BSG Working Paper Series

Providing access to the latest policy-relevant research

BLAVATNIK SCHOOL OF GOVERNMENT

UNIVERSITY OF OXFORD

Choice for the poor or poor choice?

Experimental evidence from implementation of India's school choice policy

BSG-WP-2017/022

July 2017

Vijay Kumar Damera, Blavatnik School of Government, University of Oxford

Choice for the poor or poor choice?

Experimental evidence from implementation of India's school choice policy

Vijay Kumar Damera
Blavatnik School of Government, University of Oxford
Vijay.kumar@bsg.ox.ac.uk

July 2017

Acknowledgements: I am grateful to my supervisors, Stefan Dercon and Andrew Eggers, and to Julien Labonne, Patrick Kuhn, Abhijeet Singh, Ingo Outes-Leon, and Maya Tudor for their guidance and support. I am also thankful for the feedback received from participants at the CSAE workshop, University of Oxford, UK, and at the Summer School of Development Economics, Prato, Italy for their comments and suggestions. I thank the Education Department of the Government of Karnataka for their support at different stages of the project. Data collection for project has been made possible primarily through a fellowship from IDFC foundation, India, and also through research grants from University of Oxford-George Webb Medly/ OEP research grant, Department of Economics, and CSAE Student Grants Scheme research grant from Center for Study of African Economies (CSAE).

Abstract: Private sector's role in the provision of primary education has steadily increased over the last 25 years in the developing world. Proponents of school choice and some policy makers have been keen to leverage private sector provision to expand access to quality education. In this paper, I evaluate the impact of a government-implemented school choice policy in the south Indian state of Karnataka. The policy sets aside 25 percent places in private schools for children from disadvantaged backgrounds, with the government paying the tuition fees to private schools. Exploiting the lottery-based allocation of free places, I investigate whether policy beneficiaries had improved learning and psychosocial outcomes relative to non-beneficiaries. The study cohort comprises 7-year-old children who entered class I in 2015. After 1.5 years of schooling, I find no difference in test scores of lottery winning and losing children on a range of subjects. However, the policy does appear to have a positive effect on self-efficacy. I find only girls show statistically significant test score gains. Mechanism analysis reveals that majority of the applicants would have attended similar schools irrespective of the policy. My results and analysis point to several policy implications. First, the policy is mistargeted. This is primarily due to flawed policy design. Making eligibility contingent on income, whose determination is difficult in the Indian context, has enabled many ineligible households to participate in the program. Second, government spending on the policy is a direct income transfer from taxpayers to free place winners who are not the vulnerable populations, and hence is wasteful from a public finance and welfare perspective. Third, as the policy seems to be alleviating a binding constraint for girls, reserving significant proportion of free places for girls can enhance its effectiveness.

Key Words: School choice, private sector in primary education, policy mistargeting, gender bias

1. Introduction

Private sector's role in the provision of primary education has been steadily increasing over the last two and half decades across the developing world. Private primary school enrollment increased in developing countries from 14 to 24 percentage between 1990 and 2014, while the change for OECD countries was from 9 to 10 percent (World Bank 2016). Unlike private schools in rich countries that are typically expensive and are attended by the elite, private schools in low and middle-income countries are varied in their fees structure, ranging from ultra-elite high-fee international schools to low-fee neighborhood schools. The growth of the private sector and the mushrooming of low-fees private schools have triggered continuing debates on the impact of private schools on education outcomes and societal equity, and on public versus private provision of education.

Private sector proponents argue that market mechanisms enhance efficiency leading to provision of better quality education at a lower cost. They posit that the fast growth of low-fees private schools enables even the poorest to access quality education, and argue for introduction of school choice policies like vouchers that make public funds available to the private sector. Critics of the burgeoning private sector contend that it leads to segregation of the school system as the middle class and wealthy migrate to private schools, leaving only the poor at state schools. They dispute the 'higher private school quality' argument, pointing to the lack of supportive rigorous evidence. Finally, they assert that elementary education is a public good, hence necessitating public provision (Drèze 2013), and that choice policies divert scarce financial resources away from the public sector.

Empirical evidence on the impact of private schools and choice policies are scarce and inconclusive in the context of low- and middle-income countries. Glewwe and Muralidharan (2015) summarize the few rigorous studies from India, Chile, and Columbia, and opine that the existing evidence points to private schools being more productive than government schools (produce the same results at much lower cots), but not more effective in raising test scores. They conclude that further evidence is required to understand if the higher productivity of private schools can be translated to better learning outcomes. There is, therefore, a wide gap in our understanding of impacts of private schools on learning, and if choice policies that promote private provision of education can be effective instruments of human capital development.

I present experimental evidence on the impact of a unique school choice policy introduced via India's Right to Education (RTE) Act of 2009. The policy (hereafter referred to as the RTE 25 percent mandate/the mandate) blocks 25 percent places (hereafter referred to as RTE free places/ free place) in entry grades of all private schools for children from disadvantaged households. Families from certain unprivileged social groups (castes) and families below the specified income cutoff are defined as disadvantaged and are eligible for free places. The government reimburses the tuition costs to private schools. When fully implemented, the mandate could impact 16 million children in 8 years making it the world's largest program for public funding and private provision in education (Indian Institute of Management, Ahmedabad 2013.p 17). The stated objectives are to provide better quality education to disadvantaged children and to desegregate the Indian schooling system.

Despite these lofty goals, and the legal mandate, some Indian states are skeptical about the mandate and haven't implemented it. They argue the mandate will reduce public school budgets by diverting scarce public funds to private schools, and worry about the incorrect identification of the income-disadvantaged households given the lack of information on household incomes. States implementing the mandate view it as a public-private partnership through which the state can procure higher quality education from the private sector. They believe that the mandate provides a ladder of opportunity for disadvantaged children to move from their default low-quality learning environments (government or low-fees private schools) to high-quality environments (high-fees private schools). Against this background, I investigate the impact of the 25 percent mandate on children's learning and psychosocial outcomes.

My study is located in the south Indian state of Karnataka - one of the better implementers of the mandate. Exploiting the lottery-based allocation of RTE free place, I estimate the casual impact of the policy using a reasonably large sample of 1616 children from two different regions of the state. The study cohort enrolled in class I in mid- 2015 and were aged about 7.3 years at the time of end line data collection in November 2016. After 1.5 years of schooling, I find no difference in test scores of lottery winning and losing children on four subjects - General Cognitive Ability (GCA), Math, English, and Kannada (the local language). I find statistically significant heterogeneous learning outcome effects by gender - the treatment effect for girls on the total test score measure is 0.11 σ ($p=0.096$) and on GCA is 0.15 σ ($p=0.037$). None of the treatment effects for boys is significant.

I also measured psychosocial measures to investigate the strength of anecdotal evidence (widely reported in the media) that RTE children are being discriminated against in schools. On one of the four psychosocial measures, self-efficacy, lottery winners perform better than lottery losers (0.11 σ; $p=0.020$).

I explore the mechanisms driving the results and determine that the theory of change put forward by the policy supporters didn't materialize. First, 98 percent of the treatments and 93 percent of the controls are enrolled in private schools, establishing that most of the policy applicants are default private school goers. The poorest and most deserving households - default government school goers - are not participating in the policy. Second, I use a government dataset of school fees and estimate the treatment effect on school fees to be Indian Rupees (INR) 1,550 (0.2 σ). Though this effect is statistically significant, the effect size is not meaningful from a policy perspective, given that the average government spending on the mandate per child is INR 6,800. The mandate has, therefore, not moved children to better learning environments as posited in the theory of change. If the treatment is defined as a shift to a better learning environment, then the compliance with treatment is very low, and that explains the absence of significant learning impacts for the full sample.

On heterogeneity, I find that winning the lottery has a positive impact on human capital accumulation possibilities for girls relative to boys - the probability of private school attendance increases by 7 percentage points (compared to 5 for boys), fees of enrolled schools increases by INR 2,042 (compared to INR 1,200 for boys), and probability of attending private tuition increases by 8 percentage points (compared to zero for boys). The lottery results are much less binding for boys as intra household resource allocation strongly favors them i.e., boys go to better schools and private tuitions, relative to girls, irrespective of the policy. This suggests gender bias against girls; the policy seems to work for girls by relieving this binding constraint to a certain extent.

My results and analysis point to several policy implications. First, the policy is mistargeted - this is primarily due to flawed policy design. Making eligibility contingent on income, whose determination is difficult in the Indian context, has enabled many ineligible households to participate in the program. Second, because of this mistargeting, government spending on the policy is a direct income transfer from taxpayers to RTE free place winners, and hence is wasteful from a public finance and welfare perspective. Third, as the policy seems to be alleviating a binding constraint for girls, reserving significant proportion of free places for girls can enhance its effectiveness. Finally, RTE beneficiary children are not being discriminated in their new learning environments as suggested by a large body of anecdotal evidence.

My research contributes to and enriches several strands of empirical literature in economics and public policy. This is the first experimental evidence on the impacts of a government implemented large-scale school choice program in a developing country setting[1]. Though I can't estimate the school choice effects or the private school premium owing to failure of the theory of change, my policy impact estimates contribute to the empirical school choice literature from developed and developing countries. This literature evaluates the impacts of different types of school choice programs like vouchers, charter schools, open enrollment systems etc., and points to why specific choice policies did or didn't work in the given context. Barrow and Rouse (2008), Gleason and Silverberg (2010), Musset (2012), and Glewwe and Muralidharan (2015) summarize the evidence.

My headline result of policy mistargeting speaks to the literature on means testing and targeting the poor for social programs in developing countries (Alatas et al. 2012, Cameron and Shah 2011). The heterogeneous treatments effects by gender speak to the intra household resource allocation literature. While gender bias against older girls aged 10 years or above is documented the Asian context (Zimmermann 2012, Burgess and Zhuang 1998), my results establish bias even for younger girls and contribute to this literature.

The rest of the paper is structured as follows: Section 2 describes the school choice policy, the experiment, study design and sampling. Section 3 presents the data, while section 4 presents the results. Section 5 is a detailed discussion of the mechanisms. Section 6 is a discussion on the inferences and policy implications. Section 7 concludes.

2. India's school choice policy and experiment

2.1. The Indian elementary education context

About 200 million children are part of India's elementary education system (class 1-8). The following contextual details enable better appreciation of the public versus private debate. First, private school enrollment is one of the highest in the developing world at 37 percent. The national average masks the variation in levels of private school enrollment across states and across urban and rural areas. For instance, five Indian states have private school enrollment rates of greater than 50% (Pratham Foundation 2015). There has been a steady migration of children from government to private schools over the last decade, a phenomenon described as emptying of government schools (Kingdon 2017).

Second, there is enormous variation in the type and quality of private schools, as demonstrated by the range of school fees. The median private school fee for the whole country is INR 5000, which is just about 17 percent of per capita GDP (Kingdon 2017). This suggests that private schools serve not just the wealthy, but also the middle class, and probably, a section of the poor.

