00	1.00	1.00	0.98	0.00	-0.02***	-0.01***
electric fan	0.95	0.94	0.94	0.85	-0.02*	-0.09***	-0.11***
has television	0.76	0.67	0.70	0.76	-0.09***	0.06***	0.00
has refrigerator	0.26	0.19	0.21	0.19	-0.07***	-0.02	-0.07***
has bicycle	0.70	0.79	0.76	0.76	0.09***	-0.00	0.06***
has motorcycle/scooter	0.54	0.45	0.48	0.45	-0.09***	-0.03	-0.09***

Notes: This table shows the prevalence of different characteristics for applicant households in our main sample (Column 1), a sample of applicants without any variation in the schools they are assigned to (Column 2), all applicants (a weighted average of Columns 1 and 2, in Column 3), and households in the NFHS sample (Column 4). It also displays the difference between the samples and whether this difference is statistically significant (Columns 5–7). Panel A uses the entire sample, Panel B focuses on Scheduled Caste households, Panel C on Scheduled Tribe households, and Panel D on Other Backward Caste households. We re-weight our sample to account for differential non-response by household characteristics. We estimate the probability of responding to our survey using a linear probability model that accounts for the household district, caste, and the child's age and gender. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.16: Differences in maternal education between applicants and average households in Chhattisgarh

	Stochastic Applicants	Deterministic Applicants	All Applicants	NFHS	(2)-(1)	(4)-(3)	(4)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full sample							
no education	0.08	0.10	0.09	0.29	0.02**	0.20***	0.22***
incomplete primary	0.02	0.04	0.04	0.10	0.02***	0.07***	0.08***
complete primary	0.07	0.11	0.10	0.14	0.04***	0.04***	0.07***
incomplete secondary	0.51	0.50	0.50	0.35	-0.01	-0.15***	-0.16***
complete secondary	0.21	0.17	0.18	0.06	-0.04***	-0.12***	-0.15***
higher	0.10	0.08	0.08	0.05	-0.02**	-0.03***	-0.04***
Panel B: Scheduled Caste							
no education	0.10	0.06	0.07	0.26	-0.04*	0.19***	0.16***
incomplete primary	0.03	0.04	0.03	0.10	0.01	0.07***	0.07***
complete primary	0.05	0.12	0.10	0.14	0.07***	0.04	0.09***
incomplete secondary	0.50	0.56	0.54	0.39	0.06	-0.16***	-0.11***
complete secondary	0.21	0.15	0.16	0.07	-0.06*	-0.09***	-0.14***
higher	0.10	0.07	0.08	0.03	-0.02	-0.05**	-0.06***
Panel C: Scheduled Tribe							
no education	0.11	0.18	0.17	0.44	0.07*	0.27***	0.33***
incomplete primary	0.02	0.03	0.03	0.10	0.01	0.08***	0.08***
complete primary	0.13	0.12	0.12	0.15	-0.01	0.03*	0.02
incomplete secondary	0.44	0.37	0.39	0.26	-0.07	-0.13***	-0.18***
complete secondary	0.22	0.17	0.18	0.03	-0.05	-0.15***	-0.19***
higher	0.06	0.12	0.11	0.02	0.06*	-0.09***	-0.04***
Panel D: Other Backward Class							
no education	0.06	0.08	0.08	0.23	0.02	0.16***	0.17***
incomplete primary	0.03	0.05	0.04	0.11	0.02**	0.07***	0.08***
complete primary	0.07	0.12	0.11	0.14	0.05***	0.03***	0.07***
incomplete secondary	0.53	0.50	0.51	0.40	-0.03	-0.11***	-0.13***
complete secondary	0.21	0.19	0.19	0.06	-0.02	-0.13***	-0.14***
higher	0.09	0.06	0.07	0.06	-0.03***	-0.02	-0.04***

Notes: This table shows the prevalence of different characteristics for applicant households in our main sample (Columns 1), a sample of applicants without any variation in the schools they are assigned to (Column 2), all applicants (a weighted average of Columns 1 and 2, in Column 3), and households in the NFHS sample (Column 4). It also shows the difference between the samples and whether this difference is statistically significant (Columns 5–7). Panel A uses the entire sample, Panel B focuses on Scheduled Caste households, Panel C on Scheduled Tribe households, and Panel D on Other Backward Caste households. We re-weight our sample to account for differential non-response by household characteristics. We estimate the probability of responding to our survey using a linear probability model that accounts for the household district, caste, and the child's age and gender. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.17: Differences in paternal education between applicants and average households in Chhattisgarh

	Stochastic Applicants	Deterministic Applicants	All Applicants	NFHS	(2)-(1)	(4)-(3)	(4)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full sample							
no education	0.03	0.03	0.03	0.17	0.00	0.13***	0.13***
incomplete primary	0.02	0.02	0.02	0.09	0.00	0.07***	0.07***
complete primary	0.06	0.09	0.08	0.11	0.03***	0.03***	0.05***
incomplete secondary	0.50	0.50	0.50	0.44	-0.00	-0.06***	-0.06***
complete secondary	0.22	0.23	0.22	0.09	0.01	-0.13***	-0.13***
higher	0.15	0.13	0.14	0.10	-0.02**	-0.04***	-0.05***
Panel B: Scheduled Caste							
no education	0.04	0.02	0.02	0.14	-0.02*	0.12***	0.11***
incomplete primary	0.03	0.03	0.03	0.07	0.01	0.04***	0.05***
complete primary	0.04	0.13	0.11	0.10	0.09***	-0.01	0.06***
incomplete secondary	0.43	0.39	0.40	0.47	-0.04	0.07*	0.04
complete secondary	0.26	0.32	0.31	0.12	0.07*	-0.19***	-0.14***
higher	0.21	0.10	0.12	0.09	-0.10***	-0.03	-0.11***
Panel C: Scheduled Tribe							
no education	0.07	0.07	0.07	0.27	-0.00	0.20***	0.20***
incomplete primary	0.01	0.02	0.02	0.11	0.01	0.10***	0.10***
complete primary	0.09	0.02	0.03	0.13	-0.08***	0.10***	0.04***
incomplete secondary	0.50	0.54	0.53	0.37	0.04	-0.16***	-0.13***
complete secondary	0.20	0.18	0.19	0.06	-0.01	-0.13***	-0.14***
higher	0.11	0.18	0.17	0.05	0.07*	-0.11***	-0.05***
Panel D: Other Backward Class							
no education	0.02	0.03	0.03	0.11	0.01	0.08***	0.09***
incomplete primary	0.02	0.02	0.02	0.10	-0.00	0.08***	0.08***
complete primary	0.06	0.10	0.09	0.11	0.04***	0.01	0.04***
incomplete secondary	0.52	0.54	0.53	0.48	0.01	-0.05**	-0.04***
complete secondary	0.22	0.19	0.20	0.10	-0.03*	-0.10***	-0.12***
higher	0.14	0.12	0.13	0.10	-0.02	-0.03*	-0.04***

Notes: This table shows the prevalence of different characteristics for applicant households in our main sample (Columns 1), a sample of applicants without any variation in the schools they are assigned to (Column 2), all applicants (a weighted average of Columns 1 and 2, in Column 3), and households in the NFHS sample (Columns 4). It also shows the difference between the samples and whether this difference is statistically significant (Columns 5–7). Panel A uses the entire sample, Panel B focuses on Scheduled Caste households, Panel C on Scheduled Tribe households, and Panel D on Other Backward Caste households. We re-weight our sample to account for differential non-response by household characteristics. We estimate the probability of responding to our survey using a linear probability model that accounts for the household district, caste, and child's age and gender. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.18: Proportion of Scheduled Caste and Scheduled Tribe students in public and private schools under different scenarios

	Private mean (1)	Public mean (2)	Difference (3)	Difference Block F.E. (4)	Difference Pincode F.E. (5)
% (SC+ST)	29.17	57.03	27.86***	19.92***	19.87***
	(22.37)	(33.06)	(0.45)	(0.39)	(0.39)
	[5,731]	[30,499]			
% (SC+ST-Used Seats)	24.04	57.89	33.85***	26.39***	26.51***
	(23.73)	(32.78)	(0.47)	(0.42)	(0.42)
	[5,718]	[30,496]			
% (SC+ST+Used Seats)	33.75	55.22	21.47***	12.49***	11.97***
	(22.31)	(33.97)	(0.46)	(0.40)	(0.41)
	[5,731]	[30,496]			
% (SC+ST+Available Seats)	42.31	51.75	9.44***	-1.75***	-3.17***
	(19.79)	(35.61)	(0.45)	(0.37)	(0.37)
	[5,731]	[30,495]			

