The Role of Affirmative Action in Enrollment, Test Scores, and School Quality: Evidence from India *

Monica Agarwal †
University of Wisconsin-Madison

September 14, 2023

Preliminary Draft. Please do not circulate.

Abstract

Worldwide, affirmative action policies are implemented as a means to promote social equity. India's Right to Education Act (RTE), one of the largest affirmative action policies in the world mandates all private schools to reserve 25% of incoming seats at entry-level grades for low socioeconomic status students. Despite being in existence for more than a decade, the effectiveness of this policy remains largely understudied. In this paper, I estimate the causal impact of RTE's 25% quotas on children's learning outcomes using a combination of rich administrative and survey data in a large state in India. I leverage the lottery based allocation of oversubscribed schools to identify the causal impact of being a beneficiary under this policy. I find that the policy improves children's English test scores by .18 SD via beneficiaries attending significantly better schools, and investing more time in educational activities. Furthermore, while the policy allocates children to private schools, there exists a large variation in school quality within the private sector. Motivated by the existence of this within-sector heterogeneity in quality, I uncover the distribution of effects within the private sector, and find that higher quality private schools boost English test scores by .5-.7 SD, relative to their lower quality counterparts. My findings are from a context when all learning is remote, and suggest that private schools, especially the ones at the upper end of the quality distribution, do a better job at adapting to, and implementing remote educational technologies, and in doing so, they also enhance children's learning.

*I am extremely grateful to Laura Schechter, Priya Mukherjee, and Jeff Smith for their invaluable guidance on this project. I also thank Moshi Alam, Brad Barham, Leah Bevis, Paul Dower, Tarun Jain, Ian McCarthy, Nishith Prakash, participants at the AAE Development Lab, and AMIE mentoring group for helpful comments and discussions. I thank the team at NEERMAN, Nandish Kenia, and the team of enumerators for conducting excellent fieldwork and phone surveys. I gratefully acknowledge funding support for this research by J-PAL's Post Primary Education Initiative and a Dissertation Research Grant from the Department of Agricultural and Applied Economics. Any errors are my own.

†Corresponding author: Monica Agarwal. Department of Agricultural and Applied Economics, University of Wisconsin-Madison. Email: monica2@wisc.edu

1 Introduction

Governments across the world implement affirmative action policies as a means to promote social equity. Such policies aim to redress long histories of discrimination against historically disadvantaged groups. The majority of such policies typically focus on later life stages of individuals, such as college admissions, or the workplace. However, how might individuals' life trajectories change if such disparities are reduced early in life? A growing literature suggests that reducing disparities early in life is important for the formation of cognitive skills and that later life interventions may be too late to achieve this in a cost-effective way (Cunha, Heckman and Schennach, 2010). As one of the world's largest affirmative action policy that targets children of school entry age, India's Right to Education Act (RTE) provides a unique opportunity to study this question.

The RTE mandates all private schools in India to reserve seats for disadvantaged children at entry level grades, with the goal of reducing segregation within classrooms. The scale of the policy is huge - in 2018-19 alone, the policy benefited approximately 4 million children, and has the potential to impact about 16 million children, if implemented nationally (Indus Action, 2019; Romero and Singh, 2023). As a direct effect, the policy improves accessibility to private schools for economically disadvantaged classes. Thus, a first order question is to study the effectiveness of this policy, or in other words, the impact of attending a private school under the policy.

In addition, the private schooling sector in India has been steadily growing and accounted for 45% of the primary grade enrollment in 2020.[1] Given the rapid growth in the market share of fee-charging private schools, both at the upper and lower end of the quality distribution, private school effectiveness is likely to vary within the private sector. This in turn, necessitates the importance of examining the distribution of effects *within* the private sector.

Hence, in this paper I study two main questions. First, I ask: does being a beneficiary under the RTE quotas, improve disadvantaged children's educational outcomes? Second, do the effects of this policy vary by the quality of private schools that beneficiaries attend?

I study the impact of this policy in the context of Maharashtra, the second most populous state in India. Under the policy, private schools are mandated to reserve up to 25% of the incoming seats at entry-level grades for disadvantaged children. Allocation of private school seats to applicants under the policy, is based on a lottery mechanism which ensures that applicants who submit the same school preferences, and live in

[1] World Bank data (2020)

the same neighborhood, have an equal chance of winning a seat at any given school that they listed in their application. Those who win entry to private schools under this policy are eligible to get tuition-free education from these schools till they finish grade 8, with the government reimbursing the schools up to a cap. The outside option for those who lose under the policy, is to either attend private schools of their choice as a fee-paying student, attend government schools (which are free of cost, but often of lower quality), or remain out-of-school. I use the feature of lottery-induced allocation of oversubscribed private school seats, to estimate the causal impact of the RTE policy on children's educational outcomes using an instrumental variables framework. Methodologically, I follow the recent methods by Abdulkadiroğlu et al. (2017) and utilize the within-variation in lottery outcomes of applicants who had a similar simulated ex-ante probability of winning the private school lottery under the allocation mechanism, which takes into the school preferences submitted at the time of application.

To do this I use the administrative data of the population of children who applied for grade 1 private school lotteries under RTE's 25% quotas, in the 2020-21 school year. I supplement this with phone-survey data which I designed and administered with a sample of applicant households, to collect detailed information on children's education, schooling, and performance on phone-based assessments in English and Math.[2] This gives me a sample of 2329 applicant households for whom I am able to construct a rich data of household characteristics, children's schooling, their performance on phone-based assessments, their time-use, parental investments, and school inputs.

The data, however, corresponds to the period of COVID-19 induced school closures. Like many low- and middle-income countries, pandemic induced school closures lasted for a long duration in India. While schools were closed for in-person instruction, the majority of schools transitioned to various forms of remote instruction (asynchronous and synchronous) at some point during the 2020-21 school year. Since the majority of current evidence on private school effectiveness is from in-person learning contexts, this setting provides me a unique opportunity to study whether private schools are effective when learning is remote. Thus, the findings in my paper are most relevant to the context of remote learning, however, it is worth noting that my results align closely with past evidence documented in comparable interventions, where the mode of instruction is in-person. I discuss this in more depth in the subsequent paragraphs.

My findings suggest that being a beneficiary under the RTE policy led to significant improvements in educational outcomes of children. One and a half years after ex-

[2] This assessment was adapted from the phone-based learning instruments used by Romero and Singh (2023) and Angrist et al. (2020).

posure to RTE, I find that quota children who won the private school lottery were significantly more likely to be enrolled in school in the two academic years 2020-21 and 2021-22.[3] The effect sizes are substantive - for compliers, the likelihood of being enrolled at any school increases by 13.3 and 4.6 percentage points, in 2020-21 and 2021-22, respectively. Given that primary school enrollment is near universal in India, these increases in enrollment largely reflect that the RTE was useful in insuring disadvantaged children against the risk of non-enrollment during a period of massive disruptions to learning. The gains however, are not just limited to enrollment, but also translate to gains in test scores of children - for the compliers, being a quota student at private schools improves performance in English by .18 SD (p-value < 0.05). There is evidence of suggestive gains in Math (by .14 SD), however, the impact on Math is not statistically significantly distinguishable from zero. Even though my findings come from a remote learning context, they are strikingly similar to prior estimates of private school effectiveness when learning happens in-person. Muralidharan and Sundararaman (2015) find gains of .12 SD units in English, but none in Math, for winners of private school vouchers in India after 4 years.[4] This suggests that private schools are effective not just during in-person settings, but also when learning is remote.

In order to interpret these results, it is helpful to learn about the composition of the counterfactual group. While treated compliers are a homogeneous group who attend private schools under the RTE quotas, the same is not true for the control compliers, since they have multiple outside options to choose from, such as, attending private schools as a fee-paying student, attending government schools, or being out-of-school. Looking at the extensive margin of the type of school being attended, I find that for compliers, the quota receipt increases the likelihood of attending a private school by 20 percentage points. However, this is over a base of 79% private school enrollment in the control group comprising non-quota students, which suggests that the outside option for those who lose is not necessarily to attend government schools. Following Abdulkadiroğlu et al. (2014), an analysis of *counterfactual destinies* for control compliers highlights that about 65% of lottery losers end up at private schools as fee-paying students, and only about 20% end up at government schools. The fact that attending private schools as fee-paying students happens to be the fallback option for the major-

[3]Applications for private school admissions under the RTE 25% quotas were made for the 2020-21 academic year, and I conducted phone-surveys with a sample of these applicants during the middle of the following academic year i.e., 2021-22. This allows me to study their enrollment decision in these two academic years.

[4]Singh (2015) finds effect sizes of similar magnitudes using value added estimates in the same state in India - Andhra Pradesh. Romero, Sandefur and Sandholtz (2020) study the impact of allocating private management bodies to existing government schools in Liberia, and find gains of .13 SD in language. Using data from a different state in India (Chhattisgarh), and in an earlier version of their paper, Romero and Singh (2023) look at the impact of being RTE quota student on test scores and find gains of .19 SD in foundational numeracy and literacy skills.

ity of lottery losers, highlights the aspect of regressive selection within eligible groups and inframarginality in program spending (Romero and Singh, 2023).[5]

Next, I explore three broad mechanisms to understand the channels through which gains in children's outcomes are realized - school inputs, parental inputs, and children's own time use. I find that school inputs, and children's own time use are the main channels that explain these gains. There is some evidence of parental monetary and time investments increasing as a result of winning the lottery, however, the effect sizes are small, suggesting that parental inputs explain only a small part of the story. Explaining the mechanisms in details, I find that conditional on being enrolled in school, quota students are more likely to receive remote instruction from their school in both the academic years - by 7 and 3 percentage points in 2020-21 and 2021-22, respectively. The magnitude of these effects reflect that schools being attended by treated compliers were significantly more efficient in adapting to remote learning during the period of school closures. In addition, they were significantly more likely to receive synchronous online modes of instruction (by 13.6 percentage points), relative to the non-quota students who were more likely to receive asynchronous modes like text-based communication via WhatsApp, and pre-recorded audio and video clips. While these outcomes are more reflective of school characteristics that might specifically matter during the periods of remote instruction, I also examine the impact of winning the RTE private school lottery on the overall quality of school being attended, which is reflective of quality in business-as-usual settings. I create a school quality index using Principal Component Analysis (PCA) that combines information on school infrastructure, digital facilities, and teacher qualifications, and find that relative to the non-quota students, quota students attend schools that are .6 SD units better in their overall school quality index. Quota students are also more likely to be enrolled in schools that have English as the primary language of instruction, teach more number of subjects, and have a longer school week (by 3 hours/week).

Next, I uncover heterogeneity within the private sector to examine if there are gains from attending higher quality schools within the private sector. I start with a simple case of defining schools as *elite* or *budget* based on two alternate measures of school quality. Focusing on the group of beneficiaries who won the RTE private school lottery, I leverage the randomization in lottery offers at elite private schools to compare the outcomes of ex-ante similar children who had a similar ex-ante probability of winning at elite private schools, but face a randomization in winning the lottery at elite

[5] Using RTE applications data from the state of Chhattisgarh in India, Romero and Singh (2023) find evidence of regressive selection under RTE by relatively better-off households among eligible groups. They find that 50% of the applicants who lose the RTE lottery for their top choice private school, end up attending the same school as a fee-paying student. They show that only 7.4% of the program spending under RTE quotas accrues to the bottom socioeconomic quintile, compared to 24.3% in the top quintile.

versus budget private schools. Like before, I implement this using an IV-2SLS framework which uses lottery offers at elite schools as an instrument for enrollment at elite schools as quota student, and utilizes the within-variation in lottery outcomes of children who have a similar simulated ex-ante propensity of winning the elite private school lottery, given their school preferences (Abdulkadiroğlu et al., 2017). The first measure of school quality or school eliteness is created using administrative data on school's annual fee (that it charges to fee-paying students), and the second measure is based on a PCA based school quality index that I create using a variety of school characteristics. The two measures of school quality show substantial positive correlation suggesting that schools that are elite based on the fee-measure are also likely to be elite based on the PCA-index measure. I find that attending elite private schools significantly improve English test scores by .48 SD (when eliteness is defined using school fee), and by .69 SD (when eliteness is defined using school quality index). However, as before there are no statistically significant impacts on Math.

As before, I examine potential mechanisms and find that while both elite and budget private schools were equally likely to provide remote instruction, however, elite schools are more likely to provide synchronous online instruction (by .10 - .18 percentage points), and provide longer hours of class instruction (by 2.1 - 3.1 hours/week). Relative to budget private schools, elite schools are no more likely to teach conventional subjects (Math, English, etc.) but they are significantly more likely to teach additional subjects like general knowledge, arts/crafts, music and dance. In order to further understand why remote learning might be more effective for elite schools, I compare baseline characteristics of elite and budget schools, and find that elite schools are significantly more likely to be equipped with better digital technologies (for instance, access to internet; higher per-pupil quantities of laptops, desktops, and digital boards), employ teachers with higher qualifications, and teachers trained in computers. Another stark difference is in the caste composition of students attending elite and budget private schools. Elite schools are likely to have a significantly less diverse student composition, and are likely to have significantly lesser proportion of children from disadvantaged caste categories. These differences provide additional evidence of heterogeneity within the private schooling sector, which might further explain differences in school effectiveness across elite private and budget schools, especially during periods of remote learning.

Taken together, my findings suggest that private schools attended by quota students were more effective in the delivery of remote schooling inputs, and enhanced children's learning during the period of school closures. They increased students' accountability by holding regular synchronous classes, providing student-teacher interaction, and keeping them engaged with school activities for longer hours per week. In addition, I find evidence of substantial heterogeneity within the private sector. Elite schools that levy high annual fee, and have better overall school characteristics, are significantly better in providing remote instruction and increasing student test scores. This is in line with recent evidence from (Andrabi, Bau, Das and Khwaja, 2022) who find similar evidence of within-sector heterogeneity in Pakistan, in the context of in-person learning. Furthermore, the results that look at the impact on school quality suggest that private schools are likely to be effective not just during the time of remote instruction, but also in business-as-usual settings, when learning is in person. Given that my data correspond to the period of remote learning, I am unable to test this formally, however prior evidence of private school effectiveness provides findings in support of this (Muralidharan and Sundararaman, 2015; Romero and Singh, 2023).

My contributions to the literature are threefold. First, I contribute to the literature on affirmative action in education. There is a large literature on affirmative action that looks into targeting, mismatch hypothesis, short-term and long-term impacts on the beneficiaries, and cost-benefit analysis, however, most of this work focuses on affirmative action in college admissions.[6] I add to this literature by studying one of the world's largest affirmative action policies that targets children of school entry age, when issues surrounding academic mismatch and fairness in admissions criteria are less of a concern (Romero and Singh, 2023). While this policy has been around for more than a decade, there is very little evidence on its effectiveness, partly because of the recent shift toward centralized admissions, which in turn has facilitated proper record keeping, and access to data. The only other papers that have studied the impact of the RTE quotas on children's outcomes include Damera (2018) and Romero and Singh (2023). I add to this literature by examining a host of mechanisms such as, school quality, parental monetary and time investments, and children's time use (both on the extensive and intensive margin) that might better explain the channels behind gains in children's outcomes. I also contribute by conducting a detailed analysis of the counterfactual destinies of the control compliers, which is useful for the interpretation of the causal effects. Finally I provide evidence from the state of Maharashtra, where the policy implementation rules around the allocation mechanism are very different from the allocation mechanisms implemented in most other states. This is important because the welfare effects of school choice also depend on the allocation mechanism.

Second, I contribute to the extensive literature on school choice, private schools, vouch-

[6] Some examples of this comprise works by Arcidiacono and Lovenheim (2016); Bagde, Epple and Taylor (2016); Bertrand, Hanna and Mullainathan (2010); Bleemer (2022); Card and Krueger (2005), and Khanna (2020).

ers, public-private partnerships in education, and education policies in general.[7] In the US, a vast majority of research on school choice focuses on studying the effectiveness of charter schools, which have been found to improve learning outcomes of disadvantaged students (Cohodes, Setren and Walters, 2021). In low- and medium-income countries, the debate surrounding private schools revolves around concerns of economic stratification and weakening of public schools caused by fee-charging private schools, and potential ways to curtail this, for example, by promoting voucher-like models using public-private partnerships (Glewwe and Muralidharan, 2016). Literature on relative impact of public and private schools provides mixed evidence.[8] I contribute to this literature by studying lottery based admissions to private schools through India's RTE policy. My paper provides one of the first estimates from a remote learning context, which offers a unique opportunity to understand private school effectiveness in the context of remote learning, since most of what we know so far about private school effectiveness is from in-person learning contexts.[9] To the best of my knowledge, I provide the first estimates of the distribution of effects within the private sector in the Indian context, and the closest study to do this in a similar context is by Andrabi, Bau, Das and Khwaja (2022) who use value added models and find substantial heterogeneity within the private and public schooling sectors, in Pakistan.[10]

Third, I contribute to the growing literature on learning loss due to school closures, and ways to mitigate these losses using remote education and technology interventions. A growing number of studies have estimated large learning losses among school going children, as a result of the pandemic induced school closures, and recommend post emergency programs (Azevedo, Hasan, Goldemberg, Geven and Iqbal, 2021; Guariso and Björkman Nyqvist, 2023). Another set of studies look at remote technology interventions on mitigating learning loss (Angrist, Bergman and Matsheng, 2020;

[7]Glewwe and Muralidharan (2016) provide a review that synthesizes research on education policies combining various country contexts.

[8]With the exception of null impacts of private school vouchers on children's learning in Chile (Hsieh and Urquiola, 2006), most other studies find positive impacts of private schools on learning - PACES program in Columbia (Angrist, Bettinger, Bloom, King and Kremer, 2002; Angrist, Bettinger and Kremer, 2006), private school vouchers in Andhra Pradesh (Muralidharan and Sundararaman, 2015), school value-added in Andhra Pradesh (Singh, 2015). Specifically in the context of India, Muralidharan and Sundararaman (2015) find that private schools achieve these gains at a substantially lower cost per student making it more cost-effective.

[9]A related paper is by Crawfurd, Evans, Hares and Sandefur (2023), who randomize primary school students in Sierra Leone to receive phone tutoring calls from public or private school teachers during the period of COVID-19 school closures. The teachers supplemented government provided radio instruction, but the intervention did not increase children's test scores, whether provided by private or public school teachers. They attribute this non-impact to limited take-up by children.

[10]Prior evidence on heterogeneity within schooling sectors from other country contexts provides mixed evidence - Pop-Eleches and Urquiola (2013) and Kirabo Jackson (2010) find positive impacts of attending a better school in Romania, and Trindad and Tobago, respectively. Whereas, Abdulkadiroğlu, Angrist and Pathak (2014); Dobbie, Fryer et al. (2011) and Cullen, Jacob and Levitt (2006) find no additional gains on test scores as a result of attending elite and high performing schools in the US.

Carlana and La Ferrara, 2021; Mukherjee, Beam and Navarro-Sola, 2021). One such study is in the context of Tamil Nadu, India, by Singh, Romero and Muralidharan (2022), who study a government-run after-school remedial program in Tamil Nadu, India, and find that it was successful in recovering two-thirds of the learning loss in primary school-aged children. I add to this literature by providing evidence of how well-implemented affirmative action policies can act as a safety net for the disadvantaged, during times of severe economic disruptions. In particular, I provide evidence that the policy insured vulnerable children against the risk of non-enrollment, and maintaining grade progression, and at the same time improved their learning outcomes.

The rest of this paper is structured as follows. Section 2 describes the policy and context (RTE quotas in Maharashtra, and the lottery algorithm); Section 3 describes the data sources (administrative data, and primary data collection) and sampling strategy; Section 4 describes the empirical strategy and also talks about balance, attrition, and external validity; Section 5 discusses results and mechanisms; Section 6 discusses the within-private sector heterogeneity; Section 7 talks about robustness checks, followed by Appendix tables and figures at the end.

2 Background and Policy

The Right to Education (RTE) Act was enacted by the Indian government in 2009, and made education a fundamental right of every child aged 6-14 years. I focus on a specific Clause 12(1)(c) of this act under which all private schools in India are mandated to reserve at least 25% of the seats in entry-level grades for children belonging to low socioeconomic (SES) families.[11] Children who get admitted to private schools under this policy are eligible to get free education from the respective schools till they complete grade 8. The government reimburses private schools to cover the school's tuition fee for children admitted under the quota. Children admitted under this quota are also eligible to get free textbooks and uniforms from the respective schools but the enforceability of this varies across states and schools. These quotas were motivated in part due to the rapid increase in fee-charging private schools. Fee-charging private schools accounted for a total of 5.8% enrollment in rural India in 2002 (Kingdon, 2007), and in more recent years, this has shot up to about 31% primary school enrollment in rural areas, and 50% in urban areas (Pratham, 2019). Due to the rapid growth in demand, there were growing concerns about the rise in segregation within classrooms with the well-off moving to private schools, and the relatively worse-off being in the

[11] Religious and linguistic minority schools are exempted under the RTE Act. Entry level grades comprise grade 1 and pre-primary grades (for example, nursery or kindergarten).

government schools (which are free of cost). Thus, one of the goals of these quotas is to desegregate classrooms on the basis of socioeconomic status and improving access to quality schooling for all. The quota requirement has been met with restrain across states, and while it was adopted by several states over the years, the policy remains unimplemented in several states (Romero and Singh, 2023).

2.1 RTE quotas in Maharashtra: context and lottery mechanism

2.1.1 Private school quotas in Maharashtra

I study the impact of this policy in the context of the second most populous state in India, Maharashtra. Maharashtra adopted this policy starting 2010 and the eligibility criteria includes children from historically disadvantaged caste groups, low income background, and children with disability.[12] The government reimburses schools for each child who is enrolled under this policy by sponsoring the school fee up to a certain limit and schools are not allowed to charge any fee to the quota students.[13]

2.1.2 Online applications

Maharashtra adopted a centralized online application system under this policy, starting the academic year of 2017-2018. The online application to apply to schools under this policy begins in the month of February and is open for a month, following which the allocation of students to schools begins based on a centralized lottery algorithm. The majority of schools in the state follow the June to April school year.[14] The process of online application includes filling out the child's details along with household characteristics, for example child's name, date of birth, gender, and household characteristics like religion, caste and income (if applying under the low income quota). The most important information that is filled out is the house address details, after which the system generates a list of all private schools available under the policy, in the child's neighborhood in three distance bins - all schools available within 1 km radius of the house address, within 1-3 km of the house address, and beyond 3 km of the

[12] Historically disadvantaged castes include Scheduled Castes, Scheduled Tribes, and Other Backward Classes (OBC). Low income families are defined as those earning less than INR 100,000 per annum ($4746 in PPP). In my administrative data for the year 2020-21, the majority of applications were received under the low income and disadvantaged caste category. Applications received under the disability category comprised 0.6% of the total applications.