Third, despite lack of rigorous empirical evidence, there is a general perception that private schools are of superior quality than government schools (Juneja 2014). Survey data suggests that parents prefer private schools for the following reasons: better learning environments in private schools, unsatisfactory

[1] Muralidharan and Sundararaman's (2015) seminal work on impacts of vouchers in India is based on an NGO run voucher program implemented across a limited number of villages.

quality of education in government schools, and absence of English medium education in government schools (Saha 2016).

Finally, private coaching (referred to as private tuitions) attendance has been increasing steadily with about 26 percent of all students enrolled (Saha 2016). Parents consider private tuitions as a necessary complement to school attendance.

2.2. RTE 25 percent mandate

India's Right to Education (RTE) Act, 2009 is a landmark legislation that guarantees universal access to elementary education and ushers in several far-reaching changes in the elementary education system of the country. The act has drawn most attention and controversy for it's section 12 (1) (c) which mandates that all private schools (except minority institutions) reserve 25 percent of places in their entry level class for students from disadvantaged groups- both social and economic. Social disadvantage is based on caste. Scheduled Castes (SC) and Scheduled Tribes (ST) are eligible to apply for the mandate. Economic disadvantage is based on income and state governments (the implementing agencies) are to set income eligibility cutoffs. The tuition fee reimbursement to private schools is at a rate that is the lower of the actual amount charged from the child by the school, or per child expenditure incurred in government schools (Ministry of Law and Justice, Government of India 2009).

The 25 percent mandate, therefore, introduced a limited school choice by creating a fixed proportion of free places in every private school of the country. While school choice in a market sense was available to the poor in India for the last decade or so, thanks to the growth of a strong private sector providing low-fee private schooling, poverty constrained the exercise of that choice(Härmä 2011; Srivastava 2008). The mandate, therefore, at least in theory makes school choice a reality for the poor by relaxing the binding income constraint.

Choice introduced through the mandate is a unique policy intervention and should be differentiated from the traditional choice models like voucher programs. A typical voucher program provides vouchers of a specified value and thus limits the choice set of income constrained families (who cannot top up the voucher) to private schools whose tuition fees is equivalent to the value of the voucher. For instance, in the Chilean voucher program, 6-7 percent of elite schools did not participate as their fees were much higher than the value of the voucher (Hsieh and Urquiola 2006). In contrast, the RTE free places are available in every private school and hence the school choice set, subject to the neighborhood criterion, is much larger as families are allowed to apply to more than one school within their neighborhoods. However, the limitation of the RTE model is that it is not universally available even within the eligible groups given the cap of 25 percent on seats and that the targeted population of socially and economically disadvantages groups is much more than the seats available[2]. Kingdon (2017)estimates that number of free places is less than 10 percent of the population of eligible children.

The goals of the 25 percent mandate are two fold. First, to desegregate Indian schooling system and create an inclusive learning environments for children from different backgrounds to share interests and knowledge on a common platform (Indian Institute of Management, Ahmedabad 2013). Second, to provide better quality education for children from disadvantaged backgrounds. Educationalists and

[2] The population of Scheduled Castes (SC) and Scheduled Tribes (ST), the socially disadvantaged groups together constitutes about 25 percent of the population or roughly about 50 million of the 200 million children in elementary grades. Hence, at each grade there are about 6 million (50 divided by 8) socially disadvantaged children, against the 2 million free places. There is no clean way of even approximating the percentage of income-constrained households given the absence of income data. However, even without considering the economically disadvantaged, there is a huge supply constraint in the policy.

public intellectuals assert the primacy of the desegregation goals, while politicians, policy makers, economists, and choice activists assert the centrality of the improved learning goal. The primary focus of this inquiry is also the latter, though I will shed light on the impact of the policy on both student outcomes and on achieving desegregated learning environments.

2.3. Theory of change

Though not clearly articulated, supporters of the mandate seem to have the following theory of change in mind.

1. There is a near universal preference for private schooling. However, binding income constraints compel poor parents to send their children to public schools.
2. The 25 percent mandate, by targeting the disadvantaged groups, would open the doors of high quality private schools to children from poorer backgrounds, lead to provision of better quality education and consequently, improve learning outcomes for these children.
3. The enrollment of 25 percent of poorer children in private schools will also desegregate the schooling system.

It is therefore expected that the 25 percent mandate would move disadvantaged/income-constrained children from low quality (government/low-fees private) to high-quality (higher-fees private) educational environments, and thus improve their learning outcomes. The two underlying assumptions of this theory of change are the private school learning premium (private schools produce better learning than government schools), and the high-fees learning premium (high-fees leads to higher quality learning). Figure 1 provides a pictorial description of the theory of change underlying the 25 percent mandate.

2.4. Implementation of the mandate

Implementation of RTE 25 percent mandate has become a contentious issue amongst education policy makers and private schools[3]. Despite the legal requirement, large states like Andhra Pradesh, and Telengana are not implementing the mandate. Policy makers from non- implementing states opine that the mandate will have a two-pronged negative impact. First, it will divert limited government resources to the private sector and weaken the government schooling system. Second, it will lead to some kind of 'elite capture' and policy mistargeting whereby only the better-off among the disadvantaged benefit from the program. These arguments are exactly mirrored in theory and in global policy and scholarly debates on choice and private schools. State governments (that invested aggressively in the government school system over the last decade through infrastructure up-gradation, teacher appointment, training, etc.,) and critics of the mandate are concerned that it could lead to a dually unsustainable financial burden by paying high salaries to government teachers who cannot be retrenched and reimbursing the costs to private schools. In addition, some policy makers also argue that there is no credible and consistent evidence that private schools are more effective than government schools in improving student outcomes and hence don't see the need for implementing this mandate.

On the other hand, the front runner states in implementing the mandate, like Karnataka, Rajasthan, Madhya Pradesh, argue that the mandate provides a unique opportunity to provide better quality education to children from disadvantaged backgrounds. They view it as a new model of service provision in elementary education that could alleviate the capacity constraints faced by government schools, particularly in urban areas.

2.5. Existing research on the mandate

[3] High-fees, elite private schools vigorously opposed the mandate viewing it as the state's encroachment on their autonomy, and reluctantly fell in line only after the courts ruled against them.

Since states started implementing the mandate in 2012, it has become the focus of enquiry by the media and to a limited extent by practitioners and academics. The media's focus has been on stories highlighting the travails of parents and children going through the admission process and the experiences of children enrolled in the free places. Academics and practitioners have written on the administrative mechanisms in the implementation of the mandate (Indian Institute of Management, Ahmedabad 2013), the institutional and ideological debate surrounding it (Srivastava and Noronha 2014, Juneja 2014) and on its early implementation (Noronha and Srivastava 2013).

There are no empirical studies exploring the impact of the mandate on children's outcomes. As pointed in the State of the Nation Report on RTE (2016), "opinion is divided on the impact of the mandate on children themselves," with claims of positive and negative impacts made by enthusiasts and skeptics of the mandate respectively. The report adds, "surprisingly, despite many of these claims being empirical, there is little large- scale empirical work informing these claims". Further, Muralidharan and Sundararaman (2015) comment, "Indian states are currently starting to implement the RTE Act, and there is much fertile ground for future research to better understand education markets in low-income settings and directly contribute to better education policy".

Against this background, I seek to investigate if expanded choice under the RTE 25 percent mandate has impacted student outcomes. Though learning outcomes measured by test scores are the primary outcome variables of the analysis, I also focus on psychosocial outcomes as they are welfare indicators in their own right, and play an instrumental role in learning (Singh 2012). Further, Kaufman and Rosenbaum's (1992) 'relative deprivation hypothesis' suggests that admitting ill- prepared poor children into schools where they might feel out of place (due to socio-economic reasons) would put them at a competitive disadvantage. This is a concern in the context of the mandate, given anecdotal evidence and the widely accepted popular narrative that children admitted to RTE free places in elite private schools are not well-integrated into their new learning environments.

2.6. Study setting

My research is located in Karnataka state in south India, a top implementer of the mandate, both in terms of the number of children benefitted and the quality of implementation. The state started implementing the 25 percent mandate from the academic year 2012-13. The Education Department of the state Government of Karnataka is the nodal agency implementing the mandate. A unique feature of the implementation of the 25 percent mandate in Karnataka is that the 25 percent quota is further subdivided amongst the three major eligible disadvantaged groups: 7.5 percent to Scheduled Castes (SCs), 1.5 percent to Scheduled Tribes (STs), and 16 percent to Other Backward Classes (OBCs).

For the first three years of its implementation, 2012-15, RTE free places were filled through a decentralized non-computerized application process with the schools playing a key role. Parents were required to physically submit paper applications to schools. Schools in turn would scrutinize the eligibility of applicants, and conduct admission lotteries in cases of over-subscription. In cases of under-subscription or exact-subscription (number of applications is equal to the number of free places), free places would be offered to all applicants. Though the education department supervised the entire process, there were complaints of non-transparent lotteries and schools subverting the system through rejecting eligible applicants.

The Education Department introduced a centralized online admissions system from the year 2015-16 to redress complaints and to improve the efficiency of the entire admission process. Parents were allowed to choose up to five schools in their neighborhoods. In the new system, parents access the online application, upload their eligibility documents, choose their neighborhood, and select up to five schools giving a preference order[4]. Private schools have no role in the process except indicating the number of free places available in their entry classes.

Once the application deadline is past, the Education Department does an intensive data cleaning exercise to weed out ineligible applications and duplicates. Post this, the lottery is held on a pre-fixed date in the presence of the media, and the political and bureaucratic heads of the education department. The admissions algorithm (described below) generates admission offers and information is sent to parents through text messaging on the same day. Parents who have been offered free places are given a time window to go to the school and claim their spot. As the school year in Karnataka begins from June, the admissions process starts in February/ March and is completed by the end of April.

2.7. Lottery

Karnataka's RTE admissions lottery algorithm is based on a random serial dictatorship mechanism (Abdulkadiroğlu and Sönmez 1998). A single central lottery is conducted on all the eligible applications and admission offers are made as follows:

1. Each child/application is randomly assigned a unique 15 character alphanumeric code.
2. The randomly generated codes are arranged in ascending order thus giving a numerical rank to each child.
3. Every applicant is considered rank-wise and each of her preference is sequentially matched with the seat availability in the particular school and eligibility group (SC, ST, OBC) combination. A match leads to an admission offer and the child becomes a lottery winner; failure to find a match means the child becomes a lottery loser.

The lottery winning probability depends on the neighborhood, the eligibility category (caste), and the choice profile (the schools that the parents put down in the application and the preference order). Combinations of neighborhood, eligibility category, and choice profile constitute a randomization stratum. Within randomization stratum, treatment (offer of a free place) is random. Hence the lottery losers from the same neighborhood, same eligibility category, and with the same choice profile provide a clean counterfactual for the lottery winners i.e., the ex-ante probability of admission is identical for applications within a randomization stratum.