Notes: %(SC+ST) is the percentage of Grade 1 enrollment taken by students who are from a Scheduled Caste or a Schedule Tribe. %(SC+ST-Used Seats) estimates the percentage of Grade 1 enrollment taken by students who are from a Scheduled Caste or a Schedule Tribe in the absence of the RTE policy assuming all used RTE seats are taken by children from those groups and that these students would otherwise go to a public school. %(SC+ST+Used Seats) estimates the percentage of Grade 1 enrollment taken by students who are from a Scheduled Caste or a Schedule Tribe if all (currently used) RTE seats are taken by children from those groups (and these students come from public schools). %(SC+ST+Available Seats) estimates the percentage of Grade 1 enrollment taken by students who are from a Scheduled Caste or a Schedule Tribe if all (available) RTE seats are taken by children from those groups (and these students come from public schools). Column 1 shows the mean in private schools (standard deviation in parenthesis), while Column 2 shows the mean in public schools (standard deviation in parenthesis). Column 3 presents the difference (with its standard error in parenthesis), Column 4 presents the difference with block fixed effects (with its standard error in parenthesis), and Column 5 presents the difference with pincode fixed effects (with its standard error in parenthesis). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table A.19: School characteristics across the public and private sector

	Private mean (1)	Public mean (2)	Difference (3)	Difference Block F.E. (4)	Difference Pincode F.E. (5)
% (SC+ST)	26.78	56.39	29.61***	20.77***	20.75***
	(19.78)	(31.53)	(0.46)	(0.38)	(0.39)
	[5,792]	[30,825]			
English medium (%)	34.47	1.46	-33.01***	-28.10***	-26.87***
	(47.53)	(12.00)	(1.21)	(0.95)	(0.94)
	[5,792]	[30,825]			
Facility index	0.73	0.13	-0.60***	-0.45***	-0.45***
	(0.55)	(0.77)	(0.01)	(0.01)	(0.01)
	[5,782]	[30,824]			
Enrollment	686.40	107.28	-579.12***	-454.65***	-426.86***
	(825.25)	(96.13)	(38.23)	(19.04)	(18.01)
	[5,792]	[30,825]			
Teachers	19.94	4.43	-15.51***	-12.68***	-12.03***
	(22.11)	(2.81)	(1.02)	(0.59)	(0.41)
	[5,792]	[30,825]			
PTR	47.86	26.16	-21.70***	-17.65***	-15.51***
	(109.39)	(39.80)	(5.46)	(2.80)	(2.55)
	[5,763]	[30,718]			

Notes: %(SC+ST) is the percentage of enrollment taken by students who are from a Scheduled Caste or a Schedule Tribe. English medium (%) is the percentage of schools with English medium. Facility index is a principal component analysis (PCA) index based on whether the school has computer assisted learning, a homeroom, electricity, a library, a playground, a solid building, a boundary wall, functioning toilets, and solid classrooms. Enrollment is the total size of the school, teachers is the total number of teachers, and PTR is the pupil-teacher ratio. Column 1 shows the mean in private schools (standard deviation in parenthesis), while Column 2 shows the mean in public schools (standard deviation in parenthesis). Column 3 presents the difference (with its standard error in parenthesis), Column 4 presents the difference with block fixed effects (with its standard error in parenthesis), and Column 5 presents the difference with pincode fixed effects (with its standard error in parenthesis). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

B Tables and figures controlling for students' preferences

B.1 Main tables

Table B.1: Balance across lottery winners and losers, controlling for students' preferences

	Admin data		Phone survey #1		Phone survey #2	
	Control mean (1)	Treatment differential (2)	Control mean (3)	Treatment differential (4)	Control mean (5)	Treatment differential (6)
Female	0.49	0.00	0.49	-0.00	0.49	-0.01
	(0.50)	(0.01)	(0.50)	(0.02)	(0.50)	(0.03)
	[4,932]	[10,079]	[2,053]	[4,000]	[1,108]	[2,152]
Age (Jan 1st, 2019)	4.06	-0.01*	3.99	-0.03**	3.97	-0.01
	(0.93)	(0.01)	(0.91)	(0.01)	(0.88)	(0.02)
	[4,932]	[10,079]	[2,053]	[4,000]	[1,108]	[2,152]
Scheduled Caste	0.17	-0.00	0.16	0.01	0.15	0.02
	(0.38)	(0.01)	(0.37)	(0.01)	(0.36)	(0.02)
	[4,932]	[10,079]	[2,053]	[4,000]	[1,108]	[2,152]
Scheduled Tribe	0.17	-0.00	0.12	-0.00	0.10	0.00
	(0.38)	(0.01)	(0.32)	(0.01)	(0.31)	(0.01)
	[4,932]	[10,079]	[2,053]	[4,000]	[1,108]	[2,152]
Other Backward Class	0.54	-0.00	0.58	0.00	0.60	-0.01
	(0.50)	(0.01)	(0.49)	(0.02)	(0.49)	(0.02)
	[4,932]	[10,079]	[2,053]	[4,000]	[1,108]	[2,152]
Rural	0.40	-0.00	0.32	0.00	0.32	0.00
	(0.49)	(0.00)	(0.47)	(0.00)	(0.47)	(0.00)
	[4,932]	[10,079]	[2,053]	[4,000]	[1,108]	[2,152]
Surveyed			0.44	0.02**	0.26	0.03***
			(0.50)	(0.01)	(0.44)	(0.01)
			[4,932]	[10,079]	[4,932]	[10,079]
Allocated a seat			0.17	0.77***	0.17	0.78***
			(0.38)	(0.01)	(0.38)	(0.02)
			[2,007]	[3,962]	[1,088]	[2,138]

Notes: Odd columns contain the control (lottery losers) mean, standard deviation of the mean (in parentheses), and the number of observations in the control group (in square brackets). Even columns report the treatment effect (difference between lottery winners and losers), the standard error of the effect (in parentheses), and the number of observations in the treatment group (in square brackets). Columns 1–2 focus on the full sample. The p-value of the null hypothesis that the differences across all the observable applicant characteristics (Column 2) are jointly zero is .81. Columns 3–4 focus on those who answered our first phone survey. The p-value of the null hypothesis that the differences across all the observable applicant characteristics (Column 4) are jointly zero is .5. Columns 5–6 focus on those who answered our second phone survey. The p-value of the null hypothesis that the differences across all observable applicant characteristics (Column 6) are jointly zero is .31. All treatment estimates control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.2: Effect on the extensive margin of enrollment, controlling for students' preferences

	Any school				Private school			
	Control mean	ITT	CCM	LATE	Control mean	ITT	CCM	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All	0.85	0.13***	0.83	0.18***	0.80	0.17***	0.78	0.22***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
		[6,053]		[5,969]		[5,904]		[5,842]
Nursery	0.80	0.19***	0.77	0.24***	0.77	0.21***	0.74	0.27***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
		[3,103]		[3,062]		[3,017]		[2,987]
KG	0.87	0.12***	0.85	0.16***	0.80	0.17***	0.79	0.22***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
		[1,766]		[1,741]		[1,728]		[1,710]
Grade 1	0.98	0.01	0.98	0.02	0.90	0.08***	0.88	0.10***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
		[1,184]		[1,166]		[1,159]		[1,145]

Notes: Columns 1 and 5 report the control (lottery losers) mean and the standard error of the mean (in parentheses). Columns 2 and 6 list the itent-to-treat (ITT) effect (difference between lottery winners and losers), the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). Columns 3 and 7 report the control complier mean (CCM) — the mean outcomes for lottery loser compliers — and the standard error of the CCM (in parentheses). Columns 4 and 8 list the local average treatment effect (LATE) of being assigned an RTE seat (instrumented by winning the lottery), the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). All treatment estimates control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.3: Heterogeneity on school enrollment LATE, controlling for the probability of being assigned to a private school and students' preferences