[13] The reimbursement received by schools is equal to the value determined using the smaller of the these two amounts: school fee charged to fee-paying students, or the upper cap set by the government based on per-pupil expenditure in government schools in the state. The reimbursements have to be borne by centre and state governments in 60:40 ratio. The policy has been slightly controversial since private schools may choose not to comply with RTE quotas if their fee levels exceed the reimbursement limits. As of year 2020-21, the per child reimbursement under RTE in Maharashtra was capped at INR 17,640 per annum (approximately 213 USD).

[14] A small number of schools follow the May to March school year.

house address (within the district). This is an important detail of the application process, which I come back to in my estimation strategy. Parents are allowed to choose a maximum of ten schools combining all three distance bins, but they cannot rank schools in order of their preference. They are also required to indicate the eligibility criteria which could be any one of these: low income category, disadvantaged caste category, or child disability category. Finally, parents sign an online declaration which says that in the event of winning a seat, parents are required to show a proof of house address (which must match the address reflected in the online application) and a valid proof that establishes their eligibility criteria under this policy. According to the rules, admission at allotted schools is guaranteed conditional on the house address documentation and other eligibility proofs being valid.[15] Importantly, the declaration states that the documents must be genuine, and in the case that any documents are found to be false or counterfeit, it may lead to charges of monetary penalty and cancellation of the admission offer.[16] Since the policy is targeted towards disadvantaged households, help centers are organized during the weeks of the online application window (oftentimes in schools, and community centers) to specifically assist interested households with filling out the online application and answer questions. Similarly, in the weeks leading upto the start of the online application, the policy is advertised through notifications and billboards outside school premises, community centers, and local newspapers.

2.1.3 Lottery algorithm

States have considerable autonomy in how they implement the RTE quotas. Thus, the lottery mechanism that determines the allocation of students to schools under this policy also varies across states. In Maharashtra, it is designed such that each school assigns the highest priority to applicants who reside and applied in the nearest distance bin of the school (within 1 km radius of school, henceforth, distance bin 1), followed by those who reside and applied in the next distance bin (within 1-3 km radius of the school, henceforth, distance bin 2), followed by those who reside and applied in the farthest distance bin (beyond 3 km radius of the school, henceforth, distance bin 3). Hence, the overarching goal is to allocate applicants to schools which are closer to their house address. Importantly, parents are not allowed to submit rank ordered lists and can choose a maximum of ten schools. The lottery mechanism is a two part process where the first part involves determining applicants who end up winning at a school,

[15] This could be an income certificate, caste certificate, or disability certificate based on whether the eligibility condition chosen is low income category, disadvantaged caste category, or disability category. Disability quota is very rare and constitutes only 0.6% of the applications in the population.

[16] In the administrative data I see that 0.6% of the admission offers were cancelled ex-post, due to false or improper documentation.

and the second part involves determining applicants who end up being waitlisted at a school. Applicants who are neither winners, nor waitlisted by the end, are those who lost at each and every school they applied to. The end result is that each applicant has one final lottery outcome which is tied to a unique school - they are either a winner at a unique school, or, waitlisted at a unique school (with a waitlist priority), or, have lost everywhere. In other words, if an applicant is a winner then they only won at one unique school; if they are waitlisted, then they did not win anywhere, but were waitlisted at one unique school; if they are neither a winner, nor waitlisted, then they lost at each and every school they applied to. The detailed mechanism is explained in Appendix Section B.1

2.1.4 RTE School lotteries in Maharashtra, 2020-21

My administrative data corresponds to the universe of applications made under the RTE Act, for private school admissions in the academic year of 2020-21. Private school lotteries in the state were extremely competitive in the 2020-21 academic year. A total of 8848 private schools across the state participated in RTE quota admissions, and received applications from 291,365 children. Of these applicants, 35% won, 39% lost and 26% were waitlisted. Most applications were made under the disadvantaged caste category (63.5%), followed by low income category. Since the applications under the RTE school lotteries were open only till the end of February 2020, the decision to apply to these school lotteries was made before the COVID-19 pandemic hit India (early March, 2020). However, the decision to take admission (in the event of winning a seat) is likely to have been disrupted due to the nationwide lockdown which was imposed in mid-March and thus unexpectedly coincided with the time when schools were offering admissions.[17]

3 Data

My data comes from four sources. First is the administrative data, which gives me details of the universe of children who applied to private school lotteries for grade 1 under the RTE quotas, in the entire state of Maharashtra, for the academic year of 2020-2021. Second is the phone-survey data, which I collected during the months of Nov-Dec 2021, by contacting a sample of households who applied to these lotteries (using the phone number provided by the household in their RTE application). Third, I use the U-DISE (Unified District Information System for Education) data which contains the administrative data of school characteristics of the population of schools in India. I

[17]Because of the nationwide COVID imposed lockdown beginning 24 March 2020, RTE admissions continued to be open till the month of December, 2020. Parents were offered the flexibility to complete the admission formalities either remotely or in-person.

use data from the 2019-2020 school year as that contained the most recent information on school characteristics prior to the RTE applications. Finally, I use the administrative data on annual school fee for all RTE private schools in the state of Maharashtra from the 2019-2020 school year.

3.1 Administrative data of RTE quota applications

This data provides the details of the universe of applicants who applied for grade 1 private school lotteries in the state of Maharashtra for the academic year of 2020-2021. These were publicly available at the Maharashtra's Education Department website. This gives me detailed information on the children who applied to these lotteries and for each child who applied, there was information about the child's name, child's date of birth, parents' name, parent's contact number, house address, religion, caste, household income, list of private schools chosen by the applicant in the three distance bins (within 1km, 1-3km, beyond 3km), and the distance of each school to the house address of the applicant. For each child who applied, there was detailed information about their lottery outcome and how it evolved over time. To be precise, for every child who applied, there was data on the initial status of the application - whether their application was selected, wait-listed, or not-selected anywhere. Each child could only have one of these statuses to begin with.

To explain this in further details, if a child's application status was declared as selected, then it meant that they had won a seat at one of their preferred schools (if they win, they only win at one school and are excluded from all other schools that they had indicated); if the application status was wait-listed, then it meant that their application was wait-listed at just one of their preferred schools, and they were in the consideration set for admission to this school if a previously selected candidate forgoes their seat (each wait-listed child would get a wait-list priority number such that priority number of 1 would mean that this child would be the next in line for admission, if a vacancy was created at this school. This child was also excluded from all other schools if they had applied to multiple schools); if a child's application was not-selected anywhere, then it meant that they were neither selected, nor wait-listed at any school that they had indicated in their application. Over time, the status of the application of a child evolves, and for each selected application, there is data on whether the child formally secured admission to the private school that was allotted to them and the corresponding date on which admission was secured (some students forgo their admissions and this creates vacancies for wait-listed children); for each wait-listed child, whether this child was finally admitted to the school that wait-listed them and if so, when did they secure admission to the respective school.[18]

[18]The status would evolve over time and the website put a notice of the deadlines by which selected

3.2 Primary survey data collection

I conducted phone-surveys with a sample of applicants during the months of Nov-Dec 2021, to collect a rich data on children's outcomes, and household characteristics. A total of 4259 applicant households were contacted during this period, and successful interviews were completed with 2329 households (response rate of 55%). For each successful interview attempt, I also conducted a short interview with the applicant child to collect data on their learning outcomes in English and Math.[19] Among the full sample, a total of 695 households provide data on children's learning outcomes.[20] Response rates among winning and non-winning applicant households was about 57.7% and 52.4%, respectively. I discuss about attrition and non-response bias in Section 4.2 and find that my results are robust to differential attrition, using inverse probability reweighting.

3.2.1 Sampling strategy

To select the sample of applicant households for conducting phone-surveys, I desgin a sampling strategy. It is carefully designed to select a sample of comparable winners and losers under the policy, who are otherwise ex-ante similar in their household location and the school preferences that they listed in the RTE application.

The most ideal comparison would involve comparing winners and losers who had the same school preferences by each distance bin, to begin with (as indicated at the time of submitting the online application). However, full stratification of applicants based on their distance bin-specific school preferences eliminates many schools and students from consideration (Abdulkadiroğlu, Angrist, Narita and Pathak, 2017).[21] In order to remedy this, I pick my sample such that the applicants who win and lose the private school lottery are comparable to one another to the extent that they made the same school choices in the *nearest distance bin*, i.e., schools chosen within 1 km radius

candidates must approach their allotted schools to secure admission after which their admission would be null and void. Similar notices were put for the waitlisted candidates along with their priority numbers, and the process would continue to extend to candidates with lower priorities, until all seats were filled. Candidates were also sent SMS notifications about the deadlines on their registered contact numbers.

[19]The questions to test children on phone-based assessments come from Romero and Singh (2023) and Angrist, Bergman and Matsheng (2020) and are designed to capture foundational language and numeracy skills. The questions administered with children are showed in Figure A3 in the Appendix.

[20]To minimize non-response bias, the following rule was followed for calling households - each household was attempted to be called up to five times before discarding that number. The protocol was to attempt to call each household once during: the morning, afternoon and evening of a weekday; once on a Saturday, and once on a Sunday.

[21]The most ideal comparison would involve comparing children who differ in their lottery outcome but indicated the same school choice in each of the three distance bins, as this takes care of their endogenous choice of schools, household location, and their ex-ante likelihood of winning entry into schools as determined by the lottery algorithm. However, implementing this is difficult in practice given the high dimensionality of possible school choices over the full population of applicants.

of the house address; or, in other words, had chosen the same *school vector* in the nearest distance bin.[22] This in turn facilitates the comparison of winners and losers under the policy, who were ex-ante similar in their school preferences in the nearest distance bin and resided in the same geographic location. An important point to note is that the sampling strategy is designed to take into account only those schools which were *oversubscribed*, i.e., schools that conducted lotteries to admit applicants, and those applicants who were subjected to lotteries. This is a limitation in studies that rely on lottery based designs since oversubscribed schools may differ from undersubscribed schools, which in turn makes it hard to generalize the findings. The strategy is explained in details in Appendix Section B.2, and a schematic flowchart for the same is given by Appendix Figure B2.

3.2.2 Summary statistics

Table A1 summarizes the characteristics of applicants in the phone survey and also shows the key variables associated with the applicants and their household characteristics. The average applicant is about 7.6 years old at the time of interviews, slightly more likely to be male, and applied to about 5 schools in the RTE application. Some instance of non-enrollment exists in both the academic years, however, there is improvement in enrollment rates in 2021-22, with the easing of pandemic related restrictions. Conditional on school enrollment, there is variation in the likelihood of schools providing instruction. The average applicant is more likely to come from Hindu households, and the Other Backward Class (OBC) group, and also likely to have more educated mothers than fathers. Several other variables are summarized, such as monetary and time investments in children, their time use, and performance in phone-based assessments and these comprise my outcome variables.

3.3 Administrative data of school characteristics

To get at the characteristics of the school being attended by each child in the sample, I use publicly available data on school characteristics from U-DISE for the 2019-2020 school year. This data constitutes the population of all private and public schools in India and has rich information on schools. I use this data to construct one of my two measures of school quality. I create a school quality index using Principal Components Analysis (PCA) using data on school infrastructure details, digital facilities, teacher quality, and peer composition. I explain this in more details in Section 6.1.

[22] Throughout the paper, I frequently use the term *school vector* to refer to a unique combination of schools chosen in distance bin 1.

3.4 Administrative data of school fee

I use administrative data on school fee for all the RTE private schools in the state which participated in the RTE lotteries in the 2020-21 year. The data comes from the official website of the State Department of Education, Maharashtra and reflects school fee for the 2020-21 year. This data is used in creating the second measure of school quality, where I define schools to be elite based on the annual fee charged. I explain this in more details in Section 6.1.

4 Empirical Strategy

Using the administrative data of applicants who applied for private school admissions for grade 1 under the RTE quotas in the academic year of 2020-21, my goal is to estimate the impact of enrolling in a private school as a quota student on children's educational outcomes. The treatment group comprises the beneficiaries under the policy i.e., those who are enrolled as RTE quota students in private schools and the control group comprises non-quota students who may be attending private schools (as fee-paying students), or government schools (free of cost), and those not enrolled anywhere.

There are two endogeneity concerns here, and I address both of them. First, schools selected at the time of submitting the application are endogenous, and second, conditional on winning, the decision to enrol as a quota student is also an endogenous choice. Both these choices might correlate with unobserved household characteristics which might be simultaneously correlated with children's outcomes. I address both these concerns by using a conditional instrumental variables strategy. The idea is that, given the lottery algorithm, conditional on the school choices listed in the application, winning the lottery to a private school is random.[23]While conditioning on the school choices listed in the application solves the endogeneity in unobserved preferences for schools, the second endogenity problem is solved by instrumenting quota enrollment with the indicator of winning the lottery, which in turn is random conditional on controlling for the school choices that were listed in the application. Thus, I estimate the

[23]This follows from the lottery algorithm which satisfies the Equal Treatment of Equals (ETE) property (Abdulkadiroğlu, Angrist, Narita and Pathak, 2017). ETE is said to satisfy when students with the same preferences and priorities have the same chance of getting allocated at any given school. If the object of interest is winning a lottery at a school chosen in distance bin 1, then ETE is satisfied each time there is a group of applicants who had listed the exact same schools in distance bin 1. If the object of interest is winning a lottery at a school chosen in distance bin 2, then ETE is satisfied each time there is a group of applicants who had listed the exact same schools in distance bin 1, and distance bin 2. Finally, if the object of interest is winning a lottery at a school chosen in distance bin 3 or, winning a lottery at *any* school in any of the three distance bins, then ETE is satisfied each time there is a group of applicants who had listed the exact same schools in each of the three distance bins.

local average treatment effect of being enrolled as a quota student on children's outcomes in an instrumental variables framework.

As I explain in the previous section, given the high dimensionality of school preferences, my sampling strategy is designed such that I can condition for the vector of schools chosen in bin 1 and compare applicants who are similar to the extent that they had the same school preferences in bin 1. Conditioning on the vector of schools chosen in bin 1 is one way of addressing the endogeneity in school preferences listed at the time of the application. However, note that given the lottery algorithm and the ETE property, the relevant instrument to be used in such a case is winning the lottery in distance bin 1, which in turn means that the causal effect is estimated for compliers, defined by those who attend private schools as quota students because of winning the lottery in bin 1, and those who don't because they lost lotteries at bin 1 schools. On the other hand, if the instrument is winning the lottery in *any* distance bin, that leads to a much more interesting composition of compliers, i.e., those who are quota students because of winning the lottery in any bin, and those who are not quota students because of losing the lottery in all bins.

Such an estimation can be estimated by conditioning on the simulated ex-ante propensity scores of winning the private school lottery (Abdulkadiroğlu, Angrist, Narita and Pathak, 2017). This strategy is useful because it helps reduce the dimensionality of preferences and does not require to explicitly control for the school preferences made at the time of application. The idea is the following - taking the distance bin specific school preferences of applicants as given, one can simulate the lottery algorithm a large number of times to arrive at the simulated ex-ante likelihood of winning the private school lottery, for each applicant. Since the simulated likelihood or propensity scores take into account the school preferences that were listed by the applicant, hence, controlling for these propensity scores essentially performs a similar function as is achieved by explicitly controlling for the full set of schools chosen at the time of application. Since the goal is to estimate the LATE of being enrolled as a quota student under the RTE policy, the identifying assumption in this estimation strategy is that winning the lottery to a private school is conditionally exogenous after controlling for the ex-ante propensity scores of winning the lottery. Below I discuss the implementation of this strategy which is my preferred specification.[24,25]

[24] I discuss the calculation of these propensity scores in Appendix Section B.3. I also show the distribution of these ex-ante propensity scores (Appendix Figure A2). Appendix Table B1 shows the detailed distribution of simulated propensity scores for the full population, and the sample.

[25] This strategy is powerful to deal with issues of stratification and sampling such as the one caused by fully stratifying applicants on the basis of their distance bin specific school preferences. It relies on comparing winners and losers who had a similar ex-ante likelihood of winning and does not require them to have chosen the exact same set of schools, thus bypassing some of the power-issues which may occur if comparisons are based on controlling for school fixed effects.

Following Abdulkadiroğlu, Angrist, Narita and Pathak (2017), my preferred estimation strategy involves controlling for the vector of dummies of narrow bins of ex-ante propensity scores of winning a lottery in any distance bin.[26] This strategy relies on comparing the winners and losers of private school lotteries, who had a similar ex-ante propensity of winning the lottery to an RTE private school (in any distance bin). This exploits the within-variation that results from comparing winners and losers who had a similar ex-ante propensity of winning any private school lottery, and does not require them to have chosen overlapping sets of schools. I estimate this using a two-stage least squares (2SLS) procedure, where the first stage is the effect of a random assignment of a private school seat on enrollment, and the second stage estimates the impact of quota enrollment on student outcomes.

I estimate the following equations via 2SLS:

$$RTE\_Enrolled_i = \alpha_1 WinningLotteryAnyBin_i + X_i'\alpha_2 + \sum_{x=1}^{100} \gamma_x d_i(x) + \epsilon_i \tag{1}$$

$$Y_i = \beta_1 \widehat{RTE\_Enrolled_i} + X_i'\beta_2 + \sum_{x=1}^{100} \gamma_x d_i(x) + e_i \tag{2}$$

where, $d_i(x)$ are dummies taking a value of 1 if child i's estimated propensity score of winning a lottery at a private school in any bin lies in the respective 0.01 wide probability bin, X_i is the vector of child and household characteristics like sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. These covariates are added only to increase the precision of my estimates and the results are robust to excluding these covariates. The coefficient of interest is given by β_1, which captures the LATE of attending a private school as a quota student on child outcomes, for the compliers. The compliers are those who attend private schools as quota students because they won the lottery to a private school (in any bin), and those who are without a quota because they lost the lottery at all schools that they listed in their application, and may be attending private schools as fee-paying students, or government schools (or may be out-of-school).

For some of the surveyed households, responses on certain conditioning variables are sometimes missing. Instead of a listwise deletion of observations that have missing values for covariates variables, I re-code missing values of covariates to their mean value in the sample and control for these re-coded covariates, and a separate missing

[26] In the Appendix, I present results that condition on the school vector chosen in bin 1, and compare these results to the case which conditions on the simulated ex-ante propensity of winning in bin 1. Table A9, A10, A11 show the results for the main outcomes. The two specifications produce very similar results thus providing confidence in the fact that conditioning on simulated ex-ante propensity scores performs a similar function as is achieved by conditioning on the school fixed effects.

value indicator in all the specifications. Listwise deletion of observations missing any of the conditioning variables would mean non-randomly dropping a substantial fraction of the sample (King, Honaker, Joseph and Scheve, 2001; Black, Smith and Daniel, 2005).

4.1 Balance

I test for balance across winning and non-winning applicants to examine if they are similar on baseline observed characteristics. Table A2 presents the results, by conditioning on the ex-ante propensity of winning at any bin. The majority of the characteristics are balanced across the two groups, with some exceptions - for example, father's education, religion, and household SES index. This suggests that the winners and non-winners are modestly balanced on baseline observed characteristics.

4.2 Attrition

A concern that could potentially bias estimates is whether there is a selection into who agrees to be a part of the phone surveys. For example, if winners were more likely to participate in the survey, and at the same time also benefited from the quota seat, then this could bias the effect sizes in the upward direction. Table A3 shows whether there is a selection into participation in phone surveys based on observable characteristics of households at baseline, after conditioning on the ex-ante propensity of winning in any bin.[27] The table shows this for household's participation in phone surveys, and for household's participation in phone-based assessments with the applicant child, conditional on being part of the phone surveys. As can be seen from Panel A, attrition is slightly unbalanced - winning applicants were 5.7pp more likely to agree to be interviewed relative to the non-winning applicants. However, there is no systematic attrition by winning status, on participation in child assessments, conditional on survey participation. I test for robustness of my results on the main outcome (phone-based assessments), using inverse-probability reweighting to account for differential attrition (Table A19).

4.3 External validity

My results are based on a lottery-based research design. While lottery-based estimates help in removing selection bias, there are several challenges with this design. First, these estimates are specific to oversubscribed schools, which might be different from

[27]Results are robust to conditioning on school vector fixed effects of winning in bin 1 or ex-ante propensity of winning in bin 1.

undersubscribed schools. For example, oversubscribed schools might be overrepresentatitve of urban areas, relative to rural areas.[28] Second, it relies on applicants who faced lotteries to get admitted to schools, a group that may differ from nonapplicants (Angrist, Cohodes, Dynarski, Pathak and Walters, 2016). Third, the LATE identifies a treatment effect only for compliers which is a very specific sub-population of the treated (Black, Joo, LaLonde, Smith and Taylor, 2022). Nevertheless, Kline, Rose and Walters (2022) show that LATE is the policy-relevant parameter in case of a marginal increase in the number of available seats among lottery applicants (Angrist, Hull and Walters, 2022). I discuss the issue of external validity in more detail in the next Section 4.3.1 where I discuss complier characteristics - these can provide a partial guide to external validity of in context of lottery-based IV estimates (Angrist, Hull and Walters, 2022).

4.3.1 Characterizing Compliers

The instrumental variables strategy identifies a unique causal parameter, which is specific to the sub-population of compliers for that instrument. Different valid instruments for the same causal relation therefore estimate different things, because the compliers are essentially different based on the instrument (Angrist and Pischke, 2009). Since the IV identifies the average treatment effect for the compliers, it is a useful exercise to learn more about the characteristics of the compliers. Another important reason to study complier characteristics is that they can provide insights about external validity of a set of lottery-based IV estimates (Angrist, Hull and Walters, 2022).