2.8. Study design

Given the lottery algorithm, I choose a pair-wise matching design (Bruhn, McKenzie 2009). I generate matched treatment and control pairs, by randomly matching treatments (lottery winners) in each randomization stratum with the controls (lottery losers). The population of matched pairs is the sampling frame.

Two types of applicants get eliminated in this matching scheme – all applicants in randomization strata where all or none of the applicants are treated, and some applicants in randomization strata where the number of treatments and controls are not balanced. The sampling frame is therefore smaller than the overall population of applicants. This has implications only for external validity of the results, and doesn't change their internal validity. Figure 2 summarizes the study design and timeframe.

[4] The Education Department set up kiosks across the state to facilitate the application process. Further, private internet kiosks are available across urban Karnataka. Parents seem to have used both the government and the private facilities to apply for the mandate.

2.9. Study cohort and sampling

The sample for this study is drawn from the cohort of children who applied for admission to class I[5] in February/ March 2015 and started class I in June 2015. I got access to the admissions data in March 2016 after the entire admission process was completed and when the study cohort was already finishing class I[6]. Table 1 provides a summary of the 2015-16 Class I admission database for Karnataka. In total 126,728 children applied for admission into class I, of which about 62,046 (49 percent) got an offer of admission.

The state is divided into 34 education districts with significant differences in the education markets (level of private school activity and government school effectiveness), socio-economic status, demographics, and the number of school admissions. For instance, Bangalore Urban district (one part of the capital city) alone accounts for 14 percent of all the applications. Existing theoretical and empirical literature on the impacts of school choice suggests that the effects are heterogeneous across contexts. In particular, there is emphasis on how the impacts should vary across urban and non-urban contexts (Hoxby 2007; Muralidharan and Sundararaman 2015). To particularly explore this heterogeneity, and to improve generalizability of the results to a larger population, the sample for the study is chosen from amongst four districts, Bangalore South, Bangalore Rural, Bellary, and Gulbarga. Table 1 summarizes district-wise, the number of applications, and number of treated (offered RTE free place) and control (not offered RTE free place) children. The four districts account for 28 percent of the total applications received and 25 percent of all the matched pairs in the state.

The planned sample size for the study was 1600 children - 800 matched treatment-control pairs- powered to detect a minimum effect size of 0.1 of a standard deviation and to explore heterogeneous effects. The sampling frame was the population of matched pairs (of treatment and control children) from the four sample districts. Despite the differences in districts in the population of eligible children, number of applications, and number of matched pairs, the sample of 1600 children was distributed equally across all four districts- 400 children/ 200 matched pairs per district. A suitable weighting mechanism will be used to account for the differences in the district sizes while calculating the final impact estimate.

3. Data

3.1. Validity of the design

The study cohort was randomized and assigned to treatment (offer of a free place) almost a year before the start of the research. Hence, I don't have baseline data. This entire analysis is primarily based on the end line data collected in September-December 2016. The sample children had about 1.5 school years of schooling at end line. Table 2 establishes the validity of the design by presenting treatment- control balance on several observable characteristics that are potentially correlated with the primary dependent variable- children's outcomes.

3.2. Outcome variables

There are three sets of outcome variables in the analysis- learning and psychosocial outcomes of children, household expenditure on children's education, and the school fees. Data on the learning outcomes of children was collected through administering an age appropriate test on all sample children. The test was developed in collaboration with the Center for Early Childhood Development (CECED), New

[5] The age of entry into class I is typically 6 years.
[6] The school year in Karnataka is from June to March.

Delhi, and the Education Department of the Government of Karnataka. It has 90 items that check student's ability on four broad skills, General Cognitive Ability (GCA), mathematics, English, and Kannada (the local language). It was administered one-on-one, at school, on all sample children, taking care that the same enumerator tested the treatment and control children in a matched pair. There is enough variation on the total score measure (unweighted sum of the four competency scores) and the individual competency score measures. In a multivariate regression of test scores on a range of covariates, the coefficient estimates on the child's age, and mother's education are positive and statistically significant; being a Scheduled Caste or being a Muslim are negative and statistically significant. These results, which are in line with the literature, and the substantial variation in test scores, establish the strength of the primary dependent variable. Graph 1 shows the distribution of learning outcomes and Table 3 presents a summary of the raw test scores.

Data on psychosocial outcomes was obtained through administering a psychosocial test comprising 16 yes or no statements that tested the child on four psychosocial attributes - self-efficacy, peer support, school support, and teacher support. Four summary indices - self-efficacy index, peer support index, school support index, and teacher support index- are then constructed using two methods. This was intended to measure potential discrimination and how well schools are integrating the RTE free placeholders.

First, given that some of the statements are negatively worded, I recoded the responses so that a one always indicates a better outcome than a zero. Then I weigh each response equally and generate indices by aggregating the responses of each statement. Each index ranges from 0-4, a higher value indicating better outcome on the measure. Second, I follow the inverse covariance weighting approach (Anderson 2008) and generate indices, each with a mean of zero and standard deviation of one. Table 4 presents the summary of the psychosocial measures using both the simple aggregation/ naïve and the inverse covariance weighting approach.

Data on household expenditure on children's education are self- reported and are collected through the parental/ household survey. Data on school fees is obtained from the education department of Karnataka.

3.3. Independent and control variables

Information on the primary independent variable of interest, the treatment status is available in the admissions database. Information on control variables, household level and child level variables, is collected through a detailed household survey administered on the household head.

4. Results- Policy impact estimates

4.1. Estimation model

I estimate the impact of policy on children's outcomes using the following model

$$\boldsymbol{Y_{isd} = \beta_0 + \beta_1 (T_i) + \beta_{Zi} Z_i + \beta_{Xi} X_i + \varepsilon_{isd}} \qquad (1)$$

Where $\boldsymbol{Y_{isd}}$ is the outcome (learning and psychosocial) of child *i* in subject *s* in district *d*. T_i is a binary treatment variable taking a value of 1 if child *i* is offered expanded choice (i.e., if the child is a lottery winner). I use district fixed effects ($\boldsymbol{Z_i}$) to absorb geographic variation and to improve precision. I also check if the results are robust to inclusion of pair-fixed effects, as the randomization is at the pair level. I show the main results both with and without covariates ($\boldsymbol{X_i}$), though my preferred specification is the model with district fixed-effects and a rich set of household (socio-economic) and child-related controls (age of the child, gender of the child, indicator for belonging to Scheduled Caste, indicator for being Muslim, mother's age, mother's education (in years), indicator for a working mother, and a household asset index).

As treatment is randomly assigned, β_1 is an unbiased Intent To Treat (ITT) estimate of the impact of being treated for children whose parents chose to apply to RTE free places. The ITT parameter is not an estimate of the impact of being enrolled into an RTE free place; it is an estimate of the effect of winning the lottery i.e., being offered an RTE free place. Enrollment into a free place is non-random and is determined both by the lottery outcome and parental behavior. For instance, some children who win the lottery might not enroll into a free place and other children who lose the lottery may end up enrolling into a free place, as will be explained in section 4.2. The Treatment on Treated (ToT) estimation, which uses the lottery outcome as an instrument for enrollment to an RTE free place, would answer the question of impact of being enrolled to an RTE free place. The ITT is, however, the best policy parameter given that parental decisions to apply for RTE places and to enroll their kids when offered admission are not in the direct control of the policy maker (Deming et al. 2009; Duflo et al. 2007). The analysis in this paper will focus on the ITT estimates – the policy impacts.

4.2. Final sample and Compliance with treatment

The final realized sample for the study comprises of 808 matched treatment-control pairs with 25 percent of the sample from each of the four sample districts.

Like in most experimental studies, there is a modest non-compliance with treatment. Here compliance is defined as enrollment in an RTE free place if offered admission, and not being enrolled in an RTE free place if not offered admission. Table 5 provides details of the compliance amongst treatment and controls. The 18.2 percentage non-compliance among the controls (also referred to as crossover rate) is puzzling given that the State Education Department monopolizes the offer of treatment. Lottery losers' enrollment into an RTE free place could only have happened due to manipulation of the admission system by the grassroots bureaucracy. Interviews with officials across the bureaucratic hierarchy points to how grassroots functionaries, in collusion with private schools, allotted some of the unclaimed free places to the lottery losers[7].

The compliance rate for the study, calculated as the difference between the compliance rate of the treatments (89.5) and the crossover rate (18.2), is 71.3. This is a reasonably high value. Further, non-compliance and crossover don't effect the estimation of the ITT or introduce any bias in the result. They have implications for estimating the ToT and the Average Treatment Effect (ATE), which will be discussed later.

4.3. Policy impact on test scores

Table 6 presents the ITT impact estimates for test scores. The dependent variable, test scores, is converted into standardized z-scores, hence the coefficient estimate should be interpreted as the effect size. The offer of an RTE free place does not have a statistically significant impact on learning outcomes measured across a range of subjects, though the sign of the coefficient is positive across all measures. These results are for the preferred specification with district dummies and covariates; the standard errors are clustered at the pair level to account for the pairwise randomization design. The pattern of

[7] Given the set up of the admissions process and the lottery algorithm (where parents are allowed only up to five preferences), it is possible that, even within a neighborhood, there are both non-allotted free places in some schools and lottery losers.

results for the main treatment effect holds across specifications. Further, age, being Muslim, being Scheduled Caste, and mother's education variables are all statistically significant and have the expected signs.

4.4. Policy impact on psychosocial outcomes

Table 7 presents the ITT estimates of the impact of policy on the four measured psychosocial outcomes. The self-efficacy index is positive and statically significant across specifications. The point estimate of 0.11 (p-value: 0.020) in Panel B indicates that winning the lottery increases the sense of self-efficacy by about 0.11 of a standard deviation (σ). The result is statistically significance even after correcting for multiple hypothesis testing – with four tests, the critical value with a Bonferroni correction for significance at the 10 percent level is 0.025.

The result on the self-efficacy index alone, unfortunately, doesn't point to any definitive inference; but the fact that the self-efficacy index is statistically significant, and that there is no significant difference on the other three measures- school experience, peer support, and teacher experience- is conclusive proof against the anecdotal evidence and popular views articulated in the mass media that children admitted into free places are at a psychosocial disadvantage due to discrimination/ lack of integration at schools.

4.5. Heterogeneous Effects

Heterogeneous effects are estimated using equation (2) below by introducing an interaction term between the child characteristic (CC) and the treatment dummy in the ITT estimation model. The coefficient on the interaction term β_2 is the parameter of interest.

$$Y_{isdp} = \beta_0 + \beta_1 (T_i) + \beta_2 (T_i * CC_i) + \beta_{Zi} Z_i + \beta_{Xi} X_i + \varepsilon_{isd} \qquad (2)$$

Heterogeneous test score impacts by three child characteristics (CC), gender, religion, and caste, are presented in Panel A of Table 8. Each cell is a separate regression and the reported value is the coefficient (β_2) and standard error of the interaction term. The interaction term is the difference in the treatment effect between students identified by the covariate and others. For instance, the value of 0.139 in the first cell means that there is a 0.139σ difference in total score treatment effect between girls and boys. The treatment effects are heterogeneous by gender and religion, but not by caste. There is positive and statistically significant differential effect of being a girl on general cognitive ability score, while the differential effect of being a Muslim is negative on both the total and the Kannada scores.