	Any school (19-20)		Private school (19-20)	
	All	Grd 1	All	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Heterogeneity by gender				
Allocated a seat	.16***	.0086	.22***	.13***
	(.016)	(.017)	(.018)	(.037)
Allocated a seat × Female	.028	.015	.014	-.05
	(.022)	(.019)	(.025)	(.046)
Female	-.015	-.0072	-.0064	.037
	(.02)	(.017)	(.023)	(.039)
N. of obs.	5,969	1,166	5,842	1,145
CCM	.83	.98	.78	.88
Panel B: Heterogeneity by parental education				
Allocated a seat	.19***	.018	.24***	.11***
	(.013)	(.015)	(.015)	(.028)
Allocated a seat × Mother HS	-.07**	-.021	-.11***	-.085
	(.031)	(.015)	(.035)	(.065)
Mother HS	.064**	.015	.091***	.068
	(.027)	(.012)	(.031)	(.052)
N. of obs.	5,783	1,136	5,663	1,114
CCM	.83	.98	.78	.88
Panel C: Heterogeneity by caste				
Allocated a seat	.18***	.025	.2***	.13**
	(.027)	(.025)	(.032)	(.064)
Allocated a seat × OBC	-.017	-.025	.0016	-.089
	(.029)	(.023)	(.034)	(.066)
Allocated a seat × ST	.027	.0026	.013	-.046
	(.042)	(.028)	(.051)	(.087)
Allocated a seat × SC	.042	.023	.11**	.13
	(.04)	(.06)	(.046)	(.098)
Other Backward Class (OBC)	-.00027	.023	-.019	.059
	(.026)	(.018)	(.031)	(.051)
Scheduled Tribe (ST)	-.041	.0045	-.03	.035
	(.038)	(.022)	(.045)	(.068)
Scheduled Caste (SC)	-.058	-.035	-.12***	-.14*
	(.035)	(.048)	(.041)	(.078)
N. of obs.	5,969	1,166	5,842	1,145
CCM	.83	.98	.78	.88

Notes: This table presents the LATE of being assigned an RTE place (instrumented by winning the lottery). CCM denotes the mean outcomes for lottery loser compliers. The outcome in Columns 1–2 is whether the child was enrolled in any school in 2019–2020 (=1). The outcome in Columns 3–4 is whether the child was enrolled in a private school in 2019–2020 (=1). Mother HS indicates whether the mother completed high school. Columns 1 and 3 use the full sample, while Columns 2 and 4 use only Grade 1 students. All regressions control for "full preference" list fixed effects. Table B.14 provides the ITT effect of winning a lottery seat. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.4: Effect on the likelihood attending different schools, controlling for students' preferences

	English medium (1)	% students ST & SC (2)	Facility index (3)	Enrollment (4)	Teachers (5)	PTR (6)
Panel A: ITT						
Lottery seat	.042	.27	-.0085	12	.15	.45
	(.031)	(.72)	(.04)	(23)	(.59)	(1.2)
N. of obs.	1,139	808	810	769	785	754
Control mean	0.57	29.00	0.72	415.77	13.45	30.94
Control mean | enrolled	0.58	29.75	0.74	427.48	13.80	31.82
% Enrolled (Control)	98.53	97.49	97.49	97.26	97.42	97.26
Panel B: LATE						
Allocated a seat	.06	.51	-.022	5.2	-.0079	.77
	(.041)	(.89)	(.049)	(27)	(.71)	(1.6)
N. of obs.	1,124	800	802	761	777	746
CCM	0.52	28.05	0.71	429.98	13.97	29.21
CCM | enrolled	0.53	29.19	0.73	452.33	14.66	30.56
% Enrolled (CCM)	97.68	96.51	96.55	96.30	96.32	96.20

Notes: Panel A presents the ITT effects of winning a seat through the lottery on different characteristic of the school the child is enrolled in. Panel B presents the LATE of being allocated an RTE (instrumenting with the outcome of the lottery) on different characteristics of the school the child is enrolled in. CCM denotes the mean outcomes for lottery loser compliers. In Column 1, the outcome is whether the child attends an English medium schools or not. In Column 2, the outcome is the percentage of enrollment taken by Scheduled Castes and Tribes in the school the child attends. In Column 3, the outcome is a principal component analysis (PCA) facility index based on whether the school has computer assisted learning, a homeroom, electricity, a library, a playground, a solid building, a boundary wall, functioning toilets, and solid classrooms. In Columns 4-6 the outcomes are enrollment, number of teachers, and the pupil-teacher ratio (PTR). All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.5: Effect on fees, controlling for students' preferences

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Causal effect (LATE)				
Allocated an RTE seat	3,659***	5,322***	2,514***	1,577***
	(288)	(471)	(439)	(531)
CCM	5,538	5,916	4,611	6,210
CCM in private	8,001	9,922	6,213	7,284
% out of school (CCM)	17	23	15	2.2
% in public (CCM)	4.9	2.6	6	9.2
N. of obs.	4,296	2,046	1,385	865
Panel B: Decomposition (non-causal)				
Participatory effect (%)	32	43	27	13
Average fee (treatment)	8,992	11,013	7,404	7,789
Extensive margin	2,848	4,681	2,006	1,036
Conditional-on-positive effect	991	1,091	1,191	505
% private school (CCM)	69	59	74	85
Intensive margin	686	644	886	429
Total effect	3,534	5,324	2,892	1,465
N. of obs.	4,266	2,032	1,376	858

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the LATE of being allocated an RTE (instrumenting with the outcome of the lottery) on the market price of the school a child attends. Table B.15 presents the ITT effect of winning a lottery seat. All regressions control for "full preference" list fixed effects. CCM denotes the mean outcomes for lottery loser compliers. Panel B presents a non-causal decomposition of the effect among the extensive and intensive margins. The total effect in Panel B may be different from that reported in Panel A as the sample is slightly different, requiring information on both fees and private school enrollment. Likewise, the participatory effect is different from that presented in Tables B.2 due to sample differences. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.6: Effect on enrollment in top choice controlling for students' preferences

	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Lottery seat at first choice	.49***	.56***	.42***	.44***
	(.014)	(.018)	(.025)	(.033)
N. of obs.	6,053	3,103	1,766	1,184
Control mean	0.33	0.28	0.38	0.37
Control mean | enrolled	0.37	0.34	0.42	0.38
Control mean | enrolled & no RTE seat	0.43	0.42	0.44	0.42
% Enrolled (Control)	87.35	82.13	88.46	98.59
% RTE seat (Control)	26.70	28.89	23.03	26.28

Notes: This table presents the ITT effects of winning a place in the first-choice school through the lottery on the likelihood of enrolling in this top-choice school. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.7: Effect on government expenditure, controlling for students' preferences

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Market price				
Allocated an RTE seat	3,659***	5,322***	2,514***	1,577***
	(288)	(471)	(439)	(531)
CCM	5,538	5,916	4,611	6,210
CCM in private	8,001	9,922	6,213	7,284
% out of school (CCM)	17	23	15	2.2
% in public (CCM)	4.9	2.6	6	9.2
N. of obs.	4,296	2,046	1,385	865
Panel B: Reimbursed fee				
Allocated a seat	5,889***	6,497***	5,038***	5,578***
	(63)	(79)	(122)	(136)
N. of obs.	4,978	2,440	1,529	1,009
Panel C: Non-limit reimbursed fee				
Allocated a seat	9,659***	11,482***	7,206***	8,579***
	(252)	(368)	(432)	(489)
N. of obs.	4,978	2,440	1,529	1,009

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the LATE of being allocated an RTE seat (instrumenting with the outcome of the lottery) on the market price of the school a child attends. Panel B presents the LATE of being allocated an RTE seat (instrumenting with the outcome of the lottery) on the reimbursed fee (set to zero for children without an RTE seat). Panel C presents the LATE of being allocated an RTE (instrumenting with the outcome of the lottery) on the hypothetical reimbursed fee in the absence of the maximum reimbursement limit (set to zero for children without an RTE seat). All regressions control for "full preference" list fixed effects. CCM denotes the mean outcomes for lottery loser compliers. Table B.21 presents the ITT estimates of winning a lottery seat. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.8: Effect on school enrollment during the COVID-19 pandemic, controlling for students' preferences

	Any school	Private school
	(1)	(2)
Panel A: ITT		
Lottery seat	.01	.062***
	(.01)	(.02)
COVID	-.035***	-.075***
	(.012)	(.014)
Lottery seat $\times$ COVID	.017	.05***
	(.013)	(.016)
N. of obs.	3,060	2,912
Control mean (2019-2020)	0.977	0.908
COVID + COVID $\times$ Lottery seat	-0.018	-0.025
p-value(H_0:COVID+COVID$\times$Lottery seat= 0)	.00089	.000088
Panel B: LATE		
Allocated an RTE seat	.013	.081***
	(.014)	(.026)
COVID	-.04***	-.09***
	(.015)	(.018)
Allocated an RTE seat $\times$ COVID	.025	.071***
	(.018)	(.021)
N. of obs.	3,018	2,878
CCM (2019-2020)	0.978	0.883
COVID + COVID $\times$ Allocated an RTE seat	-0.015	-0.019
p-value(H_0:COVID+COVID$\times$Allocated an RTE seat= 0)	.0095	.0055

Notes: This table estimates difference-in-differences models of the effect of an RTE place before and after the COVID-19 pandemic began. The data includes outcomes for the 2019–2020 academic year (pre COVID-19) and for the 2020–2021 academic year (post COVID-19). Panel A contains the intent-to-treat (ITT) effect of winning a lottery seat. Panel B includes the local average treatment effect (LATE) of being allocated a seat (instrumented by the outcome of the lottery). COVID is a dummy equal to 1 for the 2020–2021 academic year. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.9: Effect on on instructional inputs, controlling for students' preferences