I use Angrist et al. (2022)'s implementation of the methods discussed in Abadie (2002), to compute complier characteristics. Table A4 shows the differences in baseline characteristics of the compliers, always- and never-takers in Maharashtra's RTE lottery. The table shows the mean of baseline characteristics for each of these groups (see Appendix Section C.1 for details on implementation). Untreated and treated compliers are very similar across all characteristics as shown in columns (1) and (2). Colums (3) and (4) show the mean characteristics for always- and never-takers. Relative to all other groups, always-takers are slightly more likely to be low income quota applicants, Muslims, and households with mothers having finished primary education. Relative to the other two groups, the average complier is slighlty more likely to be Hindu, and less likely to be from Scheduled castes. However the magnitude of the differences

[28] Romero and Singh (2023) compare the lottery based estimates to a random sample of applicants who are always assigned to a private school and find that the lottery-based sample of students is moderately better off than the sample of students with a guaranteed private school allocation. They point out that this might be a function of urban areas being over-represented in their core sample, which have more oversubscribed schools. This has also been observed in charter school lotteries in the US (Cohodes, Setren and Walters, 2021).

are small suggesting that overall group characteristics are quite similar across groups. Overall, this suggests that compliers are representative of the full sample of applicants and that the external validity of the LATE extends to always- and never-takers.

5 Results

5.1 First stage

Table 1 shows the first-stage which captures the relationship between lottery offers and enrollment as a quota student. The endogenous variable of interest, i.e., enrollment in a private school as a quota student is instrumented by the indicator of winning the lottery at a private school, under the RTE policy. The instrument is random conditional on controlling for the narrow bins of simulated ex-ante propensity scores of winning. The results show that winning the lottery is strongly and positively correlated with enrollment as a quota student. The first-stage estimates are smaller than one because of non-compliance among lottery winners - some lottery winners choose to opt out of the quota seat at allotted schools as they may prefer other schools.[29] Another reason for the reduced estimate of the first-stage is that some applicants who did not win any lotteries in the beginning, received an offer through the waitlist (at a later date). The first-stage estimates differ across outcomes due to changes in sample composition - for example, the phone-based assessments is for a sub-sample of the surveyed households and some outcomes have missing responses leading to a reduced sample.

Table 1: First stage of winning the RTE lottery on enrollment as a RTE quota student

	Enrolled as RTE student
	(1)
Instrument = Winning the Lottery (any bin)	0.792***
	(0.013)
Outcome mean	0.44
Control mean	0.08
Observations	2,329
R^2	0.66
Pscores of winning	Yes
Controls	Yes

Notes: This table shows the first stage effects of winning the RTE private school lottery in any distance bin on enrollment as an RTE quota student in a private school. This first stage corresponds to the 2SLS regression where the outcome of interest is school enrollment. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

[29] Some of this non-compliance may also be stemming from the fact that the timing of seeking admissions at allotted schools under the RTE policy, coincided with the COVID-19 lockdown. However, the extent of COVID-19 induced non-compliance among lottery winners was reduced to some extent, as a result of schools allowing admission formalities to be completed over phone.

5.2 Primary outcomes

There are two primary categories of outcomes of interest. First, enrollment in the two academic years and second, performance on phone based assessments. Table 2 shows significant gains in enrollment in both academic years for treated compliers. Gains in enrollment are approximately three times higher in 2020-21 relative to 2021-22 suggesting that some of the children who were out-of-school during the first year of the pandemic (in 2020-21), are enrolled in schools in the following year (2021-22), with easements in restrictions surrounding the pandemic. Column (3) sheds light on another important aspect which is that RTE quota students were more likely to be able to maintain the right grade-for-age trajectory following their timely enrollment.

Table 2: LATE of being a RTE quota student on enrollment

	Enrollment (2020-21)	Enrollment (2021-22)	Grade 2 and above (2021-22)
	(1)	(2)	(3)
Enrolled as RTE student	0.141***	0.048***	0.194***
	(0.016)	(0.009)	(0.017)
First stage F-stat	3,911.06	3,938.19	3,934.19
Outcome mean	0.89	0.97	0.86
Control mean	0.84	0.94	0.78
Observations	2,328	2,328	2,327
R^2	0.10	0.07	0.15
Pscores of winning	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on children's enrollment. The outcomes in columns (1) and (2) measure the indicator of school enrollment in the two academic years. Column (3) measures the indicator for whether the child is in grade 2 or grade 3 in the 2021-22 academic year. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Being a quota student not only increases the likelihood of being enrolled and being enrolled in a higher grade, but also leads to gains in performance on phone-based assessments. As can be seen in Table 3, there is a .18 SD unit increase in English performance for the treated compliers. Although, the effect on Math is statistically indistinguishable from zero (at conventional levels), it is quite similar in magnitude to English.

Another thing to pay attention to, is to understand the composition of compliers, since the causal impact of interest is relevant to this group. In particular, while the treated compliers comprise a homogeneous group of students (enrolled as quota students because of winning the lottery), the same is not true for the control compliers. The latter group comprises fee-paying students at private schools, students who go to government schools, and students who are out-of school. Thus, it is helpful to characterize the distribution of enrollment status across these various sectors (Angrist, Hull and Walters, 2022). Abdulkadiroğlu, Angrist and Pathak (2014) and Chabrier, Cohodes and

Table 3: LATE of being a RTE quota student on phone based assessments

	Test score (standardized)	
	English	Math
	(1)	(2)
Enrolled as RTE student	0.187**	0.144
	(0.089)	(0.091)
First stage F-stat	1,129.88	1,129.88
Outcome mean	-0.00	-0.00
Control mean	-0.10	-0.09
Observations	695	695
R^2	0.17	0.13
Pscores of winning	Yes	Yes
Controls	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on children's performance on phone-based assessments. Outcomes measure children's standardized test scores on English and Math. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Oreopoulos (2016) refer to this as *counterfactual destinies*. Table A5 shows the counterfactual destinies for control compliers - 65% of the lottery losers end up enrolling in private schools as fee-paying students, 20% in government schools and about 5% are out-of-school.[30] Aside from the caveat that the control compliers are not a homogeneous group, and that these gains in test scores reflect differences in the mean outcomes of treated compliers relative to control compliers (who might vary in their enrollment status on the extensive margin, and type of school in the intensive margin), the effect sizes are similar to those observed in Muralidharan and Sundararaman (2015) who estimate the intent-to-treat (ITT) effects of being awarded vouchers to private schools. After 4 years of program implementation, they find that winners of private school vouchers perform .12 SD units better on English in the state of Andhra Pradesh, India. Similar to my findings, they find null impacts on Math. Their mechanisms show that these effects are primarily driven by the differences in instructional time spent across subjects.

In the next section, I investigate multiple mechanisms that might cause these gains. One mechanism that stands out and might explain gains in English, is the increase in likelihood of attending English medium schools - Table 4 shows that treated compliers are approximately 9 percentage points more likely to be in English medium schools relative to control compliers.[31] However, other channels such as, instructional time spent across subjects, and quality of instruction, may also play a role in explaining

[30] Among lottery losers, there are some children for whom the school name and the official school code could not be matched with the administrative data on the population of schools. Thus, for these children, the school sector – private, government, or out-of-school – is missing. It is for this reason that the counterfactual destinies don't add up to one.

[31] In India, English medium schools refer to schools where the primary language of classroom instruction is English. English-medium instruction is also perceived to have large labor market returns Azam, Chin and Prakash (2013).

these effects. While my survey does not collect data on instructional time per subject, it contains other rich information on school specific instruction that I talk about in the next section.

The majority of existing evidence of private school effectiveness is in the context of in-person learning. More recently, since the COVID-19 pandemic, there has been a growing interest in understanding the impact of remote instruction on children's educational outcomes, but we know relatively little about how schools adapt to changes in learning environments, and how this varies by school sectors, and whether private schools are still effective in the context of remote learning.[32] If private schools are seen to be effective in remote learning environments, then this might be especially relevant in developing country contexts where private school penetration is low or skewed. While constructing schools to provide uniform access to quality schooling might be the long term goal of governments, a short term cost-effective solution could be to increase access to private schools through remote learning. My results suggest that virtual learning can be effective, and that private schools do a better job at adapting to, and implementing remote educational technologies, and in doing so, they also enhance children's learning.

5.3 Mechanisms

Children's cognitive achievement and human development is considered to be a cumulative process that depends on the history of family and school inputs, and on children's innate ability (Becker and Tomes, 1976; Todd and Wolpin, 2003). Following that, I explore the various mechanisms that might explain these improvements in test scores. In particular I study three channels - school inputs, parental inputs, and children's own time use and educational effort.

5.3.1 School inputs

First, I discuss the channel of school inputs and school quality. Table 4 looks at various aspects of school characteristics that might matter in children's educational production function. The first two columns look into the outcomes of attending a private school and whether the school's primary language of instruction is English. Being enrolled as an RTE quota student increases the likelihood of both these outcomes, for the compliers. A notable observation is the magnitude of these effects - the likelihood of

[32] A related paper is by Crawfurd, Evans, Hares and Sandefur (2023), who randomize primary school students in Sierra Leone to receive phone tutoring calls from public or private school teachers during the period of COVID-19 school closures. The teachers supplemented government provided radio instruction, but the intervention did not increase children's test scores, whether provided by private or public school teachers. They attribute this non-impact to limited take-up by children.

attending a private school increases only by 20 percentage points. However, it is not surprising to see a small effect size, given the evidence of gradual exodus of children from government schools as a result of the increased affordability and demand for private schools (Kingdon, 2020). In the control group (non-quota students), about 79% of the children are enrolled in private schools, suggesting that for many applicant households, the RTE policy might just be a way of upgrading to a *better* or a more *expensive* school within the private sector - a finding that Romero and Singh (2023) investigate in greater detail in the context of RTE lotteries in the state of Chhattisgarh, India. This also points to the fact that there is substantial variation in the quality of schools *within* the private sector, and that this might matter in determining children's educational outcomes - I investigate this point in further details in Section 6.

Table 4: LATE of being a RTE quota student on school inputs

	School type		School instruction		School instruction modality		
	Private (2021-22)	English medium (2021-22)	Provides instruction (2020-21)	Provides instruction (2021-22)	Synchronous (online) (2021-22)	Recordings shared (audio/video) (2021-22)	Activity plans (WhatsApp/SMS) (2021-22)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enrolled as RTE student	0.199***	0.089***	0.072***	0.030***	0.136***	-0.034*	-0.066**
	(0.016)	(0.013)	(0.017)	(0.007)	(0.021)	(0.018)	(0.026)
First stage F-stat	3,856.22	3,859.05	3,472.00	3,877.71	3,788.83	3,788.83	3,788.83
Outcome mean	0.88	0.94	0.89	0.98	0.77	0.14	0.55
Control mean	0.79	0.89	0.85	0.97	0.70	0.15	0.57
Observations	2,249	2,250	2,083	2,255	2,210	2,210	2,210
R^2	0.16	0.08	0.11	0.05	0.15	0.08	0.10
Pscores of winning	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on school inputs. Columns (1) and (2) show the indicator of child being in a private school, and the school having English as the primary language of instruction. Columns (3) and (4) show outcomes for whether school provides instruction in the two academic years. Columns (5) - (7) show the type of instruction modality offered at child's school in the 2021-22 academic year. The question was: in the past month, what were the types of instruction offered by child's school (select all that apply) - (i) online classes with teacher, other students (ii) pre-recorded lectures were sent (audio/video) (iii) written learning activity plans were shared via Whatsapp/SMS (iv) other, specify. This question was asked only to children who were enrolled in school in 2021-22, and whose school was providing instruction. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table 4 also shows that conditional on being enrolled, treated compliers were more likely to be enrolled in schools that were actively providing instruction in the two academic years (columns (3) and (4)). The magnitude of the effect size is larger in the 2020-21 academic year (7 percentage points), relative to the 2021-22 academic year (3 percentage points). This suggests that RTE schools were especially more effective in providing remote instruction during the year that coincided with pandemic induced school closures, and that this effect size was reduced by about half in the following academic year. The reduction might be coming from a combination of these channels - schools being attended by control compliers might be getting better at providing remote instruction over time, and the extent of being out-of-school is falling among control compliers.

Finally, in the last three columns I look at the modality of instruction being offered at the school, conditional on being enrolled. Treated compliers are 13.6 percentage points more likely to be enrolled in schools that offer synchronous (online) classes, whereas control compliers are more likely to be in schools that offer recordings of lectures and share text-based activity plans (via WhatsApp or SMS).

Another outcome of interest is how winning the school lottery improves the overall quality of the school that a child attends, as it is reflective of school's quality in business-as-usual settings. This is of specific interest in the context of school choice in US where school quality is defined using peer achievement and/or peer socioeconomic composition etc. Appendix Table A6 shows the impact on being a quota student on school quality, conditional on enrollment. In absence of data on peer achievement, I measure school quality by creating indices of specific broad categories of school level characteristics using principal component analysis (PCA). I find that quota students are more likely to be enrolled in schools that have better infrastructure facilities, digital facilities, teacher quality, and have a less diverse student composition. Interestingly, comparing the magnitudes across specific indices suggests that digital and teacher indices increase more than infrastructure, suggesting that gains in performance of children are likely to be driven more because of school characteristics that actually matter for remote instruction, rather than infrastructure facilities which are less likely to directly matter for remote instruction.

Finally, Table A7 shows the LATE of winning the lottery on the likelihood that school teaches any given subject, after conditioning on enrollment. Relative to the non-quota students, quota students are significantly more likely to be in schools that teach English, Hindi, Environmental studies, Computers, General knowledge, Arts, Music and Dance. However, they are no more likely to teach Math. A caveat is that this information comes from parental responses, and not from the school, for which there could be measurement error in the estimates. But with that caveat aside, these estimates suggest that gains in English could be driven through a combination of reasons. Firstly, schools are more likely to teach English. Secondly they teach more number of subjects, and since the primary language of instruction is English it is likely to further complement students' understanding of the English language.

Overall, these results suggest that treated compliers are likely to end up in schools that were more likely to provide instruction and also provide synchronous modes of instruction - which is arguably more effective and holds both teachers and students more accountable, by offering real-time interaction between students and teachers. Furthermore, these schools are likely to be better equipped with digital facilities and have teachers with more qualifications, both of which might matter for augmenting children's learning during the period of remote construction.

5.3.2 Parental inputs

Second, I explore the channel of parental investments in children as receipt of a quota seat may also change household inputs into education (Das et al., 2013; Pop-Eleches and Urquiola, 2013). If parental investments change as a result of a quota receipt, then the LATE of attending a private school as a quota student on test scores, reflects an overall effect of school inputs and home inputs on children's achievement (Becker and Tomes, 1976; Todd and Wolpin, 2003). Recent research in this literature attempts to understand whether public investments in children encourage or crowd out parental investments - knowledge of this can inform policy and improve the targeting of public funds towards school inputs that encourage parental effort (Rabe, 2020). I test the extent to which parents adjust their time and monetary investments in children, in response to winning the quota seat. I find that parents increase their investments in winners, however the effect sizes are small, thereby suggesting that even though parental inputs are increasing, they only explain a small part of the story.

Table 5: LATE of being a RTE quota student on parental investments

	Time investments		Monetary investments	
	Receives help with homework	Hours of help (hrs/week)	Any expense (past year)	Expenditure (past year)
	(1)	(2)	(3)	(4)
Enrolled as RTE student	0.020	0.543*	0.065***	-90.182
	(0.013)	(0.308)	(0.014)	(158.303)
First stage F-stat	3,915.91	3,915.91	3,822.52	3,822.52
Outcome mean	0.93	9.50	0.93	3,462.86
Control mean	0.92	9.31	0.91	3,467.37
Observations	2,329	2,329	2,227	2,227
R^2	0.08	0.06	0.06	0.06
Pscores of winning	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on parental time and monetary investments in children - both on the extensive and intensive margin. Column (1) measures the extensive margin of whether child receives any help with educational activities in the household, and column (2) measures the intensive margin of the number of hours of help. The survey questions were: "Does the child receive any help with educational activities from any members of the household?" followed by details of each person who helps and their relationship with the child. Next, it was asked: "Among all those who help, who is the person who most often helps the child with educational activities?", followed by details about number of hours per day of help on a typical day, and number of days per week in the past week, to calculate weekly hours of help coming from the main helper. Hence, data on hours of help are collected only for the main helper. Column (3) measures the extensive margin of any educational expenses in the child in the *past one year* (on curriculum books, notebooks, and stationary), and column (4) measures the intensive margin of the amount of expenditure incurred on child's education in the past one year. There are some missing values for the monetary investment questions due to item non-response. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

I collect detailed data on parental monetary and time investments which help me in studying both the extensive and intensive margin impacts. Table 5 shows that while there is no significant impact on the extensive margin of children receiving household help with educational activities, however, there is evidence on the intensive margin - treated compliers are likely to receive a little more help per week with educational activities (approximately 30 mins more per week), relative to the control compliers. Further analyzing this, I find that among all the household members, mothers are the ones who are significantly more likely to help children with educational activities (Table A8). Turning to monetary investments, I find that parents of lottery winners are 6 percentage points more likely to spend on educational needs of children in the past year (on curriculum books, and stationary). However, there is no impact on the intensive margin.

Together, these results suggest that parents seem to respond to the receipt of the RTE quota seat by reinforcing investments in children, however the effect sizes suggest that this channel explains only a small part of the story.

5.3.3 Children's time use

Third, I explore the channel of children's own effort by looking at children's time use. Table 6 shows the LATE of being a quota student on children's time use in educational activities, and I find that being a quota student significantly increases children's time use in educational activities. Treated compliers spend 3 more hours per week doing school related activities, and approximately 20 more minutes per week doing homework. I don't find any statistically significant differences in time spent on private tuition (after school classes) and in non-educational activities. Overall, children seem to significantly increase their educational effort as a result of winning the lottery and this might also contribute to gains in their educational outcomes. These findings are also consistent with Muralidharan and Sundararaman (2015) who find no impact of winning private school vouchers on home study- and play-habits except increased time spent in school for voucher winners.

6 Winning in Elite versus Budget RTE private schools

The results so far provide evidence that winning entry to RTE private schools significantly improves children's learning outcomes. The mechanisms suggest that school's mode of instruction, and children's effort in educational activities play an important role in achieving these gains. However, even among the class of RTE private schools attended by winners, there might be variation in school quality that makes some private schools better relative to others. Private schools that levy high yearly school-fee are likely to have highly qualified and motivated teachers with high teacher salaries, and thus offer higher quality of education, better resources, and as a result might have

Table 6: LATE of being a RTE quota student on children's time use

	Educational activities			Non-educational activities		
	School (hrs/week)	Tuition (hrs/week)	Homework (hrs/day)	Playing (hrs/day)	Television (hrs/day)	House chores (hrs/day)
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled as RTE student	2.945***	-0.405	0.262***	-0.099	-0.073	-0.006
	(0.400)	(0.315)	(0.038)	(0.062)	(0.047)	(0.021)
First stage F-stat	3,915.91	3,915.91	3,915.91	3,909.99	3,938.97	3,915.91
Outcome mean	12.12	4.67	1.40	2.45	1.10	0.39
Control mean	10.79	4.92	1.31	2.50	1.15	0.40
Observations	2,329	2,329	2,329	2,328	2,322	2,329
R^2	0.13	0.08	0.06	0.05	0.09	0.06
Pscores of winning	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on children's time use in educational and non-educational activities. School hours are set equal to zero for those who report being not-enrolled in any school. Tuition hours (differs from formal schooling, typically happens after school) are also set equal to zero for those who report being not enrolled in any private tuition. The question for homework hours is not always zero for not enrolled children, as the question asked - "how much time does child spend doing homework, or any educational activities after school?". There are some missing values for playing and watching television due to item non-response. All time use data are winsorized at the top 99th percentile. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

a higher value-added.[33] In contrast, private schools that charge lower fee might have lesser teachers with fewer qualifications, and as a result have lower value-added. Thus, even among the set of RTE winners who benefit from a quota seat at private schools, school quality is likely to differ, which may lead to differences in children's achievement. But do these differences in school quality matter during periods of remote learning?

There is no evidence on the how the distribution of private school effectiveness varies within the private sector (Romero and Singh, 2023). The only such evidence from a similar context is from Punjab in Pakistan, by Andrabi et al. (2022). Using value-added models (VAMs), they find evidence of substantial within-village variation in school quality within the private schooling sector. Contrary to the existing literature that has largely focused on a single private school premium, their findings suggest a range of causal estimates of the private school premium, resulting due to a substantial *within-sector* variation in school quality. If there exists variation in quality within private schools, then it might be misleading to focus on a single estimate of the private school premium. The next question that arises is how should one arrive at a reliable measure of school quality? In this section, I uncover the impacts of relative differences in private school quality on children's educational outcomes by using two alternative

[33] Previous literature has used school fee as a proxy for school quality. Rao (2019) defines elite schools as those charging fee greater than 2000 INR per month, in New Delhi. Andrabi et al. (2022) find positive correlation between school value added (SVA) and school-fee in Pakistan. Romero and Singh (2023) analyze the impact of winning a quota seat under RTE on the market price of the school being attended, in the state of Chhattisgarh, India, and find that quota students are enrolled in costlier schools.

measures of school quality which I discuss in details in Section 6.1. The next paragraph briefly summarizes how the literature defines school quality.

The idea of school quality is a latent concept, and the literature has looked at various ways of measuring the *true* school quality. A bulk of the literature on school quality in the US focuses on achievement-based measures of quality, and more recently on outcomes other than student achievement, for example, crime, employment, earnings and non-cognitive outcomes (Angrist et al., 2022).[34] Several papers use peer ability and socioeconomic composition of peers as proxy for school quality (Abdulkadiroğlu et al., 2014; Pop-Eleches and Urquiola, 2013; Dobbie and Fryer Jr, 2013). Greaves et al. (2023) use school inspection ratings as a source of information on school quality in the context of England. School management interventions that improve the quality of leadership practices have also been utilized to get at measures of school quality.[35] Specific dimensions of school inputs have also been used to measure school quality, such as class size (Datar and Mason, 2008; Fredriksson et al., 2016) or school resources (Houtenville and Conway, 2008; Das et al., 2013). Another important and related strand of literature is on college quality, where the goal is to study the labor market effects of the quality of college that individuals attend. Black and Smith (2006) discuss the issues with using a single proxy of college quality (such as the average SAT score of the entering class) as it leads to substantial measurement error in quality measure, and propose several solutions. One of the proposed solutions is to create a quality index that combines multiple individual quality measures via factor analysis. The more the number of quality variables, the less is the measurement error in the index (Black, Smith and Daniel, 2005). I take inspiration from them to define one of my school quality measures in a similar way, and I discuss this in more details in the next section.

6.1 Two alternate measures of school quality and eliteness

In absence of panel data on standardized test scores across schools, and school level peer achievement, I consider two ways of defining school quality - one that uses school fee, and other that uses data on a rich set of school level characteristics.[36] I start with

[34] Angrist et al. (2022) provide a useful review of this literature by summarizing the various econometric strategies for estimating school effectiveness - school lotteries using the instrumental variables approach, regression-discontinuity approach where students are admitted based on a cutoff score, centralized school assignment where school allotment happens via conditional randomization based on rank ordered lists submitted by parents, and finally value-added models (VAMs) which control for lagged outcomes and covariates by making use of panel data of student test scores.

