Choice for the poor or poor choice? Experimental evidence on the impact of India's school choice policy

1 Introduction

School choice programs, like voucher systems, have been important and contentious education reforms in the last few decades. They entail introducing market mechanisms in schooling by making public funds available to any type of school (public, private, or faith-based) chosen by the parents. This parental ability to choose schools, as consumers, is expected to trigger competition in schooling markets, leading to overall improvements in school performance. Further, choice has an equity aspect as it potentially benefits poor households the most: these households face binding income constraints that oblige them to enroll their children at failing neighborhood public schools. Theory, hence, predicts that school choice would promote student outcomes, school level efficiency, and equity. However, these expectations are strongly contingent on the underlying assumptions of parental behavior and nature of schooling markets. Critiques of choice have long argued that it would have the detrimental effect of increasing school segregation. Given this theoretical ambiguity, understanding the impact of school choice policies becomes an empirical question.

The empirical evidence is also, unfortunately, inconclusive: the size and direction of the impacts of choice seem to vary by context and design/type of the choice program. Barrow and Rouse (2008) and Musset (2012) summarize the evidence. Further, most of the existing empirical literature is focused on US and OECD countries. The literature from developing country contexts is at a very nascent stage (Morgan et al., 2013) as large-scale choice programs have not been widely implemented in these settings. Calls for introduction of school choice policies have, of late, been increasing in developing countries like India, given the large and emergent private sector and the growing perceptions of failure of government schools. India's Right to Education (RTE) Act of 2009 introduced a unique choice policy that, when fully implemented, could impact 16 million children making it the world's largest program for public funding and private provision in education (Indian Institute of Management, 2013).

In this chapter, I present experimental evidence on the impact of this policy. 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. Government pays the tuition costs for these places to private schools. Families from certain unprivileged social groups (castes) and those below a specified income cutoff are defined as disadvantaged and are eligible for free places. The stated objectives of the policy are to provide better quality education to disadvantaged children and to desegregate the Indian schooling system.

Despite these lofty goals, and the legal mandate, several Indian states are skeptical about the program and haven't implemented it. They argue the mandate will reduce public school budgets by diverting scare 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 enabling them to shift from their default low quality learning environments (government or low-fees private schools) to high quality ones (high-fees private schools). Against this background, I investigate the impact of the 25 percent mandate on childrens' learning and psychosocial outcomes.

My study sample of 1,616 children comes from two regions of the south Indian state of Karnataka. I exploit the lottery-based allocation of RTE free place and use a pairwise matching design (Bruhn and McKenzie, 2009) to estimate the casual impact of the policy. My matching design accounts for the admission process and the lottery algorithm and generates treatment -control pairs with both the members having the same ex-ante probability of admission. 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.

My results show that after 1.5 years of schooling, there is 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). On one of the four psychosocial measures, self- efficacy, lottery winners perform better than lottery losers (0.12 σ ; p=0.017). Further, there is no heterogeneity in treatment effects for different sub-groups in the sample.

I explore the mechanisms driving the non-impact and determine that the policy didn't shift children from their default low-quality schools to high-quality schools as expected. First, 99 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- have not participated in the policy. Second, both the treatment and control children, on average, attend high-fee private schools that charge around the median school fee. Hence,there is no qualitative difference between the type of schools attended by treatment and control children and the average policy applicant is a default private school goer who can afford the median school fee. All this leads to the central policy result of this chapter: the 25 percent mandate is mistargeted. I replicate this result on a much larger sample of children drawn from across the state.

I demonstrate that design flaws in the 25 percent mandate have led to the mistargeting. Making eligibility contingent on income, whose determination is difficult in the Indian context, has enabled many ineligible households to participate in the program. Further, the policy only covers the tuition costs of education and not the non-tuition costs, which I estimate to be around 1.3 times the tuition costs. This partial nature of the RTE subsidy has probably deterred the poorest parents from applying to the mandate.

My research contributes to and enriches several strands of empirical literature in economics and public policy. First, I add to the large global literature on the impact of choice programs on childrens' outcomes. This literature comprises investigations of impacts of various types of school choice programs: universal and targeted vouchers, charter schools, open enrollment, magnet schools. Barrow and Rouse (2008); Musset (2012) summarize the evidence. My policy impact estimates and mechanism analysis contribute to this literature by illuminating how the design of the choice program interacts with the context and determines impact or the lack of it. Within this broader literature, my contribution is particularly relevant to the smaller body of empirical evidence on impacts of choice policies in developing countries. Glewwe and Muralidharan (2015) summarize the few rigorous studies from Chile, Columbia, and India and opine that more evidence is needed to better understand school choice in these contexts. Zooming in further, my findings contribute significantly to the small literature on the impacts of school choice in India. Additionally, this is the first evaluation of the impact of a government implemented large-scale school choice policy in the Indian context, and has much higher generalizability than previous evaluations that are based on NGO run programs (Muralidharan and Sundararaman, 2015).

Second, my headline result of policy mistargeting and the detailed mechanism analysis speak to the literature on targeting the poor for social programs in developing countries. A large part of this literature is centered on poverty alleviation programs like food subsidies, cash transfers, and public works. Coady et al. (2004) summarize the evidence on 122 programs from 48 countries across the developing world. There is also a significant body of research demonstrating the mistargeting of welfare programs in Indian (Jha et al., 2013; Mazumdar and Sharma, 2013; Svedberg, 2012; Drèze and Khera, 2010). I contribute to this literature by demonstrating the failure of income based targeting, a relatively new targeting approach in the Indian context. Further, my evidence hold relevance across the developing world, where the difficulty of household-income-determination has been well established (Alatas et al., 2012).

The rest of the paper is structured as follows: Section 2 presents the school choice theory and the debate. Section 3 presents the institutional setting and describes the school choice policy being evaluated. Section 4 goes into the research design and the study sampling. Section 5 presents the data, while Section 6 presents the results. Section 7 is a detailed analysis of the mechanism driving the results. Section 8 is a detailed discussion along with policy implications. Section 9 explores the external validity of the results. Section 10 concludes.

2 Theory and Literature

2.1 School Choice: Theory and Debate

School choice programs allow poor parents to choose the type of schools their children attend. In the context of US where the choice debate, theory, and literature is largely focused, the default school option for children, especially from underprivileged socio-economic backgrounds, is the neighborhood public school. Their parents don't have the income and other means to afford fee- charging private schools or to move to better school districts. These twin constraints of income and residence are binding for families from particular socio-economic groups, for instance low-income racial minorities. School choice policies seek to relax binding constraints of income and place of residence and to move poor children to better schools.

Proponents of choice contend that making public funds available for private institutions, linking public school budgets to student enrollment, and giving parents the choice to decide where they want to educate their child would introduce competition amongst education providers (competition effect) leading to enhanced productivity of the entire education system and improve childrens' learning outcomes. The assumptions of this 'market model' of school choice are that public schools are inefficient because of their monopolistic nature and that private schools are more effective than public schools (Hsieh and Urquiola, 2006). The underlying mechanism driving the overall improvement in productivity in both public and private schools is parental ability as consumers to 'vote with their feet' in accordance with the Tiebout (1956) choice model. Finally, choice would also promote equity if its benefits were targeted to poor and disadvantaged households (Friedman and Solo, 1955; Friedman, 1982; Hoxby, 2003; Wolfe, 2009).

Critiques of expanding choice maintain that it leads to greater segregation in schools, as private schools would 'cream- skim' better students than improving their productivity to survive in the market place.[1] Further, 'choice improves education outcomes' argument is premised on parental ability to choose schools that produce the best outcomes. However, school effectiveness is unobservable, and parents might end up choosing schools based on criteria other than quality of education. This in turn could create more incentives for cream- skimming leading to greater stratification at schools (Hanushek, 1981; Hsieh and Urquiola, 2006; Rothstein, 2006; Wolfe, 2009; Musset, 2012). Scholars have also argued that overreliance on market mechanisms cannot be the default solution for improving school education given its public goods nature (Drèze, 2013).

Theoretical uncertainty on the impacts of choice and the contentious nature of

[1]The cream skimming argument goes that faced with competition, a private school can either improve productivity through the longer and costly route of investing in their teachers etc., or through the shorter and less-expensive route of selecting better students (Hsieh and Urquiola, 2006).

the choice debate has opened doors for empirical research into the questions of impact of choice on school productivity, equity, and student outcomes. Unfortunately, the empirical evidence is also ambiguous on the impact questions; the results seem to be context and policy design specific. Barrow and Rouse (2008) and Musset (2012) summarize the evidence from US and other OECD countries. The literature from developing country contexts is at a very nascent stage (Morgan et al., 2013; Glewwe and Muralidharan, 2015).

2.2 School choice in developing countries

The operation of school choice is different in low and middle-income countries, like India, relative to OECD countries, given the contextual differences in functioning and funding of the public education system, and the extent and nature of private schooling. First, on public schooling, national and state governments fund public schools without any role for local taxes, thus precluding the possibility of operation of Tiebout choice mechanism to discipline public schools. Second, most public school teachers are tenured civil servants with pay and tenure delinked from performance measures like student test scores or enrollment. Hence, it is unlikely that choice policies could trigger competition between public and private schools.

Second, private primary school enrollment in these contexts is much higher than that in rich countries.[2] And there is huge variation in the type and quality of private schools. Unlike private schools in rich countries that are typically expensive and are attended only by the elite, private schools in developing countries are varied in their fees structure and cater to a broader section of the society (Patrinos et al., 2009). Choice proponents in these contexts assume that private schools are better than public schools, and that high-fee private schools are better than low-fee private schools at producing education outcomes. They contend that expanded choice will improve

[2]For instance, private primary school enrollment in India, Pakistan, and Bangladesh is higher than 30 percent, compared to less than 10 percent in US and UK.

learning outcomes of disadvantaged children, not so much because of the choice-induced-competition improving overall productivity of the education system, but because of the pre-existing higher productivity of private schools in general, and of high-fee private schools in particular.

3 Institutional Setting

3.1 Elementary education system in India

About 200 million children are part of India's elementary education system (grade 1-8). The following contextual details enable better appreciation of the policy/ program being investigated. First, private school enrollment is one of the highest in the developing world at 35 percent (The World Bank, 2016). The national average masks variation in levels of private school enrollment across states and between urban and rural areas. For instance, 49 percent of children in urban India are enrolled at private schools (Kingdon, 2017) and five Indian states have private school enrollment rates of greater than 50 percent (Pratham Foundation, 2014). 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). This can be attributed to the general perception that private schools are of superior quality than government schools (Juneja, 2014). There is, however, no rigorous evidence supporting this perception. Survey data suggests that parents prefer private schools for the following reasons: better learning environments in private schools, unsatisfactory quality of education in government schools, and absence of English medium instruction in government schools (Saha, 2016).

Second, private schools serve not just the wealthy and the middle class, but also a section of the poor. The private school fees distribution in India is log-normal with a pronounced rightward skew; the highest fee is around

Indian Rupees 200,000 (USD 3,100), 40 times the median fee of Indian Rupees 5,000 (USD 77). Further, the mean private school fee is only about 9.2 percent of the per capita GDP, and about 26 percent of rural private schools have a monthly fee that is below the daily minimum wage (Kingdon, 2017).

3.2 The policy: 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 grades for students from disadvantaged groups: both social and economic. Social disadvantage is based on caste: Schedules Castes (SC), Scheduled Tribes (ST), and some backward castes are eligible. Economic disadvantage is means tested and state governments are to set income eligibility cutoffs. Families with income below the defined cutoff become eligible for the program. Government reimburses the tuition fee to private schools[3], thus subsidizing the education of program beneficiary children.

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 vibrant private sector providing low- fee schooling, poverty constrained the exercise of that choice (Härmä, 2011). The mandate, hence, makes school choice a reality for the poor by relaxing the binding income constraint.

[3]The fee reimbursement to private schools is at a rate that is the lower of the actual amount charged from non-poor children (75 percent) by the school, or the per child expenditure incurred in government schools (Ministry of Human Resource Development, 2010). The typical reimbursement rate is around the median private school fee. So, private schools that charge above the median fee incur a revenue loss through participation in the program. Further, government regulation makes it difficult for schools to increase or decrease the number of places. Hence high fee schools (loosely, those charging above the median fee) have opposed the mandate.

Choice introduced through the mandate is a unique policy intervention and should be differentiated from 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 was 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 disadvantaged groups is bigger than the seats available. Kingdon (2017, p. 31)estimates that number of free places is less than 10 percentage 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, 2013, p. 15); second, to provide better quality education for children from disadvantaged backgrounds. Educationalists and 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.

3.3 Implementation of the mandate

Implementation of RTE 25 percent mandate has become a contentious issue amongst education policy makers and private schools[4]. Front-runner states in implementing the mandate like Karnataka, Rajasthan, and Madhya Pradesh argue that the mandate provides a unique opportunity to deliver 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.

On the other hand, states like Andhra Pradesh, and Telengana are not implementing the mandate despite the legal requirement. 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, and 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 school choice. [5]

3.4 Theory of change

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

- There is a near universal preference for private schooling (Härmä, 2011).

[4]High-fee 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.

[5]Further, state governments that invested aggressively in the government school system over the last decade through infrastructure up-gradation, teacher appointment, and training are concerned that it could lead to a dual unsustainable financial burden: paying high salaries to government teachers who cannot be retrenched and paying private schools for 25 percent places.