	All		Nursery		Kindergarten		Grade 1	
	CCM	LATE	CCM	LATE	CCM	LATE	CCM	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is open	0.05	0.01	0.04	0.01	0.04	0.04	0.10	-0.03
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)
		[3,202]		[1,656]		[967]		[579]
School: academic support	0.41	0.12***	0.38	0.19***	0.38	0.12**	0.54	-0.08
	(0.02)	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)
		[3,108]		[1,616]		[934]		[558]
Lectures last month	0.39	0.21***	0.38	0.27***	0.31	0.27***	0.54	-0.04
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.05)	(0.07)
		[3,070]		[1,598]		[925]		[547]
Video lectures	0.28	0.21***	0.26	0.27***	0.23	0.24***	0.45	-0.01
	(0.02)	(0.03)	(0.03)	(0.05)	(0.03)	(0.05)	(0.06)	(0.08)
		[2,446]		[1,236]		[790]		[420]
Audio lectures	0.13	0.15***	0.14	0.18***	0.10	0.19***	0.19	-0.04
	(0.02)	(0.03)	(0.02)	(0.05)	(0.02)	(0.05)	(0.05)	(0.07)
		[1,660]		[801]		[591]		[268]
HH: academic support	0.81	0.00	0.83	0.01	0.79	0.00	0.82	-0.03
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.05)	(0.04)	(0.06)
		[3,117]		[1,619]		[938]		[560]
Tutor: academic support	0.15	-0.02	0.13	-0.02	0.18	-0.07*	0.12	0.05
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)
		[3,119]		[1,626]		[932]		[561]

Notes: Odd columns contain the CCM — the mean outcomes for lottery loser compliers — and the standard error of the CCM (in parentheses). Even columns report the LATE of being assigned an RTE seat (instrumented by winning the lottery), the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). Columns 1–2 focus on the full sample, Columns 3–4 on Nursery students, Columns 5–6 on Kindergarten (KG) students, and Columns 7–8 on Grade 1 students. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.10: Effect on test scores, controlling for students' preferences

	All		Nursery		Kindergarten		Grade 1	
	CCM	LATE	CCM	LATE	CCM	LATE	CCM	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall								
IRT score	-0.01	0.11*	-0.12	0.02	-0.09	0.17	0.37	0.27*
	(0.04)	(0.06)	(0.06)	(0.08)	(0.07)	(0.11)	(0.10)	(0.15)
% correct	49.30	3.62*	45.42	0.86	46.69	5.66	62.21	7.82
	(1.19)	(1.99)	(1.90)	(2.78)	(2.39)	(3.53)	(3.22)	(4.82)
Panel B: Numeracy								
Counting (%)	70.07	3.10	66.60	0.24	68.37	6.24	80.31	5.71
	(1.52)	(2.51)	(2.53)	(3.63)	(3.03)	(4.49)	(3.80)	(5.42)
Number comparison (%)	50.05	4.53*	46.18	1.19	47.38	7.56	60.22	8.61
	(1.57)	(2.64)	(2.53)	(3.72)	(3.07)	(4.69)	(4.06)	(6.23)
Addition (%)	46.83	2.78	41.13	-0.99	44.63	6.70	65.46	6.55
	(1.59)	(2.63)	(2.46)	(3.64)	(3.18)	(4.81)	(4.06)	(6.18)
Subtraction (%)	47.01	3.87	42.15	-1.50	44.67	6.47	59.73	14.36**
	(1.49)	(2.54)	(2.31)	(3.53)	(3.09)	(4.63)	(3.88)	(5.94)
% math	53.42	3.72*	48.41	0.48	51.68	6.47*	67.03	8.06
	(1.29)	(2.14)	(2.04)	(2.98)	(2.62)	(3.90)	(3.39)	(5.07)
Panel C: Hindi								
Letters (%)	37.65	3.67	36.00	0.15	34.79	7.35*	48.72	7.19
	(1.38)	(2.35)	(2.18)	(3.21)	(2.74)	(4.23)	(3.80)	(5.85)
Vocabulary (%)	61.06	3.79	57.39	-0.74	58.71	8.22*	72.85	8.83
	(1.58)	(2.64)	(2.55)	(3.71)	(3.30)	(4.87)	(3.88)	(5.82)
Sentences (%)	51.09	4.20	46.31	1.81	49.01	4.97	65.09	9.52
	(1.62)	(2.72)	(2.56)	(3.86)	(3.25)	(4.91)	(4.16)	(6.19)
Listening (%)	51.52	3.69	48.32	0.57	47.97	7.46*	62.40	5.97
	(1.50)	(2.54)	(2.38)	(3.59)	(2.99)	(4.45)	(3.94)	(6.02)
% Hindi	48.92	3.82*	45.78	0.41	46.19	7.04*	60.76	7.80
	(1.26)	(2.11)	(2.02)	(2.95)	(2.53)	(3.78)	(3.37)	(5.03)
Panel D: English								
Vocabulary (%)	30.37	3.87**	27.98	3.66	25.21	2.19	44.18	7.30
	(1.16)	(1.90)	(1.78)	(2.63)	(2.07)	(3.19)	(3.68)	(5.15)
% English	41.76	3.06	38.78	2.41	37.59	1.56	55.17	7.38
	(1.26)	(2.08)	(2.02)	(2.98)	(2.37)	(3.49)	(3.60)	(5.20)
Number of obs		3,226		1,669		978		579

Notes: Odd columns report the control complier mean (CCM) — the mean outcomes for lottery loser compliers — and the standard error of the CCM (in parentheses). Even columns contain the local average treatment effect (LATE of being assigned an RTE seat (instrumented by winning the lottery) and the standard error of the effect (in parentheses). Columns 1–2 focus on the full sample, Columns 3–4 on Nursery students, Columns 5–6 on Kindergarten (KG) students, and Columns 7–8 on Grade 1 students. All regressions control for "full preference" list fixed effects. Table A.10 presents the intent-to-treat (ITT) effect of winning a lottery seat. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

B.2 Appendix tables

Table B.11: Attrition by child characteristics, controlling for students' preferences

	Survey #1	Survey #2
	(1)	(2)
Female	.0042	.005
	(.0083)	(.0073)
Age (Jan 1st, 2019)	-.015	-.0097
	(.013)	(.011)
Scheduled Caste	-.037**	-.024
	(.018)	(.016)
Scheduled Tribe	-.074***	-.065***
	(.019)	(.017)
Other Backward Class	-.012	.0011
	(.016)	(.014)
Rural	.042	.031
	(.038)	(.033)
N. of obs.	15,011	15,011
Outcome mean	.43	.25

Notes: The outcome is whether we were able to conduct the interview (=1). All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.12: Compliance, controlling for students' preferences

	Allotted an RTE seat			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Allocated a seat	.77***	.78***	.76***	.77***
	(.011)	(.015)	(.021)	(.026)
N. of obs.	5,969	3,062	1,741	1,166
Control mean	0.17	0.18	0.17	0.18

Notes: This table presents the effect of winning a lottery seat on being allotted an RTE seat. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.13: Effect on the extensive margin of enrollment, controlling for the probability of being assigned to a private school: Lee bounds and stratas with low attrition controlling for students' preferences

	Strata without attrition		Low attrition strata			Lee bounds	
	ITT	LATE	Differential attrition	ITT	LATE	ITT	
						LB	UB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All grades							
Private school (19-20)	0.15***	0.19***	0.00	0.17***	0.22***	0.08	0.25
	(0.04)	(0.04)	(0.00)	(0.01)	(0.02)	(0.02)	(0.02)
	[361]	[358]	[6,294]	[3,060]	[3,033]	[2,874]	[2,874]
Any school (19-20)	0.11***	0.14***	0.00	0.13***	0.17***	0.06	0.18
	(0.03)	(0.04)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)
	[240]	[236]	[6,294]	[3,138]	[3,097]	[2,937]	[2,937]
Panel B: Nursery							
Private school (19-20)	0.17***	0.20***	-0.00	0.21***	0.26***	0.10	0.26
	(0.05)	(0.06)	(-0.00)	(0.02)	(0.02)	(0.02)	(0.03)
	[142]	[140]	[2,983]	[1,523]	[1,511]	[1,397]	[1,397]
Any school (19-20)	0.16***	0.20***	-0.00	0.18***	0.23***	0.09	0.24
	(0.04)	(0.05)	(-0.00)	(0.02)	(0.02)	(0.02)	(0.03)
	[150]	[146]	[2,983]	[1,567]	[1,549]	[1,436]	[1,436]
Panel C: Kindergarten							
Private school (19-20)	0.11	0.12	-0.01	0.14***	0.19***	0.07	0.33
	(0.10)	(0.11)	(-0.01)	(0.02)	(0.03)	(0.04)	(0.04)
	[20]	[20]	[2,098]	[959]	[948]	[906]	[906]
Any school (19-20)	.	.	-0.01	0.10***	0.14***	0.08	0.22
	(.)	(.)	(-0.01)	(0.02)	(0.02)	(0.02)	(0.03)
	[.]	[.]	[2,098]	[981]	[965]	[920]	[920]
Panel D: Grade 1							
Private school (19-20)	0.13*	0.18*	0.02	0.09***	0.12***	0.07	0.12
	(0.06)	(0.09)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
	[68]	[68]	[1,213]	[578]	[574]	[571]	[571]
Any school (19-20)	0.05	0.07	0.02	0.02*	0.03*	0.00	0.01
	(0.04)	(0.06)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
	[70]	[70]	[1,213]	[590]	[583]	[581]	[581]