[35] Anand et al. (2023) conduct a meta analysis of the impact of school management interventions on student learning using data from multiple evaluations, and provide a systematic review of this literature.

[36] The school fee based measure is in spirit of previous literature that uses fee to define school eliteness and quality, such as Rao (2019); Romero and Singh (2023) and Andrabi, Bau, Das and Khwaja (2022). The school quality based measure is in spirit of the college quality literature that uses multiple dimen-

a simple case of categorizing schools as *elite* or *budget*, based on two measures of quality.[37] The first measure uses administrative data of schools' annual fee to categorize each school as elite or budget. Figure A4 shows the distribution of annual school fee for private schools in the state using the administrative data of annual school fee charged by RTE private schools in the state. As the figure shows, most of the private schools are concentrated on the lower end of the fee distribution suggesting that most private schools are low-fee. This is in line with Kingdon (2020), who documents that the vast bulk of private schools in India are low fee schools, when benchmarked against the state per capita income and daily wage laborer's incomes. The author also points out that this increase in affordability has led to a rapid migration of students towards private schools, and an emptying of government schools. Taking the distribution of annual school fee for all the RTE private schools in the state, I define a school as elite if the annual school fee exceeds the 75^{th} percentile in the distribution of fee of all private schools in the state, and budget, otherwise.[38]

The second measure of school eliteness is based on a school quality index that I construct using Principal Component Analysis (PCA). The data for this analysis comes from UDISE, which allows me to make use of a rich dataset on school characteristics – infrastructure details, digital facilities, teacher qualifications, and peer SES composition – which might matter in determining the overall school quality. The complete list of variables that are used for creating this index is shown in Table A5 in the Appendix. I use the first component of the PCA to create the quality index. The table also shows the factor loadings on each of the variable - it shows that all these different types of school inputs are positively associated with school quality.

These two measures of school quality display a strong and positive correlation - Table A13 shows the results from a simple OLS where I regress school fee on school's PCA based quality index.[39] While both measures capture a measure of quality, I prefer the

sions of college characteristics to create an index of college quality, such as Black, Smith and Daniel (2005) and Black and Smith (2006).

[37]Note that, while both my measures of quality are continuous measures of quality, the discretization of schools into elite and budget is done following the identification strategy which relies on the within-variation in lottery outcomes of children with similar ex-ante propensity of winning the lottery *at elite* schools. This in turn requires that each school that was chosen during the time of application, be categorized as a binary of either elite or not-elite to get at the simulated ex-ante propensity score of winning at an elite school. Black and Smith (2006) discuss that such discretization might lead to loss of information and in turn cause researcher induced measurement error in the quality index.

[38]I vary the bar of eliteness by lowering and increasing the threshold to 50^{th} and 90^{th} percentile, respectively. In the sub-sample of lottery winners, only 2% of the children attend elite schools but not as an RTE student. In the sub-sample of lottery winners (which is the relevant sample for this exercise), approximately 4% of the children attend government schools. I assume that government schools (free of cost) are budget schools. Most of the government schools being attended by the non-compliers among the lottery winners are zilla parishad schools, which lie at the lower end of government school quality distribution.

[39]Tabulating schools on these two measures of eliteness shows that majority of the schools are con-

fee based measure of the PCA index. The reason for this is that school fee is likely to encapsulate various dimensions of school quality - both observed and unobserved - which may not be apparent in the school characteristics data that is used to construct the PCA index. The school characteristics data is useful to the extent that it provides information about observed characteristics of the school. The fee on the other hand is a measure that is likely to take into account all the aspects about schools which may be hard to quantify.[40]

6.2 Estimating the impact of attending elite private schools as a quota student

For the group of lottery winners, what is the impact of attending elite private schools as a quota student, relative to attending budget private schools? I estimate this using two-stage least squares framework on the sub-sample of lottery winners:

$$RTE\_Enrolled\_Elite_i = \alpha_1 WinningLotteryEliteAnyBin_i + X_i'\alpha_2 + \sum_{x=1}^{50} \gamma_x d_i(x) + \epsilon_i \quad (3)$$

$$Y_i = \beta_1 \widehat{RTE\_Enrolled\_Elite}_i + X_i'\beta_2 + \sum_{x=1}^{50} \gamma_x d_i(x) + e_i \quad (4)$$

where, $RTE\_Enrolled\_Elite_i$ is the indicator that child i attends an Elite private school as a quota student, $WinningLotteryEliteAnyBin_i$ is the indicator that child i won the lottery at an Elite school in any bin, X_i is the vector of child and household characteristics, $d_i(x)$ are dummies taking a value of 1 if child i's estimated propensity score of winning a lottery at an elite private school lies in the respective 0.02 wide probability bin. As before, identification comes from within variation in lottery offers at elite or budget schools for groups of applicants who are otherwise similar in their ex-ante propensity of winning at elite schools.[41]

sistent in the elite definition across the two measures (Figure A6). About 65% of the schools are elite on both measures, about 25% of the schools differ in classification of eliteness across the two measures, the rest have missing data on one of the two measures.

[40]Note that using a fee based measure as a quality measure assumes that schools don't increase fee in response to the policy. The government reimburses schools up to a cap of INR 17,640 per-pupil per annum, an amount that might fall short of the actual fee for some schools. Intuitively, it is plausible that high-fee charging schools that charge in excess of what the government reimburses them, might increase their fee levels further, to compensate for the lost revenue per quota seat by extracting more revenues from the fee-paying students, keeping in mind the price elasticity of the fee-paying students. However, I cannot test this in the data. However, I do not think that schools increase fee indiscriminately, in response to the policy - this is corroborated to some extent by the regression of school fee on the PCA index of school quality, which shows a strong positive correlation between school quality and school fee.

[41]The detailed step by step process of calculating these ex-ante propensities of winning at elite schools is explained in Appendix Section B.3.

Table 7: First stage of winning the RTE lottery at elite school on enrollment at an elite school

	RTE student at Elite school	
	Elite (PCA)	Elite (Fee)
	(1)	(2)
Won RTE lottery at Elite school	0.880***	0.869***
	(0.027)	(0.028)
Outcome mean	0.39	0.51
Control mean	0.00	0.00
Observations	1,019	973
R^2	0.85	0.82
Pscores of winning at elite	Yes	Yes
Controls	Yes	Yes
Avg quality (Elite=1)	4.37	43046.62
Avg quality (Elite=0)	2.34	12320.27

Notes: This table shows the first stage effects of winning the RTE private school lottery at an elite school on enrollment at an elite school as a quota student. The sample is restircted to lottery winners. Eliteness is defined using the 75^{th} percentile cutoff. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

6.2.1 First stage

Table 7 shows the first stage - winning the lottery at an elite school, defined as schools lying above the 75^{th} percentile of the quality distribution, increases the likelihood of attending one, by 87pp - 88pp depending on the quality measure. The dependent mean shows the proportion of children enrolled at elite schools under the quota - this is 39% based on the PCA index measure, and 51% based on the fee-based measure. These differences stem from each measure identifying a different aspect of school quality and relatedly the fact that the same school might be categorized as elite based on one measure, but as budget on the other. It is also informative to learn about the average quality of schools that are categorized as elite versus those categorized as budget. Table 7 shows this for both the quality measures - the mean school fee for elite schools is about 3.5 times higher than that for budget schools, and this ratio is about 1.8 for the PCA index.[42]

6.2.2 Primary outcomes

Table 8 shows the LATE of attending an elite school on children's performance on phone-based assessments, using both measures of school quality. Treated compliers are children who attend elite private schools under the RTE quota because they won the lottery to an elite private school, and control compliers are those who do not attend elite schools under the quota because they lost the lottery to all the elite schools. Results show that elite schools increase English test scores on both the quality measures, however, there are no gains in Math. As before I explore several mechanisms

[42] Appendix Table A15 shows how results vary when the percentile cutoff is changed to 50^{th} and 90^{th} percentile.

Table 8: LATE of attending elite schools on performance on tests

	English	Math	English	Math
	Elite (PCA)		Elite (Fee)	
	(1)	(2)	(3)	(4)
RTE student at Elite school	0.699***	0.370	0.485**	0.138
	(0.270)	(0.267)	(0.242)	(0.240)
First stage F-stat	590.47	590.47	389.43	389.43
Outcome mean	0.06	0.04	0.06	0.05
Control mean	0.03	-0.05	-0.18	-0.20
Observations	318	318	303	303
R^2	0.14	0.13	0.20	0.17
Pscores of winning at elite	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on children's performance on phone based assessments. The sample is restricted to lottery winners. As before, the number of observations is smaller here because the phone-based assessment on English and Math is available only for a subsample of lottery winners. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

below.

6.2.3 Mechanisms

My survey data allows me to explore several school inputs such as, school's instructional modality, subjects taught and other school characteristics, and children's time use as potential mechanisms. I discuss these in depth in the subsequent paragraphs.

6.2.3.1 School inputs

While elite schools are no more likely to provide instruction in the two academic years (as measured on the extensive margin), they are however more likely to provide better instruction modalities. Table 9 shows that treated compliers are more likely to report receiving synchronous online classes (between 10 - 18 percentage points, based on the PCA and the fee-based measure, respectively), and less likely to receive text-based instruction (by 17 pp, on fee-based measure) during the period of remote instruction.

Table A12 shows the differences in characteristics of elite and budget schools, and helps in understanding key differences across these schools. Controlling for the village fixed effects, elite schools are consistently more likely to have internet, more digital boards per pupil, more likely to be English medium, have a higher proportion of teachers trained in computers, teachers with Bachelor's in Education degrees, and a higher proportion of general caste category students. The magnitude of differences in Bachelor's in Education degree is substantive, at 20 percentage points, suggesting that elite schools are significantly more likely to hire teachers who have specifically

Table 9: LATE of attending elite schools on school instruction

	Synchronous classes (online)	Recordings shared (audio/video)	Text-based activity plans (WhatsApp/SMS)	Synchronous classes (online)	Recordings shared (audio/video)	Text-based activity plans (WhatsApp/SMS)
	Elite (PCA)			Elite (Fee)		
	(1)	(2)	(3)	(4)	(5)	(6)
RTE student at Elite school	0.102*	0.024	-0.064	0.180***	-0.036	-0.174**
	(0.060)	(0.054)	(0.077)	(0.051)	(0.048)	(0.069)
First stage F-stat	1,151.81	1,151.81	1,151.81	1,129.44	1,129.44	1,129.44
Outcome mean	0.83	0.13	0.53	0.83	0.13	0.53
Control mean	0.77	0.15	0.59	0.69	0.17	0.60
Observations	1,005	1,005	1,005	959	959	959
R^2	0.09	0.05	0.12	0.18	0.08	0.13
Pscores of winning at elite	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on school's instruction modality and children's time use in educational activities. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

trained to pursue a school teaching career.[43] These patterns suggest that the relative effectiveness of elite schools in remote instruction is likely to be a function of teachers being more qualified and also being more adept at dealing with digital technologies. In addition, these results are also robust to changing the elite cutoff to the 50^{th} and the 90^{th} percentile of annual fee (Table A18 in Appendix), which provides a consistent story that elite schools were doing better in terms of providing instruction during the period of remote instruction.

Another channel that might explain gains in English might relate to differences in quality of English instruction, and differences in instructional time spent across subjects, across elite and budget schools. Appendix Table A14 shows that while elite school goers are no more likley to be taught the conventional subjects (Math, English, Marathi and Hindi), however, they are more likely to have other subjects in their curriculum, such as General Knowledge, Arts, Music and Dance. These other subjects are likley being taught in English (as suggested in balance table A12 which shows elite schools being more likely to be English medium,) and this in turn might be indirectly improving children's exposure to English thereby improving their test scores.

6.2.3.2 Children's time use

Finally, studying the impacts on children's time use (Table 10), I find that elite schools are also more likely to provide higher hours of instruction per week (2.1 - 3.1 hours/week) relative to budget private schools.

[43] In the Indian context, Bachelors in Education is degree program that is specifically designed for those who aspire to become school teachers. It is typically a two-year program that one pursues after a three/four year undergraduate degree program, in order to become a school teacher.

Taken together, these channels suggest some of the plausible mechanisms which might be at play and might matter for children's performance. While the lack of data on instructional time by subject precludes me for testing the role of that channel in explaining the results, the results on differences in school characteristics, in terms of their baseline digital facilities, teacher quality, overall time spent in school, and likelihood of studying specific subjects provide some understanding of why elite schools were more effective in providing remote instruction and also enhancing children's learning.

Table 10: LATE of attending elite schools on children's time use

	School (hrs/week)	Tuition (after school) (hrs/week)	Homework (hrs/day)	School (hrs/week)	Tuition (after school) (hrs/week)	Homework (hrs/day)
	Elite (PCA)			Elite (Fee)		
	(1)	(2)	(3)	(4)	(5)	(6)
RTE student at Elite school	3.185***	-1.936**	0.007	2.181**	-0.314	-0.140
	(1.176)	(0.957)	(0.113)	(1.066)	(0.869)	(0.103)
First stage F-stat	982.18	982.18	982.18	937.84	937.84	937.84
Outcome mean	13.58	4.43	1.51	13.57	4.49	1.52
Control mean	12.98	4.91	1.51	12.38	5.00	1.48
Observations	1,019	1,019	1,019	973	973	973
R^2	0.10	0.09	0.04	0.11	0.09	0.03
Pscores of winning at elite	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on school's instruction modality and children's time use in educational activities. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

7 Robustness checks

I perform a number of robustness checks to validate my findings.

7.1 Excluding applicants who are age-eligible to re-apply for RTE

The eligibility to apply for grade 1 admissions under the RTE in 2020-21 was that one had to be born no before July 2013, and no later than October 2014. This means that the eligibility criteria to apply for grade 1 applications in the following academic year of 2021-22 was that one had to be born between July 2014 and October 2015. Hence, the age-eligibility to apply under the RTE policy for grade 1 spans more than a year, which implies that the very young applicants who applied for grade 1 lotteries in 2020-21, would be age-eligible to apply again under the policy, in the following year (academic year 2021-22). Appendix Figure A8 shows the distribution of the birth year-months in the population of the applicants.

Figure A8 shows that my sample of surveyed applicants contains some children who are eligible to re-apply under the policy in the following academic year of 2021-22 - these are the applicants who were born between July 2014 and October 2014. This leads to two concerns. The first concern is that among the young applicants (who are age-eligible to re-apply) those who lost the lottery, can wait to try again next year, however those who won the lottery in the first year, might accept and enroll. This is concerning because such applicants can cause selection bias and also affect the composition of the control group. Since the control group comprises non-quota children, the presence of such applicants might lead to the control group having some children who became quota beneficiaries in the following year.[44] The second worry is that these young applicants might be different from relatively older applicants (age-ineligible to re-apply) on unobservable dimensions, for example, parental motivation to apply or child ability, which might be simultaneously correlated with children's performance on tests, and might bias estimates. Thus, I address these issues by limiting my analysis to the subsample of those who are age-ineligible to re-apply in the following year. I find that the results on phone-assessments are robust to excluding young applicants. Results are shown in Table A20.

7.2 Using school level values to measure outcomes

My results use survey data from household level reports on children's outcomes. However, some of these variables could be measured with error. For example, certain variables correspond to school level information, like - does the child's school provide instruction; what is the modality of instruction at child's school; frequency of classes at child's school etc. The ideal scenario would be to obtain administrative data from schools on these school level information, however, since such data is not available/collected by schools, I rely on household level reports for these school related variables. It is possible that the household level responses to school level variables might have measurement error such that there are inconsistencies in how children attending the same exact school might respond differently to any given question about the school, which in turn might lead to biases in the estimates.[45] I attempt to address this potential noise in the household level responses, by creating a new variable that captures the school level unique responses to these questions. I do this by coding the value of the new variable as the response that was most frequently chosen by stu-

[44] I define my treatment as enrollment as a RTE quota student where the quota receipt is based on the 2020-21 school year.

[45] For instance, consider a scenario where a total of five children attend school A, and of these five children, four children respond by reporting yes to the question that asks whether school A was providing instruction in the previous academic year, while one child responds no to this question. This would be problematic in the regression where the outcome measures the binary indicator of school providing instruction.

dents attending the same school with the goal that these new variables have lesser noise.[46]

Results based on this cleaning of school level variables are shown in Appendix Table A21. I see that the results are still robust - RTE schools are still significantly more likely to provide school instruction in the academic year of 2021-22, they are still significantly more likely to provide synchronous and live classes. The standard errors on the estimates have also shrunk which is a mechanical result due to a decrease in the variation in the new outcome variable. These robustness check strengthen the validity of the main results.

7.3 Varying the ex-ante propensity scores of winning

The identification in my estimations comes from using the within-variation in children who had a similar ex-ante likelihood of winning the RTE lottery but varied in their final lottery outcome. To do this I control for narrow bins of ex-ante propensity scores of winning by simulating the lottery algorithm. I show that my results are robust to increasing the number of narrow bins of these ex-ante propensities, or in other words reducing the bin width. Reducing the bin-width would lead to stricter within comparisons, comparing children who had a very similar ex-ante likelihood of winning under the lottery. Appendix Table A7 shows this in a coefficient plot which shows how the LATE coefficient on test scores changes as the bin-width is reduced.

8 Conclusion

Affirmative action policies are implemented to promote social equity. While most policies target late-life equality (in higher education and the workplace), some focus on early-life equality. Policies implemented earlier in life have the potential to mitigate disparities that tend to amplify over time, thus diminishing structural disadvantages. This paper studies the impact of India's Right to Education Act, an affirmative action policy that targets children of school entry age, on children's educational outcomes. Given that India has one of the world's largest schooling systems, the scale of the policy is huge and impacts millions of disadvantaged children.

I leverage the lottery based allocation of schools to students to estimate the causal impact of attending a private school as a beneficiary under this policy on children's educational outcomes. The context is that of remote learning during the period of

[46] I clean this by making a new clean variable at the school level based on whether the proportion of children who answer yes to the question at a given school exceeds half. This is under the assumption that the reports of more than 50% of the students are less likely to be wrong answers.

pandemic indiced school closures, and I find that being a beneficiary under the policy significantly insured disadvantaged children from the risk of non-enrollment, helped with grade progression, and maintaining the right grade-for-age trajectory. I find that these gains in enrollment also translate to gains in performance in English and improve children's English test scores by .18 SD units. Exploring mechanisms, I find that relative to the non-beneficiaries, beneficiaries are more likely to attend private schools of higher overall school quality, where the main language of instruction is English, and that are more effective in providing synchronous remote education technologies, and have a longer school week.

Next, given that the private schools themselves are differentiated in quality, I focus on the beneficiaries to estimate the causal impact of attending elite or higher quality private schools, relative to budget, or lower quality private schools, by leveraging the randomization in offers to elite schools. As before, I find significant gains in English as a result of attending elite schools, which suggests that a single estimate of private school premium might be misleading. As before, the mechanisms point to elite schools being significantly better at providing remote instruction and having a longer school week. Baseline differences in school characteristics further show that elite schools have better digital technologies, have higher proportion of teachers with better qualifications, and higher proportion of teachers trained in computers, all of which may matter in making remote learing more effective. Overall, my results suggest that private schools, and especially those at the upper end of the quality distribution, are effective in adapting to, and providing remote learning and in doing so they also enhance children's learning.

While the policy is successful in delivering these gains, however, there are concerns about whether the applicant pool is representative of the eligible groups in the population. Given that the fallback option for the majority of lottery losers (about 65% of control compliers) is to enroll as fee-paying students in private schools, this points to the concerns of regressive selection among eligible groups. Romero and Singh (2023) focus on the aspect of mistargeting and regressive selection in RTE and find that various constraints (information, documentation, application complexity) prevent poor households from applying under the RTE policy, despite them having a high demand for private schools. Thus, this points to the neccissity of improving targeting under the policy such that the benefits can percolate to the ones who most need it.

References

Abadie, A. (2002), 'Bootstrap tests for distributional treatment effects in instrumental variable models', *Journal of the American statistical Association* **97**(457), 284–292.

Abadie, A. (2003), 'Semiparametric instrumental variable estimation of treatment response models', *Journal of Econometrics* **113**(2), 231–263.

Abdulkadiroğlu, A., Angrist, J. D., Dynarski, S. M., Kane, T. J. and Pathak, P. A. (2011), 'Accountability and flexibility in public schools: Evidence from Boston's charters and pilots', *The Quarterly Journal of Economics* **126**(2), 699–748.

Abdulkadiroğlu, A., Angrist, J. D., Narita, Y. and Pathak, P. A. (2017), 'Research design meets market design: Using centralized assignment for impact evaluation', *Econometrica* **85**(5), 1373–1432.

Abdulkadiroğlu, A., Angrist, J. and Pathak, P. (2014), 'The elite illusion: Achievement effects at Boston and New York exam schools', *Econometrica* **82**(1), 137–196.

Abdulkadiroğlu, A., Pathak, P. A., Schellenberg, J. and Walters, C. R. (2020), 'Do parents value school effectiveness?', *American Economic Review* **110**(5), 1502–1539.

Alasino, E., Ramırez, M. J., Romero, M., Schady, N. and Uribe, D. (2023), 'Learning losses during the COVID-19 pandemic: Evidence from Mexico'.

Anand, G., Atluri, A., Crawfurd, L., Pugatch, T. and Sheth, K. (2023), 'Improving school management in low and middle income countries: a systematic review'.

Andrabi, T., Bau, N., Das, J., Karachiwalla, N. and Khwaja, A. I. (2023), 'Crowding in private quality: The equilibrium effects of public spending in education', *National Bureau of Economic Research* .

Andrabi, T., Bau, N., Das, J. and Khwaja, A. I. (2022), Heterogeneity in school value-added and the private premium, Technical report, National Bureau of Economic Research.

Andrabi, T., Daniels, B. and Das, J. (2021), 'Human capital accumulation and disasters: Evidence from the Pakistan earthquake of 2005', *Journal of Human Resources* pp. 0520–10887R1.

Angrist, J., Bettinger, E., Bloom, E., King, E. and Kremer, M. (2002), 'Vouchers for private schooling in Colombia: Evidence from a randomized natural experiment', *American Economic Review* **92**(5), 1535–1558.