So, the poor attend public schools not by choice, but because of their inability to pay for private education.

- The 25 percent mandate, by targeting 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.
- 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 (high-fees private)[6] educational environments, and thus improve their learning outcomes. The two underlying assumptions of this theory of change are that of a private school learning advantage (private schools produce better learning than government schools), and the high-fee school learning advantage (high-fees leads to higher quality learning). A detailed pictorial description of the theory of change underlying the mandate is at Appendix A.

3.5 Existing research on the mandate

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 at free places. Academics and practitioners have written on the administrative mechanisms in the implementation of the mandate (Indian Institute of Management, 2013, 2016), the institutional

[6] Srivastava (2008) defines low-fee private schools as those with monthly fee less than the daily minimum wage. However, there is no agreed upon definition in the literature on what constitutes a high-fee school. For the analysis in this thesis, I define all schools charging more than the median-school fee as high-fee private schools.

and ideological debate surrounding it (Srivastava and Noronha, 2014; Juneja, 2014), and on its early implementation (Noronha and Srivastava, 2013).

There have been calls to evaluate the impact of the policy on children's outcomes. As pointed in the State of the Nation Report on RTE (Indian Institute of Management, 2016, p. 13), "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, p. 1063) 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, 2015). Further, Kaufman and Rosenbaum (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. In a similar vien, Cullen et al. (2006) argue about the importance of match-quality and argue that what is good for one child, might not necessarily be good for another. Both these arguments are very relevant 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.

4 Research design

4.1 Experiment

Demand for RTE free places is much higher than supply, necessitating random allocation of places through school lotteries. The RTE act itself provides for the lottery based allocation deeming it a fair way of filling places in cases of over subscription. These lotteries divide the policy participants into two groups: a treated group that has been given the policy benefit and a control group that hasn't. The control group of lottery losers creates the perfect counterfactual and simulates the behavior of policy participates in the absence of the policy. Comparing the outcomes of treatments and controls should, therefore, provided unbiased evidence of the impact of expanded school choice on childrens' outcomes.

4.2 Setting

My research is located in Karnataka, a large south Indian state.[7] The government is enthusiastic about the potential of the mandate to improve learning outcomes and is hence investing significant administrative effort into its implementation. The state started fulfilling 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 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 Economically Weaker Sections (EWS). Thus 64 percent of the free places are blocked for income-disadvantaged, while the rest are for the socially disadvantaged. Eligibility determination is based

[7] Karnataka has a population of 60 million, with 8 million children in the elementary education age.

on certificates issued by grassroots functionaries- a caste certificate for the socially disadvantaged groups, and an income certificate for the the economically weaker sections. Procedures for ascertaining caste are well established in this context, while those for income determination are vague. The income cutoff for being eligible is INR 100,000 (USD 1,546).[8]

For the first three years of it's implementation, 2012-15, RTE free places were filled through a decentralized 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 this new system, parents access the online application, upload their eligibility documents, choose their neighborhood, and select up to five schools giving a preference order. Hence, all parents in a neighborhood have the same feasible choice set of schools.

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

[8]The state average per capita income is INR 126,976.

June, the admissions process starts in February/ March and is completed by the end of April.

4.3 Lottery

Karnataka's RTE admissions lottery algorithm is based on the random serial dictatorship mechanism (Abdulkadiroğlu and Sönmez, 1998, 2003). A single central lottery is conducted on all the eligible applications and admission offers are made as follows:

- Each applicant is randomly assigned a unique 15 character alphanumeric code.
- These randomly generated codes are arranged in ascending order thus giving a numerical rank to each applicant.
- 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, EWS) combination. A match leads to an admission offer and the applicant becomes a lottery winner; failure to find a match means the applicant becomes a lottery loser.

The lottery winning probability depends on the eligibility category (SC, ST or EWS), and the choice profile (the schools that the parents put down in the application and the preference/ rank order). Combinations of eligibility category and choice profile constitute a randomization stratum. Within randomization stratum, treatment (offer of a free place) is random. Hence the lottery losers within each eligibility category and choice profile group provide a clean counterfactual for the lottery winners i.e., the ex-ante probability of admission is identical for applicants within a randomization stratum.

4.4 Study design

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

Two types of applicants get eliminated in this matching scheme: every applicant 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.

4.5 Study cohort and sampling strategy

The sample for this study is drawn from the cohort of children who applied for admission to grade I[9] in February/ March 2015 and started school 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 grade I.[10] Hence, I only collected endline data on which this analysis is based. Table 1 (panel A) provides a summary of the 2015-16 grade I admission database for Karnataka.[11] In total 126,728 children applied for admission into grade I, of which about 62,046 (49 percent) got an offer of admission.

Karnataka state is divided into four board administrative regions, and 34 districts with significant differences in the education markets (level of private

[9] The age of entry into grade I is typically 6 years.

[10] The school year in Karnataka is from June to March.

[11] RTE admissions happen at entry grades, which in Karnataka are defined as Lower Kindergarten and grade I. I study only grade I applicants, given the difficulty with testing children in Kindergarten.

school activity and government school effectiveness), demographics, and the number of school admissions across divisions and districts. For instance, Bangalore Urban district (one part of the capital city) alone accounts for 14 percent of all the applications. To explore possible heterogeneities in the targeting of the program across these administrative units, I chose the sample from four districts, with two of them belonging to the same region: Bangalore Urban and Bangalore Rural[12] districts from the more developed southern region of the state, and Bellary and Gulbarga districts from the underdeveloped Northern Karnataka. Figure 1 is a map of Karnataka with the sample districts identified.

[12] Bangalore Rural comprises suburbs of Bangalore city, Karnataka's capital, and is predominantly an urban area. The rural tag is purely administrative; the district is rural only relative to the heavily urban Bangalore city.

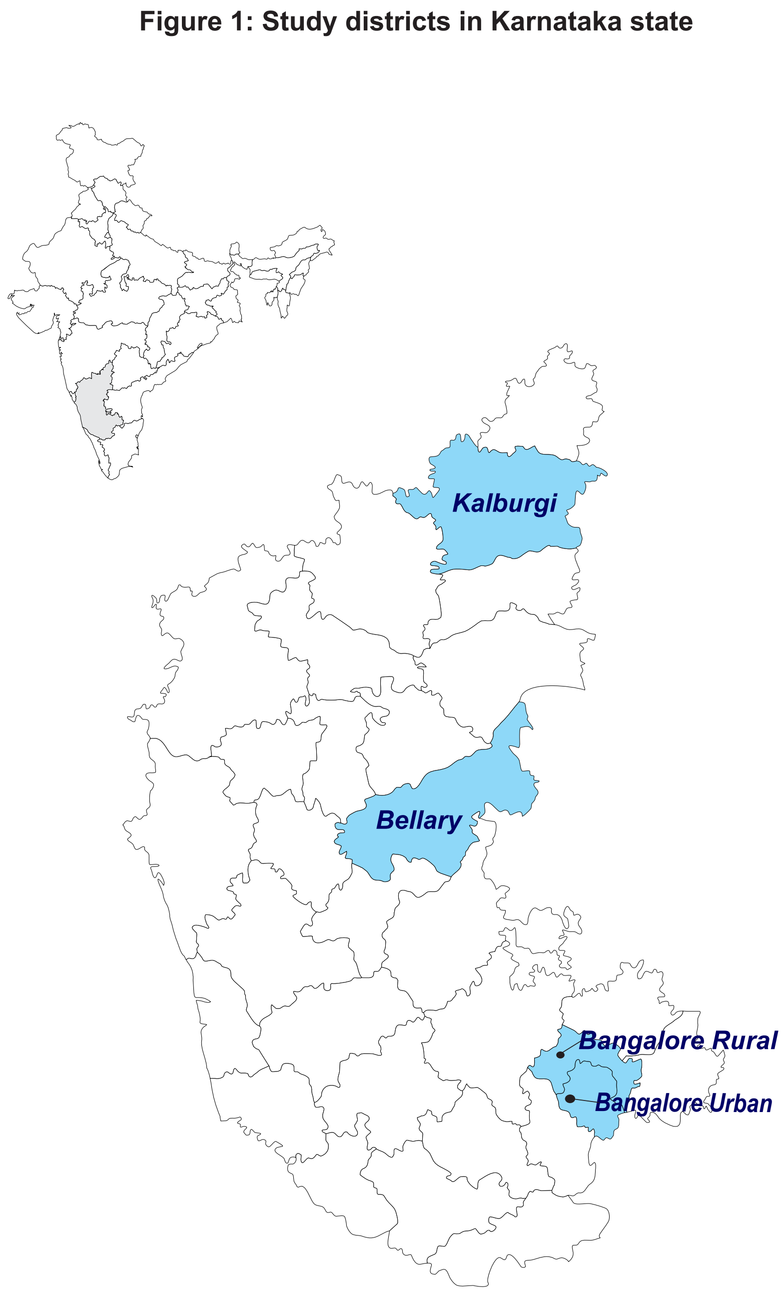


Figure 1: Map of Karnataka with the sample districts

Table 1 (panel A) 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 sample districts account for 28 percent of the total applications received and 25 percent of all the matched pairs in the state. Panel B presents the proportion of total applicants that claimed social and income disadvantage for participation in the mandate. The proportion of income disadvantage claimants for the whole state is around 69 percent, while that in the sample districts is 65 percent.

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 is distributed equally across all four districts: 400 children/ 200 matched pairs per district.

Table 1: **Summary of 2015-16 class I applications**

Panel A: District-wise applicants (treated and matched)					
	Applicants	Percentage of total	Treatment	Percent 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

Panel B: District -wise applicants by eligibility criteria				
	Applicants	Socially disadvantaged	Income disadvantaged	Percent of income disadvantaged
All 34 districts	126,728	39,617	87,111	68.7
SAMPLE DISTRICTS				
Bangalore Rural	2,893	820	2,073	71.7
Bangalore Urban	17,362	5,760	11,602	66.8
Bellary	9,082	3,616	5,466	60.2
Gulbarga	6,538	2,217	4,321	66.1
Total	35,875	12,413	23,462	65.4

4.6 Study timeline

Figure 2 below presents the study timeline. As mentioned earlier, this research was conceived in early 2016, almost an year after the RTE lottery in April 2015. Finalizing the design and sampling were done in June- July 2016, after which data collection was done from September- December 2016. The final dataset has 1,616 children/ 808 matched treatment- control pairs.

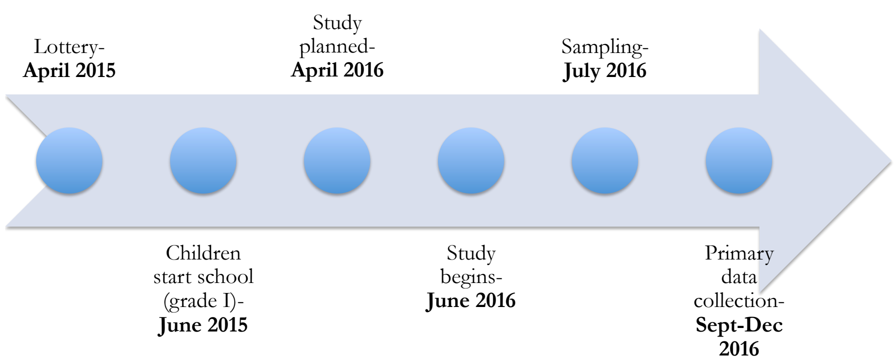


Figure 2: Study timeline

5 Data

5.1 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 based on the endline data. Table 2 establishes the validity of the design by presenting treatment- control balance on several observable child and household characteristics. Most of these variables are time-invariant or couldn't have been impacted by treatment.

Table 2: **Validity of the design**

	Treatment Mean	Control Mean	Difference	p-value
	(1)	(2)	(3)	(4)
Age (in years)	7.33	7.32	0.01	0.62
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	30.16	30.35	-0.19	0.32
Mother's education (in years)	8.26	8.28	-0.02	0.86
Working mother	0.22	0.24	-0.02	0.34
Father's age	36.69	37.01	-0.32	0.17
Father's education (in years)	8.67	8.62	0.05	0.77
Working father	0.94	0.95	-0.01	0.36
Birth order	1.59	1.63	-0.04	0.23
Number of siblings	1.07	1.09	-0.02	0.61
Attended pre-primary school	0.9	0.91	-0.01	0.65
Asset index	8.92	8.81	0.11	0.52
House ownership	0.5	0.53	-0.03	0.15
Number of rooms in the house	3.01	3	0.01	0.77
N	808	808	1616	

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Table presents the treatment and control means on a range of variables. Column (3) is the difference between means, and column (4) is the p- value on the treatment indicator with the balance variable regressed on treatment and sub-district dummies, and with standard errors clustered at the pair level. The p-value for an F-test of joint significance of all the balance variables when regressed on treatment along with sub-district dummies is 0.80.