Notes: Columns 1–2 report the results restricting the sample to strata without attrition. Column 1 shows the ITT effect of winning the lottery, and Column 2 the LATE of being assigned an RTE seat (instrumented with winning the lottery). Columns 3–5 show the results after dropping the 25% of the strata with the most differential attrition. Column 3 shows the results of the differential attrition, Column 4 the ITT effect, and Column 5 the LATE of being assigned an RTE seat. Columns 6–7 show Lee (2009) style bounds — Column 6 has the lower bound (LB), while Column 7 has the upper bound for (UB) — for the ITT effect of winning the lottery. Standard errors are in parentheses. The number of observations in the treatment effects estimates is in square brackets. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.14: Heterogeneity on school enrollment ITT, controlling for the probability of being assigned to a private school and students' preferences

	Any school (19-20)		Private school (19-20)	
	All	Grd 1	All	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Heterogeneity by gender				
Lottery seat	.12***	.0063	.17***	.097***
	(.012)	(.013)	(.015)	(.028)
Female	-.0051	-.0049	.002	.029
	(.016)	(.014)	(.019)	(.031)
Lottery seat × Female	.018	.012	.0081	-.038
	(.017)	(.014)	(.02)	(.034)
N. of obs.	6,053	1,184	5,904	1,159
Control mean	.86	.99	.81	.91
Panel B: Heterogeneity by parental education				
Lottery seat	.14***	.014	.18***	.084***
	(.01)	(.011)	(.012)	(.021)
Mother HS	.05**	.011	.07***	.048
	(.022)	(.009)	(.025)	(.043)
Lottery seat × Mother HS	-.053**	-.015	-.08***	-.063
	(.024)	(.011)	(.028)	(.05)
N. of obs.	5,858	1,152	5,724	1,128
Control mean	.86	.99	.81	.91
Panel C: Heterogeneity by caste				
Lottery seat	.14***	.015	.16***	.085**
	(.021)	(.015)	(.025)	(.039)
Other Backward Class (OBC)	-.0014	.018	-.014	.034
	(.022)	(.012)	(.026)	(.037)
Scheduled Tribe (ST)	-.025	.0044	-.019	.025
	(.029)	(.015)	(.035)	(.049)
Scheduled Caste (SC)	-.051*	-.035	-.1***	-.14**
	(.029)	(.039)	(.034)	(.062)
Lottery seat × OBC	-.016	-.016	-.0065	-.052
	(.023)	(.014)	(.027)	(.042)
Lottery seat × ST	.0054	.0045	-.005	-.03
	(.031)	(.018)	(.038)	(.06)
Lottery seat × SC	.028	.022	.078**	.12*
	(.031)	(.045)	(.036)	(.07)
N. of obs.	6,053	1,184	5,904	1,159
Control mean	.86	.99	.81	.91

Notes: This table presents the ITT estimates of being assigned a seat by winning the lottery. The outcome in Columns 1–2 is whether the child was enrolled in any school in 2019–2020 (=1). The outcome in Columns 3–4 is whether the child was enrolled in a private school in 2019–2020 (=1). Mother HS indicates whether the mother completed high school. Columns 1 and 3 use the full sample, while Columns 2 and 4 use only Grade 1 students. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.15: Intent-to-treat effect of winning the lottery on fees, controlling for students' preferences

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Causal effect (ITT)				
Lottery seat	2,942***	4,444***	1,940***	1,235***
	(227)	(378)	(333)	(426)
Control mean	5,474	5,609	4,783	6,369
Control mean in private	7,680	9,354	6,341	7,369
% out of school (control)	21	36	16	2.3
% in public (control)	7.3	4.4	8.6	11
N. of obs.	4,337	2,061	1,402	874
Panel B: Decomposition (non-causal)				
Participatory effect (%)	25	35	20	11
Average fee (treatment)	8,608	10,451	7,194	7,622
Extensive margin	2,185	3,702	1,472	814
Conditional-on-positive effect	800	906	946	375
% private school (control)	71	62	76	86
Intensive margin	571	558	717	324
Total effect	2,756	4,259	2,189	1,138
N. of obs.	4,305	2,047	1,392	866

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the ITT effects of winning a seat through the lottery on the market price of the school a child attends. All regressions control for "full preference" list fixed effects. Panel B presents a non-causal decomposition of the effect among the extensive and intensive margins. The total effect in Panel B may be different from the effect in Panel A as the sample is slightly different, requiring information on both fees and private school enrollment. Likewise, the participatory effect is different from that presented in Table B.2 due to sample differences. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.16: Effect of winning different lottery seats on enrollment in the top-choice school, controlling for students' preferences

	(1)	(2)	(3)	(4)	(5)	(6)
Won lottery	.42***					
	(.015)					
Won seat in first choice		.49***	.62***	.59***	.62***	.61***
		(.014)	(.025)	(.027)	(.041)	(.043)
Won seat in second choice				-.15***	-.23***	-.24***
				(.048)	(.079)	(.08)
Won seat in third choice						-.18**
						(.075)
N. of obs.	6,053	6,053	1,694	1,694	649	649
Sample	Full	Full	≥ 2 choices	≥ 2 choices	≥ 3 choices	≥ 3 choices

Notes: This table presents the effect of winning different lottery seats on the likelihood of enrolling in the top-choice school. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.17: Effect on the diversity of the student body, controlling for students' preferences

	% SC	% ST	% SC+ST
	(1)	(2)	(3)
Panel A: ITT			
Lottery seat	.12	-.61	-.49
	(.9)	(.89)	(1.4)
Scheduled Tribe	.039	-1.2	-1.2
	(.99)	(1.5)	(2)
Scheduled Caste	2.9*	-1.1	1.8
	(1.5)	(1.1)	(2)
Other Backward Class	-.15	-1.2	-1.4
	(1.1)	(1)	(1.6)
Lottery seat × Scheduled Tribe	.46	1.3	1.8
	(1)	(1.5)	(2)
Lottery seat × Scheduled Caste	-2	1.1	-.89
	(1.4)	(1.1)	(2)
Lottery seat × Other Backward Class	.31	.89	1.2
	(1.1)	(1)	(1.6)
N. of obs.	808	808	808
Control mean	12.60	16.40	29.00
Control mean | enrolled	12.93	16.82	29.75
% Enrolled (Control)	97.49	97.49	97.49
Panel B: LATE			
Allocated a seat	.35	-.83	-.48
	(1.3)	(1.3)	(2.1)
Allocated a seat × Scheduled Caste	-2.6	1.5	-1.1
	(1.9)	(1.5)	(2.7)
Allocated a seat × Scheduled Tribe	.31	1.7	2
	(1.5)	(2.1)	(2.9)
Allocated a seat × Other Backward Class	.3	1.2	1.5
	(1.5)	(1.5)	(2.2)
Scheduled Caste	3.2*	-1.3	1.9
	(1.8)	(1.4)	(2.4)
Scheduled Tribe	.18	-1.5	-1.3
	(1.4)	(2.1)	(2.7)
Other Backward Class	-.23	-1.5	-1.8
	(1.4)	(1.4)	(2.1)
N. of obs.	800	800	800
CCM	12.88	15.17	28.05
CCM | enrolled	13.38	15.81	29.19
% Enrolled (CCM)	96.51	96.51	96.51

Notes: Panel A presents the ITT effects of winning a seat through the lottery on the proportion of students from Scheduled Castes (SC) and Scheduled Tribes (ST). Panel B presents the LATE of being allocated an RTE (instrumenting with the outcome of the lottery) on the proportion of students from SC and ST. CCM denotes the mean outcomes for lottery loser compliers. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.18: Intent-to-treat effect of winning the lottery on test scores, controlling for students' preferences

