How can progressive vouchers help the poor benefit from school choice? Evidence from the Chilean voucher system *

Josefa Aguirre†

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Abstract. This paper considers a major educational reform in Chile that increased vouchers by 50 percent for students in the lowest 40 percent of the income distribution. This increased the revenues that schools received for these students and lowered the relative prices of private voucher schools for eligible parents. I use a national dataset to implement a RDD exploiting that eligibility is a discontinuous function of a socioeconomic ranking. Results reject that eligible students chose schools with higher test scores or average SES, and that they are doing better than non-eligible students in math and language test scores.

Keywords: School choice, Stratification, Chile

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†Columbia University, Teachers College. Email: `ja2958@tc.columbia.edu`

Advocates of school vouchers typically argue that these help promote consumer choice, personal advancement, and competition. From their perspective, they can lower educational costs even as they increase school quality.[1] Despite this expectation, there is still no consensus on whether voucher programs improve average outcomes. Moreover, there is concern that they can generate a sorting of students across schools along characteristics like income and ability, which possibly leads to lower educational outcomes for less-advantaged students (see Manski, 1992; Epple and Romano, 1998; and MacLeod and Urquiola, 2012, 2015 for theoretical work; and Chakrabarti, 2009; Hsieh and Urquiola, 2003, 2006; McEwan et al., 2008; and Muralidharan and Sundararaman, 2015 for empirical evidence).

A question that remains unanswered is whether a more careful voucher design could preserve the potential efficiency benefits from competition while mitigating socioeconomic stratification. For instance, several authors have suggested that deviating from a flat voucher to one that conditions the subsidy on student characteristics like income could ameliorate sorting impacts and help low-income students benefit from voucher systems (Epple and Romano, 2008; Nechyba, 2000, 2003). The underlying idea is that differentiated vouchers would give schools an incentive to serve low-income students, which mitigates the temptation to admit only students with relatively high socioeconomic status or ability. Despite much discussion, however, there is still little rigorous empirical evidence on actual impact of such differentiated vouchers.

In this paper I look at a major educational reform implemented in Chile, a country that has used vouchers for over three decades. Specifically, in 2008 the voucher amount was increased by 50% for students in the lowest 40% of the income distribution. A number of papers have studied this change, but while most of them, with the exception of Feigenberg et al. (2017), have found a positive effect on educational outcomes (MINEDUC, 2012; Correa et al., 2014; Villarroel, 2012; Mizala and Torche, 2013; Neilson, 2013; Navarro-Palau, 2017), there is an ongoing discussion on the potential mechanisms through which progressive vouchers could have helped improve the educational outcomes of the poor.

In part the lack of agreement on the potential mechanisms reflects an identification challenge. While several researchers have noted that it would be natural to assess the program using a regression discontinuity design, this has not been feasible due to a lack of information. In this paper I use a new matched administrative dataset to implement such an approach and to explore the direct effect that differentiated vouchers had on their beneficiaries.

Chile is one of the few countries with a universal voucher system, where both public and private voucher schools get paid a voucher amount for each student. Progressive vouchers were implemented in Chile to acknowledge the fact that the costs of educating low-income students are higher and also with the aim of giving low-income parents access to a sub-group of private

[1] see Hoxby (2003) for a review of how school choice might affect school productivity.

voucher schools that charge add-ons to parents and that have on average higher test scores than public schools and private voucher schools that do not charge add-ons to parents. Because of the latter, in order to receive the extra resources from targeted vouchers, schools had to sign up for the policy and agree, among other things, not to charge add-ons to low-income parents, although they could still charge add-ons to high-income parents.

There are several mechanisms through which the reform could have increased educational outcomes for eligible students. First, public schools and private schools that chose to join the policy received additional revenue for each eligible student that could be used to improve educational results. Importantly, the law entailed that money from targeted vouchers had to be spend on educational improvement plans aimed to increase the educational results of eligible students. Second, the higher revenues provided schools an incentive to compete for the enrollment of low-income students. This could have led schools to increase their overall quality or the quality that they provide to low-income students. Third, because eligible parents no longer had to pay add-ons to attend private voucher schools and because schools had higher incentives to admit these students, the policy could have expanded choice sets for eligible parents. This could have led to better educational outcomes for students if eligible parents were now able to choose better schools.