5.2 Attrition

Though I only collect one round of data, there is attrition in the sample as all the children in the initially randomized matched pairs couldn't be located. This happened because I recruited participants for the study post-randomization. After randomly sampling 200 pairs per district from the sampling frame of matched pairs, the survey team tried to trace the applicant's parents using their telephone numbers in the admissions database. However, a significant proportion of telephone numbers had become unavailable/ non-functional in the 18-month gap between RTE application time (February 2015) and the sampling time (July/August 2016).[13] To arrive at the final study sample, we moved down the randomized sampling sequence[14] and replaced unavailable pairs with pairs lowered down in the sequence. The survey team called/ contacted 2,942 applicants (1471 matched pairs) to finalize the study sample of 1,616 children (808 matched pairs).

Table 3 presents district-wise data on the contacted and the study sample numbers. Of the 2,942 children that were contacted, 1,616 (55 percent) are part of the final sample and are surveyed; 856 (29.1 percent) could not be contacted over telephone, and I refer to these observations as 'missing.' I dropped another 470 children (16 percent) despite their willingness to participate as their partner in the matched pair was missing. Given the pairwise matching design and the planned use of pair fixed effects in the analysis, collection of data on lone member in a pair would have been a wastage of resources. I refer to these observations as 'dropped.'

The shift from the contacted sample of 2,942 children to the study sample of 1,616 children constitutes attrition: missing and dropped observations are the attritors. The attrition rate, however, is not differential on treatment

[13] It is not uncommon for Indians to change mobile phone connections in response to continuously changing tariff plans.

[14] I generated a random sampling sequence of matched pairs in each district (using Stata's seed and runiform commands), and then selected the first 200 pairs to be part of the sample. Moving down the sampling sequence involved going beyond the first 200 pairs.

Table 3: **Sample construction**

DISTRICT	Contacted sample	Study sample	Missing	Dropped
Bangalore Rural	698	400	185	113
Bangalore Urban	699	398	203	98
Bellary	695	420	202	73
Gulbarga	850	398	266	186
Total	2,942	1616	856	470
Percent (of 2,942)		54.9%	29.1%	16.0%

as both the contacted and the study samples are balanced.To check for differential attrition, I regressed an indicator for being in the study sample on all available covariates and covariates interacted with treatment. I am unable to reject the null of joint significance on an F-test for this regression. Further, I separately regressed indicators for 'missing' and 'dropped' on treatment. Table 4 presents the results that show a 3 percent point statistically significant difference in attrition rate between treatment and control. However, there is no difference in the observable characteristics between attritors across treatment and control. Finally, table 5 shows the results for three sub-samples: missing observations, dropped observations, and for both missing and dropped observations. These results, demonstrating the balance of attrition on all observable characteristics both individually and jointly for the attritors, read together with the strong treatment control balance shown in table 2, make it unlikely that the study sample is imbalanced on unobservables that affect outcomes (Muralidharan and Sundararaman (2015, p. 1030) make a similar argument).

Table 4: **Is attrition random?**

	(1)	(2)	(3)	(4)
	Missing indicator		Dropped indicator	
Treatment	-0.03**	-0.03**	0.03**	0.03**
	(0.02)	(0.02)	(0.01)	(0.01)
Age (in years)		-0.01		-0.02
		(0.01)		(0.01)
Gender- female		-0.01		0.03**
		(0.02)		(0.01)
Schools applied to		-0.01		0.00
		(0.01)		(0.01)
Caste-Scheduled Caste		0.04		-0.02
		(0.03)		(0.02)
Caste-Scheduled Tribe		0.03		-0.05***
		(0.02)		(0.02)
Bangalore Urban		0.04*		0.06***
		(0.02)		(0.02)
Bellary		-0.02		0.00
		(0.02)		(0.02)
Gulbarga		-0.03		0.01
		(0.05)		(0.04)
Constant	0.31***	0.38***	0.14***	0.24***
	(0.01)	(0.10)	(0.01)	(0.07)
Observations	2,942	2,940	2,942	2,940
F-stat (without district dummies)		1.315		2.255
F-stat (model)		1.308		5.915

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Dependent variable is an indicator for missing (in columns 1 and 2) and an indicator for dropped (columns 3 and 4). The coefficient on the treatment indicator shows there is a 3 percent point difference in attrition rate between treatment and control.

Table 5: **Differences in observable characteristics between attritors**

	(1)	(2)	(3)
Sample	Missing only	Dropped only	Both missing and dropped
		Treatment indicator	
Age (in years)	0.004	-0.050	-0.013
	(0.023)	(0.037)	(0.020)
Gender- female	-0.029	0.004	-0.011
	(0.035)	(0.047)	(0.028)
Schools applied to	-0.006	0.007	-0.001
	(0.015)	(0.021)	(0.012)
Caste-Scheduled Caste	0.064	-0.088	0.002
	(0.045)	(0.058)	(0.036)
Caste-Scheduled Tribe	-0.059	0.104	0.005
	(0.102)	(0.135)	(0.085)
Bangalore Urban	0.063	-0.113	0.003
	(0.055)	(0.075)	(0.044)
Bellary	-0.010	0.047	-0.003
	(0.052)	(0.075)	(0.043)
Gulbarga	-0.055	0.061	-0.001
	(0.048)	(0.060)	(0.038)
Constant	0.459***	0.895***	0.599***
	(0.164)	(0.265)	(0.141)
Observations	848	471	1,319
F-stat (without district dummies)	0.678	1.099	0.119
F-stat (model)	1.081	1.758	0.0770

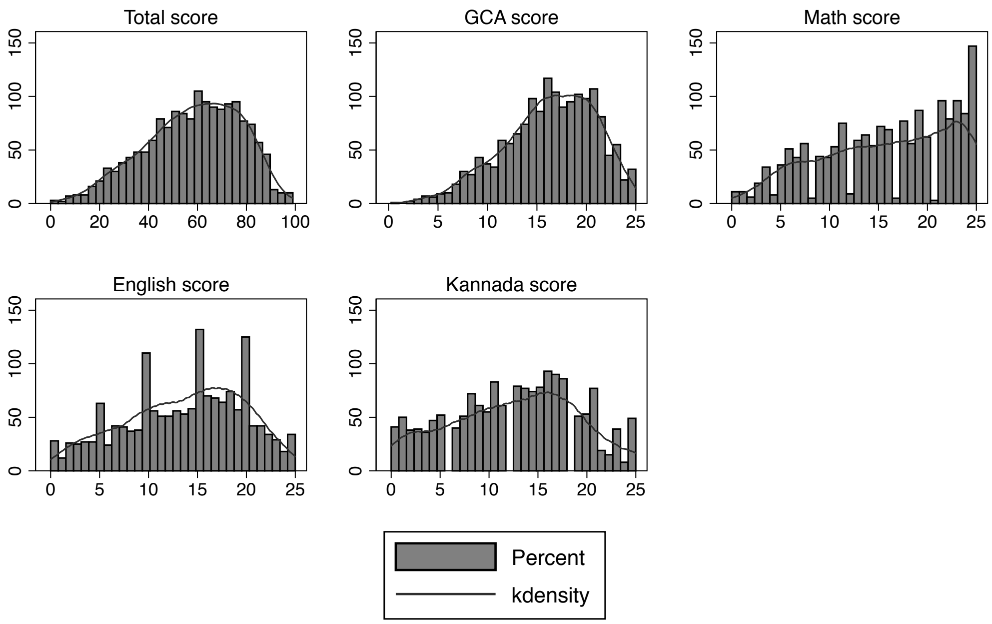
Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Dependent variable in all regressions is an indicator for being treated. Regression in column 1 uses 'missing' observations only, that in column 2 uses 'dropped' observations, and that in column 3 uses both missing and dropped observations. These results show there is no difference in observable characteristics between attritors.

5.3 Outcome Variables

I collected data on two sets of outcomes for all the sample children: test scores and attitudinal (psychosocial) outcomes. Test score data was collected through administering an age appropriate 90-item test that assesses students' 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.[15] All four of my test score measures and the total score measure (unweighted sum of the four competency scores) show considerable variation. Figure 3 shows their distribution.

To further analyze the reliability of the test score measures, I ran a multivariate regression of test scores on a range of family covariates. The coefficient estimates on the child's age, and mother's education are positive and statistically significant; being a Scheduled Caste (SC) or being a Muslim are negative and statistically significant. The results (shown in Appendix) are consistent with the existing literature where educational outcomes are positively correlated with age and mother's education and negatively correlated with belonging to socially disadvantaged groups, in this case, being SC or Muslim (Basant, 2007; Magnuson, 2003). Table 6 presents a summary of the raw test scores.

[15] The testing tool was developed in collaboration with the Center for Early Childhood Development (CECED), New Delhi, and the education department of the Government of Karnataka. The tool is based on the School Readiness Instrument (SRI), developed and validated by the World Bank.



Notes:
1.Total score is the sum of the individual scores
2.GCA- General Cognitive Ability
3.N= 1616

Figure 3: Histogram of test scores

Table 6: **Summary of 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				

In addition to test scores, I also collected data on attitudinal items that capture the child's subjective schooling experience (school experience, interaction with peers, and interactions with teachers) and her sense of self-efficacy. The motivation for collecting data on non-cognitive outcomes was two fold. First, a strand of the school choice literature labeled the 'relative deprivation hypothesis' (Kaufman and Rosenbaum, 1992; Cullen et al., 2006), posits that admitting poor children into schools where they might feel out of place (due to socio-economic reasons) would put them at a competitive disadvantage. Second, in the context of the RTE 25 percent mandate, there is sizable anecdotal evidence and widely accepted popular narrative that children admitted to RTE free places in elite private schools are not well-integrated into their new learning environments. Measuring children's responses to attitudinal/ psychosocial items would shed light on whether the relative deprivation hypothesis is in operation in this context.

I developed a psychosocial test based on the instrument used in the Young Lives study (Singh, 2015). My test has 16- binary-response statements that enable me to asses the child on four psychosocial attributes: self-efficacy, peer support, school support, and teacher support. Out of 16 statements, only 9 are positively phrased. I recode the yes-no responses to each statement in a way that a one always indicates a better outcome than a zero. Based on this recoded values, I construct four summary indices - self-efficacy index, peer support index, school support index, and teacher support index- using two methods. First, I use simple/ naïve aggregation by giving equal weight to each statement. In this method, each index ranges from 0-4 with 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 7 presents the summary of the psychosocial measures using both the simple/ naïve aggregation and the inverse covariance weighting approach. The indices constructed using the latter approach are used in this analysis.

Table 7: **Summary of attitudinal/psychosocial outcomes**

Index	Mean	Std.Dev.	Min	Max
Panel A-Index construction: simple aggregation (naive approach)				
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				
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

There could be two potential concerns about my outcome variables. First, the average age of the children in my sample is 7.3 years, and one might worry about the reliability of outcomes measured at such a young age. However, evidence shows that test scores measured even at age 7 are significant predictors of both future test scores and labor market outcomes (Currie and Thomas, 1999). Further, the two recent and most cited studies in this literature measure outcomes on children of comparable age groups. While Singh (2015) younger cohort is tested at ages 5, 8 and 9, Muralidharan and Sundararaman (2015) tests their study sample at ages 8 and 10 respectively. The second concern is that the outcomes are measured after only 1.5 years post-treatment. It can be questioned if 1.5 years is sufficient time for school effects to manifest. Here again, (Muralidharan and Sundararaman, 2015) measure affects both at 2 years and at 4 years post- treatment. Their results broadly remain unchanged between the two test rounds, suggesting that results after 1.5 years of being exposed to treatment are good indicators of the treatment effect.

5.4 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

6 Results

6.1 Estimation model

I estimate the impact of being offered an RTE free place (i.e., the impact of the policy) on the learning and psychosocial outcomes of children using the following model.

$$Y_{id} = \beta_0 + \beta_1(T_i) + \boldsymbol{\beta}_{Di}\mathbf{D}_i + \epsilon_{id} \tag{1}$$

Where Y_{id} is the outcome variable for child i in in sub-district d[16]. T_i is a binary treatment variable taking a value of 1 if child i is offered an RTE free place i.e., if the child is a lottery winner. I use sub-district dummies ($\mathbf{D}_i$) to absorb geographic variation and to improve precision. Though use of pair dummies is recommended, given the randomization (Bruhn and McKenzie, 2009), equation 1 doesn't have either pair dummies or pair fixed effects because I lose observations with this approach (especially while estimating heterogeneous effects).[17] However, given the strongly balanced panel, clustering the standard errors at the pair level gives the same results as using pair fixed effects. So, equation 1 is my preferred estimation model. I

[16] The sample is drawn from 4 districts and 28 sub-districts, referred to as blocks in India

[17] Pair fixed effects only uses within pair variation, so a lot of pairs become unusable for the estimation of heterogeneity.

will, however, demonstrate the robustness of the main results to the inclusion of controls[18] and pair fixed effects.

As treatment is randomly assigned, β_1 is an unbiased Intent To Treat (ITT) estimate of the impact of being offered a free place for children whose parents chose to apply to 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. 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 at 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 (Duflo et al., 2007; Deming et al., 2009). The analysis in this chapter will focus on the ITT estimates, the policy impacts.