	All		Nursery		Kindergarten		Grade 1	
	Control mean	ITT	Control mean	ITT	Control mean	ITT	Control mean	ITT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IRT score	-0.02	0.09*	-0.14	0.02	-0.11	0.12	0.39	0.21*
	(0.03)	(0.05)	(0.05)	(0.07)	(0.06)	(0.08)	(0.09)	(0.12)
% correct	49.13	2.92*	44.75	1.04	46.55	4.02	63.33	6.15
	(1.02)	(1.54)	(1.59)	(2.19)	(1.92)	(2.63)	(2.72)	(3.77)

Notes: Odd columns contain the control mean and standard error of the control mean (in parentheses). Even columns report the ITT effect of winning an RTE seat through the lottery, the standard error of the effect (in parentheses), and the number of observations used to estimate the effect (in square brackets). Columns 1–2 focus on the full sample, Column 3–4 on Nursery students, Columns 5–6 on Kindergarten (KG) students, and Columns 7–8 on Grade 1 students. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.19: Heterogeneity in learning outcomes by child characteristics, controlling for students' preferences

	(1) Female	(2) Age	(3) SC	(4) ST	(5) OBC	(6) Rural
Panel A: ITT						
Lottery seat	.075	-.07	.11**	.093*	.023	.084
	(.061)	(.21)	(.051)	(.049)	(.071)	(.058)
Covariate	.025	.26***	.11	-.046	-.098	-.44***
	(.067)	(.067)	(.1)	(.11)	(.074)	(.17)
Lottery seat × Covariate	.025	.041	-.12	-.065	.11	.012
	(.082)	(.053)	(.12)	(.14)	(.087)	(.097)
N. of obs.	3,260	3,260	3,260	3,260	3,260	3,260
Panel B: LATE						
Allocated a seat	.098	-.13	.14**	.12*	.027	.1
	(.077)	(.27)	(.067)	(.062)	(.092)	(.074)
Allocated a seat × Covariate	.026	.062	-.15	-.067	.14	.027
	(.1)	(.069)	(.15)	(.19)	(.11)	(.13)
Covariate	.023	.24***	.14	-.047	-.12	-.46**
	(.083)	(.075)	(.12)	(.15)	(.091)	(.18)
N. of obs.	3,226	3,226	3,226	3,226	3,226	3,226

Notes: The outcome is the child's IRT score. Panel A presents the ITT effects of winning a place through the lottery. Panel B reports the LATE of being allocated an RTE seat (instrumenting with the outcome of the lottery). CCM denotes the mean outcomes for lottery loser compliers. Each column displays the interaction of a different covariate with the treatment variable. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.20: Learning outcomes, controlling for the probability of being assigned to a private school: Lee bounds and stratas with low attrition controlling for students' preferences

	Strata without attrition		Low attrition strata			Lee bounds	
	ITT	LATE	Differential attrition	ITT	LATE	ITT	
						LB	UB
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.44**	0.43	0.02	0.09*	0.12*	-0.08	0.38
	(0.21)	(0.28)	(0.02)	(0.05)	(0.07)	(0.04)	(0.03)
	[164]	[158]	[4,422]	[1,432]	[1,419]	[3,260]	[3,260]
Nursery	0.42	0.27	0.03	0.01	-0.00	-0.13	0.37
	(0.29)	(0.35)	(0.03)	(0.08)	(0.10)	(0.07)	(0.06)
	[110]	[104]	[1,975]	[688]	[680]	[1,686]	[1,686]
KG	0.77	0.95	-0.00	0.12	0.16	0.05	0.42
	(0.57)	(0.69)	(-0.00)	(0.09)	(0.12)	(0.08)	(0.07)
	[15]	[15]	[1,723]	[515]	[512]	[989]	[989]
Grade 1	0.32	0.48	0.02	0.31**	0.40**	-0.09	0.38
	(0.34)	(0.58)	(0.02)	(0.14)	(0.18)	(0.12)	(0.10)
	[39]	[39]	[726]	[235]	[233]	[585]	[585]

Notes: Columns 1–2 report results of restricting the sample to strata without attrition. Column 1 shows the ITT effect of winning the lottery, and Column 2 the LATE of being assigned an RTE seat (instrumented with winning the lottery). Columns 3–5 show the results after dropping the 25% of the strata with the most differential attrition. Column 3 shows the results of the differential attrition, Column 4 has the ITT effect, and Column 5 has the LATE of being assigned an RTE seat. Columns 6–7 show Lee (2009) style bounds — Column 6 has the lower bound (LB), while Column 7 has the upper bound for (UB) — for the ITT effect of winning the lottery. Standard errors are in parentheses. The number of observations in the treatment effects estimates is in square brackets. All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Table B.21: Intent-to-treat effect on government expenditure, controlling for students' preferences

	INR			
	All	NU	KG	Grd 1
	(1)	(2)	(3)	(4)
Panel A: Market price				
Lottery seat	2,942***	4,444***	1,940***	1,235***
	(227)	(378)	(333)	(426)
Control mean	5,474	5,609	4,783	6,369
Control mean in private	7,680	9,354	6,341	7,369
% out of school (control)	21	36	16	2.3
% in public (control)	7.3	4.4	8.6	11
N. of obs.	4,337	2,061	1,402	874
Panel B: Reimbursed fee				
Lottery seat	4,837***	5,607***	3,860***	4,486***
	(76)	(100)	(134)	(173)
N. of obs.	5,019	2,456	1,545	1,018
Panel C: Non-limit reimbursed fee				
Lottery seat	7,937***	9,901***	5,546***	6,886***
	(220)	(327)	(353)	(444)
N. of obs.	5,019	2,456	1,545	1,018

Notes: Fee information comes from administrative data. Students in public schools or not enrolled in school are assigned zero fees. Panel A presents the ITT effects of being allocated an RTE through the lottery on the market price of the school a child attends. Panel B presents the ITT effects of being allocated an RTE through the lottery on the reimbursed fee (set to zero for children without an RTE seat). Panel C presents the ITT effects of being allocated an RTE through the lottery on the hypothetical reimbursed fee in the absence of the maximum reimbursement limit (set to zero for children without an RTE seat). All regressions control for "full preference" list fixed effects. Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

C Phone based test

We engaged students directly to conduct a short age-appropriate test of basic literacy and numeracy competencies. To make students feel at ease with the enumerator, the test was designed as a conversation (see the full survey instrument in Section C.1). The answers to some of the questions we asked were not recorded (e.g., name an animal that lives in the jungle or whether they have siblings) as they were meant to build a rapport with the children and get them comfortable talking to us.

The questions broadly cover three domains: literacy in Hindi, numeracy, and literacy in English. The test instrument was piloted extensively with considerable care and responsibility to ensure the comfort of the student by only asking questions related to the familiar contexts of home or school.

We estimate a two-parameter item response theory (IRT) model to obtain a proxy for students' ability. We run several tests of the validity of the test. First, Cronbach (1951)'s alpha (a measure of how well test items correlate with each other) is 0.948. A test is considered to have good internal consistency if alpha is above 0.9 (Nunnally, 1994; Bland and Altman, 1997; DeVellis, 2016). Second, as expected, the test scores are higher for older students and for children from economically better-off households (see Table C.22). Finally, there is a good empirical fit with estimated Item Characteristic Curves from the IRT model and no evidence of differential item functioning across grades (see Figures C.1 and C.2).

Table C.22: Learning outcomes by child characteristics

	(1)	(2)	(3)
Panel A: With caste dummies			
Female	.038	.047	.042
	(.029)	(.038)	(.032)
Age (Jan 1st, 2019)	.27***	.3***	.25***
	(.016)	(.059)	(.05)
Scheduled Caste	-.15***	-.045	-.085
	(.053)	(.076)	(.062)
Scheduled Tribe	-.23***	-.13	-.22***
	(.06)	(.089)	(.07)
Other Backward Class	-.2***	-.066	-.15***
	(.043)	(.064)	(.051)
Rural	-.17***	-.35*	-.15***
	(.032)	(.18)	(.05)
Asset Index	.078***	.062***	.07***
	(.0096)	(.013)	(.011)
N. of obs.	4,191	3,120	3,859
Fixed Effects	None	Preferences	Probability
Panel B: Without caste dummies			
Female	.04	.048	.045
	(.029)	(.038)	(.032)
Age (Jan 1st, 2019)	.27***	.3***	.25***
	(.016)	(.059)	(.05)
Rural	-.19***	-.35*	-.17***
	(.032)	(.18)	(.05)
Asset Index	.08***	.062***	.072***
	(.0095)	(.013)	(.011)
N. of obs.	4,191	3,120	3,859
Fixed Effects	None	Preferences	Probability

Notes: The outcome is the child's IRT score. Column 1 does not include any controls. Column 2 controls for "full preference" list fixed effects. Column 3 controls for the probability of being assigned to a private school by the assignment mechanisms following Abdulkadiroğlu et al. (2017). Statistical significance at the 1, 5, 10% levels is indicated by ***, **, and *.