To further unpack these differential effects of being a girl and a Muslim, I estimate equation (3), the results of which are presented in Panel B of Table 8.

$$Y_{isd} = \beta_0 + \beta_1 (CC_i) + \beta_2 (CC_i * T_i) + \beta_3 ((1-CC_i) * T_i)) + \beta_{Zi} Z_i + \beta_{Xi} X_i + \varepsilon_{isd} \qquad (3)$$

The first two rows present the effects for girls and boys - β_2 and β_3 with CC being girl. The treatment effects for girls are positive and statistically significant for both the total score measure and for the GCA measure, while the effects for boys are negative and not statistically significant. The Muslim and the non-Muslim sub-group results are not statistically significant, but the difference in the sign of coefficients is striking- on all the outcome variables, it's negative for being a Muslim, and positive for being a non-Muslim.

My results on test score effects for the full sample are similar to the results of the AP school choice experiment (Muralidharan and Sundararaman 2015), the only other experimental study on the impacts of school choice in India. The study was conducted in rural Andhra Pradesh, another south Indian state, and establishes that there is no difference between test scores of voucher-winners and voucher-losers after two and four years of the program on the four subjects of math, English, science/ social studies, and Telugu (the native language). The study, however, doesn't document any heterogeneous effects by gender or religion.

Table 9 presents sub-group impacts on the psychosocial measures. The positive and the statistically significant effect on self- efficacy for girls, non- Muslims, and non-SCs, suggests that the overall positive treatment effect on self-efficacy (Table 7) is driven by these three groups. The negative and statistically significant treatment effect on peer support for Muslim children and the positive and statistically significant treatment effect on non- Muslim children are striking, though they don't remain significant after correcting for multiple hypothesis testing.

5. Mechanism analysis

5.1. Theory of change

The research question is to estimate the impact of the policy (offer of an RTE free place) on student outcomes. The three key results from the previous section are that

i. The policy has no statistically significant impact on learning outcomes after 1.5 years of treatment.
ii. The impact on psychosocial outcomes is also not statistically significant, though policy beneficiary children seem to display a positive and statistically significant sense of self-efficacy.
iii. The treatment effects on girls are different from those of boys, with girls showing positive and statistically significant effects on two learning outcome measures: total score and general cognitive ability score.

As detailed in section 2, the theory of change underlying the 25 percent mandate is that it creates a ladder of opportunity for children from disadvantaged/income-constrained households, moves them from low-quality to high-quality educational settings, and thus improves learning outcomes. There are, therefore, three necessary conditions for the mandate to lead to better outcomes. First, it should cause a qualitative change in the learning environments of children, ideally moving them from government to private schools. Second, the new learning environments should provide better quality education than the older ones. And, finally the policy shouldn't lead to significant changes in home inputs to education. The second condition is more of an assumption underlying the policy; hence, I investigate the validity of the first and third conditions to unpack the mechanisms driving the results, change in learning environments and constancy of home inputs.

5.2. Home inputs to education

The education production literature views school inputs, home inputs, child characteristics, and household characteristics as the typical inputs into the production of learning (Glewwe and Kremer 2006; Glewwe et al. 2011). Studies that evaluate the impact of changes in school inputs (or in the extreme, the school itself) on learning are usually estimating the partial derivative of learning with respect to school inputs holding everything else constant. This estimation approach, however, ignores the real possibility of parents changing home inputs in response to policies that change school inputs. Das et al. (2013), demonstrate that households re-optimize resources and inputs in response to policies, and argue that naïve impact estimates that do not account for household substitution could potentially be biased. They posit that while the partial derivative is the production-function effect, a technology parameter, the total derivative of learning with respect to school inputs, accounting for household substitution, is the policy effect – an unbiased estimate of the policy. Therefore, household substitution could potentially bias the estimation of the partial derivative and is a mechanism in understanding the total derivative.

In the context of the RTE 25 percent mandate, household re-optimization is a distinct possibility- for instance, as lottery winning children get an opportunity to attend better schools, parents could reduce investments in private tuitions and/or in time spent on learning at home; parents of lottery losing children could engage in the opposite behavior.

Table 10 presents treatment-control differences in home inputs to education on five variables- an indicator for attending private tuition, annual private tuition fees, time spent by the child at private tuition, on studying at home, and on playing at home. There are no statistically significant differences in the measured home inputs- the p-value for an F-test of joint significance for all the variables is 0.86. Household substitution of resources, therefore, doesn't seem to be a mechanism driving the results.

5.3. Policy-induced change in learning environments

To understand policy-induced change in learning environments, I first analyze the types of schools attended by lottery winners and losers in terms of public versus private. Table 11 (column 1) shows there is only a 5 percentage point treatment effect of enrollment in a private school (i.e., the difference between the private school attendance rate of treatments and controls is 5 percentage points). So, if treatment is defined as private school attendance conditional on winning the lottery, the compliance rate is a mere 5 percent. Further, the control mean of 0.94 demonstrates that 94 percent of the applicants would have attended a private school, even in the absence of the policy. This inference stems from the fact that the control group provides a counterfactual for a world sans the policy.

Table 12 presents the raw data on public versus private school attendance. Panel C shows population statistics of public versus private school attendance in elementary grades for the four sample districts. While 41 percent of population attends public schools, only about 7 percent of the study sample are default government school goers. This points to non-participation of the poorest households, the default government school goers.

This result that majority of the lottery winners and losers are attending private schools, however, doesn't mean that there is no difference in the quality of schools attended by lottery winners and losers. As explained in section 2.1, there is wide variation in private school quality and fees charged. Absent a more reliable measure of school quality like standardized test scores, I use annual school fees to proxy for school quality, as it captures parent's willingness to pay for education and other services provided at school. Table 13 provides a snapshot of the variation in the private school market both within and across the districts in the sample. This variation in private schooling, and that half of the children in urban India study in private schools (Kingdon 2017), suggests that a significant proportion of the children currently attending low-fees private schools belong to income-constrained or marginally income-constrained families. Therefore, despite the majority of the sample children being default private school goers, the 25 percent mandate should have improved the quality of schools attended by the lottery winners vis-à-vis losers by moving the winners to higher fees private schools. This should, potentially, have improved learning outcomes assuming that high-fees schools are indeed better at education production.

To investigate this aspect, I examine the treatment-control difference in the annual school fees. The fees data is accessed from the education department of the Government of Karnataka. There are two caveats to this analysis. First, fees information is not available for 157 of the 1,616 children in the sample (about 10 percent of the observations); second, given the huge variation in school fees, I trim the sample by dropping observations in the top 2 percentile of the fees distribution. This still leaves 1429 children with fees information, 88 percent of the original sample. The Cumulative Density Functions (CDFs) of school fees for treatments and controls is in Graph 2. The control CDF first-order dominates the treatment CDF establishing that across the distribution of school fees, treatments, on average, attend high-fees schools than the controls. However, the gap between the two CDFs is relatively narrow suggesting that the difference is not meaningful from a policy perspective. The same story can be seen in Graph 3 where the school fees kernel densities of treatment and control are plotted. The right side of the distribution of the density functions clearly shows that in the fees range of INR 10,000- 25,000, there are more treatments than controls. The gap between the two curves is not large.

I explore the story further using regression analysis, estimating the impact of winning the lottery on school fees. Table 11, column (2) presents ITT estimates, which show that being a lottery winner is associated with attending a school whose average fees is higher by INR 1,546, 0.2 σ (the mean of the fees for the sample is INR 12,330 and the standard deviation is 7,897). So, the policy did move treated children to better schools, but the magnitude of the shift, though statistically significant, is not meaningful from a policy perspective.

Table 14 presents the heterogeneous school fees effects for being a girl, Muslim, and scheduled caste student. While none of the differential impact estimates in Panel A are significant, the treatment effects on girls are bigger in magnitude than those for boys (Panel B). Lottery winning girls have a 7 percentage points higher probability of attending private schools, compared to 5 for lottery winning boys; the school fees effect is INR 2,042 compared to INR 1,200 for boys. School fees CDFs (Figure 4) for the boys and girls' sub-samples demonstrate this difference. Further, the probability of private tuition attendance increases by 8 percentage points for girl lottery-winners, compared to zero for boys (Table 14, Panel B, column 3). This is probably because parents are investing a portion of the money saved by the lottery win in education. This suggests that winning a lottery not only shifts girls to better private schools, but also increases their overall possibilities for human capital accumulation. That probably explains the statistically significant test scores reported for girls in table 8.

In summary, the mechanism driving the results is as follows: For the full sample, the policy didn't bring about a meaningful change in learning environments (i.e., the policy didn't alleviate any binding constraints). Hence it didn't change outcomes significantly. For girls, the policy seems to have relaxed the binding constraint of lower educational investments relative to boys, and therefore led to better outcomes. Further, significant effects on the girls subgroup doesn't translate into treatment effects for the full sample as girls constitute only 42 percent of the sample, and the subgroup effect sizes are modest (Total score- 0.11 σ (p=0.096) and GCA - 0.15 σ (p=0.037)).

6. Discussion

6.1. Policy mistargeting

The policy goal of moving poor kids to better schools they couldn't otherwise afford hasn't been achieved demonstrated by the fact that the lottery losers are attending similar types of schools as the winners demonstrates that the schools to which households apply to, through the policy, are already within their (affordable) choice set. The fees reimbursements made by the government to private schools are, in effect, a direct transfer/ subsidy to lottery-winning households, who are not strictly income-constrained in making school choices. The key policy inference is that there has been a failure of targeting. This then begs two important questions. First, how are ineligibles able to apply for the program? Two, why are eligibles not applying for the program?

Program eligibility is determined on the basis of caste or income and is very broadly defined. Some estimates put the number of eligible children to be 80 percent of the school-going children (Kingdon 2017). While caste based eligibility determination is procedurally well established and unambiguous, income determination is notoriously difficult given the lack of reliable data on household incomes. Further, a higher proportion of applicants claim income eligibility. In Karnataka, 69 percent of the applicants claimed income-eligibility compared to only 31 percent of caste based eligibility claimants. The impossibility of establishing an effective measure of income eligibility seems to have opened the doors for ineligible households to apply for the program. A very board definition of eligibility based on income is, therefore, a fatal design flaw in this case.

Next is the issue of non-participation of eligible households. Anecdotal evidence points to information constraints, transaction costs of application (financial and time costs of procuring eligibility documents, for instance), and non-tuition expenses associated with private school education. The role of information constraints and transaction costs in excluding the poor from demand driven social programs has been well documented.