6.2 Compliance with treatment

Like in most experimental studies, there is a modest non- compliance with treatment in this study. Compliance is defined as enrollment in a free place if offered admission, and not being enrolled in a free place if not offered admission. Non-compliance occurs if lottery winners don't enroll at RTE free places (never -takers), or if lottery losers are enrolled at free places (always-takers). Table 8 provides details of the compliance amongst treatments and

[18] I use the following controls and check for robustness of the reults : age of the child, gender of the child, indicator for belonging to Scheduled Caste, indicator for being Muslim, mother's education (in years), indicator for a working mother, and two measures of the economic status of the household, a household asset index measure and the number of rooms in the house.

controls. That 18.2 percent of the sample are always-takers is puzzling as only the lottery winners should be able to enroll in the free places. Lottery losers' enrollment into a 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.[19]

The compliance rate for the study, calculated as the difference between the compliance rate of the treatments (89.5) and the percent of always-takers amongst the controls (18.2) is 71.3 percent. This is a reasonably high value. Further, non-compliance and the presence of always-takers and never-takers neither affects the estimation of the ITT nor introduces any bias in the results.

Table 8: **Compliance with treatment assignment**

	(1) Total	(2) Enrolled at free place	(3) Not-enrolled at free place	(4) Compliance rate
Treatment	808	723	85	89.50
Control	808	147	661	18.20
	1616	870	746	

Notes: The 85 treated children who didn't enroll at a free place are the never-takers, while the 147 control children who did enroll at a free place are the always-takers. Compliance rate for the study (71.3 percent) is the difference between the compliance rate amongst the treated (89.50 percent) and the non-compliance rate amongst the control children (18.2 percent).

6.3 Policy impact on childrens' outcomes

Table 9 presents the ITT impact estimates for test scores. The dependent variable, test scores, is converted into standardized z-scores, hence the coefficient

[19] Given the setup of the admissions process and the lottery algorithm (where parents are allowed only up to five preferences), it is possible that in some neighborhoods there are lottery losers alongside non-allotted free places (in some schools). This creates an opportunity for grassroots functionaries to assign these free slots to the lottery losers.

estimate should be interpreted as the effect size.The odd numbered columns show the results of the estimation model at equation (1), my preferred specification. The even numbered columns show the results of the multivariate regression model, which includes sub-district dummies and covariates. In both the models, the standard errors are clustered at the pair level to account for the pairwise randomization design.

None of the treatment effects is significant, demonstrating that the offer of an RTE free place does not have a statistically significant impact on learning outcomes measured across a range of subjects. The sign of the coefficient, however, is positive across all measures. Further, age, being Muslim, being Scheduled Caste, and mother's education variables are all statistically significant and have the expected signs.

Table 9: **Impact of policy on test scores (ITT estimates)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables	Total		GCA#		Math		English		Kannada	
Treatment	0.038	0.027	0.013	0.009	0.041	0.036	0.030	0.017	0.033	0.025
	(0.044)	(0.042)	(0.045)	(0.043)	(0.044)	(0.044)	(0.044)	(0.043)	(0.042)	(0.041)
Age (in years)		0.294***		0.324***		0.281***		0.216***		0.154***
		(0.035)		(0.035)		(0.036)		(0.034)		(0.033)
Gender (boy==1)		-0.070		0.085*		0.010		-0.117***		-0.147***
		(0.044)		(0.046)		(0.046)		(0.045)		(0.044)
Scheduled Castes		-0.365***		-0.214***		-0.260***		-0.323***		-0.303***
		(0.068)		(0.067)		(0.065)		(0.068)		(0.064)
Muslim		-0.451***		-0.328***		-0.253***		-0.168***		-0.698***
		(0.060)		(0.061)		(0.062)		(0.059)		(0.062)
Mother's education		0.032***		0.025***		0.022***		0.053***		0.010
		(0.006)		(0.006)		(0.006)		(0.006)		(0.006)
Working mother		0.001		-0.006		0.026		-0.092		0.104*
		(0.059)		(0.057)		(0.061)		(0.058)		(0.056)
Asset index		0.008		0.009		0.001		0.017**		-0.002
		(0.007)		(0.007)		(0.007)		(0.007)		(0.007)
House size		0.007		-0.012		0.003		0.033		0.004
		(0.021)		(0.022)		(0.022)		(0.022)		(0.021)
Constant	0.045	-2.151***	-0.477***	-2.920***	0.294**	-1.790***	-0.245	-2.132***	0.389***	-0.585**
	(0.150)	(0.321)	(0.125)	(0.301)	(0.135)	(0.319)	(0.205)	(0.335)	(0.120)	(0.283)
Observations	1,616	1,616	1,614	1,614	1,614	1,614	1,614	1,614	1,614	1,614
R-squared	0.104	0.202	0.106	0.185	0.096	0.155	0.078	0.173	0.152	0.253

#-GCA is General Cognitive Ability

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Regression in columns 1, 3, 5, 7, and 9 are models without covariates, regressing outcome variable on treatment dummy and sub-district dummies. Regressions in columns 2, 4, 6, 8, and 10 have sub-district dummies and controls. Standard errors are clustered at the pair level in all models to account for pairwise matching design. The dependent variables, test scores, are standardized z-scores; hence, the coefficient estimate is the effect size. Gender, Scheduled Caste, Muslim and working mother are indicator variables. Age, asset index, mother's education (in years) and House size (number of rooms in the house) are continuous.

Table 10 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 0.117 (p-value: 0.017) in column 2 indicates that winning the lottery increases the sense of self-efficacy by about 0.12 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.

Table 10: **Impact of policy on psychosocial outcomes (ITT estimates)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	Self-efficacy		Peer support		School experience		Teacher support	
Treatment	0.117**	0.113**	0.029	0.030	0.033	0.031	-0.028	-0.027
	(0.048)	(0.049)	(0.045)	(0.045)	(0.048)	(0.047)	(0.045)	(0.045)
Age (in years)		0.093**		0.018		0.019		0.048
		(0.038)		(0.038)		(0.037)		(0.035)
Gender (boy==1)		-0.075		0.024		0.016		-0.006
		(0.051)		(0.051)		(0.049)		(0.047)
Scheduled caste		-0.133*		-0.107		0.070		-0.148**
		(0.073)		(0.070)		(0.059)		(0.066)
Muslim		-0.255***		0.067		0.022		0.086
		(0.063)		(0.063)		(0.058)		(0.061)
Mother's education		0.015**		0.000		-0.002		0.011*
		(0.006)		(0.006)		(0.006)		(0.006)
Working mother		-0.022		-0.041		-0.101		-0.003
		(0.066)		(0.065)		(0.062)		(0.060)
Asset index		-0.006		-0.008		-0.002		-0.015**
		(0.008)		(0.008)		(0.008)		(0.007)
House size		-0.003		-0.008		-0.022		0.018
		(0.025)		(0.024)		(0.028)		(0.022)
Constant	-0.163	-0.790**	-0.061	-0.118	-0.709***	-0.762**	-0.699***	-1.069***
	(0.133)	(0.311)	(0.111)	(0.307)	(0.231)	(0.324)	(0.223)	(0.330)
Observations	1,553	1,553	1,577	1,577	1,585	1,585	1,557	1,557
R-squared	0.052	0.071	0.115	0.120	0.153	0.156	0.168	0.178

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Regression in columns 1, 3, 5, 7, and 9 are models without covariates, regressing outcome variable on treatment dummy and sub-district dummies. Regressions in columns 2, 4, 6, 8, and 10 have sub-district dummies and controls. Standard errors are clustered at the pair level in all models to account for pairwise matching design.. The dependent variables, test scores, are standardized z-scores; hence, the coefficient estimate is the effect size. Gender, Scheduled Caste, Muslim and working mother are indicator variables. Age, asset index, mother's education (in years) and number of rooms in the house are continuous.

6.4 Heterogeneous effects

In this section, I investigate if the impact of policy on childrens' outcomes are heterogeneous across four-child characteristics: gender, religion, caste, and policy eligibility category (income disadvantaged versus socially disadvantaged). The choice of the characteristics is motivated both by existing literature and the specific context of the policy.

- Gender: There is large literature demonstrating differential household allocation of resources by gender (White et al., 2016; Zimmermann, 2012).
- Religion: Muslims compromise one of the most economically backward groups in India (Muralidharan and Sundararaman, 2015); hence, I investigate the impact of the policy on Muslims vis-à-vis non-Muslims.
- Caste: I check the impact of policy on Scheduled Castes (again, one of the most disadvantaged communities in India) vis-à-vis the non-Scheduled Castes.
- Eligibility criteria: As explained before, policy beneficiaries are either socially or income-disadvanatged. Most social programs target the socially- disadvantaged; the RTE mandate is one of the first large social policies that explicitly supports the income-disadvanatged, hence making eligibility criteria an interesting dimension to explore heterogeneity.

I estimate heterogeneous effects 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.

$$Y_{id} = \beta_0 + \beta_1(T_i) + \beta_2(T_i * CC_i) + \boldsymbol{\beta}_{Di}\mathbf{D}_i + \epsilon_{id} \tag{2}$$

The results are presented in table 11 for both test scores and psychosocial outcomes. 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 student identified by the covariate and others. The results are positive and statistically significant on one measure for girls (GCA score effects, p-value: 0.038) and negative and significant on two measures for Muslims (Kannada score (p-value: 0.059) and peer support index (p-value: 0.003)).

Table 11: **Heterogeneous impacts by student characteristics (ITT estimates)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Test scores					Psychosocial outcomes			
	Total	GCA	Math	English	Kannada	Self efficacy	Peer support	School experience	Teacher support
Treatment*covariate									
Girl	0.076	0.169*	0.052	0.061	-0.011	0.060	0.083	-0.120	0.062
	(0.099)	(0.098)	(0.098)	(0.101)	(0.094)	(0.100)	(0.097)	(0.094)	(0.095)
Muslim	-0.162	-0.147	-0.111	-0.104	-0.173*	-0.028	-0.290***	-0.018	-0.106
	(0.108)	(0.111)	(0.107)	(0.111)	(0.103)	(0.121)	(0.101)	(0.107)	(0.103)
Scheduled Caste(SC)	0.082	0.097	0.141	0.039	-0.003	-0.029	0.180	0.085	-0.117
	(0.121)	(0.123)	(0.120)	(0.122)	(0.115)	(0.130)	(0.125)	(0.112)	(0.130)
Income disadvantaged	-0.053	-0.098	-0.132	-0.058	0.101	-0.045	-0.064	0.033	0.156
	(0.103)	(0.103)	(0.103)	(0.102)	(0.097)	(0.105)	(0.101)	(0.105)	(0.105)
Observations	1,616	1,614	1,614	1,614	1,614	1,553	1,577	1,585	1,557

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Each cell is a separate regression, the point estimate and the standard error on the interaction term are reported here. All regressions include subdistrict dummies; standard errors are clustered at the pair level

To further unpack these differential effects and to understand the treatment effects for different sub-groups, I estimate equation (3), the results of which are presented in table 12. It has four panels of two rows each with every panel showing the result for one child characteristic CC. For instance, the first two rows present the effects for child characteristic gender, with CC being girl: the first row shows the coefficients γ_2 and and the second row γ_3 .

$$
\begin{aligned}
Y_{id} = \gamma_0 + \gamma_1(CC_i) + \gamma_2(CC_i * T_i) + \gamma_3((1 - CC_i) * T_i)) \\
+\boldsymbol{\gamma}_{Di}\mathbf{D}_i + \epsilon_{id}
\end{aligned}
\tag{3}
$$

On test scores, the treatment effects are not significant for any sub-group. On psychosocial effects, the self-efficacy index is positive and the statistically significant for girls, non- Muslims, and non-SCs. This suggests that the positive treatment effect on self-efficacy for the full sample (table 10 is driven by these three groups. The differences between Muslim and non-Muslim children on the peer support index are also striking, though they don't remain significant after correcting for multiple hypothesis testing.

Table 12: **Sub-group impacts by student characteristics(ITT estimates)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Total score					Psychosocial outcomes			
	Total	GCA	Math	English	Kannada	Self efficacy	Peer support	School experience	Teacher support
Treatment*covariate									
Girl	0.079	0.114	0.072	0.060	0.021	0.149**	0.078	-0.036	0.008
	(0.070)	(0.072)	(0.069)	(0.071)	(0.068)	(0.075)	(0.077)	(0.074)	(0.069)
Boy	0.003	-0.055	0.019	-0.000	0.032	0.089	-0.004	0.084	-0.054
	(0.062)	(0.061)	(0.063)	(0.063)	(0.059)	(0.064)	(0.056)	(0.060)	(0.062)
Muslim	-0.087	-0.100	-0.044	-0.049	-0.100	0.095	-0.191**	0.019	-0.109
	(0.094)	(0.096)	(0.092)	(0.097)	(0.090)	(0.104)	(0.087)	(0.092)	(0.089)
Non-Muslim	0.075	0.048	0.067	0.054	0.073	0.123**	0.099*	0.037	-0.002
	(0.050)	(0.051)	(0.052)	(0.050)	(0.047)	(0.057)	(0.052)	(0.056)	(0.052)
Scheduled Caste(SC)	0.103	0.092	0.155	0.060	0.030	0.093	0.176	0.102	-0.123
	(0.110)	(0.111)	(0.107)	(0.110)	(0.104)	(0.119)	(0.114)	(0.100)	(0.119)
Non-SC	0.022	-0.005	0.015	0.021	0.033	0.122**	-0.004	0.017	-0.006
	(0.048)	(0.049)	(0.050)	(0.049)	(0.047)	(0.053)	(0.049)	(0.054)	(0.048)
Income disadvantaged	0.023	-0.015	0.003	0.014	0.063	0.103*	0.010	0.043	0.020
	(0.051)	(0.052)	(0.053)	(0.052)	(0.050)	(0.058)	(0.054)	(0.057)	(0.052)
Socially disadvantaged	0.076	0.083	0.135	0.071	-0.038	0.149*	0.074	0.009	-0.136
	(0.087)	(0.088)	(0.087)	(0.087)	(0.081)	(0.088)	(0.085)	(0.088)	(0.089)
Observations	1,616	1,614	1,614	1,614	1,614	1,553	1,577	1,585	1,557

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$ Each cell is a separate regression, the point estimate and the standard error on the interaction term are reported here. All regressions include subdistrict dummies; standard errors are clustered at the pair level

7 Mechanism Analysis

7.1 Understanding non-impact

The evidence demonstrates that the policy (offer of an RTE free place) didn't have any impact childrens' outcomes. In this section, I explore the various mechanisms driving the null-effect. Given the theory and policy context, there are three potential candidate explanations:

- *General equilibrium effects:* As discussed in the theory section, the 25 percent mandate may have improved the overall productivity of the education system, which led to the null results i.e., the schools attended by both the treated and control children responded to competition and improved their academic effectiveness leading to no difference in test scores between the two groups of children.