Figure C.1: Items 1-12

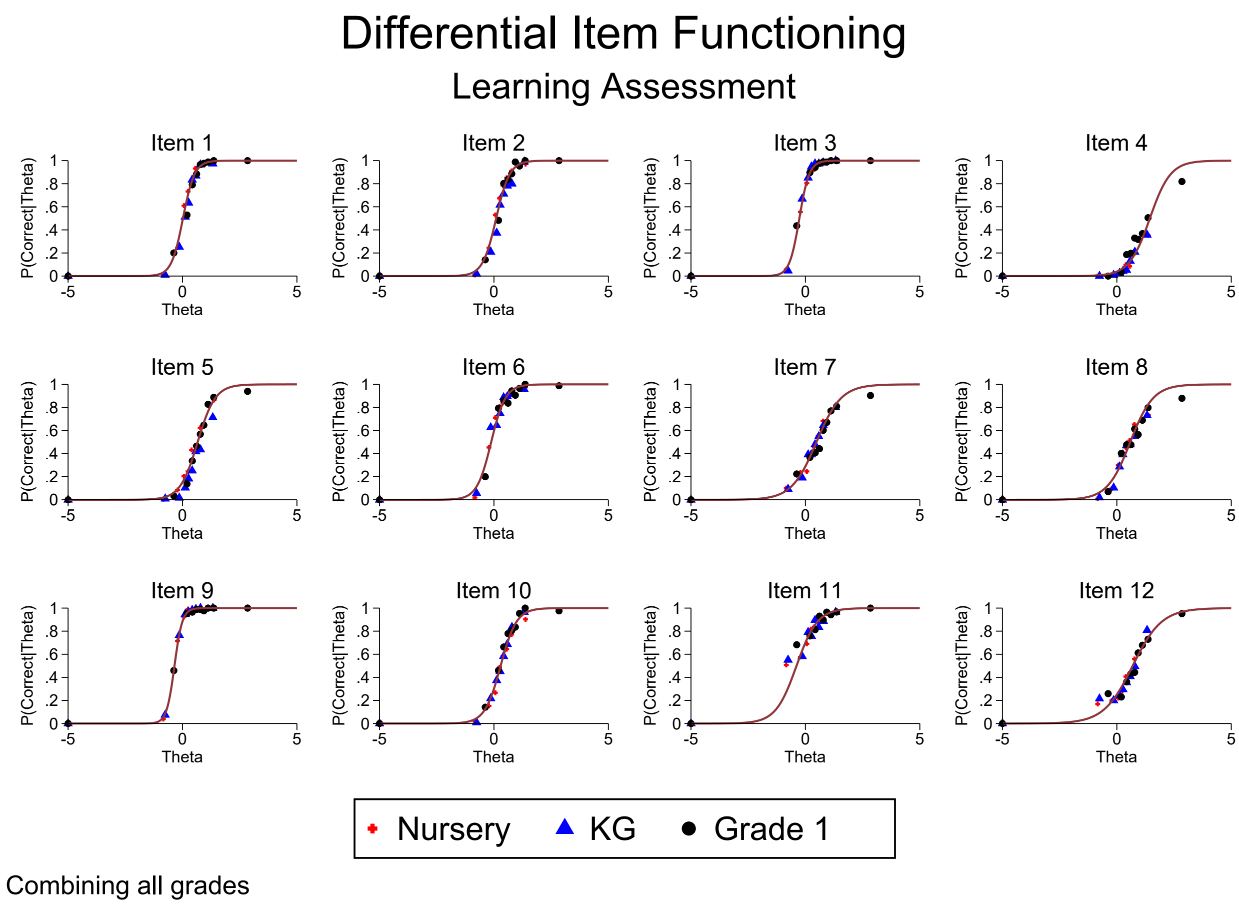
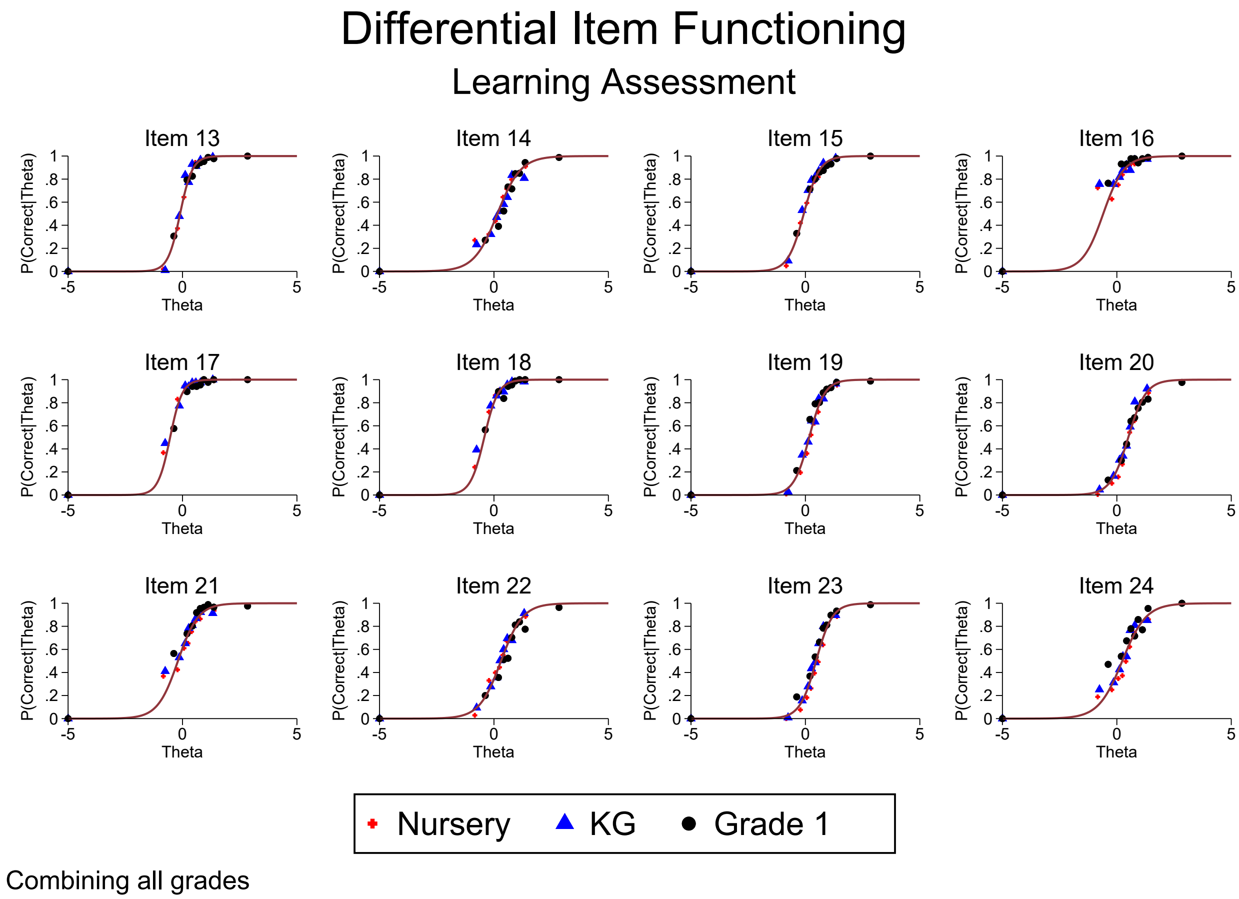