Nevertheless, previous mechanisms could also prove to be ineffective. If, regardless of the voucher increase, higher performing schools are unwilling to serve low-income students, then they may have abstained from participating in the policy, limiting parents' choices. Alternatively, if parents face barriers aside from price —such as distance or lack of information— that prevent them from attending better performing schools, then the policy might have been unable to change students' distribution across schools. Also, if schools have local market power, it is possible that the policy simply led to increased revenues for existing schools without any impact on educational outcomes for eligible students.

In this paper, I exploit the fact that eligibility for progressive vouchers is a discontinuous function of a socioeconomic ranking to implement a regression discontinuity design. This allows me to estimate the causal impact of being eligible for a targeted voucher on the school choices and educational outcomes of students who entered 1st grade in 2012, four years after the program was first implemented. I find that being eligible for a targeted voucher had no impact on the probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the average class size of the chosen school, or the distance traveled to school. The results do show that eligible students chose schools that charge higher add-ons to non-eligible parents, but this effect is small in magnitude, with eligible students choosing schools that charge approximately 3USD more to non-eligible parents. In terms of educational outcomes, results show no impact of being eligible for a targeted voucher on students' performance on a standardized language test that is given to students in second grade, and on a standardized language and math test that is given to students in fourth grade. Importantly, I am able to reject a positive impact above 0.04 standard deviations on each of these test scores.

A first contribution of this paper is to show that being eligible for a targeted voucher had no impact on parents' school choices. I argue this is driven by both demand and supply side mechanisms. On the supply side, I am able to show that even though all public schools and roughly all private voucher schools that charge no add-ons to parents joined the policy, only 50% of private voucher schools that charge add-ons chose to join. Private voucher schools that chose to participate have lower prices, test scores, and socioeconomic status, compared to schools that chose not to participate. This result is in line with that of Abdulkadiroglu et al. (2015) who find that low-quality private voucher schools tended to select themselves into the Louisiana Scholarship Program. These results suggest that, despite the voucher increase, schools with the highest test scores were unwilling to participate in the policy and serve low-income students.

On the demand side, I am able to show suggestive evidence indicating that there are other barriers, aside from price, that prevent parents from attending higher test score schools. Even though private voucher schools with the highest test scores abstained from participating in the policy, there was still a substantial number of above average test score schools that charge add-ons and chose to join the program. These schools were now free for eligible parents and could have represented an improvement over public schools and private voucher schools that charge no add-ons to parents. Importantly, the extra voucher amount was typically higher than the add-ons charged by these schools, indicating that they might have had a special incentive to admit eligible as opposed to non-eligible students. I perform three exercises aimed at better understanding what barriers could be preventing parents from responding to this price decrease. I look at heterogeneous effect by mothers' education, heterogeneous effects by distance to the nearest private voucher school that charges add-ons to parents and joined the program, and I extend results further away from the discontinuity.

The effects could vary by mothers' education either because higher socioeconomic status parents might have a higher preference for school quality (see Hastings and Weinstein, 2008 and Bayer et al., 2007 for evidence on this), or because schools might choose to serve, among eligible students, those of higher socioeconomic status. In practice, however, I find no evidence of a differential impact on students whose mothers' have less than high school education, or more than high school education.

Results could also vary by students' distance to a private voucher school that charges add-ons to parents and joined the policy, as distance has been found to be a major determinant of school choice (Hastings and Weinstein, 2008; Bayer et al., 2007). However, when looking at the impact of the policy on students who live relatively close to a private voucher school that charges add-ons to parents and joined the policy, I find no effect on parents' school choices or students' educational outcomes.

Finally, I look at whether results look any different for those students who are further away from the discontinuity. A possible concern is that, because eligibility for targeted vouchers is determined on a yearly basis, eligible parents close to the cutoff might be afraid of losing their benefit and ineligible parents close to the cutoff might be expecting to gain the benefit. To address this issue I follow Angrist and Rokkanen (2015) and I use a matching strategy to estimate the impact of the program on a wider sample of students, comparing eligible and ineligible students who are further away from the discontinuity. This allows me to use a broader treatment and control group that is unlikely to change status from one year to the next. It also allows me to determine what the impact of being eligible is for students who are further away from the cutoff and therefore have lower or higher SES. Results from this analysis confirm previous findings and indicate that being eligible for a targeted voucher had no impact on the characteristics of the schools chosen or educational outcomes for students who are further away from the discontinuity.