- *Household input substitution:* Glewwe and Muralidharan (2015) and Das et al. (2013) argue that null-impacts of education policies could result when households reoptimize their resources in response to the policy. In this context, household re-optimization could occur if, for instance, parents of lottery winning children reduce their investments in private tuition and/or in time spent on learning at home or if lottery losing parents increase their investments.

- *Revisiting the theory of change:* As detailed in section 3.4, 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. A necessary condition for improvement in learning outcomes is that there should be a qualitative change in the learning environments of children: children in the treatment group should be attending qualitatively superior schools than those in the control group.

7.2 General Equilibrium Effects

I contend that the general equilibrium effects argument is not valid here. First, the mandate became fully operation only from 2013 and I measure outcomes in 2016. Three years is hardly enough time for schools to change their production technologies, say by hiring new teachers or changing class sizes. Second, and more fundamentally, the mandate's design doesn't generate the 'competition effect' as theoretically envisaged:

- Public school budgets are not tied to enrollment and teacher salaries continue to be paid by the government. Hence the incentive to improve doesn't exist for government schools.
- High cost private schools incur a revenue loss through participation in the mandate: their fee (paid by 75 percent children) is higher than the government subsidy. They are being compelled by the government to participate in the mandate and have absolutely no inventive to improve outcomes for attracting more free-places applicants.
- Low fee private schools are paid exactly their fee amount for the 25 percent children. They, theoretically, have some incentive to improve performance, assuming they were not at 100 percent capacity prior to the mandate. Even then, however, the cost of improving quality (for everyone) might be higher than the revenue gains from the 25 percent children.[20]

To summarize, the 25 percent mandate doesn't create the 'perform or perish' incentives for any of three broad categories of schools, thus making general equilibrium effects extremely unlikely.

[20] They can't raise the school fee or size as Government regulation allow only for a 10 percent annual increase.

7.3 Household Input Substitution

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, as explained earlier. To investigate this, I estimate treatment-control differences in home inputs to education on five variables-an indicator for attending private tuition, annual private tuition fees, time spend by the child at private tuition, on studying at home, and on playing at home. Table 13 presents the results. 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 0.127. Household substitution of resources, therefore, doesn't seem to be a mechanism driving the results.

Table 13: **Treatment control differences in home inputs to education**

	(1) Treatment Mean	(2) Control Mean	(3) Difference	(4) p-value
Time spent at private tuition	1.15	1.15	0.00	0.91
Time spent on studying at home	2.22	2.27	-0.05	0.15
Time spend on playing at home	3.06	3.08	-0.02	0.75
Attending private tuition (dummy)	0.45	0.42	0.03	0.15
Annual private tuition fees	2220.90	2452.91	232.01**	0.04
N	808	808	1616	

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Table presents the treatment and control means on five measured home inputs to education. Column (3) is the difference between means, and column (4) is the p- value on the treatment indicator with the home-input variable regressed on treatment and sub-district dummies, and with standard errors clustered at the pair level. The p-value for an F-test of joint significance of all the five home-input variables is 0.127. The statistical significance of the private tuition fees variable is driven by 7 outlier observations; the significance disappears when the annual tuition fee data is winsorized at the 0.02 level (high only).

7.4 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 14 presents the policy impact on private school enrollment. The treatment effect on private school enrollment is only 6 percent points i.e., the difference between the private school attendance rate of treatments and controls is 6 percent points. So, if treatment is defined as private school attendance, the compliance rate is a mere 6 percent. Further, the control mean of 0.93 demonstrates that 93 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 14: **Impact of policy on private school enrollment**

	(1)	(2)
	Enrolled at private schools	
Treatment effect	0.06***	0.06***
	(0.01)	(0.01)
Constant/ Control mean	0.93***	0.93***
	(0.01)	(0.07)
Observations	1,616	1,616
R-squared	0.01	0.07
Sub-district dummies	No	Yes
Controls	No	Yes

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Standard errors are clustered at the pair level.

The 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 is section 3.1, there is large heterogeneity in private schooling markets, which is well captured by the school fee measure.[21]

Table 15 provides a snapshot of the variation in the private school markets both within and across sample districts. As can be seen, private schooling is available for as little as INR 4,000 at the 10th percentile (p10) of the fees distribution, and can go up to INR 32, 761 at p90. Further the highest fee is about 10-25 times the p90 value. All this indicates that there is a substantial proportion of low-fee schools that even the poor can probably afford.[22] Therefore, despite the majority of the sample children being default private

[21] 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.

[22] Low-fee school in Karnataka context can be defined as schools that charge fee around INR 5,000 or less, which is at the 15th percentile of the fee distribution.This comes from Srivastava (2008) definition of low-fee schools as those with monthly fee less than the daily minimum wage (The daily minimum wage in urban Karnataka is INR 300-500).

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.

Table 15: **Summary of school fee data**

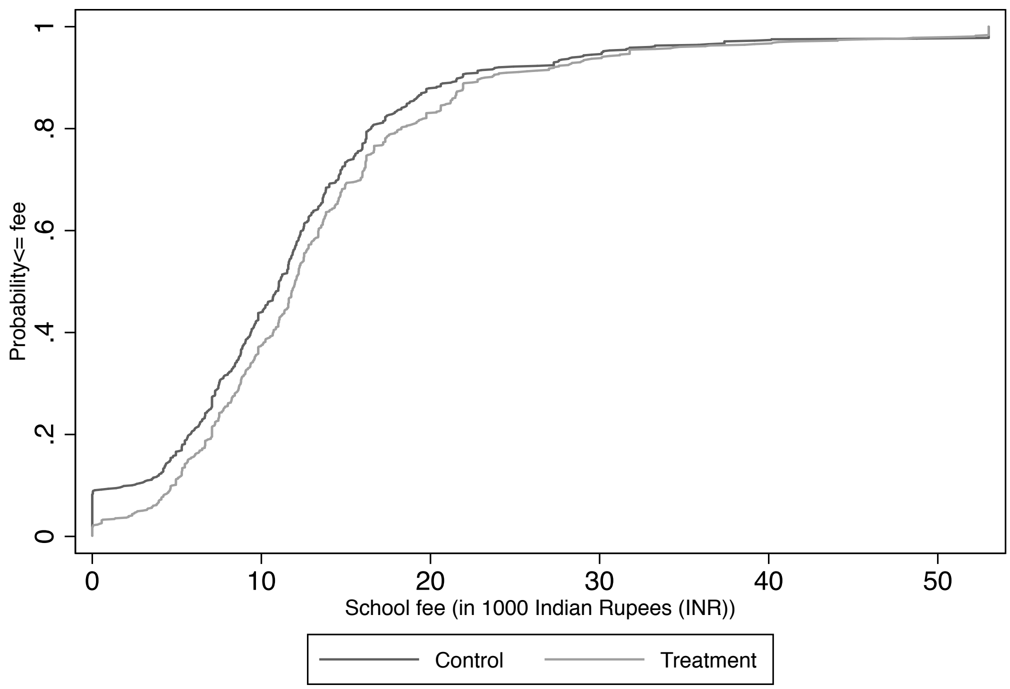
District	(1) Number of schools	(2) Average	(3) Maximum	(4) Median	(5) p10	(6) p90
		Points on the fee distribution (in Indian Rupees(INR))				
Bangalore Rural	183	17,087	274,771	13,269	6,011	26,769
Bangalore Urban	1,419	25,346	897,626	14,460	6,925	47,241
Bellary	454	10,625	121,527	8,737	4,196	16,752
Gulbarga	490	9,211	112,050	7,782	1,993	15,706
Total	2,546	19,022	897,626	12,059	4,373	32,761

Notes: p10 and p90 are tenth and ninetieth percentiles values of the fee distribution. Fees data is available only for 85 percent of the registered private schools in the government fees database

I, hence, 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).[23] 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 at graph **??**. 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 5 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,

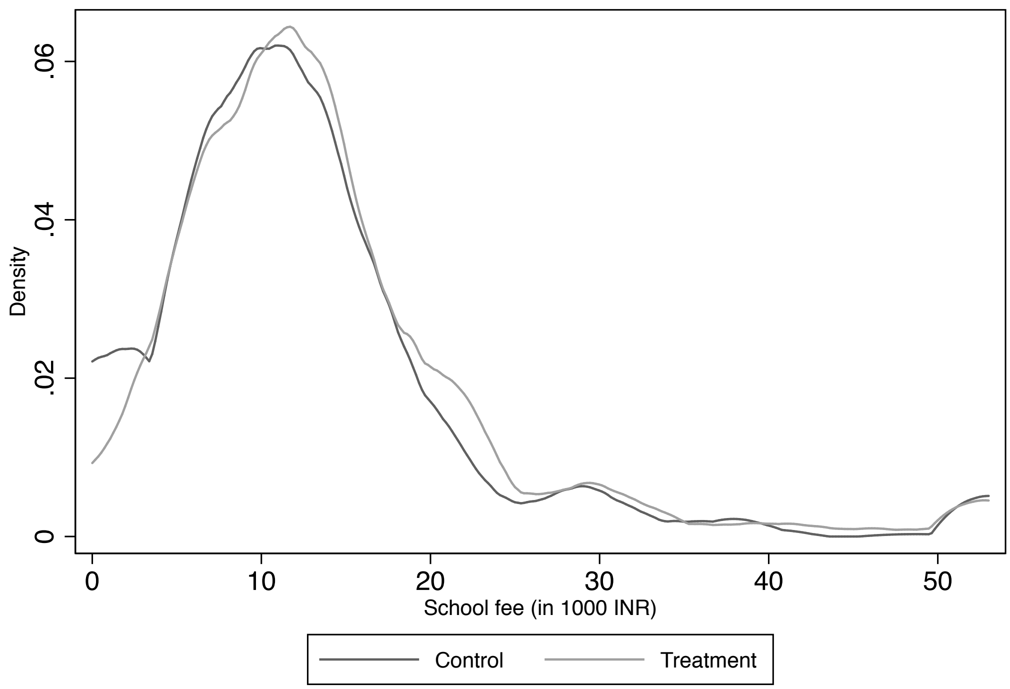
[23]This missingness of the fee data could bias the estimates if it is not orthogonal to treatment. I check this by regressing a dummy for missing fee on treatment status.

there are more treatments than controls. The gap between the two curves is small to be meaningful as I demonstrate with the next set of results.



Notes: School fees is winsorized (0.02 level) on the rightside of the distribution

Figure 4: CDF of school fee: Treatment versus Control



Notes : School fees is winsorized (0.02 level) on the rightside of the distribution

Figure 5: Density functions of school fee

Table 16 present the results of regression analysis estimating the impact of winning the lottery on school fees. While columns (1) and (2) present the ITT estimates for the raw fee data, columns (3) and (4) present results for the winsorized fee data.[24] My preferred results are those with the winsorized data, where I am certain that the results are not driven by extreme outliers in the fee-distribution. The IIT treatment effect is a statistically significant 1,342 (Column (3)). This means that being a lottery winner is associated with attending a school whose average fees is higher by INR 1,342. This is an effect of 0.14 σ (the mean of the winsorized fees is INR 13,166 and the standard deviation is 9,717). So the policy did move treated children to higher fee schools, but the effect size is small to be meaningful from a policy or economic perspective.

Table 16: **Impact of policy on school fees**

	(1)	(2)	(3)	(4)
	Annual school fees in INR			
	Non-winsorized		winsorized	
Treatment effect	1,725*	1,663*	1,342***	1,270***
	(920.49)	(922.67)	(372.75)	(373.28)
Constant/ Control mean	13,039***	9,725***	12,472***	11,499***
	(486.17)	(3,432.12)	(361.20)	(2,627.16)
Observations	1,459	1,459	1,459	1,459
R-squared	0.00	0.10	0.00	0.22
Sub-district dummies	No	Yes	No	Yes
Controls	No	Yes	No	Yes

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Standard errors are clustered at the pair level in all models. The fee data is winsoriozed at the 0.02 level (high only).