In the context of this policy, non-tuition expenses associated with private school education seem to be the primary reason. Spending on school education involves three broad kinds of costs – first, tuition fees paid to schools; second, other mandatory expenses of education like spending on books, uniforms, transportation, and compulsory activities at school; and finally, non-mandatory expenses like spending on private tuitions, and optional activities outside school. Category two and three are what I refer to as non-tuition expenses of private education. The RTE mandate provides for only the tuition fees to be covered by the government, leaving the households to cover the non-tuition costs. This implies that treatment households will still have to spend a significant amount of money despite the tuition waiver. In some cases the non-tuition costs are very high, and beyond the financial capability of a typical eligible household.

I present results in the next section demonstrating that the non-tuition costs in my sample are much higher than the tuition costs. Activists working on the mandate and parents associations have repeatedly raised this issue, pointing out that the high non-tuition expenses could be deterring the most income constrained households from applying to the mandate. The partial nature of the RTE subsidy is therefore the second design flaw of the mandate. Further research is required to precisely understand the binding constraints that prevent poor households from participating in the program.

6.2. Economic profile of policy participants

Having established that the policy is mistargeted, the next interesting question is to understand how far, the participants are, from the eligibility cutoff. I use primary data on household spending on education to estimate the treatment-controls spending differences and use this estimate to comment on the relative economic status of the RTE applicants' vis-à-vis the population.

Table 11 presents the estimates for the household spending on education. On average, lottery winners who were offered an RTE free place spent INR 5,610 less than the lottery losers on their child's education. The control mean is 13,688 implying that household spending on education of the applicants was INR 13,688 for the control households and about INR 8,000 for the treatment households. This leads to three inferences. First, despite getting a tuition waiver, the treated households spent around INR

8,000 on non-tuition education expenses. Second, the non-tuition expenses are, on average, higher than the tuition costs. Third, RTE applicants have the ability to spend INR 13,688 on their child's education, and thus, the treatment effect of INR 5,610 is a direct transfer to treated households.

The control group mean of INR 13,688 can be used as a starting point to calculate a rough estimate of the income of the applicant households. A national estimate for average percent of household income spent on education in urban India is 7 percent (Tilak 2009). With two school-going children per household, total household expenditure on education would be about INR 27,400. Therefore, the household income of an average household spending INR 27,400 on education should be about INR. 400,000. This is four times the income eligibility cutoff of INR 100,000 in Karnataka, and confirms that ineligible households have captured the policy despite eligibility checks.

6.3. Cost effectiveness

Government reimburses the costs of educating the 25 percent children to private schools. This cost is fixed as lower of the school fees or the average per child expenditure in the government system. The latter amount is INR 11,848 in Karnataka, and is around the 60th percentile of the school fees. So, 40 percent of schools (in the right side of the fees distribution) are paid the ceiling reimbursement amount of INR 11,848[8], while 60 percent get their actual fees reimbursed. The average per child reimbursement for the year 2015-16 is INR 6,800[9]. Government is, therefore, spending INR 6,800 for procurement of education (and other services provided at schools) for disadvantaged children. This amount is only about 60 percent of the cost presently incurred in the government system. Therefore, even in the absence of improved test scores, it can be argued that the program is cost effective. Government has been successful in procuring the same quality education, at 60 percent of the price, by leveraging the efficiency of the private sector. From this perspective, the lack of statistically significant test scores impacts is not a concern till the sign of these estimates is positive.

This argument, however, fails when juxtaposed against another piece of evidence from Table 11 (column 2). The treatment-control fees difference is INR 1,546, and the average per child reimbursement is INR 6,800 (i.e., the government is buying education from private schools worth INR 1,546, by spending 4.5 times that amount). This again puts the spotlight on the mistargeting issue. Theoretically the policy could be cost-effective if truly income constrained households applied for the policy. However, given the profile of the applicants, it is inequitable and ineffective.

6.4. Gender bias in intra household resource allocation

As the heterogeneous treatment estimates demonstrate, lottery results are more binding for girls than for boys. Winning the lottery improves human capital accumulation possibilities for girls relative to boys through improving their chances of attending private schools and private tuitions, and through moving them to better private schools. While increased movement to the private sector (from government) and to higher fees schools (within private) is the direct effect of the policy, increased possibility of attending private tuitions (by 8 percentage points) is due to households re-optimizing differently by gender in response to the policy. All this demonstrates a very clear gender bias against girls in intra household resource allocation.

---

[8] Only recurring costs of running schools (primarily teacher salaries) and excluding the administrative cost of running the education department. Including costs of infrastructure, administration, and school inputs (text books, free meals) makes the per child cost INR 25,500 (Karnataka education budget of INR 165,000 million divided by 6.46 million, the total number of children in government schools from class 1-10).

[9] For the year 2015-16, the government reimbursed 2,040 million INR to private schools for educating total of 300,000 children (admitted in the last four years from 2011-12 to 2015-16) from across the state- this comes to 6,800 INR per student.

Though gender bias in intra household allocation is widely debated in the Asian context, there is no empirical evidence on gender discrimination against girls aged less than 10. Zimmermann (2012), and Burgess and Zhuang (1998) document bias against older girls for India and China respectively. Kingdon (2005) finds little gender bias in educational expenditure, conditional on school enrollment in rural India. My result of gender bias in girls aged 7-8 years is a contribution to this literature.

The mandate is working better for girls by alleviating a binding constraint, in this case parental unwillingness to invest in girls relative to boys. Hence, one way to improve the effectiveness of the policy could be to reserve a significant proportion (say 50 percent) of all available free places for girls.

6.5. Benefits to program non-participants

If the net policy effect is to provide an income transfer to households that don't deserve it, the natural question is if the transfer led to spill over benefits in the household. Following the literature on intra household resource allocation, I investigate if the lottery-winning households invest the income transfer in human capital accumulation of the other children. Table 15 presents the treatment effects on household expenditure on the sample child, on all children in the household, and all children other than the sample child. As discussed in section 6.1, the treatment effect of -5,610 in column (1) implies that treatment households spent INR 5,610 less on the education of the sample child vis-à-vis control households. This saving is the transfer they received thanks to the program. If this transfer has translated into improved human capital investments on siblings, the treatment effect on education expenditure of siblings (column (2)) should be positive. However, the estimate is -1,282 and statistically significant implying that treatment households are spending less not only on program participating children, but also on their siblings. One important caveat to the result is that the expenditure data on siblings was not verified (with supporting documents) compared to expenditure data of the sample child. Further, treated households have the incentive to under report expenditures compared to controls[10]. Hence I would treat this result as purely suggestive evidence pointing to absence of household reallocation (on education) of the RTE windfall by treatment households. A more detailed analysis of this would require more detailed and reliable expenditure data.

6.6. Policy implications

My results and analysis point to the following policy implications:

1. The program is poorly targeted leading to non-participation of the most disadvantaged. I argue that this is due to flawed policy design in two aspects: determination of program eligibility and failure to provide for non-tuition costs.
2. The mandate theoretically passes the cost-effectiveness test, as it leads to provision of similar quality education at only 60 percent of the cost incurred in the government system. However, unless the benefit is passed on to the truly income-constrained households (through effective targeting), the spending on the program is just an income transfer from the taxpayers to RTE free place winners.
3. The policy leads to statistically significant treatments effects for girls by relieving the binding constraint of gender bias against them. Hence reserving a significant proportion of free places for girls could increase policy effectiveness.
4. The psychosocial treatment effects debunk the prevalent anecdotal evidence that RTE free places children are being discriminated against in their new learning environments.

[10] Survey teams consistently reported the discomfort of treated households in reporting expenditures, as several of these households knew they were strictly ineligible for the program.