Angrist, J., Bettinger, E. and Kremer, M. (2006), 'Long-term educational consequences of secondary school vouchers: Evidence from administrative records in Colombia', *American Economic Review* **96**(3), 847–862.

Angrist, J. D., Cohodes, S. R., Dynarski, S. M., Pathak, P. A. and Walters, C. R. (2016), 'Stand and deliver: Effects of Boston's charter high schools on college preparation, entry, and choice', *Journal of Labor Economics* **34**(2), 275–318.

Angrist, J. D., Pathak, P. A. and Walters, C. R. (2013), 'Explaining charter school effectiveness', *American Economic Journal: Applied Economics* **5**(4), 1–27.

Angrist, J. D. and Pischke, J.-S. (2009), *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton university press.

Angrist, J., Hull, P. and Walters, C. R. (2022), 'Methods for measuring school effectiveness'.

Angrist, J. and Imbens, G. (1995), 'Identification and estimation of local average treatment effects'.

Angrist, N., Bergman, P. and Matsheng, M. (2020), School's out: Experimental evidence on limiting learning loss using "low-tech" in a pandemic, Technical report, National Bureau of Economic Research.

Arcidiacono, P. and Lovenheim, M. (2016), 'Affirmative action and the quality–fit trade-off', *Journal of Economic Literature* **54**(1), 3–51.

Azam, M., Chin, A. and Prakash, N. (2013), The returns to English-language skills in India, Technical Report 2.

Azevedo, J. P., Hasan, A., Goldemberg, D., Geven, K. and Iqbal, S. A. (2021), 'Simulating the potential impacts of COVID-19 school closures on schooling and learning outcomes: A set of global estimates', *The World Bank Research Observer* **36**(1), 1–40.

Bacher-Hicks, A., Goodman, J. and Mulhern, C. (2021), 'Inequality in household adaptation to schooling shocks: COVID-induced online learning engagement in real time', *Journal of Public Economics* **193**, 104345.

Badaracco, N. (2020), 'Time investment responses of parents and students to school inputs'.

Bagde, S., Epple, D. and Taylor, L. (2016), 'Does affirmative action work? Caste, gender, college quality, and academic success in India', *American Economic Review* **106**(6), 1495–1521.

Bagde, S., Epple, D. and Taylor, L. (2022), 'The emergence of private high schools in India: The impact of public-private competition on public school students', *Journal of Public Economics* **215**, 104749.

Bandiera, O., Buehren, N., Goldstein, M. and Rasul, I. (2020), 'Do school closures during an epidemic have persistent effects? Evidence from Sierra Leone in the time of Ebola'.

Becker, G. S. and Tomes, N. (1976), 'Child endowments and the quantity and quality of children', *Journal of Political Economy* **84**(4, Part 2), S143–S162.

Berry, J. and Mukherjee, P. (2016), 'Pricing of private education in urban India: Demand, use and impact', *Unpublished manuscript. Ithaca, NY: Cornell University* .

Bertrand, M., Hanna, R. and Mullainathan, S. (2010), 'Affirmative action in education: Evidence from engineering college admissions in India', *Journal of Public Economics* **94**(1-2), 16–29.

Black, D. A., Joo, J., LaLonde, R., Smith, J. A. and Taylor, E. J. (2022), 'Simple tests for selection: Learning more from instrumental variables', *Labour Economics* **79**, 102237.

Black, D. A. and Smith, J. A. (2006), 'Estimating the returns to college quality with multiple proxies for quality', *Journal of labor Economics* **24**(3), 701–728.

Black, D., Smith, J. and Daniel, K. (2005), 'College quality and wages in the United States', *German Economic Review* **6**(3), 415–443.

Bleemer, Z. (2022), 'Affirmative action, mismatch, and economic mobility after California's Proposition 209', *The Quarterly Journal of Economics* **137**(1), 115–160.

Bol, T. (2020), 'Inequality in homeschooling during the Corona crisis in the Netherlands. First results from the LISS panel'.

Bonesrønning, H. (2004), 'The determinants of parental effort in education production: do parents respond to changes in class size?', *Economics of Education Review* **23**(1), 1–9.

Bruhn, J. (2019), 'The consequences of sorting for understanding school quality', *Unpublished working paper). Retrieved from https://1b50402b-a-62cb3a1a-s-sites. googlegroups. com/site/jessebruhn3/jesse_bruhn_jmp. pdf* .

Buhl-Wiggers, J., Kerwin, J. T., de la Piedra, R. M., Smith, J. and Thornton, R. (2023), 'Reading for life: Lasting impacts of a literacy intervention in Uganda'.

Card, D. and Krueger, A. B. (2005), 'Would the elimination of affirmative action affect highly qualified minority applicants? evidence from California and Texas', *ILR Review* **58**(3), 416–434.

Carlana, M. and La Ferrara, E. (2021), 'Apart but connected: Online tutoring and student outcomes during the COVID-19 pandemic'.

Chabrier, J., Cohodes, S. and Oreopoulos, P. (2016), 'What can we learn from charter school lotteries?', *Journal of Economic Perspectives* **30**(3), 57–84.

Cohodes, S. R., Setren, E. M. and Walters, C. R. (2021), 'Can successful schools replicate? scaling up Boston's charter school sector', *American Economic Journal: Economic Policy* **13**(1), 138–67.

Crawfurd, L., Evans, D. K., Hares, S. and Sandefur, J. (2023), 'Live tutoring calls did not improve learning during the COVID-19 pandemic in Sierra Leone', *Journal of Development Economics* **164**, 103114.

Cullen, J. B., Jacob, B. A. and Levitt, S. (2006), 'The effect of school choice on participants: Evidence from randomized lotteries', *Econometrica* **74**(5), 1191–1230.

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

Damera, V. K. (2018), Essays on school choice, PhD thesis, University of Oxford.

Das, J., Dercon, S., Habyarimana, J., Krishnan, P., Muralidharan, K. and Sundararaman, V. (2013), 'School inputs, household substitution, and test scores', *American Economic Journal: Applied Economics* **5**(2), 29–57.

Datar, A. and Mason, B. (2008), 'Do reductions in class size "crowd out" parental investment in education?', *Economics of Education Review* **27**(6), 712–723.

Del Boca, D., Monfardini, C. and Nicoletti, C. (2017), 'Parental and child time investments and the cognitive development of adolescents', *Journal of Labor Economics* **35**(2), 565–608.

Deming, D. J., Hastings, J. S., Kane, T. J. and Staiger, D. O. (2014), 'School choice, school quality, and postsecondary attainment', *American Economic Review* **104**(3), 991–1013.

Dobbie, W. and Fryer Jr, R. G. (2011), 'Are high-quality schools enough to increase achievement among the poor? Evidence from the Harlem children's zone', *American Economic Journal: Applied Economics* **3**(3), 158–187.

Dobbie, W. and Fryer Jr, R. G. (2013), 'Getting beneath the veil of effective schools: Evidence from New York City', *American Economic Journal: Applied Economics* **5**(4), 28–60.

Dobbie, W., Fryer, R. G. et al. (2011), Exam high schools and academic achievement: Evidence from New York City, Technical report, National Bureau of Economic Research.

Fredriksson, P., Öckert, B. and Oosterbeek, H. (2016), 'Parental responses to public investments in children: Evidence from a maximum class size rule', *Journal of Human Resources* **51**(4), 832–868.

Gelber, A. and Isen, A. (2013), 'Children's schooling and parents' behavior: Evidence from the Head Start Impact study', *Journal of Public Economics* **101**, 25–38.

Glewwe, P. and Kremer, M. (2006), 'Schools, teachers, and education outcomes in developing countries', *Handbook of the Economics of Education* **2**, 945–1017.

Glewwe, P. and Muralidharan, K. (2016), Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications, *in* 'Handbook of the Economics of Education', Vol. 5, Elsevier, pp. 653–743.

Greaves, E., Hussain, I., Rabe, B. and Rasul, I. (2019), Parental responses to information about school quality: Evidence from linked survey and administrative data, Technical report, ISER Working Paper Series.

Greaves, E., Hussain, I., Rabe, B. and Rasul, I. (2023), 'Parental responses to information about school quality: Evidence from linked survey and administrative data', *The Economic Journal* **133**(654), 2334–2402.

Guariso, A. and Björkman Nyqvist, M. (2023), The impact of the COVID-19 pandemic on children's learning and wellbeing: Evidence from India, Technical report, Stockholm School of Economics, Mistra Center for Sustainable Markets (Misum).

Hanushek, E. A. (2003), 'The failure of input-based schooling policies', *The Economic Journal* **113**(485), F64–F98.

Hassan, H., Islam, A., Siddique, A., Wang, L. C. et al. (2021), Telementoring and homeschooling during school closures: A randomized experiment in rural Bangladesh, Technical report, TUM School of Governance at the Technical University of Munich.

Houtenville, A. J. and Conway, K. S. (2008), 'Parental effort, school resources, and student achievement', *Journal of Human resources* **43**(2), 437–453.

Hsieh, C.-T. and Urquiola, M. (2006), 'The effects of generalized school choice on achievement and stratification: Evidence from Chile's voucher program', *Journal of public Economics* **90**(8-9), 1477–1503.

Imbens, G. W. and Angrist, J. D. (1994), 'Identification and estimation of local average treatment effects', *Econometrica: Journal of the Econometric Society* pp. 467–475.

Indus Action (2019), The bright spots report: Status of social inclusion through RTE section 12(1)(c), Technical report, Indus Action.

Jack, R., Halloran, C., Okun, J. and Oster, E. (2023), 'Pandemic schooling mode and student test scores: Evidence from US school districts', *American Economic Review: Insights* **5**(2), 173–190.

Jackson, C. K., Johnson, R. C. and Persico, C. (2016), 'The effects of school spending on educational and economic outcomes: Evidence from school finance reforms', *The Quarterly Journal of Economics* **131**(1), 157–218.

Khanna, G. (2020), 'Does affirmative action incentivize schooling? Evidence from India', *Review of Economics and Statistics* **102**(2), 219–233.

Khanna, G. (2023), 'Large-scale education reform in general equilibrium: Regression discontinuity evidence from India', *Journal of Political Economy* **131**(2), 549–591.

King, G., Honaker, J., Joseph, A. and Scheve, K. (2001), 'Analyzing incomplete political science data: An alternative algorithm for multiple imputation', *American political science review* **95**(1), 49–69.

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

Kingdon, G. G. (2020), 'The private schooling phenomenon in India: A review', *The Journal of Development Studies* **56**(10), 1795–1817.

Kirabo Jackson, C. (2010), 'Do students benefit from attending better schools? Evidence from rule-based student assignments in Trinidad and Tobago', *The Economic Journal* **120**(549), 1399–1429.

Kline, P., Rose, E. and Walters, C. (2022), Systemic discrimination among large US employers, Technical Report 4.

Kuhfeld, M., Soland, J., Tarasawa, B., Johnson, A., Ruzek, E. and Liu, J. (2020), 'Projecting the potential impact of COVID-19 school closures on academic achievement', *Educational Researcher* **49**(8), 549–565.

Lai, F., Sadoulet, E. and de Janvry, A. (2011), 'The contributions of school quality and teacher qualifications to student performance evidence from a natural experiment in Beijing middle schools', *Journal of Human Resources* **46**(1), 123–153.

Landerso, R., Nielsen, H. S. and Simonsen, M. (2017), 'How going to school affects the family', *Department of Economics Aarhus University* .

Moscoviz, L., Evans, D. K. et al. (2022), *Learning loss and student dropouts during the COVID-19 pandemic: A review of the evidence two years after schools shut down*, Center for Global Development.

Mukherjee, P., Beam, E. and Navarro-Sola, L. (2021), 'Take-up, use, and effectiveness of remote technologies'.

Muralidharan, K. and Kremer, M. (2006), 'Public and private schools in rural India', *Harvard University, Department of Economics, Cambridge, MA* **9**, 10–11.

Muralidharan, K. and Sundararaman, V. (2015), 'The aggregate effect of school choice: Evidence from a two-stage experiment in India', *The Quarterly Journal of Economics* **130**(3), 1011–1066.

Patrinos, H. A., Vegas, E. and Carter-Rau, R. (2022), 'An analysis of COVID-19 student learning loss'.

Pop-Eleches, C. and Urquiola, M. (2013), 'Going to a better school: Effects and behavioral responses', *American Economic Review* **103**(4), 1289–1324.

Pratham (2019), "annual status of education report, Technical report, Pratham.

Rabe, B. (2020), 'Schooling inputs and behavioral responses by families', *Handbook of Education Economics: A Comprehensive Overview* pp. chap. 16, 217—-227, 2nd ed.

Rao, G. (2019), 'Familiarity does not breed contempt: Generosity, discrimination, and diversity in Delhi schools', *American Economic Review* **109**(3), 774–809.

Romero, M., Sandefur, J. and Sandholtz, W. A. (2020), 'Outsourcing education: Experimental evidence from Liberia', *American Economic Review* **110**(2), 364–400.

Romero, M. and Singh, A. (2023), 'The incidence and effects of affirmative action: Evidence from quotas in private schools in India'.

Singh, A. (2015), 'Private school effects in urban and rural India: Panel estimates at primary and secondary school ages', *Journal of Development Economics* **113**, 16–32.

Singh, A., Romero, M. and Muralidharan, K. (2022), COVID-19 learning loss and recovery: Panel data evidence from India, Technical report, National Bureau of Economic Research.

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

Tooley, J. (2013), *The beautiful tree: A personal journey into how the world's poorest people are educating themselves*, Cato Institute.

Tooley, J. and Dixon, P. (2007), 'Private education for low-income families: Results from a global research project', *Private schooling in less economically developed countries: Asian and African perspectives* pp. 15–39.

Weiland, C., Unterman, R., Dynarski, S., Abenavoli, R., Bloom, H., Braga, B., Faria, A.-M., Greenberg, E., Jacob, B., Lincove, J. A. et al. (2023), 'Lottery-based evaluations of early education programs: Opportunities and challenges for building the next generation of evidence'.

A Appendix: Figures and Tables

Figure A1: Timeline of events

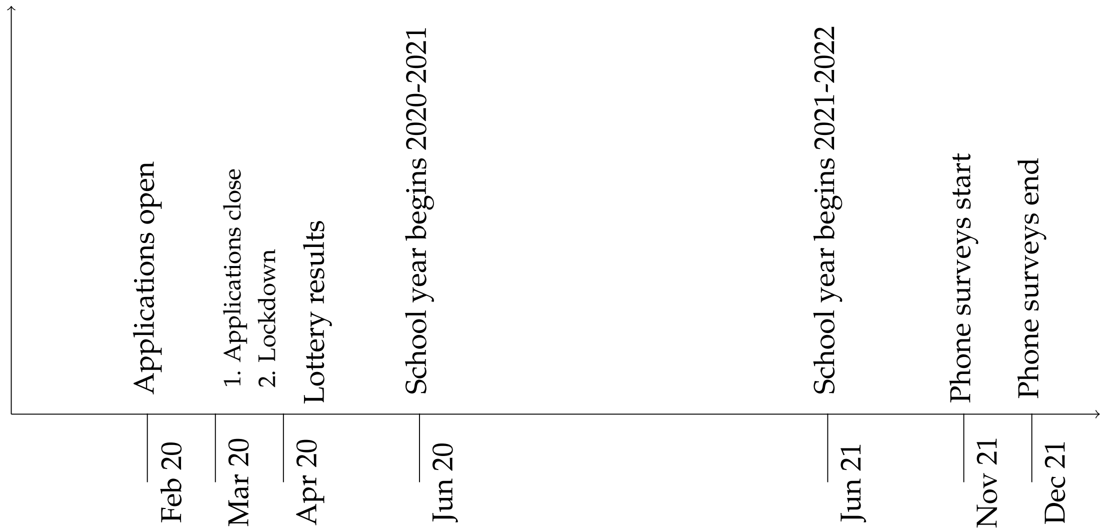
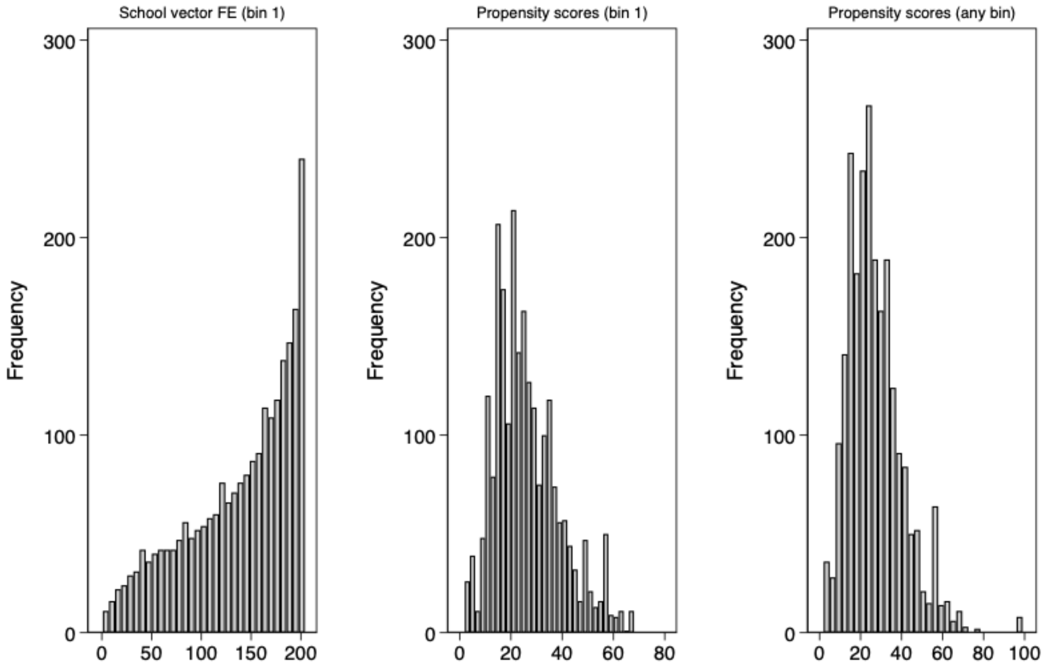


Figure A2: Distribution of school vector fixed effects and ex-ante propensity scores of winning



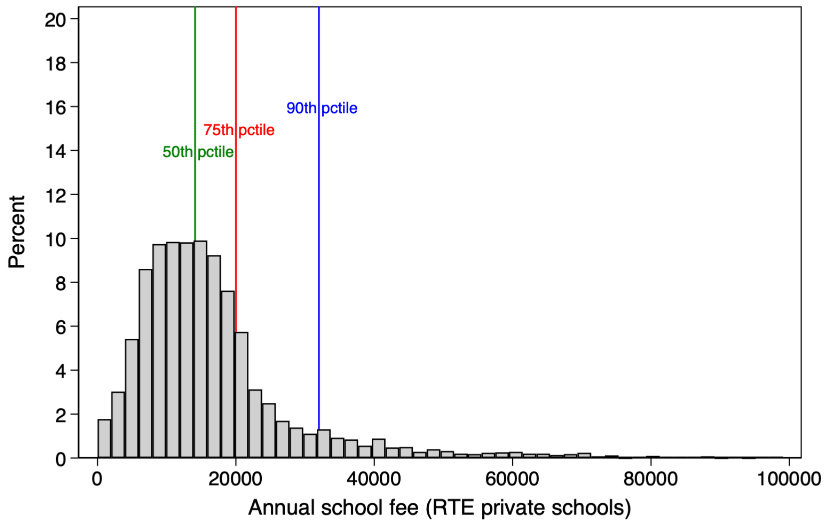
Notes: This is a histogram showing the distribution of school vector fixed effects (chosen in bin 1), and the simulated ex-ante propensity scores of winning the school lottery (in distance bin 1, and in any distance bin). The sample comprises surveyed applicants. In the sample of surveyed applicants, there are a total of 204 unique school vectors that are chosen in bin 1. A total of 193 school vectors out of these 204 vectors contribute to the identifying within-vector variation, i.e., they have at least one winner and at least one loser within bin 1. Distribution of simulated ex-ante propensity score bins in distance bin 1, and in any distance bin is also plotted. Here the propensity score bins are 0.01 interval wide.

Figure A3: English and Math questions asked during phone based assessments

Measure	Question and answers
1	If someone asks you "What is your name" and "What is your gender" then what would you reply? (phrase in quotes is said in English, the rest is said in Hindi.)
	correct; incorrect
2	Can you recite the letters of the English Alphabet?
	correct; incorrect
3	Can you tell me the spelling of "BOAT"?
	correct; incorrect
4	Can you tell me the spelling of "SWIM"?
	correct; incorrect
5	If you have 9 chocolates, and you get 1 more chocolate, how many chocolates will you have in total?
	correct (answer = 10); incorrect (answer ≠ 10)
6	If you have 22 chocolates, and you get 38 more chocolates, how many chocolates will you have in total?
	correct (answer = 60); incorrect (answer ≠ 60)
7	If you have 20 chocolates, and you give 4 chocolates to your friend, how many chocolates will you be left with?
	correct (answer = 16); incorrect (answer ≠ 16)
8	If you have 45 chocolates, and you give 26 chocolates to your friend, how many chocolates will you be left with?
	correct (answer = 19); incorrect (answer ≠ 19)
9	Can you tell me the number of "tens" and ones in the number 96?
	correct(answer = 9 tens and 6 ones)

Notes: This table shows the list of questions asked to children during phone-based assessments. For all questions, the question was said in Hindi, but the key phrases/numbers were said in English. For example, the following things were said in English - the phrase in quotes "What is your name" and "What is your gender" (for question 1); English Alphabet (for question 2); the words "Boat" and "Swim" (for question 3, 4); numbers like 9 chocolates, 20 chocolates etc. (for questions 5-9).

Figure A4: Distribution of annual school fee for RTE private schools



Notes: This histogram shows the distribution of annual school fee (in INR) for all the RTE private schools in Maharashtra. The data comes from the official website of the State Department of Education, Maharashtra.

Figure A5: Factor loadings from first component of PCA

Proportion of functional toilets (boys)	0.1804
Proportion of functional toilets (girls)	0.1517
School building is privately owned	0.309
School has pucca (brick and mortar) boundary walls	0.2265
School has library	0.0813
School has playground	0.1261
School has computer lab	0.2303
School has Internet	0.4024
Laptops per pupil	0.0751
Desktops per pupil	0.2322
Printers per pupil	0.144
Digiboard per pupil	0.173
English medium	0.3638
Proportion of teachers with undergraduate college degree or higher	0.1255
Proportion of teachers with Bachelors in Education degree or higher	0.3086
Proportion of regular teachers	0.2413
Proportion of teachers below age 55	0.0207
Proportion of teachers not involved in non-teaching tasks	0.2952
Proportion of children belonging to general caste	0.2465

Notes: This shows the factor loadings on each of the variable that is used in the construction of the school quality index using principal component analysis. The first component explains 18% variation in the data.