All this evidence demonstrates that even on the school fee measure the policy hasn't moved children to significantly different schools. Further the control mean is 12, 472 (Column (3)) demonstrating that the average child in the

[24] I winsorise the top 2 percent of the fee distribution to account for huge outliers in the fee data and to ensure that outliers do not drive the results.

control group (and hence the average applicant in the absence of the policy) attends a school of fees around INR 12,500, the median fee.

That the policy didn't move treated children to better learning environments compared to their default, is therefore, the mechanism explaining the non-impact.

8 Discussion

8.1 Policy mistargeting

The policy goal of improving outcomes of disadvantaged children hasn't been achieved due to the failure of a fundamental mechanism inherent in the 25 percent mandate's theory of change. That the treatment effect on private school enrolment is only 6 percent points with a control mean of 0.93, and that the effect on school fees is 0.14 σ indicates that the policy participants/ applicants would have attended broadly similar kind of schools with or without the policy. The default schooling choice for the average policy applicants is a private school with annual fee of around INR 12,500 (control mean in Table 16, Column 3). This fee is around the median of the fees distribution (Table 15, column 4). So the average applicant is a default private school goer, with ability and willingness to pay the fee of the median private school. This is very different from the description of the target applicant who was supposed to be default government or low-fees private school goer, without the ability to afford private school fees.

This logically implies one of two things. First, there are no default government/ low-fee school going households in the population. Or more plausibly, that the default government/ low-fee school going households didn't participate in the mandate. Table 17 shows the public versus private school attendance proportions in the sample and the population. While only 7 percent of the control applicants are government school goers (93 percent are private school goers), the public school attendance of the population is 41 percent. Hence, there is a large proportion of default government school going (poor/ disadvantaged) children in the population that hasn't applied for the policy.

Table 17: **Private and public school attendance (sample versus population)**

	Public	Private	Total	Percentage public	Percentage private
Panel A-Sample (by treatment)					
Treatment	13	795	808	1.6	98.4
Control	59	749	808	7.3	92.7
Total	72	1,544	1,616	4.5	95.5
Panel B- Sample(by district)					
Bangalore Rural	15	385	400	3.8	96.3
Bangalore Urban	4	394	398	1.0	99.0
Bellary	28	392	420	6.7	93.3
Gulbarga	25	373	398	6.3	93.7
Total	72	1,544	1,616	4.5	95.5
Panel C-Population (classes 1-8)					
Bangalore Rural	57556	53258	110814	51.9	48.1
Bangalore Urban	106029	569244	675273	15.7	84.3
Bellary	229320	119531	348851	65.7	34.3
Gulbarga	250056	179420	429,476	58.2	41.8
Total	642961	921453	1564414	41.1	58.9

Notes: In three of the four districts the proportion of public school attending children is above 50 percent.

All this points to one definitive conclusion. The target population, default government/ low-fee school goers, have not participated in the policy. The majority of the mandate participants are default private school goers who can afford the median school fees. So, the program has been mistargeted: the target households haven't applied to the program, and the non-deserving ones have applied in large numbers. In the targeting literature, these two failures are referred to as errors of exclusion (undercoverage) and inclusion (leakage) respectively (Coady et al., 2004). If the policy were successfully targeted, the point estimates on private school enrollment indicator and the school fees variable would be much higher than the present values. In the ideal case of perfect targeting the private school effect would be 1, and the fees effect would equal to the average school fees of INR 19,022 (Table 15, column (2)).

8.2 Alternate explanations- John Henry effects

An alternate explanation of the mechanism results could be that the RTE admission process and the lottery have altered the behavior of control households, and hence, the schools they are enrolled at do not provide the perfect counterfactual for a world without the policy. More precisely, lottery losing household's schooling decisions could have changed just because of participation in the lottery. These types of lottery/ experiment induced behavior changes in the control group are referred to as John Henry effects (Duflo et al., 2007).

The possibly of John Henry effects is very remote in this case for two reasons. First, altering preferences because of the lottery is costly. If binding income and/or credit constrains were preventing parents from enrolling children at private schools in the first place, losing the lottery doesn't relax those constraints. Second, the level of private school enrollment (93 percent) and the school fee of the control group children (around the median school fee) are so high that altered preferences due to losing the lottery can't plausibly account for them.

8.3 Income profile of policy participants

Having established that the policy is mistargeted, the next interesting question is to get a sense of the degree of targeting failure. I use primary data on household spending on education to estimate the treatment- control spending differences and use this estimate to comment on the income status of the mandate applicants'.

I collected detailed survey data on household spending on education of the sample child. Spending on education involves three broad kinds of costs: first, tuition fee 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,[25] and optional activities outside school. I refer to category two and three as non-tuition expenses of private education. Under the RTE mandate, government subsidizes only the tuition fees, leaving households to cover the non-tuition costs. Hence the RTE subsidy is only partial.

Table 18 presents the results for treatment effect on household education expenditure on the sample child. This expenditure involves both tuition and non-tuition expenses. 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 13,159. This implies that household expenditure on education of the applicants was INR 13,159 for the control households and about INR 7,500 for the treatment households. This leads to three inferences. First, despite getting a tuition waiver, treated households spent around INR 7,500 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 13,159 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 13,159 can be used as a starting point to generate a rough estimate of the income of the applicant households. With an average of two children per household, total household expenditure on education would be about INR 26,320. A national estimate for average percent of household income spent on education in urban India is 7 percent (Tilak, 2009). Therefore, the household income of an average household spending INR 26,320 on education should be about INR. 376,000. This is

[25] Private coaching (referred to as private tuitions) attendance has become an important aspect of education in India. About 26 percent of all primary school aged children are enrolled at private tuition (Saha, 2016). Parents consider private tuitions as a necessary complement to school attendance.

Table 18: Impact of policy on education expenditure on the sample child

	(1)	(2)
	Household education expenditure on sample child (in INR)	
Treatment effect	-5,610***	-5,647***
	(394.31)	(404.72)
Constant/ Control mean	13,159***	6,914**
	(402.93)	(3,001.14)
Observations	1,616	1,616
R-squared	0.08	0.27
Sub-district dummies	No	Yes
Controls	No	Yes

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Standard errors are clustered at pair level.

almost four times the income eligibility cutoff of INR 100,000 in Karnataka, and confirms that ineligible households have captured the policy.

I calculate income estimates for the control group children in the sample using this method and plot the kernel density function for these income estimates. Figure 6 shows the distribution for the whole sample, along with the income eligibility cutoff line at INR 100,000. Clearly, a huge part of the density of the distribution is above the eligibility cutoff, with a significant percentage of households having incomes 5 times above the cutoff.

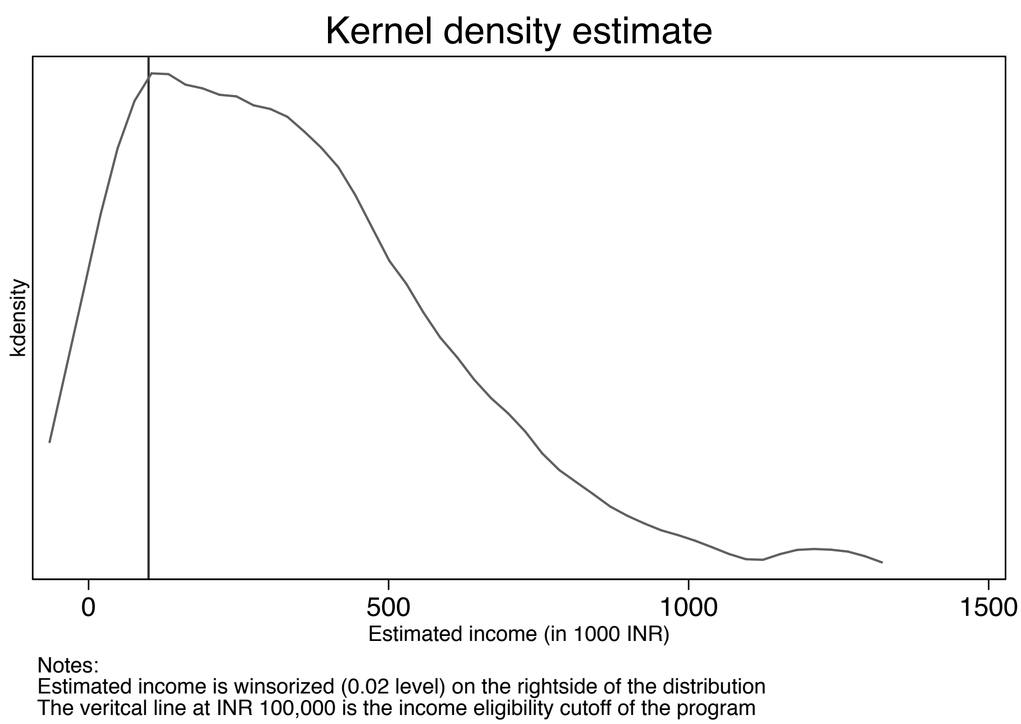


Figure 6: Estimated household income of applicants

Figure 7 plots the income estimates by district. This shows that in the more urban and developed Bangalore area (Bangalore rural and Bangalore urban), the proportion of applicants above the eligibility cutoff is much higher than those in the relatively underdeveloped Northern Karnataka districts (Bellary and Gulbarga). The failure of targeting, however, is across districts. I acknowledge that this method of estimating income is not very rigorous, but it provides strong suggestive evidence on the income profile of participants, and complements the results of table 14 and 16.

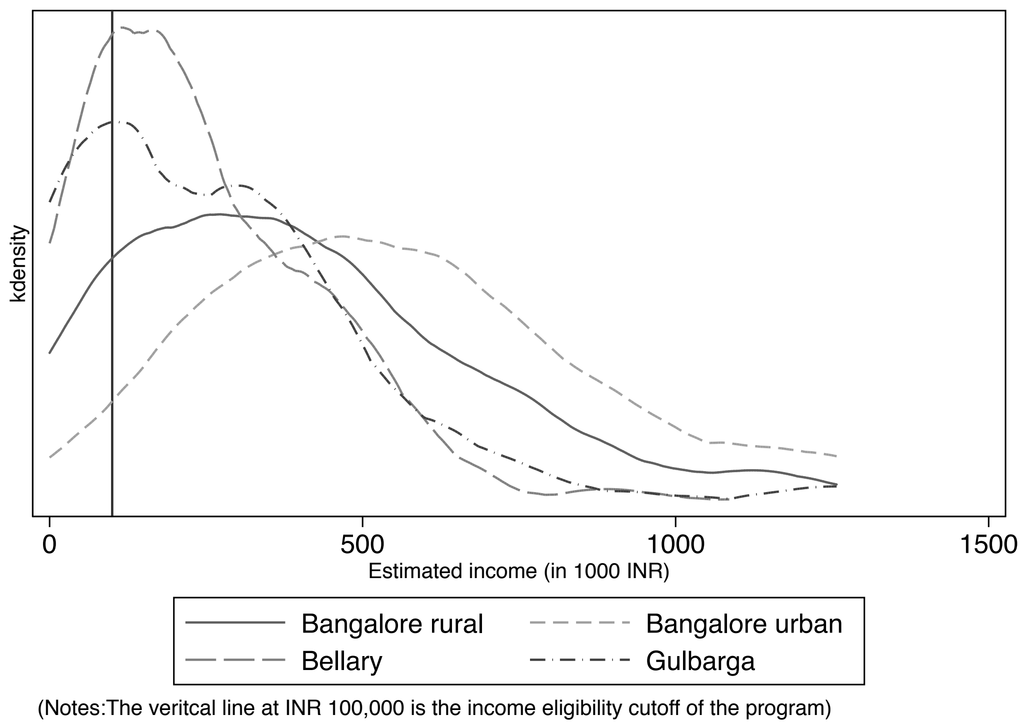


Figure 7: Estimated household income (by district)

8.4 Leakages and Undercoverage

That the program has been poorly targeted begs two central questions from a policy perspective. First, how are ineligibles able to apply for the program? Second, why are eligibles not applying for the program? In other words, what explains errors of inclusion and exclusion?

Inclusion errors are a result of the targeting strategy deployed under the mandate: an income- based targeting strategy [26] despite the well established problems with using income-eligibility determination (simple means testing) in developing country contexts (Coady et al., 2004; Banerjee et al., 2009; Alatas et al., 2012). In Karnataka (and in most other states of the country) income- eligibility is determined through a certificate issued by a grassroots government functionary without any independent verification. More often than not, these certificates are issued based on self-declarations by the household head. So, the obvious difficulty with establishing income eligibility seems to have opened doors for ineligible households to apply for the program.

Next is the issue of non-participation of eligible households. Anecdotal evidence points to information constraints, transaction costs of application, and partial nature of the RTE subsidy as barriers to participation of the poor. The role of information constraints and transaction costs in excluding the poor from school choice programs has been well documented (Musset, 2012). In the context of this policy the partial nature of the RTE subsidy, and the high non-tuition expenses associated with private school education seem to be the primary reason for exclusion of the poor. As demonstrated in the

[26] The mandate is one of the first large social policies to use income- based eligibility. Traditionally, the Indian state has used caste-based eligibility (social disadvantage) and proxy-means-testing (defining below poverty line households based on asset and land ownership) approaches for targeting poverty reduction programs. This mandate uses both social and income based targeting. I focus on the income- targeting here as it is the primary targeting mechanism, with 64 percent of the applicants claiming eligibility for being income-disadvanatged. Further, the failure of caste based targeting owing to capture of benefits by the better off amongst the socially disadvantaged has been well studied, and is an important area of public and political debate in India.

previous section, the non-tuition costs in my sample are about 1.33 times the tuition costs. Evidence points to lower socioeconomic status families not benefitting when the government subsidy (voucher) doesn't cover all the costs of education (Ryan and Watson, 2009). 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. This seems a reasonably sound explanation for exclusion errors, though further research is required to precisely understand the binding constraints that prevent poor households from participating in the program.