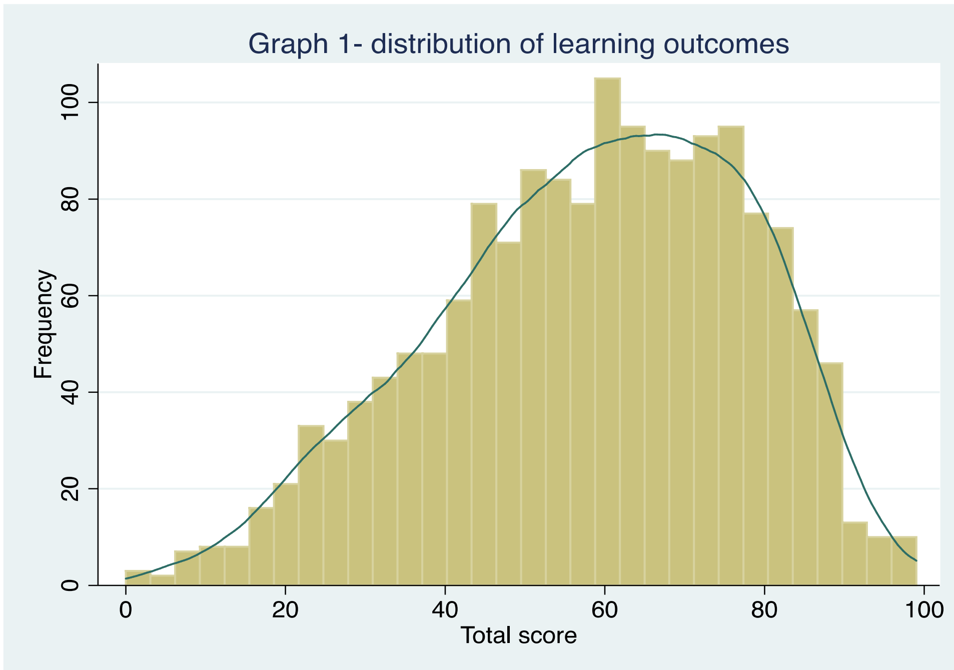
7. Conclusion

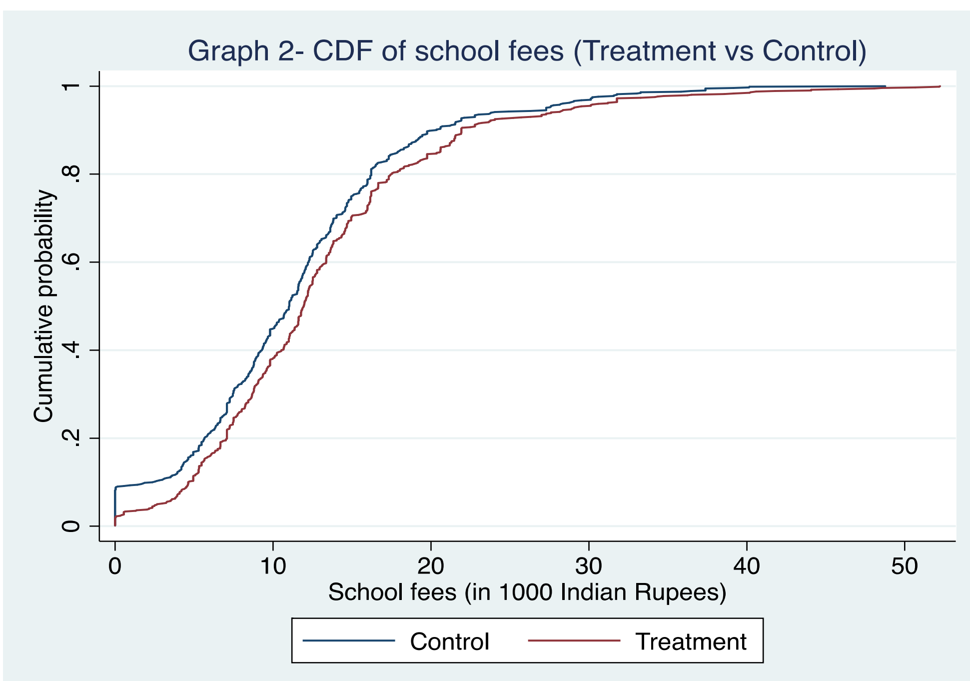
This paper presents the results of the first empirical investigation of the RTE 25 percent mandate, India's national school choice policy. Against the background of continuing debates on the role of private sector in human capital formation, school choice, and public private partnerships in education, I investigate the question of the impact of the choice policy on children's outcomes. I take advantage of the lottery-based allocation of RTE free places, and estimate the policy impacts on children's learning and psychosocial outcomes. Contrary to expectation of choice enthusiasts, I find no significant treatment effects for the full sample; the treatment effects for girls' subgroup are modest, but statistically significant. Detailed mechanism analysis reveals that the policy didn't shift lottery winners to significantly different schools vis-à-vis the lottery losers (i.e., it didn't relieve any binding constraint). I posit that the policy is mistargeted – the poor are not induced to participate and the non-poor are not effectively prevented from participating. All this explains the absence of policy impacts.

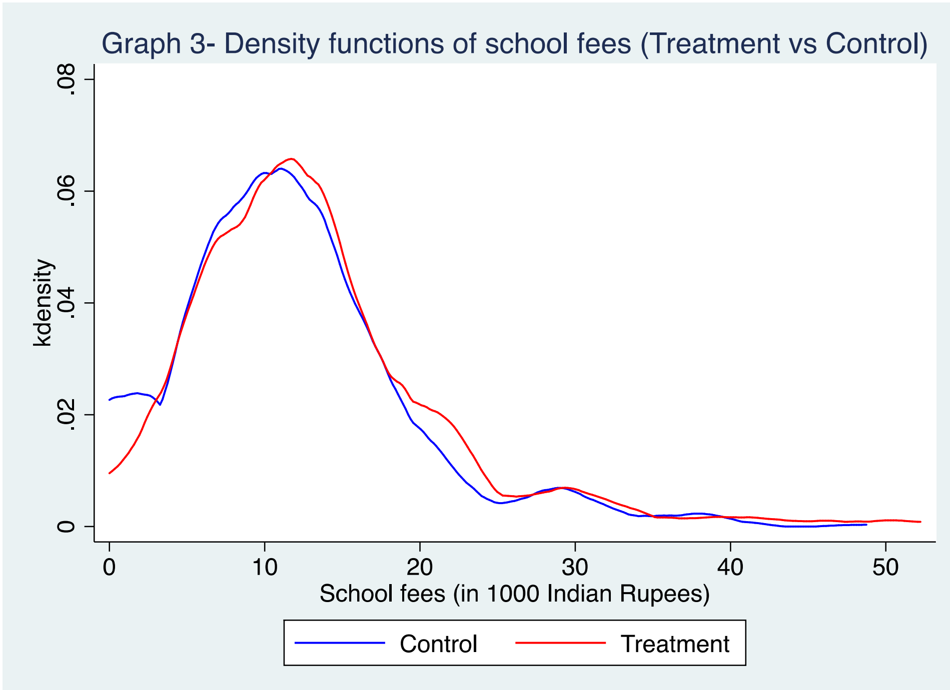
This argument is further corroborated by the results of the girls sub-sample. I document gender bias against girls in intra household resource allocation on education, and demonstrate that the policy, to a certain extent, alleviates the binding constraint of lower investment in girls' human capital formation. This potentially explains the significant test score effects for girls.

Though I can't estimate the school choice effects or the private school premium given low compliance rates, the policy impact estimates and the mechanism analysis point to ways of improving the mandate's effectiveness, and contribute to a better understanding of impacts of school choice policies, targeting of social programs, and gender bias in intra household resource allocation.

Graphs, Figures and Tables






Graph 3- Density functions of school fees (Treatment vs Control)
kdensity
0
.02
.04
.06
.08
0
10
20
30
40
50
School fees (in 1000 Indian Rupees)
Control
Treatment

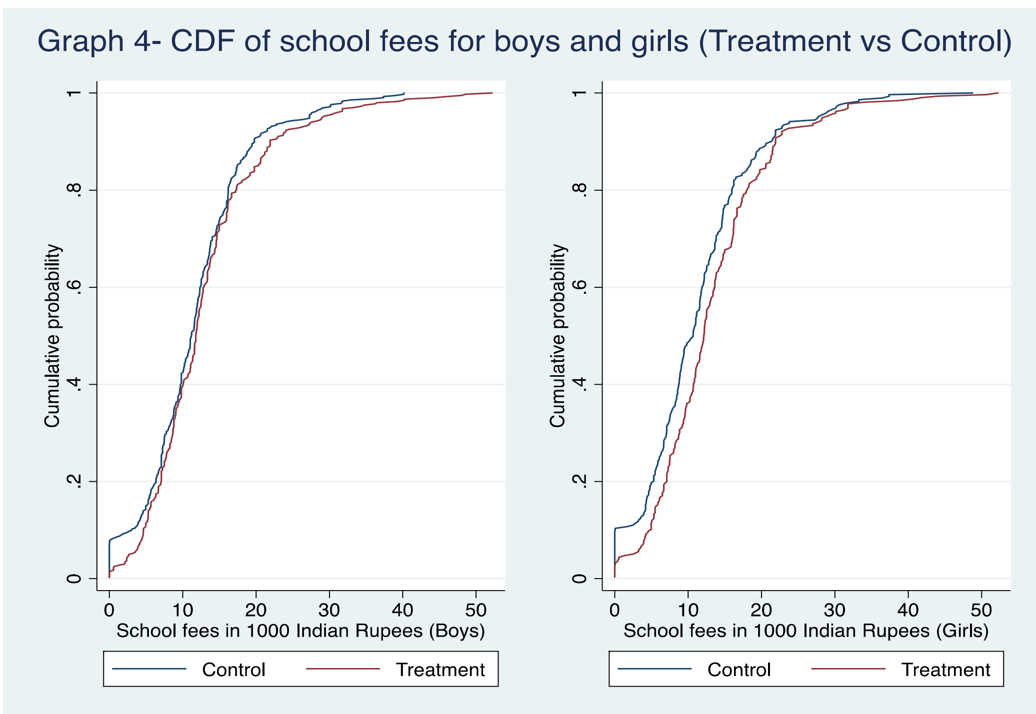

Graph 4- CDF of school fees for boys and girls (Treatment vs Control)
Cumulative probability
0
.2
.4
.6
.8
1
0
10
20
30
40
50
School fees in 1000 Indian Rupees (Boys)
Control
Treatment
Cumulative probability
School fees in 1000 Indian Rupees (Girls)
Control
Treatment

Table 1: Summary of the class I applications database					
DISTRICT	Applicants	Percentage of total	Treatment	Percentage treated	Matched pairs
All 34 districts	**126,728**	**100.0%**	**62,046**	**49.0%**	**25,123**
		Sample districts			
Bangalore Rural	2,893	2.3%	1,150	39.8%	759
Bangalore Urban	17,362	13.7%	8,819	50.8%	2,436
Bellary	9,082	7.2%	4,178	46.0%	2,022
Gulbarga	6,538	5.2%	4,296	65.7%	1,076
Total	**35,875**	**28%**	**18,443**	**51.4%**	**6,293**

Table 2: Validity of the design				
	Treatment	Control		
	Mean		Difference	p- value
	(1)	(2)	(3)	(4)
Age (in years, as on 1 Nov 2016)	7.3	7.28	0.02	0.81
Gender- male	0.56	0.59	-0.03	0.13
Caste-Scheduled Caste	0.18	0.19	-0.01	0.74
Caste- Other Backward Castes	0.61	0.6	0.01	0.42
Religion- Muslim	0.24	0.24	0	0.89
Mother's age	30.15	30.46	-0.31	0.15
Mother's education (number of years)	8.6	8.39	0.21	0.48
Working mother	0.22	0.24	-0.02	0.34
Father's age	36.64	37.09	-0.45*	0.07
Father's education (number of years)	9.04	8.96	0.08	0.80
Working father	0.94	0.95	-0.01	0.36
Birth order	1.59	1.63	-0.04	0.22
Number of siblings	1.07	1.09	-0.02	0.61
Attended pre-primary school	0.9	0.91	-0.01	0.65
Household economic status -asset index	8.93	8.82	0.11	0.51
House ownership	0.5	0.53	-0.03	0.15
Number of rooms in the house	3.01	3	0.01	0.76
N	808	808	1616	

Notes: Table presents the treatment and control means on a range of variables. Column (3) is the difference between treatment and control means, and column (4) is the p- value on the treatment indicator with the balance variable regressed on treatment, district dummies, and standard errors clustered at the pair level. The p-value for an F-test of joint significance of all the balance variables with district dummies and standard errors clustered at the pair level is 0.67. Statistical significance is denoted as: * p<0.10, ** p<0.05, *** p<0.01

Table 3: Learning outcomes- raw test scores				
Test	Mean	Std. Dev.	Min	Max
General Cognitive Ability (GCA)	16.3	4.6	0.3	25.0
Mathematics	15.8	6.7	0.0	25.0
English	13.6	6.1	0.0	25.0
Kannada	12.5	6.5	0.0	25.0
Total	58.2	19.4	0.0	99.0
Total number of observations- 1616				

Table 4: Psychosocial outcomes				
Panel A- Index construction- simple aggregation (naïve approach)				
Index	**Mean**	**Std. Dev.**	**Min**	**Max**
Self efficacy	3.0	0.8	0	4
Peer support	3.1	1.0	0	4
School support	3.5	0.8	0	4
Teacher support	3.2	0.9	0	4
Panel B- Index construction- Inverse covariance weighting approach				
Index	**Mean**	**Std. Dev.**	**Min**	**Max**
Self efficacy	0.0	1.0	-4.7	1.2
Peer support	0.0	1.0	-3.6	0.9
School support	0.0	1.0	-6.8	0.5
Teacher support	0.0	1.0	-5.2	0.7
Total number of observations- 1616				