Figure C.2: Items 13-24



C.1 Survey instrument

Section 3 : Student Assessment **भाग 3: छात्र मूल्यांकन**		
1.	आप का नाम क्या है? What is your name?	a. Said their name/अपना नाम बताया b. कोई जवाब नही/ No response
2.	आप कौन से स्कूल जाते हो? Which school do you go to?	a. Said their school name/ स्कूल का नाम बताया b. कोई जवाब नही/ No response c. पता नहीं/ Don't know
3.	**जंगल में जानवर होते है, हैं ना? आप कोई एक ऐसे जानवर का नाम बता सकते हो जो जंगल में रहता हो**? Animals live in the jungle don't they? Could you tell me the name of one animal that lives in the jungle?	
4.	**अच्छा** (बताए हुए **जानवर का नाम**) **के कितने पैर होते हैं**? **आप बता सकते हो**? Alright, can you tell me how many legs does (*Animal mentioned by the child*) have?	a. सही अंक बताया/ Correct number b. ग़लत अंक बताया/ Wrong number c. कोई जवाब नही/ No response
5.	**अच्छा मुझे बन्दर** बहुत **अच्छे लगते है। बन्दर हमेशा पेड़** पर **खेलते हैं, हैं ना**? I like monkeys a lot. They play on the trees don't they?	
6.	क्या आप बता सकते हैं की **पेड़** शब्द कौन्से अक्षर से शुरू होता है**?** Can you tell me the first letter of the word **पेड़ (tree)**	a. सही अक्षर बताया/ Named the correct letter b. ग़लत अक्षर बताया/ Named the wrong letter c. कोई जवाब नही/ No response
7.	पेड़ पर और क्या होते हैं**?** फल**,** फूल**,** है ना**? अच्छा अब एक और बात बोलिये,** क्या आप मुझे एक फल का नाम बता सकते हैं जो पीले रंग का होता है**?**	a. फल का नाम बताया/ Names a fruit b. कोई जवाब नही/ No response
	What else can we find on trees? Flowers and fruits right? Can you tell me the name of a fruit that is yellow in colour?	
	(बताए हुए **फल का नाम**)? आम भी पीले रंग के होते हैं**?** है ना**?** (Name of the fruit mentioned by the child)? Mangoes are also yellow in colour right?	
8.	**अगर मेरे पास** 5 **आम हैं और आपके पास** 6, **तो कुल मिलाकर हम दोनों के पास कितने आम होंगे**? I have 5 mangoes, and you have 6, how many do we have in total?	a. **सही अंक बताया**/ Correct number b. **ग़लत अंक बताया**/ Wrong number c. **कोई जवाब नही**/ No response
	अच्छा, मेरे पास 8 **आम हैं, अगर** 8 **आम में से मैंने** 4 **खा लिया हैं, तो कितने बचे**? If I eat 4 mangoes out of 8, how many do I have left?	a. सही अंक बताया/ Correct number b. ग़लत अंक बताया/ Wrong number c. कोई जवाब नही/ No response
9.	**"आम" शब्द "आ" अक्षर से शुरू होता है, है न**? ऐसे ही क्या आप 'क' से शुरू होने वाला कोई एक शब्द बता सकते हैं? Aam starts with A right? Similarly, can you name a word starting with 'K'?	a. सही शब्द बताया/ Named a correct word b. ग़लत शब्द बताया/ Named a wrong word c. कोई जवाब नही/ No answer
10.	**अगर हम "क" अक्षर से "** ऊ **" की मात्रा जोड़े, तो "कू" होता हैं। ऐसे ही "म" अक्षर से अगर "आ" की मात्रा लगाए, तो क्या होगा**? If we add 'oo' matra to 'k' then it becomes 'koo', similarly what will we get when we add 'aa' to 'm'	a. सही अक्षर बताया/ Named the correct letter b. ग़लत अक्षर बताया/ Named the wrong letter c. कोई जवाब नही/ No response
11.	अच्छा एक बात बताओ, आपके साथ कमरे में कौन है? Can you tell me who is in the room	

	with you?	
12.	और आपके हाथ कितने हैं? And, how many hands do you have?	a. सही अंक बताया/ Correct number b. ग़लत अंक बताया/ Wrong number a. कोई जवाब नही/ No response
13.	क्या आप गिन के बता सकते हैं उनकी उंगलिया और आपके हाथ की उंगलियाँ मिलाकर कितने हुए? Can you count the fingers in your hands and the fingers in their hands and tell me how many fingers there are in total?	a. सही अंक बताया/ Correct number b. ग़लत अंक बताया/ Wrong number c. कोई जवाब नही/ No response
	अच्छा तो आप कौनसी कक्षा में हो? आपकी कक्षा में कितने बच्चे हैं? Ok, so what class are you in? How many children are there in your class?	
14.	अच्छा, अगर आपकी कक्षा में 20 बच्चे हैं और मेरी कक्षा में 14 बच्चे हैं? तो अधिक बच्चे किसकी कक्षा में हैं? Alright, now if there are 20 students in my class and there are 14 in your class, whose class has more students?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
15.	अच्छा, 20 बच्चों में अगर 5 लड़कियां है, तो कितने लड़के होंगे? Alright, if there are 20 students and 5 are girls, how many are boys?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
16.	अच्छा आपकी बहन/ भाई भी आपके साथ स्कूल जाते है? Does your brother/sister also go to school with you? उनका नाम क्या हैं? What is their name? (skip if no siiblings)/ (अगर कोई भाई बहन नहीं है तो यह मत पूछो)	
17.	सोचो कि आपके पास 2 पुस्तकें है और आपकी बहन/ भाई /सहेली के पास 10, किसके पास ज़्यादा पुस्तकें हैं? Let's imagine you have 2 books and your	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response

	sister has 10 books. Who has more books?	
18.	आपकी 2 पुस्तकें और आपकी सहेली/ दोस्त की 10 पुस्तकें तो कुल मिलाकर कितनी पुस्तकें हुई? If you add your 2 books and their 10 books, how many books are there in total?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
19.	If three boys go to a shop and spend 5 rupees each, how much do they spend in total? मान लीजिए कि तीन लड़के एक दुकान पर जाते हैं और हर लड़का 5 रुपये खर्च करता है। वे कुल कितना खर्च करते हैं?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
20.	अब मैं एक वाक्य पढ़कर सुनाऊँगा, उसे मेरे बाद दोहराइए *Now I will read a sentence, can you repeat it after me?* ***'मोहन का कुत्ता भाग रहा है'/ Mohan's dog is running***	a. वाक्य सही दोहराया/ Repeated the sentence correctly b. वाक्य ग़लत दोहराया / Repeated the sentence incorrectly c. कोई जवाब नही/ No response
	अब हम आपको एक कहानी सुनाना चाहते हैं? आप सुनोगे? **Good, now I will read you a short story, do you want to listen?** *एक पिल्ला खाट के नीचे सो रहा था	तभी आवाज़ सुनाई दी 'म्याऊ'	पिल्ले ने इधर उधर देखा,आवाज़ कहाँ से आई? तभी पास में एक चूहा कूदा, पिल्ले ने पूछा 'क्या तुम बोले 'म्याऊ'?' चूहा बोला, नही मैं तो 'ची ची' बोलता हूँ	फिर आवाज़ आई म्याऊ,तालाब के किनारे एक मेंढ़क बैठा था| पिल्ले ने मेंढ़क से पूछा, क्या तुम बोले 'म्याऊ'? नही, मैं तो 'टर टर' बोलता हूँ	फिर बिल्ली आई सामने, कूदके बोली, 'मैं बोली 'म्याऊ'!* **जो कहानी आपने सुनी उसमे क्या आप मुझे बता सकते हैं:** **From the story you just heard can you tell me?**	
21.	ची ची कौन करता है? Who makes a 'chi-chi' sound?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response

22.	बिल्ली क्या बोलती है? What sound does the cat make?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
23.	यह आपने कुत्ते और बिल्ली की कहानी सुनी. क्या आपने गाय देखी है? क्या आप गाय के बारे में कुछ बता सकते हो? You just heard the story of a dog and a cat. Have you ever seen a cow? Can you tell me something about a Cow?	a. दो पूर्ण वाक्य बोले / Spoke atleast 2 full sentences b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
24.	बहुत अच्छे, गाय जानवर है ना? क्या आप मुझे कोई भी एक पक्षी का नाम बता सकते हैं? Well done, a cow is an animal, correct? Can you name any one bird for me?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
25.	बहुत अच्छे!! क्या आपने स्कूल में अँग्रेज़ी सीखी है? Excellent! Have you studied English in your school?	a. हाँ/ Yes b. नहीं/ No c. कोई जवाब नही/ No response
26.	क्या आप मुझे बता सकते हो, आप Boy हो या Girl हो? Can you tell me, are you a boy or a girl?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
27.	अगर आपसे कोई पूछता है "What is your name?" तो आप क्या जवाब देंगे? Very good, if someone asks "What is your name?", what will you say to them?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
28.	बहुत बढ़िया, क्या आप मुझे कोई भी एक Vegetable का नाम बता सकते हैं? Well done, can you tell me the name of any one vegetable?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer कोई जवाब नही/ No response
29.	क्या आप जानते हैं कौआ को अँग्रेज़ी में क्या बोलते हैं? 'Crow' ना? अच्छा क्या आप Crow का स्पेलिंग बता सकते हैं? Alright, can you tell me the spelling of Crow?	a. सही जवाब / Correct answer b. ग़लत जवाब / Wrong answer c. कोई जवाब नही/ No response
30.	अच्छा एक आखरी चीज़, क्या आप A to Z बता सकते हैं? Can you tell me the letters from A to Z	a. बताया गया अक्षर / Enter letter till which students was able to tell the alphabet [Enter text] b. कोई जवाब नही/ No response