In summary, the reliance on simple means testing and the partial nature of the RTE subsidy are the two design flaws in the mandate that are responsible for the failure of targeting.

8.5 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 median of the school fees distribution. So, half the schools (in the right side of the fees distribution) are paid the ceiling reimbursement amount of INR 11,848,[27] while the bottom half get their actual fees reimbursed. The average per child reimbursement for the year 2015-16 is INR 6,800 for the entire state: 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

[27] The calculation of this amount only uses the recurring costs of running schools (primarily teacher salaries). Other substantial costs incurred by the government like costs of infrastructure, administration, and school inputs (text books, free meals) are not included in this calculation. Including all costs rises the per child cost incurred by government to 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 grade 1-10).

of improved test scores, it can be argued that the program is cost effective. Government has been successful in procuring same quality education, at 60 percent of the price by leveraging on 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 given that treatment effect on school fees is only INR 1,342. This means that the government is buying education from private schools worth INR 1,342, by spending 5 times that amount! Further, and more crucially, the program beneficiaries are not default government/ low-fee school going children, whose education is the responsibility of the state. The policy is in essence, achieving a very inefficient and potentially welfare-reducing income transfer from taxpayers to non-poor policy participants.

8.6 Spillover effects within beneficiary households

If the net policy effect is to provide an income transfer to households that don't deserve it, it is important to evaluate if the transfer led to spillover benefits in the beneficiary household. Following the literature on intra household resource allocation (Das et al., 2013), I investigate if the lottery-winning households invest the income transfer in the education of the other children in the household. Table 19 present treatment effects on household expenditure on all children in the household, and all children other than the sample child. As discussed in section 8.3, 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 receive, thanks to the program. If this transfer has translated into greater investments on education of other children in the household, the treatment effect on education expenditure of siblings (column (3)) should be positive. However, the estimate is a statistically significant -1,477, implying that treatment households are spending less not only on program participating children, but also on their siblings. An important caveat to the result is that the expenditure data on siblings was not verified (with supporting documents), as was the case for the sample child. Further, treated households had the incentive to under-report expenditures compared to controls in the absence of supporting documents.[28] Hence I would treat this result as suggestive evidence pointing to absence of household reallocation (on education) of the RTE windfall by treatment households. A thorough analysis of spillover effects will require more reliable expenditure data on education and other aspects of human capital development (like health, nutrition, and so on).

Table 19: Impact of policy on household education expenditure

	(1)	(2)	(3)
	Household education expenditure (in INR)		
	Sample child	All children in household	All children excluding sample chdild
Treatment effect	-5,610***	-6,891***	-1,477***
	(394.31)	(792.01)	(545.01)
Constant/ Control mean	13,159***	23,389***	11,393***
	(402.93)	(741.58)	(450.20)
Observations	1,616	1,616	1,190

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Standard errors are clustered at pair level.Sub-district dummies and controls are not included in the models

8.7 Policy implications

I summarize the key policy implications below:

- The RTE mandate didn't lead to improved learning outcomes for the policy beneficiaries. This is owning to poor program targeting, which

[28]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.

caused the non-participation of the most disadvantaged who could have benefitted from the program.

- The targeting failure owes more to policy design than implementation: the reliance on income-eligibility and the partial nature of the RTE subsidy. The former enabled ineligible households to apply for the mandate, while the latter probably discouraged the eligible.
- 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.
- The psychosocial treatment effects debunk the prevalent anecdotal evidence that children enrolled in free-places are being discriminated against in their new learning environments.

9 External Validity

9.1 Repeating the evaluation on another sample

Given the study design (pairwise matching) and sampling strategy, the results of this study are valid only for the population of matched pairs from the four districts of Karnataka. Though the four districts account for 28 percent of the RTE applicants (table 1), the results cannot be generalized to the whole state, as the districts were not randomly chosen.Further, there is an important temporal dimension to generalizing the targeting failure result. It can be argued that the poor targeting performance is primarily due to the nascency of the policy, and that participation of eligible households will improve with time. This is the precise argument policy makers advanced when presented with my results. To address the spatial and temporal concerns on the generalizability of the policy mistargeting finding, I repeated this evaluation on another sample of RTE applicants from the academic year 2016-17 (my primary sample of 1616 children are RTE applicants from the academic year 2015-16, hereafter referred to as sample A. The new sample from academic year 2016-17 is hereafter referred to as sample B). Having conducted an extensive household and child survey on sample A, I realized that the key to evaluating targeting effectiveness was knowing the schools that the lottery winners and losers were enrolled at. This information could be obtained through a relatively inexpensive telephone survey. Hence, I repeated the evaluation on a much larger sample: sample B has 15,908 children compared to 1,616 in sample A. The evaluation on sample B, however, doesn't shed light on the learning outcomes question

9.2 Sampling, survey, and attrition

Table 20 presents the summary of the 2016-17 admissions data for the whole state. As mentioned earlier, RTE admissions happen at two entry grades in Karnataka: Lower Kindergarten and grade I. Though sample A is drawn only from grade I applicants, I use both the Kindergarten and grade I applicants for constructing sample B. A total of 184,851 children applied for admission out of whom, 64 percent were offered admission. Like in the pervious year, 70 percent of the applicants have claimed program eligibility owing to being income-disadvantaged.

The 2016-17 RTE admissions process and the place allocation lottery are the same as in the previous year. I generated the sampling frame of 29,252 matched pairs (59,184 children) by using the matching strategy explained in section 4.5. This time, however, I deployed a Probability Proportional to Size (PPS) sampling strategy wherein a certain percentage of observations are randomly chosen from each sampling unit. The sampling unit is the sub-district grade: with 193 sub-districts and 2 entry grades there are 386 sampling units.

Table 20: **Summary of the 2016-17 applications**

Panel A: Grade-wise treatment and matching data				
Entry grade	Applicants	Treatment	Percent treated	Matched pairs
Lower Kindergarten	107,864	68,520	64	17,294
Class I	76,987	48,970	64	11,958
Total	184,851	117,490	64	29,252
Panel B: Grade-wise applicants by eligibility criteria				
Entry grade	Applicants	Socially disadvantaged	Income disadvantaged	Percent of income disadvantaged
Lower Kindergarten	107,864	31,334	76,530	71.0
Class I	76,987	23,218	53,769	69.8
Total	184,851	54,552	130,299	70.5

The survey sample comprised of a randomly selected third of the matched pairs in the sampling frame: 9,612-matched pairs/ 19,242-children. I conducted a short telephone survey on these applicants in January-February 2017, and collected information on the schools children were enrolled at. As would be expected, some parents didn't respond to survey team's telephone calls. However, the attrition/ non-response rate[29] in this sample is much lower than before: only 17 percent compared to 45 percent in sample A.[30] Table 21 presents the attrition data that shows the 7 percent point difference in attrition between treatment and control (column (3)).

I obtain information on the second outcome variable for the analysis, school fees, from the education department's school fee database. The attrition problem is more acute for this variable (42 percent) as a large number of schools have not uploaded their information into the government portal. Further the attrition is differential with a 19 percent point difference in attrition rate of treatment and control.[31] I correct for differential attrition

[29]Non-respondents are of three types: those whose numbers were not working, those who were not picking up the calls, and those who refused to give information.

[30]The higher response rate in sample B is owing to the shorter time gap between the RTE admission process and the survey. The gap was 6 months for sample B compared to 18 months for sample A.

[31]The higher attrition rate amongst controls can largely be attributed to the poorer

Table 21: **Attrition in sample B**

	(1)	(2)	(3)	(4)	(5)
	Randomized sample	Respondents	Attrition rate	Fee Data available	Attrition rate
Treatment	9,621	8,271	14%	6,527	32%
Control	9,621	7,637	21%	4,699	51%
	19,242	15,908	17%	11,226	42%

using Lee bounds (Lee, 2009).

9.3 Results and discussion

Table 22 presents the policy impact estimates on the two targeting performance measures: private school enrollment and school fees. The private school effect is 8 percent points, the control mean is 0.91, the Lee bounds for this effect are very tight (lower bound of 8 percent points and upper bound of 9 percent points) and the effect confidence interval doesn't include zero. This establishes that 91 percent of the applicants in this sample were default private school goers. The fee effect is INR 2,060 (column (3) winsorized annual school fee variable), and the control mean is 9,513. This implies that the policy has moved children to schools whose fees is INR 2,060 higher, and that applicants come from households who have the ability to pay INR 9,513 (the median school fees for the whole state is INR 9,645). Figure 8 shows the treatment and control CDFs of school fees that demonstrate that there is no meaningful difference in the fees of schools treatment and control children are enrolled at. Lee bounds for the fee effect are very wide and the effect confidence interval includes zero (column (6)). This means that the

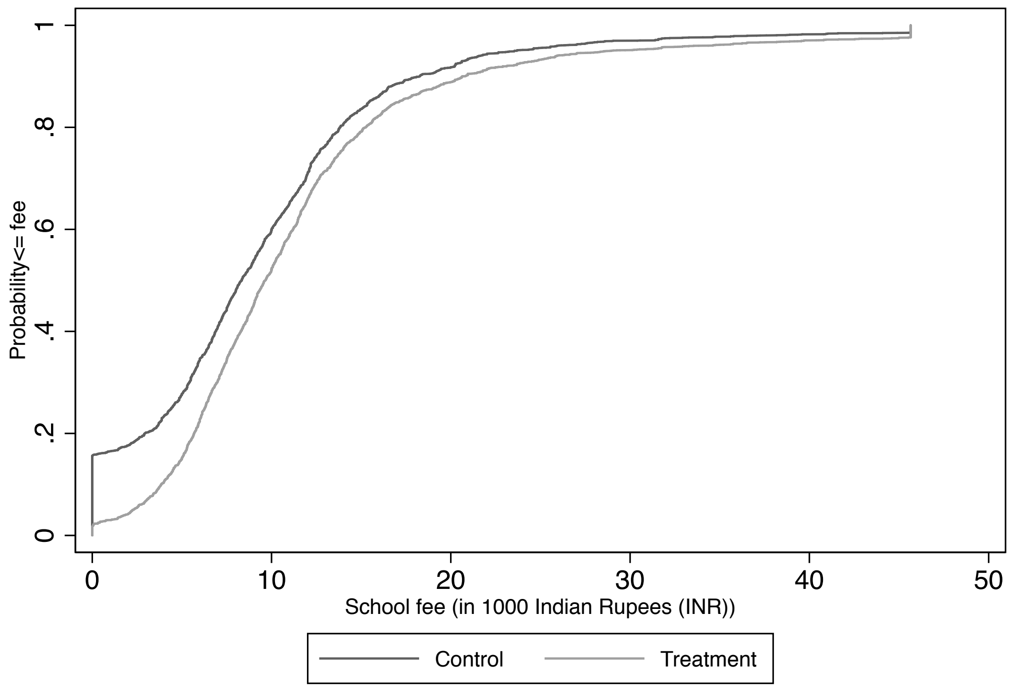
quality of data collected from the control households in the telephone survey. Parents were requested to provide the exact name of the school during the survey; the name and the neighborhood were then used to match schools with the government fees dataset. Control households were generally less forthcoming than the treatment households in providing exact name of the school the child was enrolled at. Not having the exact name led to poorer matching of schools for the control children and hence a higher level of attrition.

null of zero- impact on fees cannot be rejected. These results confirm the primary inference of policy mistargeting drawn from analysis of sample A. Though the effect sizes in sample B are slightly different, the overall story is unambigious: Majority of the policy applicants are default private school goers with ability to pay the median school fees.

Table 22: **Impact of policy on private school enrollment and school fees (all 34 districts)**

	(1) Private school	(2) School fee	(3) School fee (winsorized)	(4) Private school	(5) School fee	(6) School fee (winsorized)
Treatment	0.08***	2,253**	2,060***			
	(0.00)	(746)	(143)			
Lower				0.08***	-4,056***	-1,921***
				(0.00)	(653)	(154)
Upper				0.09***	6,007***	4,913***
				(0.00)	(975)	(208)
Constant	0.91***	11,648***	9,513***			
	(0.00)	(646)	(120)			
Observations	15,908	11,226	11,226	19,226	19,226	19,226
Selected observations				15908	11226	11226
Effect CI-lower				0.0710	-5131	-2174
Effect CI-upper				0.0982	7611	5255
Trimming proportion				0.0755	0.279	0.279

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Models (1)-(3) report the treatment effect on different outcome variables. The standard errors are clustered at the pair level. Models (4)-(6) report the lee bounds for models (1)-(3).