Table 5: Compliance with treatment assignment				
Random assignment	Treated*	Percentage treated	Not- treated	Total
Treatment	723	89.50%	85	808
Control	147	18.20%	661	808
	870	53.80%	746	1616
* - Enrolled in an RTE free place				

Table 6: Impact of policy on learning outcomes (ITT estimates)					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	**Total score**	**GCA score#**	**Math score**	**English score**	**Kannada score**
Treatment	0.035	0.016	0.046	0.022	0.028
	(0.043)	(0.044)	(0.045)	(0.044)	(0.042)
Age (in years)	0.159***	0.187***	0.141***	0.107***	0.093***
	(0.031)	(0.033)	(0.031)	(0.028)	(0.024)
Gender (boy=1)	-0.084*	0.072	-0.005	-0.128***	-0.158***
	(0.046)	(0.048)	(0.047)	(0.047)	(0.045)
Scheduled Caste	-0.392***	-0.233***	-0.299***	-0.336***	-0.323***
	(0.070)	(0.070)	(0.065)	(0.071)	(0.063)
Muslim	-0.464***	-0.317***	-0.263***	-0.167***	-0.729***
	(0.061)	(0.061)	(0.062)	(0.061)	(0.063)
Mother's age	0.020***	0.016***	0.027***	0.016**	0.011*
	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mother's education (in years)	0.022***	0.020***	0.015***	0.028***	0.012***
	(0.005)	(0.005)	(0.004)	(0.008)	(0.003)
Working Mother	0.027	0.038	0.050	-0.108*	0.139**
	(0.061)	(0.060)	(0.062)	(0.061)	(0.057)
Asset index	0.006	0.009	-0.007	0.023***	-0.006
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)
Constant	-1.978***	-2.236***	-2.105***	-1.576***	-0.838***
	(0.310)	(0.302)	(0.303)	(0.299)	(0.264)
Observations	1,599	1,597	1,597	1,597	1,597
R-squared	0.131	0.094	0.105	0.091	0.199

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
#- GCA is general cognitive ability

Notes: All regressions have district dummies and the standard errors are clustered at the pair level. The dependent variables, test scores, are standardized z-scores; hence, the coefficient estimate is the effect size.

Table 7: Impact of policy on psychosocial outcomes (ITT estimates)				
	(1)	(2)	(3)	(4)
VARIABLES	**Self-efficacy**	**Peer support**	**School experience**	**Teacher support**
Panel A- Indices constructed by simple aggregation				
Treatment	0.080**	0.033	0.023	-0.023
	(0.036)	(0.041)	(0.032)	(0.035)
Observations	1,597	1,597	1,597	1,597
R-squared	0.041	0.091	0.215	0.192
Panel B- Indices constructed by inverse covariance weighting				
Treatment	0.114**	0.037	0.035	-0.020
	(0.049)	(0.046)	(0.048)	(0.045)
Observations	1,538	1,560	1,568	1,541
R-squared	0.031	0.048	0.112	0.140

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

Notes: All regressions include covariates and district dummies; the standard errors are clustered at the pair level

Table 8: Heterogeneous test score impacts by student characteristics (ITT estimates)					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	**Total score**	**GCA score**	**Math score**	**English score**	**Kannada score**
Panel A- Differential impact estimates					
Treatment * covariate					
Girl	0.139	0.235**	0.114	0.116	0.029
	(0.097)	(0.099)	(0.098)	(0.100)	(0.090)
Muslim	-0.184*	-0.142	-0.117	-0.147	-0.187*
	(0.111)	(0.113)	(0.110)	(0.113)	(0.107)
Scheduled Caste (SC)	0.054	0.072	0.110	-0.004	0.009
	(0.123)	(0.123)	(0.120)	(0.124)	(0.116)
Observations	1,599	1,597	1,597	1,597	1,597
Panel B- Sub-group impact estimates					
Girl	0.115*	0.151**	0.112	0.089	0.045
	(0.069)	(0.072)	(0.069)	(0.070)	(0.066)
Boy	-0.024	-0.084	-0.002	-0.027	0.016
	(0.061)	(0.060)	(0.064)	(0.063)	(0.057)
Muslim	-0.105	-0.092	-0.042	-0.089	-0.114
	(0.097)	(0.098)	(0.095)	(0.099)	(0.094)
Non- Muslim	0.078	0.050	0.074	0.057	0.073
	(0.050)	(0.050)	(0.052)	(0.050)	(0.047)
Scheduled Caste (SC)	0.079	0.075	0.137	0.019	0.036
	(0.111)	(0.112)	(0.107)	(0.113)	(0.106)
Not Scheduled Caste	0.025	0.003	0.026	0.023	0.027
	(0.048)	(0.048)	(0.050)	(0.048)	(0.045)
Observations	1,599	1,597	1,597	1,597	1,597

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All regressions include covariates and district dummies; the standard errors are clustered at the pair level

Table 9: Sub- group psychosocial score impacts (ITT estimates)				
	(1)	(2)	(3)	(4)
VARIABLES	**Self-efficacy**	**Peer support**	**School experience**	**Teacher support**
Girl	0.139*	0.079	-0.038	0.002
	(0.077)	(0.078)	(0.076)	(0.070)
Boy	0.096	0.008	0.089	-0.036
	(0.065)	(0.058)	(0.061)	(0.063)
Muslim	0.077	-0.187**	0.045	-0.111
	(0.106)	(0.088)	(0.091)	(0.092)
Non- Muslim	0.125**	0.107**	0.032	0.008
	(0.057)	(0.053)	(0.056)	(0.052)
Scheduled Caste (SC)	0.070	0.177	0.118	-0.132
	(0.122)	(0.116)	(0.102)	(0.121)
Not Scheduled Caste	0.123**	0.006	0.017	0.005
	(0.054)	(0.050)	(0.054)	(0.049)
Observations	1,538	1,560	1,568	1,541

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

Notes: All regressions include covariates and district dummies; the standard errors are clustered at the pair level

Table 10-Treatment control differences in home inputs to education

	Treatment	Control	Difference	p- value
	Mean			
	(1)	(2)	(3)	(4)
Time spent at private tuition	1.15	1.14	0.01	0.92
Time spent on studying at home	2.22	2.28	-0.06*	0.10
Time spent on playing at home	3.06	3.08	-0.02	0.70
Indicator for attending private tuition	0.45	0.41	0.04	0.10
Annual private tuition fees	2220.9	2290.86	-69.96	0.46
N	808	801	1616	

Notes: Table presents the treatment and control means on five home inputs to education. The sample is pruned to remove the top 2 percentile private tuition fees observations. Column (3) is the difference between treatment and control means, and column (4) is the p- value on the treatment indicator with the home input variable regressed on treatment with district dummies, and standard errors clustered at the pair level. The p-value for an F-test of joint significance of the five home- input variables with district dummies and standard errors clustered at the pair level is 0.8649. Statistical significance is denoted as: * p<0.10, ** p<0.05, *** p<0.01

Table 11: Impact of policy on measures of school type, school fees, and education expenditure (ITT estimates)

	(1)	(2)	(3)
VARIABLES	**Private school**	**Annual school fees**	**Education expenditure**
Treatment effect	0.05***	1,545.8***	-5,610***
	(0.01)	(325.16)	(394.7)
Constant/ Control mean	0.94***	13,299.8***	13,688***
	(0.01)	(429.41)	(525.3)
Observations	1,616	1,429	1,616
R-squared	0.03	0.14	0.18

*** p<0.01, ** p<0.05, * p<0.1

Notes: All regressions include district dummies; the standard errors are clustered at the pair level

Table 12- Private and public school attendance (sample versus population)					
Panel A- Sample (by treatment)					
	Public	Private	Total	Percentage public	Percentage private
Treatment	16	792	808	2.0%	98.0%
Control	60	748	808	7.4%	92.6%
Total	76	1,540	1,616	4.7%	95.3%
Panel B- Sample (by district)					
Bangalore Rural	15	385	400	3.8%	96.3%
Bangalore Urban	7	391	398	1.8%	98.2%
Bellary	29	391	420	6.9%	93.1%
Gulbarga	25	373	398	6.3%	93.7%
Total	76	1,540	1,616	4.7%	95.3%
Panel C- Population data, classes 1-8 (by district)					
Bangalore Rural	57,556	53,258	110,814	51.9%	48.1%
Bangalore Urban	106,029	569,244	675,273	15.7%	84.3%
Bellary	229,320	119,531	348,851	65.7%	34.3%
Gulbarga	250,056	179,420	429,476	58.2%	41.8%
Total	642,961	921,453	1,564,414	41.1%	58.9%

Notes: In three of the four districts the public school attendance ratio is above 50 percent.

Table 13: Private school fees (district-wise)					
		Average fee charged	Maximum fee charged	Fee charged by the median school	Fee charged by the 90th percentile school
District	Number of schools*	**All fees data is annual and in Indian rupees (INR)**			
Bangalore Rural	182	17,179	274,771	13,289	26,370
Bangalore Urban	1,125	23,904	783,159	14,389	44,110
Bellary	378	10,419	121,527	8,608	16,212
Gulbarga	326	9,345	67,750	8,430	15,701
Total	2,011	18,398	783,159	12,094	31,602

*- Fees data is currently available only for 80 percent of the registered private schools

Source: Authenticated school fees information from the Education department, Government of Karnataka

Table 14: Heterogeneous impacts on school type, and privation tuitions (ITT estimates)				
	(1)	(2)	(3)	(4)
VARIABLES	**Private school**	**School fees**	**Private tuition**	**Private tuition fees**
Panel A- Differential impact estimates				
Treatment * covariate				
Girl	0.02	842.1	0.08	135.4
	(0.02)	(740.41)	(0.05)	(164.85)
Muslim	0.01	993.0	-0.06	170.8
	(0.02)	(799.54)	(0.06)	(181.32)
Scheduled Caste (SC)	0.02	254.3	0.00	185.4
	(0.03)	(909.59)	(0.06)	(224.37)
Observations	1,599	1,415	1,581	677
Panel B- Sub-group impact estimates				
Girl	0.07***	2,042.4***	0.08**	44.3
	(0.02)	(530.44)	(0.04)	(126.09)
Boy	0.05***	1,200.2***	0.00	-91.1
	(0.01)	(461.92)	(0.03)	(103.39)
Muslim	0.06***	2,317.4***	-0.01	89.1
	(0.02)	(684.00)	(0.05)	(148.22)
Non- Muslim	0.05***	1,324.4***	0.05*	-81.7
	(0.01)	(384.32)	(0.03)	(96.64)
Scheduled Caste (SC)	0.07**	1,764.1**	0.04	118.0
	(0.03)	(820.48)	(0.05)	(208.02)
Not Scheduled Caste	0.05***	1,509.8***	0.04	-67.4
	(0.01)	(364.92)	(0.02)	(85.05)
Observations	1,599	1,415	1,581	677

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All regressions include covariates and district dummies; the standard errors are clustered at the pair level

Table 15: Impact of policy on investment in program non-participants (ITT estimates)