Figure A6: Tabulating eliteness across fee and PCA index measure

School is Elite (PCA > 50th pctile)

School is Elite (Fee > 50th pctile)	NO	YES	MISSING	Total
NO	44	36	0	80
YES	33	132	0	165
MISSING	6	7	8	21
Total	83	175	8	266

School is Elite (PCA > 75th pctile)

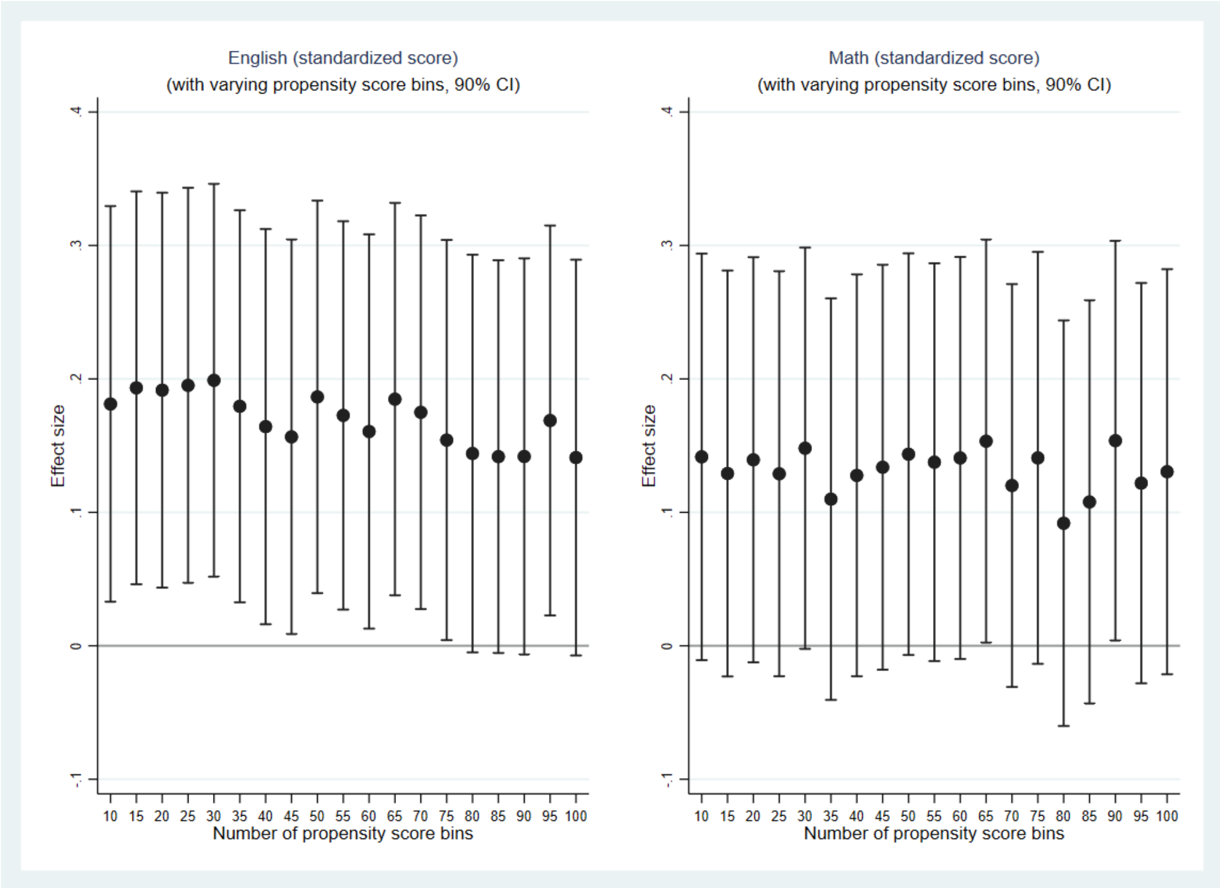
School is Elite (Fee > 75th pctile)	NO	YES	MISSING	Total
NO	103	18	0	121
YES	60	64	0	124
MISSING	6	7	8	21
Total	169	89	8	266

School is Elite (PCA > 90th pctile)

School is Elite (Fee > 90th pctile)	NO	YES	MISSING	Total
NO	159	14	0	173
YES	56	16	0	72
MISSING	13	0	8	21
Total	228	30	8	266

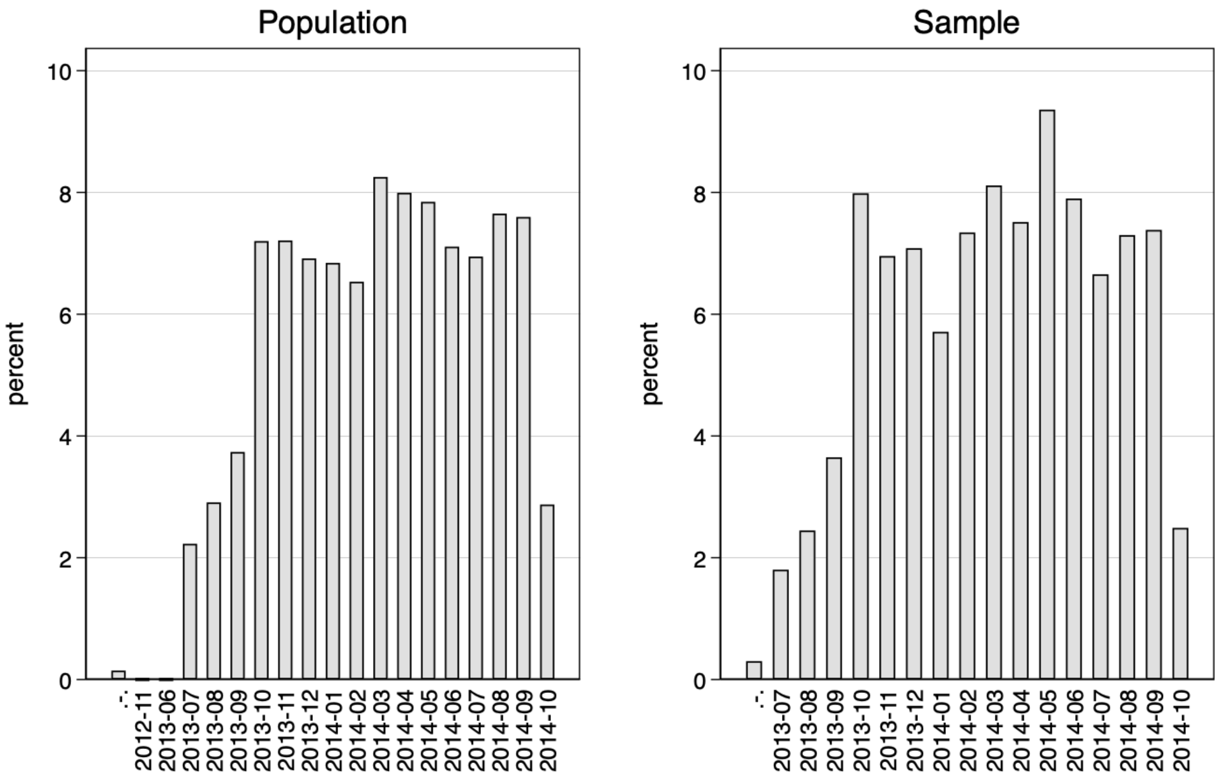
Notes: This provides a cross-tabulation of schools chosen by lottery winners, based on whether the school is categorized as elite or budget as per the PCA index measure and the school fee measure.

Figure A7: Robustness: LATE of being a RTE quota student on phone-based assessments



Notes: This figure plots the LATE of enrolling as an RTE student on children's performance in English and Math. It shows how the LATE changes as the number of bins of ex-ante propensity scores of winning are increased. The within comparisons become stricter as the number of propensity score bins are increased. The number of propensity score bins vary from 10, 15, 20, ..., 100. This utilizes the within variation resulting from comparison of treated and control students who have a similar ex-ante propensity of winning.

Figure A8: Robustness: Histogram of birth year and month



Notes: This figure shows the histogram of birth year-months for applicants to grade 1 private school lotteries under RTE policy in the 2020-21 school year. The left panel shows the distribution for the population and the right panel shows it for the sample. Some missing values exist. Birth year-months given by 2012-11 and 2013-06 are pertaining to only disability quota applicants and only appear in the population histogram. Disability quota is chosen very rarely and constitutes only 0.6% of the applications in the population. My sample does not contain any disability quota applicants. The majority of applications for grade 1 in 2020-21 school year can be seen as coming from those born in July 2013 and October 2014. Among these, applicants born between July 2014 and October 2014 are age-eligible to re-apply for grade 1 in the following year i.e., during the 2021-22 RTE lotteries. In one of my robustness checks, I remove these applicants who were still age-eligible to re-apply for the RTE lotteries in the 2021-22 school year, and find that my results are robust to removing them.

Table A1: Summary statistics

Variable	N	Mean	SD	Min	Max
Characteristics of applicants in Phone Survey					
Winner (distance bin 1)	2,329	0.44	0.50	0	1
Winner (any distance bin)	2,329	0.45	0.50	0	1
Waitlisted (any distance bin)	2,329	0.26	0.44	0	1
Loser	2,329	0.29	0.46	0	1
Age	2,329	7.62	0.33	7.05	8.34
Male	2,329	0.55	0.50	0	1
Number of schools chosen (RTE application)	2,329	4.86	2.89	1	10
Applied under low income quota	2,329	0.28	0.45	0	1
Schooling details for applicants					
Academic year: 2020-21					
School enrollment	2,329	0.89	0.31	0	1
School provides instruction	2,083	0.89	0.31	0	1
Academic year: 2021-22					
School enrollment	2,329	0.97	0.18	0	1
School provides instruction	2,255	0.98	0.14	0	1
School is Private	2,255	0.88	0.32	0	1
School is English medium	2,255	0.94	0.25	0	1
Instructional days at school	2,255	5.30	1.42	0	7
Number of subjects taught	2,107	5.93	1.85	1	12
Monetary investments in applicants					
Any educational expense (in the past year)	2,227	0.93	0.26	0	1
Annual educational expenses (INR; in the past year)	2,227	3,514	3,234	0	24,000
Time investments in applicants					
Child gets help with homework in the household	2,329	0.93	0.26	0	1
Hours of household help with homework (hours per week)	2,329	9.50	5.91	0	49
Time use of applicants					
Attending school (hours per week)	2,329	12	7.98	0	36
Attending tuition (hours per week)	2,329	4.67	6.10	0	21
Doing homework (hours per day)	2,329	1.40	0.73	0	3.30
Playing (hours per day)	2,328	2.45	1.18	0	6
Watching Television (hours per day)	2,322	1.10	0.91	0	4
Helping with household chores (hours per day)	2,329	0.39	0.40	0	2
Performance on phone assessments by applicants					
English score (standardized)	695	-0.00	1.00	-1.56	1.92
Math score (standardized)	695	-0.00	1.00	-1.81	1.91
Parental education					
Mother's education > primary	2,329	0.62	0.49	0	1
Fathers's education > primary	2,329	0.54	0.50	0	1
Household characteristics					
Number of household members	2,329	5.14	2.10	2	20
Number of siblings of applicant child	2,329	0.88	0.57	0	5
General Caste	2,329	0.26	0.44	0	1
Scheduled Caste	2,329	0.25	0.43	0	1
Scheduled Tribe	2,329	0.04	0.19	0	1
Other Backward Class (OBC)	2,329	0.46	0.50	0	1
Hindu	2,329	0.81	0.39	0	1
Muslim	2,329	0.09	0.29	0	1
Buddhist	2,329	0.09	0.29	0	1
Other religion	2,329	0.01	0.09	0	1
Household SES index (PCA)	2,329	0.00	1.21	-2.51	6.23
Annual household earnings (INR 1000)	2,001	180	132	2.40	1,200

Notes: This table shows the summary statistics of survey participants who comprise the sample. Most of the data in this table comes from phone-survey data conducted during the months of Nov-Dec 2021 (18 months after RTE results came out). Characteristics of applicants, religion, and caste information comes from the administrative data of RTE applications. Some of the variables are conditional on other variables, such as indicator of whether school provides instruction, and other variables under schooling details, are conditioned on school enrollment. Monetary investments are asked for the past year i.e., 2020-21, and includes expenses on child's education on stationary, books etc. (excluding school fee). Time investments by parents and household members is calculated by asking about time spent helping child with educational activities on a typical day of the week in the past week (along with number of days). Applicants' time use is calculated by asking about time spent on each activity on a typical day in the past week, and additionally, number of days per week for variables that measure weekly hours. English and Math scores are standardized - the English assessment had four questions, the Math assessment had five questions. Household SES index is created using Principal Components Analysis using data on asset ownership of television, air conditioner, two-wheeler, and four-wheeler.

Table A2: Balance in baseline characteristics

Variable	(1) Non winners (any bin)	(2) Winners (any bin)	(3) Difference ((2)-(1))
Age of applicant (as on 1st Nov 2021)	7.622	7.611	-0.014
	(0.326)	(0.329)	(0.014)
Male	0.545	0.560	0.012
	(0.498)	(0.497)	(0.021)
Schools chosen overall (RTE application)	4.935	4.759	-0.094
	(2.905)	(2.877)	(0.113)
Applied under low income quota	0.288	0.275	-0.015
	(0.453)	(0.447)	(0.019)
Mother's education > primary	0.871	0.890	0.022
	(0.336)	(0.313)	(0.014)
Father's education > primary	0.822	0.849	0.030*
	(0.383)	(0.359)	(0.016)
Number of household members	5.130	5.149	0.028
	(2.119)	(2.078)	(0.089)
Number of siblings of applicant	0.881	0.874	-0.011
	(0.586)	(0.544)	(0.024)
General Caste	0.261	0.251	-0.012
	(0.439)	(0.434)	(0.018)
Scheduled Caste	0.259	0.233	-0.023
	(0.439)	(0.423)	(0.018)
Scheduled Tribe	0.038	0.034	-0.006
	(0.191)	(0.181)	(0.008)
Other Backward Classes	0.442	0.482	0.042**
	(0.497)	(0.500)	(0.021)
Hindu	0.795	0.823	0.030*
	(0.404)	(0.382)	(0.017)
Muslim	0.097	0.086	-0.011
	(0.296)	(0.280)	(0.012)
Buddhist	0.098	0.086	-0.014
	(0.298)	(0.280)	(0.012)
Other religion	0.010	0.006	-0.004
	(0.100)	(0.076)	(0.004)
Household SES index (PCA)	0.057	-0.071	-0.117**
	(1.250)	(1.145)	(0.051)
Annual income from survey (INR 1000)	189.750	163.519	-25.531***
	(127.564)	(105.862)	(5.405)
Observations	1,291	1,038	2,329

Notes: This table shows the balance in baseline characteristics across non-winning and winning applicants (in any bin). The differences in column (3) control for the fixed effects of ex-ante propensity of winning the lottery in any bin such that the comparisons across winners and losers are for ex-ante similar applicants. Columns (1) and (2) contain the mean and standard deviation of the variables for non-winners and winners. Column (3) contains the coefficient in front of the dummy of being a winner from the regression of the outcome variable (displayed in the rows) on the dummy of winning, after controlling for the ex-ante propensity of winning in any bin (propensity score bins are 0.01 wide). Column (3) shows standard errors in parenthesis.

Table A3: Attrition: participation in the phone survey

Panel A: Sample includes everyone who was ever called for phone surveys

	(1) Participation, Survey = 0	(2) Participation, Survey = 1	(3) Difference ((2)-(1))
Winner (any distance bin)	0.394	0.446	0.057***
	(0.489)	(0.497)	(0.016)
Age of applicant (as on 1st Nov 2021)	7.639	7.617	-0.022**
	(0.335)	(0.328)	(0.011)
Male	0.523	0.547	0.027*
	(0.500)	(0.498)	(0.016)
Schools chosen overall (RTE application)	4.753	4.857	0.055
	(2.928)	(2.893)	(0.065)
Applied under low income quota	0.296	0.282	0.010
	(0.457)	(0.450)	(0.013)
General Caste	0.265	0.257	0.016
	(0.441)	(0.437)	(0.013)
Scheduled Caste	0.250	0.248	0.002
	(0.433)	(0.432)	(0.013)
Scheduled Tribe	0.033	0.036	-0.002
	(0.179)	(0.186)	(0.006)
Other Backward Classes	0.452	0.459	-0.016
	(0.498)	(0.498)	(0.015)
Hindu	0.794	0.807	0.020*
	(0.404)	(0.395)	(0.012)
Muslim	0.102	0.092	-0.008
	(0.303)	(0.289)	(0.008)
Buddhist	0.093	0.093	-0.008
	(0.290)	(0.290)	(0.009)
Other religion	0.011	0.008	-0.003
	(0.104)	(0.090)	(0.003)
Observations	1,930	2,329	4,259

Panel B: Sample includes everyone who agreed to participate in surveys

	(1) Participation, Phone Assessments = 0	(2) Participation, Phone Assessments = 1	(3) Difference ((2)-(1))
Winner (any distance bin)	0.437	0.466	0.026
	(0.496)	(0.499)	(0.024)
Age of applicant (as on 1st Nov 2021)	7.610	7.635	0.029*
	(0.332)	(0.316)	(0.016)
Male	0.575	0.479	-0.099***
	(0.494)	(0.500)	(0.024)
Schools chosen overall (RTE application)	4.765	5.072	0.147
	(2.881)	(2.913)	(0.094)
Applied under low income quota	0.285	0.275	-0.015
	(0.452)	(0.447)	(0.019)
General Caste	0.260	0.249	-0.012
	(0.439)	(0.433)	(0.019)
Scheduled Caste	0.242	0.260	0.002
	(0.429)	(0.439)	(0.020)
Scheduled Tribe	0.035	0.039	-0.001
	(0.184)	(0.193)	(0.009)
Other Backward Classes	0.463	0.452	0.011
	(0.499)	(0.498)	(0.022)
Hindu	0.810	0.800	-0.014
	(0.392)	(0.400)	(0.018)
Muslim	0.091	0.095	0.005
	(0.287)	(0.293)	(0.012)
Buddhist	0.091	0.098	0.011
	(0.287)	(0.297)	(0.013)
Other religion	0.009	0.007	-0.003
	(0.092)	(0.085)	(0.004)
Observations	1,634	695	2,329

Notes: This table shows the balance across survey respondents and non-respondents. The sample comprises all the applicants who were ever called for phone-surveys. Column (2) comprises those who agreed to be interviewed and with whom interviews were successfully conducted and column (1) comprises those who did not agree to be interviewed and with whom interviews were not conducted. Columns (1) and (2) contain the mean and standard deviation of the variables for non-participants and participants. Column (3) contains the coefficient in front of the dummy of participation from the regression of the outcome variable (displayed in the rows) on the dummy of participation. The regressions control for the fixed effects of ex-ante propensity of winning in any bin. Column (3) shows standard errors in parenthesis. Winner is a dummy that takes value = 1 for those who won the lottery to a school in any bin. Winning households are slightly more like to participate in the survey relative to the non-winning households.

Table A4: Characteristics of lottery compliers, always- and never-takers in Maharashtra's RTE

Variable	Compliers: Untreated (1)	Compliers: Treated (2)	Always-takers (3)	Never-takers (4)
Male	0.545	0.560	0.561	0.540
	(0.019)	(0.017)	(0.010)	(0.010)
Low income quota applicant	0.295	0.276	0.300	0.248
	(0.017)	(0.015)	(0.009)	(0.009)
Caste quota applicant	0.704	0.723	0.699	0.751
	(0.017)	(0.015)	(0.009)	(0.009)
General caste	0.274	0.258	0.263	0.201
	(0.017)	(0.015)	(0.009)	(0.008)
Scheduled Caste	0.245	0.216	0.299	0.302
	(0.017)	(0.014)	(0.009)	(0.009)
Scheduled tribe	0.043	0.035	0.019	0.016
	(0.007)	(0.006)	(0.002)	(0.002)
Other caste	0.437	0.489	0.417	0.480
	(0.019)	(0.017)	(0.010)	(0.010)
Hindu	0.796	0.833	0.796	0.773
	(0.015)	(0.013)	(0.008)	(0.009)
Muslim	0.096	0.082	0.106	0.094
	(0.011)	(0.009)	(0.006)	(0.006)
Buddhist	0.094	0.077	0.096	0.123
	(0.011)	(0.009)	(0.006)	(0.007)
Mother education > primary	0.868	0.897	0.903	0.849
	(0.013)	(0.010)	(0.006)	(0.007)
Father education > primary	0.822	0.859	0.752	0.846
	(0.015)	(0.013)	(0.009)	(0.008)
Mother works	0.233	0.212	0.230	0.260
	(0.016)	(0.014)	(0.009)	(0.009)
Father works	0.943	0.946	0.949	0.967
	(0.008)	(0.007)	(0.004)	(0.003)
Share of observations	.81		.07	.12

Notes: This table reports the estimates of average baseline characteristics of compliers, always-takers, and never-takers among lottery applicants to private schools under Maharashtra's RTE quotas. Means are computed from 2SLS and OLS regressions that control for lottery risk set indicators (or,ex-ante propensity scores of winning the lottery), as described in Abadie (2002) (see Appendix Section C.1 for details on implementation). Robust standard errors in parenthesis.

Table A5: Counterfactual densities for Maharashtra's RTE Compliers

Destiny	RTE Private school
	Z=0
	(1)
Fee-paying student at RTE Private school	0.564
	(0.019)
Fee-paying student at Non-RTE Private school	0.112
	(0.013)
Government school	0.191
	(0.015)
Out-of-school	0.052
	(0.009)
Pscores of winning	Yes

Notes: This table reports the share of untreated (Z=0) compliers enrolled at particular fallback school types among applicants to Maharashtra's RTE private school lotteries. Means are computed from 2SLS regressions that control for the ex-ante propensity scores of winning the lottery, as described in Abadie (2002). I describe the implementation of this in Appendix Section C.1. Among lottery losers, there are some children for whom the school name and the official school code could not be matched with the administrative data on the population of schools. Thus, for these children the school sector – private, government, or out-of-school – is missing. It is for this reason that the counterfactual destinies don't add up to one. Robust standard errors in parenthesis.