Notes: School fees is winsorized (0.02 level) on the rightside of the distribution

Figure 8: CDF of school fee for Sample B: Treatment versus Control

Table 23 presents the results only for grade I children from the four sample A districts. This subsample of sample B is drawn from the same population as sample A. Hence, comparison of results in table 23 with those from table 14 and 16 should point to improvements (or the lack of them) in policy targeting across time. The private school effect in table 23 is 4 percent points (against 6 in table 14), and the school fees effect is INR 1,478 (against INR 1,342 in table 16). The replication of sample A results in this subsample of sample B means that there has been no change in the type of households participating in the policy (default private school goers with ability to pay the median school fees) over the two- year period. This evidence debunks the argument that mistargeting is a result of information constraints alone, and that policy targeting would improve with time. The evidence from sample B, therefore, confirms that the mistargeting result is valid for the entire state of Karnataka, and that targeting is not improving with time. Given this, it is reasonable to argue that the non-impact on test score results can also to be generalized to the whole state of Karnataka.

Table 23: **Impact of policy on private school enrollment and school fees (sample districts and grade I only)**

	(1) Private school	(2) School fee	(3) School fee (winsorized)	(4) Private school	(5) School fee	(6) School fee (winsorized)
Treatment	0.04***	4,465**	1,478***			
	(0.01)	(2,094)	(419)			
Lower				0.03***	-3,719***	-2,639***
				(0.01)	(715)	(495)
Upper				0.05***	8,268***	4,140***
				(0.01)	(2,715)	(634)
Constant	0.95***	13,225***	12,144***			
	(0.01)	(623)	(350)			
Observations	2,755	1,623	1,623	3,363	3,363	3,363
Selected observations				2755	1623	1623
Effect CI-lower				0.022	-4894	-3453
Effect CI-upper				0.0593	12734	5184
Trimming proportion				0.0439	0.219	0.219

Notes: *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Models (1)-(3) report the treatment effect on different outcome variables. The standard errors are clustered at the pair level. Models (4)-(6) report the lee bounds for models (1)-(3).

10 Conclusion

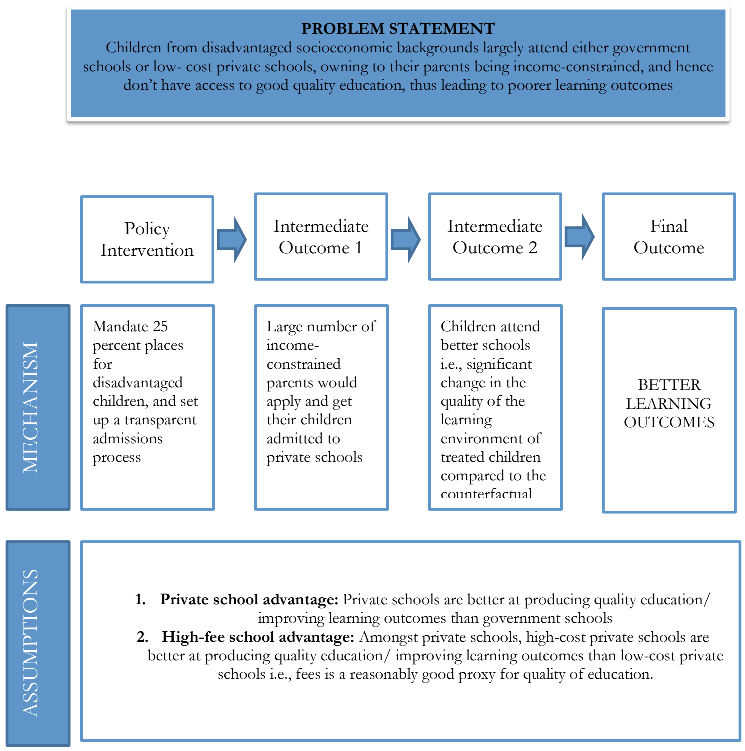
This chapter presents the results of the first empirical investigation of the RTE 25 percent mandate, India's national school choice policy. Against the background of continuing debates on school choice,the role of private sector in human capital formation, and public private partnerships in education, I investigate the question of the impact of the choice policy on childrens' outcomes. I take advantage of the lottery-based allocation of RTE free places in Karnataka state, and estimate the policy impacts on childrens' learning and psychosocial outcomes. Contrary to expectation of choice enthusiasts, I find no statistically significant difference in the outcomes of RTE beneficiary children vis-a-vis the control group. I undertake a detailed mechanism analysis and establish that there is no significant difference in schools attended by treatment and control children and that the average policy applicant is a default private school goer with ability to afford the median private school fee. The implication of all my evidence is that the policy is mistargeted. Some of the design elements of the policy make it easier for non-poor households to apply for the policy, while making it harder for poor people to apply. Given the importance of the mistargeting result from the policy design and implementation perspective, I replicate it on a much larger sample of applicant households.

My results and discussion point to several areas for future research. First, the reasons for non-participation of the targeted program beneficiaries need to be better understood. Though I conjecture that the partial nature of the RTE subsidy is the primary culprit, establishing the reasons through collecting survey and qualitative data would be important in improving the policy in the coming years. Second, my results on the spillover effects of the RTE transfer within treated households are very rudimentary at best. Analysis of these effects based on detailed household expenditure data would contribute to understanding the overall welfare effects of the policy. Finally, some Indian states like Madhya Pradesh are using alternate targeting methods to implement the policy. Evaluating the impact of the policy and it's targeting performance in these contexts could deepen our understanding of both the design and implementation of this crucial school choice mandate.

Appendices

A Theory of change

Figure 9: Theory of change underlying the RTE 25 percent mandate



References

Atila Abdulkadiroğlu and Tayfun Sönmez. Random serial dictatorship and the core from random endowments in house allocation problems. *Econometrica*, 66(3):689–701, 1998.

Atila Abdulkadiroğlu and Tayfun Sönmez. School choice: A mechanism design approach. *American economic review*, 93(3):729–747, 2003.

Vivi Alatas, Abhijit Banerjee, Rema Hanna, Benjamin A. Olken, and Julia Tobias. Targeting the poor: evidence from a field experiment in indonesia. *The American Economic Review*, 102(4):1206–1240, 2012.

Michael L. Anderson. 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):1481–1495, 2008.

Abhijit Banerjee, Esther Duflo, Raghabendra Chattopadhyay, and Jeremy Shapiro. Targeting efficiency: How well can we identify the poorest of the poor? *Institute for Financial Management and Research Centre for Micro Finance Working Paper*, 21, 2009.

Lisa Barrow and Cecilia E. Rouse. School vouchers and student achievement: Recent evidence, remaining questions. 2008.

Rakesh Basant. Social, economic and educational conditions of indian muslims. *Economic and Political Weekly*, pages 828–832, 2007.

Miriam Bruhn and David McKenzie. In pursuit of balance: Randomization in practice in development field experiments. *American economic journal: applied economics*, 1(4):200–232, 2009.

David Coady, Margaret E. Grosh, and John Hoddinott. *Targeting of transfers in developing countries: Review of lessons and experience*, volume 1. World Bank Publications, 2004.

Julie Berry Cullen, Brian A. Jacob, and Steven Levitt. The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica*, 74(5):1191–1230, 2006.

Janet Currie and Duncan Thomas. *Early test scores, socioeconomic status and future outcomes*, 1999.

Jishnu Das, Stefan Dercon, James Habyarimana, Pramila Krishnan, Karthik Muralidharan, and Venkatesh Sundararaman. School inputs, household substitution, and test scores. *American Economic Journal: Applied Economics*, 5(2):29–57, 2013.

David Deming, Justine Hastings, Thomas Kane, and Douglas Staiger. School choice and college attendance: Evidence from randomized lotteries. *NBER Working Paper*, 2009.

Jean Drèze. *An uncertain glory : India and its contradictions*. London : Allen Lane, 2013. ID: oxfaleph019533391; Includes bibliographical references and index.

Jean Drèze and Reetika Khera. The bpl census and a possible alternative. *Economic and Political Weekly*, pages 54–63, 2010.

Esther Duflo, Rachel Glennerster, and Michael Kremer. Using randomization in development economics research: A toolkit. *Handbook of development economics*, 4:3895–3962, 2007.

Milton Friedman. *Capitalism and freedom electronic resource]*. Chicago : University of Chicago Press, Chicago, 1982. ID: oxfaleph016718943; Includes bibliographical references.

Milton Friedman and Robert A. Solo. Economics and the public interest. *Economics and the public interest*, 1955.

Paul Glewwe and Michael Kremer. Schools, teachers, and education outcomes in developing countries. *Handbook of the Economics of Education*, 2: 945–1017, 2006.

Paul Glewwe and Karthik Muralidharan. Improving school education outcomes in developing countries: evidence, knowledge gaps, and policy implications. *University of Oxford, Research on Improving Systems of Education (RISE)*, 2015.

Paul W. Glewwe, Eric A. Hanushek, Sarah D. Humpage, and Renato Ravina. *School resources and educational outcomes in developing countries: A review of the literature from 1990 to 2010*, 2011.

Eric A. Hanushek. Throwing money at schools. *Journal of policy analysis and management*, 1(1):19–41, 1981.

Caroline Minter Hoxby. School choice and school productivity. could school choice be a tide that lifts all boats? In *The economics of school choice*, pages 287–342. University of Chicago Press, 2003.

Chang-Tai Hsieh and Miguel Urquiola. The effects of generalized school choice on achievement and stratification: Evidence from chile's voucher program. *Journal of public Economics*, 90(8):1477–1503, 2006.

Joanna Härmä. Low cost private schooling in india: Is it pro poor and equitable? *International journal of educational development*, 31(4):350–356, 2011.

Ahmedabad Indian Institute of Management. State of the nation: Rte section 12 (1) (c). Technical report, Indian Institute of Management, Ahmedabad, 2013.

Ahmedabad Indian Institute of Management. State of the nation: Rte section 12 (1) (c) 2015 (provisional). Technical report, Indian Institute of Management Ahmedabad and others, 2016.

Raghbendra Jha, Raghav Gaiha, Manoj K. Pandey, and Nidhi Kaicker. Food subsidy, income transfer and the poor: A comparative analysis of the public distribution system in india's states. *Journal of Policy Modeling*, 35 (6):887–908, 2013.

Nalini Juneja. India's new mandate against economic apartheid in schools. *Journal of International Cooperation in Education*, 16:55–70, 2014.

Julie E Kaufman and James E Rosenbaum. The education and employment of low-income black youth in white suburbs. *Educational Evaluation and Policy Analysis*, 14(3):229–240, 1992.

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

David S Lee. Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, 76(3):1071–1102, 2009.

Katherine Magnuson. *The effect of increases in welfare mothers' education on their young children's academic and behavioral outcomes: Evidence from the National Evaluation of Welfare-to-Work Strategies Child Outcomes Study*. Institute for Research on Poverty Madison, WI, 2003.

Sumit Mazumdar and Alakh N. Sharma. Poverty and social protection in urban india. 2013.

New Delhi Ministry of Human Resource Development. The right of children to free and compulsory education, 2009- clarification of provisions, 2010.

Claire Morgan, Anthony Petrosino, and Trevor Fronius. *A systematic review of the evidence of the impact of school voucher programmes in developing countries*. EPPI-Centre, Social Science Research Unit, Institute of Education, University of London, 2013.

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

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

C. Noronha and P. Srivastava. India's right to education act: Household experiences and private school responses. *Education Support Program Working Paper Series*, (53), 2013.

Harry Anthony Patrinos, Felipe Barrera Osorio, and Juliana Guáqueta. *The role and impact of public-private partnerships in education*. World Bank Publications, 2009.

New Delhi Pratham Foundation. Annual state of education report 2014. Technical report, ASER Center, New Delhi, 2014. URL http://img.asercentre.org/docs/Publications/ASER%20Reports/ASER%202014/District%20Estimates/karnataka.pdf.

Jesse M. Rothstein. Good principals or good peers? parental valuation of school characteristics, tiebout equilibrium, and the incentive effects of competition among jurisdictions. *American Economic Review*, 96(4):1333–1350, 2006.

Chris Ryan and Louise Watson. The impact of school choice on students' university entrance rank scores in australia. *School Choice and School Improvement: Research in State, District and Community Contexts', Vanderbilt University*, pages 25–27, 2009.

Devanik Saha. Indians increasingly prefer private education, 71 million take tuitions, 2016.

Abhijeet Singh. Private school effects in urban and rural india: Panel estimates at primary and secondary school ages. *Journal of Development Economics*, 113:16–32, 2015.

P. Srivastava and C. Noronha. Institutional framing of the right to education act: Contestation, controversy, and concessions. *Economic and Political Weekly*, 49(18):51–58, 2014.

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

Peter Svedberg. Reforming or replacing the public distribution system with cash transfers? 2012.

Washington DC The World Bank. World bank data. Data set, World

Bank, 2016. URL https://data.worldbank.org/indicator/SE.PRM.PRIV.ZS?locations=IN&name_desc=false.

Charles M. Tiebout. A pure theory of local expenditures. *The journal of political economy*, pages 416–424, 1956.

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

Gregory White, Matt Ruther, and Joan Kahn. *Educational Inequality in India: An Analysis of Gender Differences in Reading and Mathematics*. IHDS Working Paper 2016-2. 2016.

Alan Wolfe. *School choice: The moral debate*. Princeton University Press, 2009.

Laura Zimmermann. Reconsidering gender bias in intrahousehold allocation in india. *Journal of Development Studies*, 48(1):151–163, 2012.