	(1)	(2)	(3)
	Household education expenditure (in INR)		
VARIABLES	**Sample child**	**All children other than sample child**	**All children in household**
Treatment effect	-5,610***	-1,281**	-6,891***
	(394.7)	(612.6)	(792.7)
Constant/ Control mean	13,688***	10,024***	23,711***
	(525.3)	(728.5)	(1,049)
Observations	1,616	1,616	1,616
R-squared	0.200	0.060	0.157

*** p<0.01, ** p<0.05, * p<0.1

Notes: All regressions include district dummies; the standard errors are clustered at the pair level

Figure 1- THEORY OF CHANGE UNDERLYING THE 25 PERCENT MANDATE

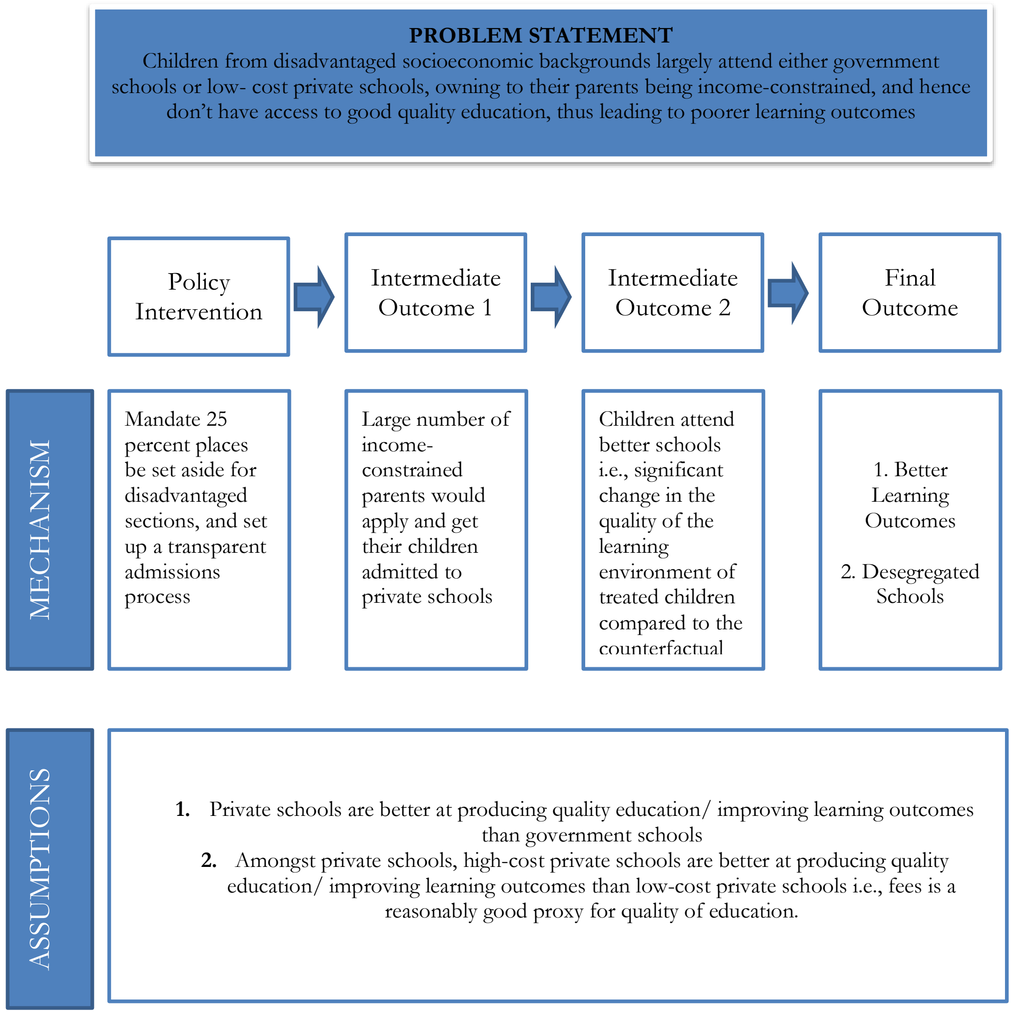


Figure 2- STUDY DESIGN AND TIMELINE

STUDY SETTING

The Karnataka state education department conducted a centralized lottery to fill the 25 percent RTE free places (for Class I) in about 11,000 private schools across the state. 49 percent of the 126,700 applicants were offered a free place. Four out of the 34 districts were chosen for the study- 2 from the more developed Southeastern Karnataka, and 2 from the backward Northeastern region.

LOTTERY

Winning the lottery is random conditional on neighborhood, caste category, and the schools chosen in the application. The ex-ante probability of winning is therefore similar within randomization cells/ strata.

April 2015

PAIR-WISE MATCHING DESIGN

Within each randomization stratum, treatments are randomly matched to controls, thus creating matched pairs. The population of matched pairs from the four sample districts constitutes the sampling frame for the study.

STUDY SAMPLE

The study sample comprises randomly sampled 800 matched pairs (1600 children- 800 treatment and 800 control) from the four districts with each district given equal weightage i.e., 200 matched pairs per district.

June-Aug 2016

PRIMARY DATA COLLECTION

Data on children's test scores and behavioral outcomes, and household characteristics were obtained through testing all the sample children and surveying their parents. Children had undergone an average of 1.5 years of treatment at the time of the survey.

Sep-Dec 2016

REFERENCES

ABDULKADIROĞLU, A. and SÖNMEZ, T., 1998. Random serial dictatorship and the core from random endowments in house allocation problems. *Econometrica,* **66**(3), pp. 689-701.

ALATAS, V., BANERJEE, A., HANNA, R., OLKEN, B.A. and TOBIAS, J., 2012. Targeting the poor: evidence from a field experiment in Indonesia. *The American Economic Review,* **102**(4), pp. 1206-1240.

ANDERSON, M.L., 2008. Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American statistical Association,* **103**(484), pp. 1481-1495.

BARROW, L. and ROUSE, C.E., 2008. School vouchers and student achievement: Recent evidence, remaining questions.

BRUHN, M. and MCKENZIE, D., 2009. In pursuit of balance: Randomization in practice in development field experiments. *American economic journal: applied economics,* **1**(4), pp. 200-232.

BURGESS, R. and ZHUANG, J., 1998. Gender biases in intrahousehold allocation in rural China. *STRICERD Development Economics Research Programme, London School of Economics, mimeograph,* .

CAMERON, L. and SHAH, M., 2011. Mistargeting of cash transfers, social capital destruction, and crime in Indonesia. *Victoria: Monash University,* .

DEMING, D., HASTINGS, J., KANE, T. and STAIGER, D., 2009. School choice and college attendance: Evidence from randomized lotteries. *NBER Working Paper,* .

DRÈZE, J., 2013. *An uncertain glory : India and its contradictions.* London : Allen Lane.

DUFLO, E., GLENNERSTER, R. and KREMER, M., 2007. Using randomization in development economics research: A toolkit. *Handbook of development economics,* **4**, pp. 3895-3962.

GLEASON, P. and SILVERBERG, M., 2010. *The evaluation of charter school impacts: Final report.* US Department of Education, National Center for Education Evaluation and Regional Assistance, Institute of Education Sciences.

GLEWWE, P.W., HANUSHEK, E.A., HUMPAGE, S.D. and RAVINA, R., 2011. *School resources and educational outcomes in developing countries: A review of the literature from 1990 to 2010,* .

GLEWWE, P. and KREMER, M., 2006. Schools, teachers, and education outcomes in developing countries. *Handbook of the Economics of Education,* **2**, pp. 945-1017.

GLEWWE, P. and MURALIDHARAN, K., 2015. Improving school education outcomes in developing countries: evidence, knowledge gaps, and policy implications. *University of Oxford, Research on Improving Systems of Education (RISE),* .

HÄRMÄ, J., 2011. Low cost private schooling in India: Is it pro poor and equitable? *International journal of educational development,* **31**(4), pp. 350-356.

HOXBY, C.M., 2007. *The economics of school choice.* University of Chicago Press.

HSIEH, C. 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), pp. 1477-1503.

INDIAN INSTITUTE OF MANAGEMENT, AHMEDABAD, 2016. *State of the Nation: RTE Section 12 (1) (c) 2015 (Provisional).* New Delhi: Indian Institute of Management Ahmedabad and others.

INDIAN INSTITUTE OF MANAGEMENT, AHMEDABAD, 2013. *State of the nation: RTE Section 12 (1) (c).* New Delhi: Indian Institute of Management, Ahmedabad.

JUNEJA, N., 2014. India's New Mandate against Economic Apartheid in Schools. *Journal of International Cooperation in Education,* **16**, pp. 55-70.

KAUFMAN, J.E. and ROSENBAUM, J.E., 1992. The education and employment of low-income black youth in white suburbs. *Educational Evaluation and Policy Analysis,* **14**(3), pp. 229-240.

KINGDON, G.G., 2017. *The private schooling phenomenon in India: A review,* .

KINGDON, G.G., 2005. Where has all the bias gone? Detecting gender bias in the intrahousehold allocation of educational expenditure. *Economic Development and Cultural Change,* **53**(2), pp. 409-451.

MINISTRY OF LAW AND JUSTICE, GOVERNMENT OF INDIA, 2009. *The Right of Children to Free and Compulsory Education Act, 2009.*

MURALIDHARAN, K. and SUNDARARAMAN, V., 2015. The aggregate effectof school choice: Evidence from a two-stage experiment in India. *Quarterly Journal of Economics,* **130**(3), pp. 1011-1066.

MUSSET, P., 2012. *School choice and equity: current policies in OECD countries and a literature review,* .

NORONHA, C. and SRIVASTAVA, P., 2013. India's Right to Education Act: Household experiences and private school responses. *Education Support Program Working Paper Series,* (53),.

PRATHAM FOUNDATION, 2015. *Annual Status of Education Report (Rural) 2014.* New Delhi: ASER Center.

SAHA, D., 2016-last update. Available: https://thewire.in/36382/indians-increasingly-prefer-private-education-71-million-take-tuitions/2017].

SINGH, A., 2012. The Private School Premium: Size and sources of the private school advantage.

SRIVASTAVA, P. and NORONHA, C., 2014. Institutional framing of the Right to Education Act: Contestation, controversy, and concessions. *Economic and Political Weekly,* **49**(18), pp. 51-58.

SRIVASTAVA, P., 2008. *School Choice in India: disadvantaged groups and low-fee private schools.* na.

TILAK, J.B.G., 2009. Household expenditure on education and implications for redefining the poverty line in India. *Planning Commission of India,* .

WORLD BANK, 2016-last update, World Bank Data. Available: http://data.worldbank.org/indicator/SE.PRM.PRIV.ZS [07/14, 2017].

ZIMMERMANN, L., 2012. Reconsidering gender bias in intrahousehold allocation in India. *Journal of Development Studies,* **48**(1), pp. 151-163.