Table A6: LATE of being a RTE quota student on school quality index

	Joint index	Infrastructure index	Digital index	Teacher index	Peer SES index
	(1)	(2)	(3)	(4)	(5)
Enrolled as RTE student	0.613***	0.329***	0.434***	0.411***	0.219***
	(0.050)	(0.053)	(0.052)	(0.052)	(0.048)
First stage F-stat	3,608.10	3,608.10	3,608.10	3,608.10	3,608.10
Outcome mean	-0.00	0.00	0.00	0.00	-0.00
Control mean	-0.27	-0.15	-0.15	-0.21	-0.11
Observations	2,086	2,086	2,086	2,086	2,086
R^2	0.20	0.10	0.14	0.14	0.28
Pscores of winning	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on school quality, when the instrument is winning the lottery in any bin. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A7: LATE of being a RTE quota student on subjects taught

	Math	English	Marathi	Hindi	Science	Environment studies
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled as RTE student	0.017	0.028**	-0.001	0.112***	0.021	0.113***
	(0.014)	(0.014)	(0.019)	(0.022)	(0.020)	(0.026)
First stage F-stat	3,877.71	3,877.71	3,877.71	3,877.71	3,877.71	3,877.71
Outcome mean	0.92	0.93	0.84	0.78	0.18	0.53
Control mean	0.91	0.92	0.84	0.73	0.17	0.48
Observations	2,255	2,255	2,255	2,255	2,255	2,255
R^2	0.07	0.06	0.05	0.09	0.05	0.10
Pscores of winning	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
	Computers	General knowledge	Art/craft	Music	Dance	Physical education
	(7)	(8)	(9)	(10)	(11)	(12)
Enrolled as RTE student	0.136***	0.096***	0.108***	0.067***	0.049***	-0.000
	(0.024)	(0.024)	(0.024)	(0.013)	(0.012)	(0.011)
First stage F-stat	3,877.71	3,877.71	3,877.71	3,877.71	3,877.71	3,877.71
Outcome mean	0.29	0.32	0.31	0.07	0.05	0.95
Control mean	0.23	0.27	0.27	0.04	0.03	0.96
Observations	2,255	2,255	2,255	2,255	2,255	2,255
R^2	0.08	0.08	0.09	0.08	0.08	0.06
Pscores of winning	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on the subjects taught at school when the instrument is winning the lottery in any bin. Outcomes measure the indicator of whether school teaches a particular subject. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A8: Time investments in children by household members: Extensive Margin

	Mother	Father	Grandparents	Siblings	Uncle/Aunt	Neighbors
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled as RTE student	0.056***	0.026	0.010	-0.014	0.000	-0.005
	(0.020)	(0.023)	(0.006)	(0.016)	(0.010)	(0.004)
First stage F-stat	3,916.37	3,916.37	3,916.37	3,916.37	3,916.37	3,916.37
Outcome mean	0.81	0.28	0.01	0.11	0.04	0.01
Control mean	0.79	0.26	0.01	0.11	0.04	0.01
Observations	2,329	2,329	2,329	2,329	2,329	2,329
R^2	0.12	0.10	0.04	0.05	0.07	0.02
Pscores of winning (any bin)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending RTE private schools as a quota student on the indicator of whether a specific household member helps the child with educational activities. The outcome variables capture the extensive margin of whether child gets any help from mom, dad, and grandparents. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A9: First stage of winning the RTE lottery (in bin 1) on enrollment as a RTE quota student

	Enrolled as RTE student	
	(1)	(2)
Instrument = Winning lottery in Bin 1	0.790***	0.787***
	(0.013)	(0.013)
Outcome mean	0.44	0.44
Control mean	0.09	0.09
Observations	2,329	2,329
R^2	0.66	0.64
School vector FE (bin 1)	Yes	No
Pscores of winning (bin 1)	No	Yes
Controls	Yes	Yes

Notes: This table shows the first stage effects of winning the RTE private school lottery in distance bin 1, on enrollment as an RTE quota student in a private school. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Column (1) controls for the fixed effects of school vector chosen in bin 1, and Column (2) controls for the ex-ante propensity of winning the lottery in bin 1. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A10: LATE of being a RTE quota student (in bin 1) on enrollment

	Enrollment (2021-22)		Enrollment (2021-22)		Grade 2 and above (2021-22)	
	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled as RTE student	0.130***	0.140***	0.045***	0.047***	0.186***	0.194***
	(0.015)	(0.016)	(0.009)	(0.009)	(0.017)	(0.017)
First stage F-stat	3,589.78	3,751.65	3,617.60	3,776.54	3,611.65	3,772.53
Outcome mean	0.89	0.89	0.97	0.97	0.86	0.86
Control mean	0.84	0.84	0.94	0.94	0.78	0.78
Observations	2,328	2,328	2,328	2,328	2,327	2,327
R^2	0.20	0.11	0.13	0.08	0.20	0.15
School vector FE (bin 1)	Yes	No	Yes	No	Yes	No
Pscores of winning (bin 1)	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on children's enrollment when the instrument is winning the lottery in bin 1. The outcomes measure the indicator of school enrollment in the two academic years. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Odd numbered columns control for the fixed effects of school vector chosen in bin 1, and even numbered columns control for the ex-ante propensity of winning the lottery in bin 1. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A11: LATE of being a RTE quota student (in bin 1) on test scores

	Test score (standardized)			
	English		Math	
	(1)	(2)	(3)	(4)
Enrolled as RTE student	0.169**	0.178*	0.093	0.135
	(0.085)	(0.091)	(0.090)	(0.093)
First stage F-stat	947.07	1,194.62	947.07	1,194.62
Outcome mean	-0.00	-0.00	-0.00	-0.00
Control mean	-0.10	-0.10	-0.09	-0.09
Observations	695	695	695	695
R^2	0.41	0.16	0.33	0.11
School vector FE (bin 1)	Yes	No	Yes	No
Pscores of winning (bin 1)	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on children's performance on phone-based assessments when the instrument is winning the lottery in bin 1. Outcomes measure children's standardized test scores on English and Math. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Odd numbered columns control for the fixed effects of school vector chosen in bin 1, and even numbered columns control for the ex-ante propensity of winning the lottery in bin 1. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A12: School characteristics in Elite and Budget schools

Variable	Fee			PCA		
	Budget (1)	Elite (2)	Difference (3)	Budget (4)	Elite (5)	Difference (6)
Proportion functional toilets (boys)	1.000	0.988	0.000	0.994	0.996	-0.000
	(0.000)	(0.101)	(0.000)	(0.079)	(0.030)	(0.000)
Proportion functional toilets (girls)	0.995	0.988	0.012	0.990	0.996	0.009
	(0.059)	(0.101)	(0.015)	(0.095)	(0.030)	(0.015)
School building is privately owned	0.437	0.602	0.172	0.356	0.870	0.464***
	(0.498)	(0.492)	(0.108)	(0.480)	(0.339)	(0.100)
School building has pucca boundary	0.778	0.971	0.009	0.812	0.986	0.142**
	(0.417)	(0.169)	(0.068)	(0.392)	(0.120)	(0.067)
School has library	0.937	0.981	0.016	0.938	1.000	0.100*
	(0.245)	(0.139)	(0.058)	(0.243)	(0.000)	(0.058)
School has playground	0.905	0.981	0.051	0.925	0.971	0.062
	(0.295)	(0.139)	(0.062)	(0.264)	(0.169)	(0.063)
School has computer lab	0.143	0.350	0.125	0.156	0.420	0.265***
	(0.351)	(0.479)	(0.078)	(0.364)	(0.497)	(0.076)
School has internet	0.849	1.000	0.139**	0.881	1.000	0.161**
	(0.359)	(0.000)	(0.065)	(0.325)	(0.000)	(0.065)
Laptops per pupil	0.003	0.005	0.004	0.003	0.005	0.005*
	(0.005)	(0.013)	(0.003)	(0.007)	(0.014)	(0.003)
Desktops per pupil	0.025	0.037	0.007	0.023	0.046	0.025***
	(0.028)	(0.034)	(0.008)	(0.026)	(0.037)	(0.008)
Printers per pupil	0.004	0.005	0.001	0.004	0.005	0.002*
	(0.003)	(0.004)	(0.001)	(0.003)	(0.005)	(0.001)
Digiboards per pupil	0.002	0.008	0.007***	0.002	0.011	0.008***
	(0.005)	(0.011)	(0.002)	(0.005)	(0.012)	(0.002)
School is English medium	0.849	1.000	0.169**	0.881	1.000	0.166**
	(0.359)	(0.000)	(0.071)	(0.325)	(0.000)	(0.072)
Prop. of teachers trained in computer	0.449	0.628	0.255***	0.511	0.571	-0.013
	(0.386)	(0.367)	(0.088)	(0.386)	(0.388)	(0.092)
Prop. of teachers who are graduates	0.736	0.859	0.074	0.759	0.867	0.079
	(0.292)	(0.197)	(0.055)	(0.281)	(0.186)	(0.055)
Prop. of teachers with Bachelors in Education	0.421	0.682	0.200***	0.462	0.716	0.213***
	(0.282)	(0.192)	(0.057)	(0.281)	(0.167)	(0.058)
Prop. of full time teachers	0.673	0.759	0.005	0.725	0.680	-0.157*
	(0.417)	(0.362)	(0.095)	(0.394)	(0.397)	(0.094)
Prop. of contract teachers	0.316	0.232	0.011	0.268	0.303	0.149
	(0.415)	(0.364)	(0.093)	(0.392)	(0.403)	(0.093)
Prop. of part-time teachers	0.011	0.009	-0.016	0.007	0.017	0.008
	(0.065)	(0.027)	(0.015)	(0.047)	(0.060)	(0.015)
Prop. of teachers < 55 years	0.953	0.965	0.009	0.951	0.975	0.049**
	(0.107)	(0.068)	(0.021)	(0.106)	(0.038)	(0.020)
Prop. of teachers not involved in non-teaching tasks	0.859	0.919	-0.015	0.855	0.959	0.068
	(0.295)	(0.234)	(0.059)	(0.302)	(0.159)	(0.060)
Teachers per pupil	0.037	0.040	0.010	0.037	0.042	0.010
	(0.029)	(0.019)	(0.006)	(0.027)	(0.021)	(0.006)
Prop. of general caste category students	0.273	0.527	0.097***	0.334	0.512	0.075***
	(0.283)	(0.271)	(0.025)	(0.296)	(0.290)	(0.026)
Observations	126	103	229	160	69	229

Notes: This table shows the balance in school characteristics for elite and budget schools, where eliteness is defined using the two measures: school fee and PCA index. Schools lying above the 75^{th} percentile value in the distribution of fee and PCA index of all the private schools in the state are classified as elite schools, and classified as budget, otherwise. The sample comprises schools being attended by lottery winners. Columns (1), (2), (3), and (4) show the mean and standard-deviations of the characteristics for budget and elite schools based on the two quality measures. Columns (3) and (4) contain the coefficient on the indicator of "school is elite" from the regression of the outcome variable (dispalyed in rows) on the indicator of school being elite, after controlling for the geography fixed effects at the village level (standard errors in parenthesis).

Table A13: Correlation between fee-based school eliteness and PCA-based school eliteness

	School Fee (log)
	(1)
School quality index (standardized)	0.272***
	(0.015)
Outcome mean	9.51
Observations	4,019
R^2	0.51
Village FE	Yes

Notes: This table shows the regression of the log of school fee on the school quality index on the population of RTE schools for whom there is non-missing data on school fee.

Table A14: LATE of attending an elite schools as a RTE quota student on subjects taught

	Math	English	Marathi	Hindi	Science	Envt studies	Comp-uters	General knowledge	Art/ craft	Music	Dance	Phys ed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Elite based on Fee												
RTE student	0.014	0.029	-0.043	-0.020	-0.111**	0.033	0.054	0.241***	0.145**	0.138***	0.085**	-0.013
Elite school	(0.036)	(0.033)	(0.051)	(0.054)	(0.055)	(0.070)	(0.067)	(0.067)	(0.069)	(0.041)	(0.036)	(0.032)
F-stat	1,090	1,090	1,090	1,090	1,090	1,090	1,090	1,090	1,090	1,090	1,090	1,090
Outcome mean	0.93	0.94	0.85	0.83	0.18	0.59	0.35	0.36	0.36	0.10	0.08	0.95
Control mean	0.92	0.93	0.84	0.79	0.18	0.52	0.26	0.28	0.32	0.03	0.02	0.95
Observations	965	965	965	965	965	965	965	965	965	965	965	965
R^2	0.08	0.08	0.08	0.08	0.07	0.08	0.12	0.12	0.06	0.17	0.16	0.10
Pscores	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Elite based on PCA												
RTE student	0.039	0.025	0.072	0.015	-0.057	0.153*	0.117	0.182**	0.041	0.121**	0.126***	0.004
Elite school	(0.042)	(0.038)	(0.058)	(0.061)	(0.063)	(0.078)	(0.076)	(0.076)	(0.077)	(0.047)	(0.042)	(0.035)
F-stat	1,099	1,099	1,099	1,099	1,099	1,099	1,099	1,099	1,099	1,099	1,099	1,099
Outcome mean	0.93	0.94	0.84	0.82	0.19	0.58	0.35	0.36	0.36	0.10	0.07	0.95
Control mean	0.92	0.93	0.85	0.80	0.20	0.53	0.31	0.33	0.36	0.07	0.06	0.96
Observations	1,011	1,011	1,011	1,011	1,011	1,011	1,011	1,011	1,011	1,011	1,011	1,011
R^2	0.07	0.08	0.07	0.07	0.05	0.08	0.08	0.09	0.07	0.09	0.05	0.10
Pscores	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on the subjects taught at child's school. The sample is restricted to lottery winners. Envt studies refers to Environment studies, and Phy Ed refers to Physical Education. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A15: First stage (varying the elite cutoff)

	RTE student at Elite school	
	Elite (PCA)	Elite (Fee)
	(1)	(2)
Panel A: Eliteness defined at 50^{th} pctile		
Won RTE lottery at Elite school	0.878***	0.881***
	(0.032)	(0.031)
Outcome mean	0.67	0.65
Control mean	0.00	0.00
Observations	1,019	973
R^2	0.70	0.72
Pscores of winning at elite	Yes	Yes
Controls	Yes	Yes
Avg quality (Elite=1)	3.79	37143.46
Avg quality (Elite=0)	1.56	10292.29

	RTE student at Elite school	
	Elite (PCA)	Elite (Fee)
	(1)	(2)
Panel B: Eliteness defined at 90^{th} pctile		
Won RTE lottery at Elite school	0.865***	0.850***
	(0.022)	(0.026)
Outcome mean	0.10	0.31
Control mean	0.00	0.00
Observations	1,019	973
R^2	0.89	0.86
Pscores of winning at elite	Yes	Yes
Controls	Yes	Yes
Avg quality (Elite=1)	4.96	54733.34
Avg quality (Elite=0)	2.71	16090.5

Notes: This table shows the first stage effects of winning the RTE private school lottery at an elite school on enrollment at an elite school as a quota student. Here, I present the results with two different percentile cutoffs of eliteness - at 50^{th} and 90^{th} percentile in panel A and B, respectively. The sample is restricted to lottery winners. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A16: LATE of attending elite schools on performance on tests (varying the elite cutoff)

	English	Math	English	Math
	Elite (PCA)		Elite (Fee)	
	(1)	(2)	(3)	(4)
Panel A: Eliteness defined at 50^{th} pctile				
RTE student at Elite school	0.083	0.256	0.189	0.241
	(0.252)	(0.250)	(0.217)	(0.220)
First stage F-stat	229.44	229.44	333.36	333.36
Outcome mean	0.04	0.06	0.05	0.06
Control mean	-0.12	-0.08	-0.18	-0.19
Observations	318	318	303	303
R^2	0.12	0.15	0.16	0.18
Pscores of winning at elite	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

	English	Math	English	Math
	Elite (PCA)		Elite (Fee)	
	(1)	(2)	(3)	(4)
Panel B: Eliteness defined at 90^{th} pctile				
RTE student at Elite school	0.219	0.510	0.024	-0.094
	(0.421)	(0.422)	(0.308)	(0.309)
First stage F-stat	1,821.94	1,821.94	567.59	567.59
Outcome mean	0.04	0.06	0.05	0.06
Control mean	0.02	0.02	-0.07	-0.02
Observations	318	318	303	303
R^2	0.11	0.12	0.14	0.17
Pscores of winning at elite	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on children's performance on phone based assessments. The results correspond to the 50^{th} and 90^{th} percentile cutoffs of eliteness in panel A and B, respectively. The sample is restricted to lottery winners. As before, the number of observations is smaller here because the phone-based assessment on English and Math is available only for a subsample of lottery winners. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A17: LATE of attending elite schools on school instruction (varying the elite cutoff)

	Synchronous classes (online)	Recordings shared (audio/video)	Text-based activity plans (WhatsApp/SMS)	Synchronous classes (online)	Recordings shared (audio/video)	Text-based activity plans (WhatsApp/SMS)
	Elite (PCA)			Elite (Fee)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Eliteness defined at 50^{th} pctile						
Quota student at Elite school	0.195***	-0.021	-0.162**	0.244***	-0.110**	-0.194***
	(0.050)	(0.046)	(0.068)	(0.046)	(0.045)	(0.066)
First stage F-stat	788.37	788.37	788.37	864.10	864.10	864.10
Outcome mean	0.83	0.13	0.53	0.83	0.13	0.53
Control mean	0.73	0.13	0.55	0.65	0.18	0.60
Observations	1,005	1,005	1,005	959	959	959
R^2	0.16	0.07	0.09	0.23	0.07	0.11
Pscores of winning at elite	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
	Synchronous classes (online)	Recordings shared (audio/video)	Text-based activity plans (WhatsApp/SMS)	Synchronous classes (online)	Recordings shared (audio/video)	Text-based activity plans (WhatsApp/SMS)
	Elite (PCA)			Elite (Fee)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Eliteness defined at 90^{th} pctile						
Quota student at Elite school	0.108	-0.005	0.257*	0.196***	-0.018	-0.128
	(0.103)	(0.092)	(0.139)	(0.063)	(0.058)	(0.083)
First stage F-stat	1,453.58	1,453.58	1,453.58	1,326.33	1,326.33	1,326.33
Outcome mean	0.83	0.13	0.53	0.83	0.13	0.53
Control mean	0.81	0.13	0.52	0.77	0.15	0.58
Observations	1,005	1,005	1,005	959	959	959
R^2	0.08	0.04	0.04	0.11	0.06	0.11
Pscores of winning at elite	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on school's instruction modality. The results correspond to the 50^{th} and 90^{th} percentile cutoffs of eliteness in panel A and B, respectively. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A18: LATE of attending elite schools on children's time use
(varying the elite cutoff)

	School (hrs/week)	Tuition (after school) (hrs/week)	Homework (hrs/day)	School (hrs/week)	Tuition (after school) (hrs/week)	Homework (hrs/day)
	Elite (PCA)			Elite (Fee)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Eliteness defined at 50^{th} pctile						
Quota student at Elite school	2.874***	-0.405	0.091	3.272***	-0.825	-0.006
	(0.985)	(0.815)	(0.095)	(0.988)	(0.810)	(0.095)
First stage F-stat	725.95	725.95	725.95	750.79	750.79	750.79
Outcome mean	13.58	4.43	1.51	13.57	4.49	1.52
Control mean	12.60	4.30	1.45	12.18	4.49	1.45
Observations	1,019	1,019	1,019	973	973	973
R^2	0.13	0.09	0.06	0.13	0.10	0.06
Pscores of winning at elite	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

	School (hrs/week)	Tuition (after school) (hrs/week)	Homework (hrs/day)	School (hours/week)	Tuition (after school) (hrs/week)	Homework (hrs/day)
	Elite (PCA)			Elite (Fee)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Eliteness defined at 90^{th} pctile						
Quota student at Elite school	2.885	-2.674	0.312	0.528	0.040	-0.290**
	(2.053)	(1.650)	(0.194)	(1.304)	(1.047)	(0.124)
First stage F-stat	1,331.97	1,331.97	1,331.97	985.43	985.43	985.43
Outcome mean	13.58	4.43	1.51	13.57	4.49	1.52
Control mean	13.38	4.58	1.50	12.86	4.58	1.51
Observations	1,019	1,019	1,019	973	973	973
R^2	0.07	0.08	0.04	0.08	0.09	0.03
Pscores of winning at elite	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending elite RTE private schools as a quota student, on children's time use. The results correspond to the 50^{th} and 90^{th} percentile cutoffs of eliteness in panel A and B, respectively. Control variables include - sex and age of child, dummy of father's and mother's education being greater than the respective means, indicator of low income quota applicant, household's SES index, indicator of caste categories, and religion. Simulated ex-ante propensity scores of winning the lottery in any distance bin are controlled. Results are robust to increasing the number of propensity score bins. Robust standard errors in parenthesis.

Table A19: Robustness to Attrition using Inverse-Probability Reweighting

	English	Math
	(1)	(2)
Enrolled as RTE student	0.332***	0.293**
	(0.126)	(0.119)
Total observations	695	695
Treatment observations	324	324
Control observations	371	371

Notes: This table shows the results for the LATE of attending private schools as a quota student on children's test scores, by using inverse probability weighting to account for the differential probability of attrition or non-response based on baseline observables.

Table A20: Robustness: Excluding young applicants to estimate LATE of being a quota student on test scores

	Test score (standardized)	
	English	Math
	(1)	(2)
Enrolled as RTE student	0.169*	0.180*
	(0.097)	(0.103)
First stage F-stat	1,054.22	1,054.22
Outcome mean	-0.01	-0.01
Control mean	-0.10	-0.11
Observations	590	590
R^2	0.14	0.08
Pscores of winning	Yes	Yes
Controls	Yes	Yes

Notes: This table shows the results for the LATE of attending private schools as a quota student on children's test scores, by excluding young applicants from the sample who are age-eligible to apply again under RTE in the year 2021-22.

Table A21: Robustness: LATE of being a quota student on school instruction

	School provides instruction (2021-22)	Synchronous (online) (2021-22)	Recordings shared (audio/video) (2021-22)	Text based activity plans (WhatsApp/SMS) (2021-22)
	(1)	(2)	(3)	(4)
Enrolled as RTE student	0.020***	0.152***	-0.069***	-0.085***
	(0.005)	(0.019)	(0.015)	(0.025)
First stage F-stat	3,562.84	3,557.02	3,557.02	3,557.02
Outcome mean	0.99	0.80	0.10	0.57
Control mean	0.98	0.75	0.13	0.59
Observations	2,255	2,238	2,238	2,238
R^2	0.05	0.18	0.10	0.11
Pscores of winning	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficient β^{LATE} from the 2SLS regression that estimates the local average treatment effect of attending a private school as a quota student on school instruction and school modality. This table uses a new variable which is generated such that it is unique at the school level, and captures a unique response to each school being attended in the sample. To do this, I recode the new variable equal to the value that is reported by the majority of the applicants (at least 50%) attending that school. For example, if more than 50% children attending school A say that school was providing instruction, then I code the variable to reflect that school A was providing instruction (for each child who is enrolled at that school, regardless of their original response). Column (1) looks at the dummy of whether school provides any instruction in the 2020-21 academic year, and columns (2), (3), and (4) look at the instructional modality offered by school. Thus, the outcome here is recoded such that there is a unique value associated with each school. Controls include - sex and age of child, dummy of father's and mother's education being greater than the mean, dummy of low income quota applicant, SES index, dummies of caste categories, and religion. Robust standard errors in parenthesis.

B Appendix: Lottery Algorithm, Sampling, and Simulation of Algorithm

B.1 Lottery algorithm

Here I explain the lottery algorithm that is implemented for RTE 25% quotas in the state of Maharashtra.

Part 1: Direct offer of admission to winners

1. Schools are arranged in the descending order of total applications received under the policy in the *previous* year. Based on this rank ordering, each school gets their turn to do the allotment of students in the *current* year.

2. There are three rounds in which the allotment happens. Each round corresponds to one of the three distance bins in which schools receive applications.

   Round 1- schools that receive applications in bin 1.

3. The first round comprises each school that received non-zero applications from students who applied to the school in distance bin 1 and allotment is done only for students who applied to these schools in bin 1.

4. The top school (as determined by the rank ordering of schools) allocates seats by lottery if the count of applications received in bin 1 $>$ seats at the school. The school allocates seats to all bin 1 applicants without any lottery if the count of applications received in bin 1 $\leq$ seats. Within the bin, all applicants are treated equally and thus have the same ex-ante probability of being selected in the lottery.[47] All the applicants who are matched to this school are removed from the consideration set and only unmatched applicants are considered for further matching. The school is removed from further matching if it has exhausted all its vacancies.[48]

5. Revised bin-level demand is calculated for all the remaining schools. The previous step is repeated for the next school based on the rank ordering list of schools. The school conducts a lottery based admission if the revised demand by bin 1 applicants exceeds the number of vacancies at school. This process is iterated over all the schools, while maintaining the same initial rank ordering.

[47] This mechanism satisfies the Equal Treatment of Equals (ETE) property following Abdulkadiroğlu, Angrist, Narita and Pathak (2017). ETE is said to satisfy when students with the same preferences and priorities have the same chance of getting allocated at any given school.

[48] If a school conducts a lottery to admit children in round 1 (i.e., for those who applied in the nearest distance bin), then this means that the school will not admit students who applied in the other two distance bins.

6. After the end of round 1, all applicants have been considered at all their bin 1 school choices and all schools have tried to allot any available seats by offering them to their respective bin 1 applicants.

   Round 2- schools (with vacancies) that have applications from unalloted applicants in bin 2

7. Next is the second round. The second round comprises schools which have non-zero vacancies and have non-zero applications from those who applied here in bin 2, based on revised bin-level demand at the end of round 1. In this round, allotment is only done for applicants who (i) failed to get a seat in round 1 and had applied somewhere in bin 2, and (ii) applicants who only applied to bin 2 schools.

8. The allotment process is same as before. The top school (based on the same initial rank ordering of schools) allots seats by lottery if the count of revised applications in bin 2 $>$ seats. School allots seats to everyone who applied here in bin 2 without a lottery if the count of revised applications in bin 2 $\leq$ seats.

9. Revised bin level demand is calculated for all the remaining schools, and the previous step is iterated over all the remaining schools, following the same initial rank ordering of schools.

10. At the end of round 2, all applicants who were remaining to be matched after round 1 and were bin 2 applicants somewhere, plus applicants who only applied to schools in bin 2 have been considered at all their bin 2 school choices, conditional on the fact that these school still had seats to offer.

    Round 3- schools (with vacancies) that have applications from unalloted applicants in bin 3

11. Next is the third round. The idea is same as before. Schools which feature here are those that still have vacancies after rounds 1 and 2. Hence round 3 considers applicants who are (i) remaining to be matched after the end of round 2 and had applied somewhere in bin 3, and (ii) applicants who only applied to schools in bin 3.

12. The allotment process is same as before. The top school (based on the same initial rank ordering of schools) allots seats via lottery if the count of revised applications in bin 3 $>$ vacant seats. School allots seats to everyone who applied here in bin 3 without a lottery if the count of revised applications in bin 3 $\leq$ vacant seats.

13. This marks the end of direct offer of admissions to winners.

Part 2: Waitlist determination

Even after the previous steps described in Part 1, there are many applicants who are yet to be matched. These applicants are either waitlisted at a unique school or are rejected from all the schools. There are 3 rounds in which the waitlist determination happens. The process is exactly similar to Part 1 and is explained as follows:

1. Schools are arranged in the same initial rank ordering as before and take turns to do the allotment based on this rank ordering. The rule is that the maximum number of waitlisted students at a school is equal to the number of winners at the school (where the number of winners per school is established in Part 1).

   Round 1- schools that have applications from unalloted applicants in bin 1

2. Round 1 comprises schools which have unmatched applications from those residing in bin 1 (these are applicants who did not get matched in Part 1).

3. The top school provides offers of waitlist by lottery if the count of unmatched applications in bin 1 > seats available under waitlist. Within the bin, all applicants are treated equally in the event of a lottery. Each matched applicant is assigned a waitlist priority at the school which determines the ordering in which they will be called for admission in the event that any winner at this school forgoes their seat.[49] All matched applicants are removed from the consideration set and the school is removed from any further matching if it has exhausted all its vacancies. Revised bin-level demand is calculated for all remaining schools. This process is iterated for all the remaining schools, following the same initial ranking.

   Round 2- schools (with vacancies) that have applications from unalloted applicants in bin 2

4. Round 2 comprises schools which have unmatched applications from those residing in bin 2 (these are applicants who did not get matched either in Part 1 or round 1 of waitlist). Similar as before, step 3 is iterated at each eligible school, taking into account unmatched applications received in bin 2.

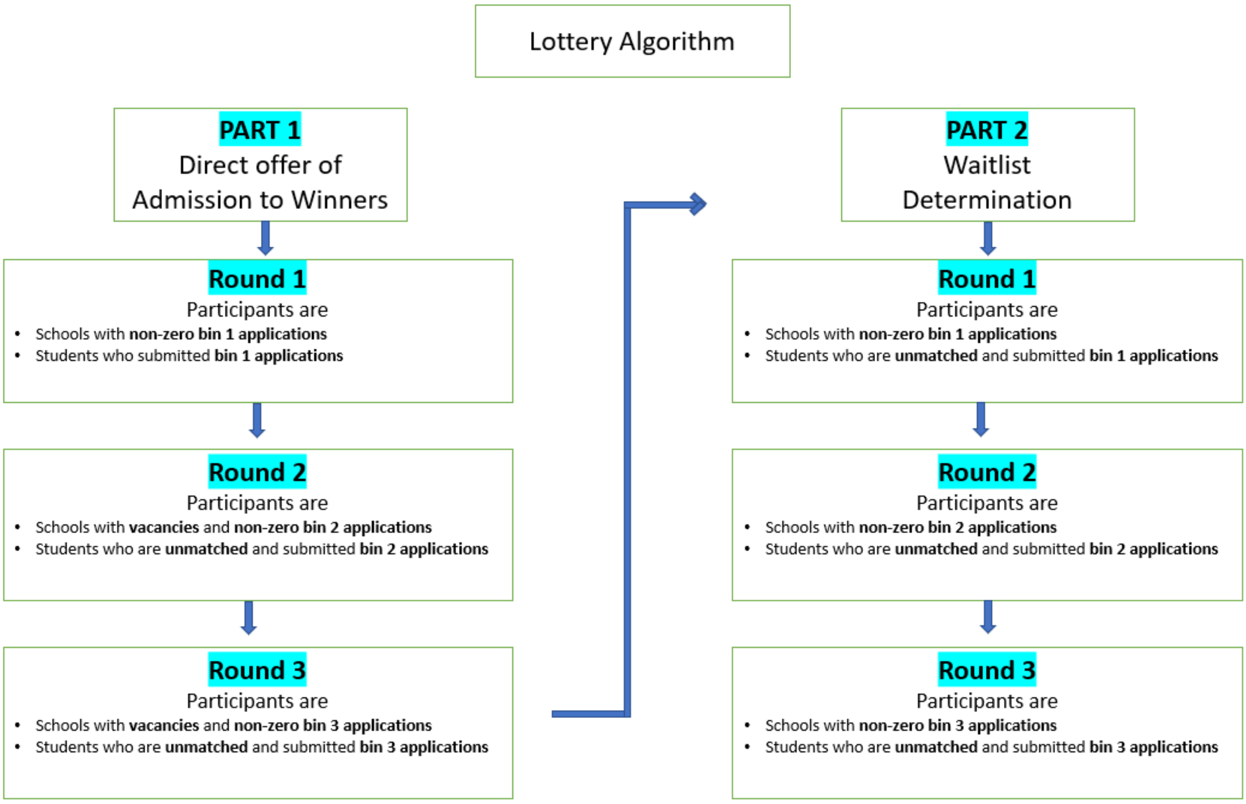
   Round 3- schools (with vacancies) that have applications from unalloted applicants in bin 3

5. Round 3 marks the final round. This comprises schools which have unmatched applications from bin 3 students (these are applicants who did not get matched either in Part 1 or in round 1, and 2 of the waitlist determination). Step 3 is iterated at each eligible school, taking into account unmatched applications received in bin 3.

6. At the end of Round 3, there are still some applicants who are remaining to be matched anywhere. These are the applicants who are not selected anywhere and I refer to them as overall lottery losers.

[49] The waitlist priority assigned to applicants at each school is randomly generated.

Figure B1: Schematic flowchart explaining the lottery algorithm



Notes: This flowchart explains the lottery algorithm which the state of Maharashtra uses to allocate schools to applicants under the RTE 25% reservation policy at private schools. The allocation mechanism is a two part process, starting with determining the winners (Part 1, as shown in the left panel), followed by determining the waitlisted candidates (Part 2, as shown in the right panel).

B.2 Sampling strategy

1. I focus on the districts for whom I have the most complete administrative data (Mumbai, Nagpur, Pune, and Thane), and focusing on these districts I make a list of all unique combinations of schools chosen by applicants in the nearest distance bin (henceforth, bin 1). This gives me all the unique *school vectors* that were chosen in bin 1. By virtue of this, some school vectors consist of a single school, and some consist of multiple schools.

2. For each school vector chosen in bin 1, I compute the count of winners who win at any school in the vector (given by the sum of winners at each school in the vector) and count of non-winners who did not win at any school they listed in bin 1.

3. Thus, non-winners for a given school vector comprise applicants who might be: (a) winners at a school that was chosen in distance bin 2 or 3, (b) waitlisted at a school that was chosen in distance bin 1, 2, or 3, and (c) overall losers who lost their chance at each and every school that they listed in each distance bin.[50]

4. Next, I focus on those school vectors which meet the following criteria:

   (i). Count of winners in the vector is at least 4.

   (ii). Count of overall losers in the vector is at least 4.

   (iii). Share of overall losers (among non-winners) in the vector is at least 0.75.

   As an aside - The rule about count of winners and overall losers being at least 4, was imposed taking into account the possibility of low response rates at the time of phone surveys.

5. Finally, from each school vector which satisfies the above three criteria, I perform a stratified random sampling where the two strata are winners and non-winners corresponding to a given school vector chosen in bin 1. Furthermore, the sampling is done such that the count of applicants sampled per school vector = min(winners, non-winners, 25)* 2.[51]

[50] This stratification based on the school vector chosen in distance bin 1, satisfies the Equal Treatment of Equals (ETE) property following Abdulkadiroğlu, Angrist, Narita and Pathak (2017). The ETE property is satisfied as all applicants who chose the exact same combination of schools in bin 1 are treated equally at the time of each school's randomization. They are subjected to the same randomization at each school which is listed in the vector, until they get matched at a school. Thus, on average the winners and non-winners who chose the same school vector in bin 1, are comparable to each other.

[51] I restrict the maximum count of applicants per vector in order to maximize the count of unique school vectors in my sample. Based on all these criteria, the minimum number of applicants selected per school vector is equal to 8. Importantly, when the school vector consists of multiple schools chosen in bin 1, I make sure to sample a non-zero count of winning applicants (among winners) from each school in the vector.

Figure B2: Schematic flowchart explaining sampling strategy

Step 1

Example of School Choices by Applicants			
	Bin 1	Bin 2	Bin 3
Applicant 1	A B C	D	E F
Applicant 2	A B C		
Applicant 3	A	B C	
Applicant 4	A	B C D	
Applicant 5	B C	D E F	
Applicant 6	B C	D	
Applicant 7	G	H I J	K
Applicant 8	G	H I	J K
Applicant 9	G	H I J	
Applicant 10	G	H I	

Step 2

	School vector in bin 1	Frequency chosen
Vector 1	A B C	30
Vector 2	A	40
Vector 3	B C	70
Vector 4	G	100

Step 3

	School vector in bin 1	# Winners in vector	# Non-Winners in vector	
			# Overall losers	# Non-losing kids
Vector 1	A B C	10	15	5
Vector 2	A	15	30	5
Vector 3	B C	40	25	5
Vector 4	G	50	40	10

Who are non-losing kids?

- Winners at a school outside the vector
- Waitlisted (either inside or outside vector)

Step 4

Stratified Random Sampling:

For each school vector (that satisfies certain criteria), randomly sample kids from the strata of winners-in-vector and nonwinners-in-vector

B.3 Calculation of ex-ante propensity scores of winning under the lottery mechanism

Below, I explain the step-by-step process for calculating the simulated ex-ante propensity scores of winning under Maharashtra's lottery mechanism for RTE.

1. I conduct a large number of simulations of the lottery mechanism as explained in Section 2.1 ($N \sim 10{,}000$).
2. For each simulation, I record the school allotted to each child.

Then for each child, I compute:

1. Simulated ex-ante probability of winning at *each* school that the child listed in application. I do this by averaging across simulations, the probability of winning at that school.
2. Simulated ex-ante probability of winning *in bin 1*. This is given by the sum of simulated ex-ante probability of winning at each school that the child listed in bin 1. The individual simulated probabilities for each chosen school are computed in the previous step.
3. Simulated ex-ante probability of winning *in any bin*. This is given by the sum of simulated ex-ante probability winning at each school that the child listed (combining bin 1, bin 2, bin 3).
4. Simulated ex-ante probability of winning at *elite schools*. I have two measures of elitness - PCA based index and school fee-based measure.
   i. For each child in the sample, and correspondingly for each RTE school that they listed in their application, I make an indicator of whether the school is elite or budget, based on the percentile cutoff. I code the indicator variable = 1 if the school lies above the respective percentile cutoff value, and I code it = 0 if the schools lies below the respective percentile cutoff value. The indicator variable is assiged a missing value in the case where there is missing data on PCA index or fee for the school.
   ii. Next, I compute the simulated ex-ante propensity of winning at elite schools. To do this I simply take the sum of the simulated ex-ante propensities for each school that is coded to be elite based on the respective percentile cutoff.
5. Note that this is always satisfied: simulated probability $\in [0,1]$
6. Next, I divide these into 100 bins of width = .01 each (for some estimations I reduce the number of bins to 50, in which case the width becomes .02, respectively).

7. Finally, I create dummies of narrow bins of simulated ex-ante propensity scores. In the case where I have 100 bins of propensity scores, this creates 100 dummies of narrow bins: [0,0.01] , (0.01,0.02], ..., (0.99, 1], such that only one of these 100 dummies gets activated for each applicant child.

8. In the estimations I control for dummies of narrow bins of ex-ante propensities of winning, as this facilitates the within-comparison between ex-ante similar applicants who vary in their lottery outcome.

Table B1: Distribution of simulated ex-ante propensity scores of winning

Panel A: Population

Variable	N	10th pctile	25th pctile	50th pctile	75th pctile	90th pctile	95th pctile	99th pctile
NAGPUR								
For winners	6,330	0.20	0.31	0.51	0.80	1	1	1
For waitlisted	5,913	0.03	0.16	0.30	0.45	0.58	0.67	0.81
For losers	9,974	0	0	0.06	0.18	0.27	0.33	0.45
PUNE								
For winners	15,198	0.25	0.42	0.65	0.94	1	1	1
For waitlisted	13,606	0	0.12	0.30	0.47	0.62	0.71	0.86
For losers	13,385	0	0	0.01	0.15	0.26	0.32	0.43
THANE								
For winners	8,041	0.42	0.75	1.00	1	1	1	1
For waitlisted	3,756	0	0.09	0.28	0.47	0.64	0.77	0.93
For losers	1,392	0	0	0.00	0.17	0.27	0.32	0.42
MUMBAI								
For winners	4,721	0.33	0.55	0.94	1	1	1	1
For waitlisted	2,776	0	0.16	0.33	0.48	0.65	0.73	0.89
For losers	1,727	0	0	0.08	0.19	0.27	0.36	0.45

Panel B: Sample

Variable	N	10th pctile	25th pctile	50th pctile	75th pctile	90th pctile	95th pctile	99th pctile
NAGPUR								
For winners	584	0.14	0.20	0.27	0.35	0.48	0.55	0.62
For waitlisted	318	0.17	0.25	0.33	0.41	0.49	0.56	0.62
For losers	396	0.12	0.16	0.22	0.29	0.34	0.38	0.53
PUNE								
For winners	275	0.10	0.15	0.20	0.33	0.52	0.57	0.68
For waitlisted	154	0.12	0.16	0.22	0.38	0.56	0.58	0.68
For losers	228	0.08	0.11	0.16	0.21	0.26	0.30	0.38
THANE								
For winners	134	0.22	0.26	0.30	0.39	0.46	0.71	1
For waitlisted	108	0.22	0.25	0.34	0.40	0.44	0.52	0.64
For losers	43	0.21	0.22	0.28	0.31	0.39	0.44	0.45
MUMBAI								
For winners	45	0.21	0.26	0.36	0.42	0.56	0.64	1
For waitlisted	28	0.25	0.26	0.39	0.53	0.62	0.63	1
For losers	16	0.19	0.20	0.25	0.26	0.27	0.39	0.39

Notes: This table shows the distribution of simulated ex-ante propensity scores of winning under the lottery mechanism.

C Appendix: Estimating Complier Characteristics and Counterfactual Destinies

C.1 Estimation

I follow the Angrist et al. (2022)'s implementation of methods used in Abadie (2002) to compute complier characteristics and counterfactual destinies for untreated compliers. Below I discuss the steps as mentioned in Angrist et al. (2022).

The notation is as follows: $Z_i \in$ {0,1} is the instrument which denotes whether i wins the RTE private school lottery. $D_i(1)$ and $D_i(0)$ refer to potential treatments, indicating i's RTE enrollment status as a quota student, when Z_i = 1 and Z_i = 0, respectively. $Y_i(0)$ and $Y_i(1)$ denote the potential outcomes for individual i as a function of RTE enrollment.

The following assumptions are made:

Assumption 1. Independence/exclusion: $(Y_i(0), Y_i(1), D_i(0), D_i(1)) \perp Z_i$.

Assumption 2. First stage: $\mathbb{E}[D_i|Z_i = 1] > \mathbb{E}[D_i|Z_i = 0]$.

Assumption 3. Monotonicity: $D_i(1) \geq D_i(0)$ ∀ i.

Angrist et al. (2022) explain the process of backing out complier characteristics, which I discuss next. While individual compliers are not coded in any data, complier characteristics can be described using methods of Abadie (2002). The monotonicity assumption implies that the population contributing to the IV analysis only consists of always-takers, never-takers, and compliers. Some of the always and never takers can be identified by the following cells of the data: $D_i = 0$ and $Z_i = 1$ are always-takers while, $D_i = 1$ and $Z_i = 0$ are never-takers. The other cells of the data contain mixtures of compliers with the other two groups: $D_i = 0$ and $Z_i = 0$ contain compliers and never-takers, while $D_i = 1$ and $Z_i = 1$ contain compliers and always-takers. The size of the compliers is given by the first stage. The data also helps in infering the share of never-takers and always-takers as these correspond to the proportion of those who reject the offer of enrollment as a quota student, and the proportion of those who choose to enroll as a quota student when not offered.

Like them, I estimate the following system of equations via 2SLS

$$g(X_i, Y_i) \times 1\{D_i = d\} = \pi_d + \gamma_d 1\{D_i = d\} + v_{id} \tag{5}$$

$$1\{D_i = d\} = \phi_d + \beta_d Z_i + e_{id}, d \in \{0, 1\} \tag{6}$$

, where $g(X_i, Y_i)$ is a function of student baseline characteristics (X_i) or post-lottery outcomes (Y_i). Complier characteristics for the treated are obtained by setting $d = 1$ which amounts to using Z_i as the instrument for D_i where the outcome in the second stage is given by $g(X_i, Y_i)$ multiplied by D_i. Similarly setting $d = 0$, estimates the complier characteristics for the untreated which means using Z_i as an instrument for (1-D_i) where the outcome in the second stage is $g(X_i, Y_i)$ multiplied by (1-D_i).

Estimating complier characteristics: Setting $g(X_i, Y_i) = X_i$ yeilds the average complier characteristics for baseline covariates. Estimating equations (5) and (6) as explained in the previous paragraph (along with ex-ante propensities of winning) produces the columns (1) and (2) for Table A4. Column (3) shows always-taker means which are computed by regressing $X_i D_i(1 - Z_i)$ on $D_i(1 - Z_i)$ (with ex-ante propensities), column (4) shows never-taker means which are computed by regressing $X_i D_i Z_i$ on $(1 - D_i)Z_i$ (with ex-ante propensities).

Estimating counterfactual destinies: Table A5 shows the distribution of enrollment across sectors for lottery losers. Lottery losers could be enrolled at private schools as fee-paying students, government schools, or remain out-of-school. I first create dummies of enrollment at a particular school sector. Next, I estimate (5) and (6) by setting d=0, for a total of four times (since there are four outside options), each time setting $g(X_i, Y_i)$ as the dummy for enrollment at that specific outisde option.