These results indicate that, with a voucher system already in place, progressive vouchers were ineffective in terms of changing students' distribution across schools. This could be a result of students who are typically left behind in public schools or bad performing private voucher schools facing additional barriers that prevent them from attending higher test score schools. Barriers could include lack of information, the complexity associated with evaluating a substantial number of options, or issues of social belonging that lead them to choose schools where their own social group is majority. This result adds to an increasing literature looking at behavioral barriers that prevent individuals from making what could be optimal educational choices (Radford, 2013; Pallais, 2015; Smith et al., 2015; Thaler and Sunstein, 2008; Ross et al., 2013).

A second contribution of this paper is to show that targeted vouchers did not have a direct impact on the educational outcomes of their beneficiaries. There is no evidence that increased revenues or increased competition improved educational results for eligible as opposed to non-eligible students in the short or medium term. This could be reflecting that educational outcomes did not improve for low-income students as a result of this policy; or that results did improve, but that the benefits were captured by all students who are similar in terms of wealth, regardless of whether they were or not eligible.

This paper provides a thorough analysis of the mechanisms through which progressive vouchers could have helped to increase the educational outcomes of the poor in Chile, thus contributing to the ongoing empirical discussion on the effects of the 2008 reform. The results are also highly relevant from a public policy perspective. As school vouchers continue to be implemented across the world, and as the US moves towards increasing school choice, much can be learned from the Chilean voucher experience. This paper in particular helps to document that progressive vouchers in Chile did not lead to a resorting of students across schools, nor did they lead to an increase in educational outcomes for eligible as opposed to non-eligible students.

1 Related Literature

Results from this paper relate to several strands of the literature. First, they relate to the literature looking at the impact of the 2008 Chilean reform on school choices and educational outcomes. The period from 2008 to 2012 saw an increase in test score results for low-income students in Chile that has been attributed to the 2008 reform by many.[2] Existing studies include MINEDUC (2012), Correa et al. (2014), Villarroel (2012), and Mizala and Torche (2013), all of which compare the academic outcomes of schools that chose to join the policy to the academic outcomes of schools that chose not to join and find a positive impact on test scores that ranges between 0.08 and 0.2 standard deviations. Neilson (2013), instead, compares the academic outcomes of low and high-income students and finds that targeted vouchers raised test scores for low-income students in 0.2 standard deviations. Navarro-Palau (2017), instead, uses variation from date of birth enrollment cutoffs to compare the outcomes of students who had a different exposure to the reform, and finds a significant though more modest effect of the reform on school achievement.

An exception is a more recent paper by Feigenberg et al. (2017) that argues that changes in parental education and household income could account for much of the decline in the achievement differential between low and high socioeconomic status students observed in the data. According to the authors, there is little evidence that the reform had a substantial effect on school inputs or that it altered the education market in a manner that could have raised achievement for low-income students.

Now, although most studies, aside from Feigenberg et al. (2017), agree in that the policy improved educational outcomes for poor students, there is an ongoing discussion about the mechanisms that could be driving these results. Studies that explicitly look at mechanisms include Neilson (2013) and Navarro-Palau (2017). Neilson (2013), on the one hand, uses a structural model of school demand and supply to constructs counter-factual simulations and isolate the different mechanisms through which the policy could have affected outcomes; concluding that approximately one third of the observed improvement is due to eligible families being able to choose better schools with the larger voucher, and two thirds of it is due to the rise in quality of existing schools in response to the policy. A major concern though, is that the structural model requires making important assumptions about school supply and demand, including, for example, that students can attend any school they are willing to travel and pay for, and that schools have no capacity constraints. To the extent that these assumptions are flawed, demand estimates could be biased and we could be over or under estimating the extent to which families responded to the policy by resorting across schools.

Navarro-Palau (2017), on the other hand, exploits variation from date of birth enrollment cut-

[2] Figure A.1 and Table A.1 in Appendix A perform a simple difference in difference analysis and shows that by 2012 the gap between eligible and ineligible students had decreased by roughly 0.08 standard deviations.

off to compare the choices and educational outcomes of students who entered school right before or right after the policy was implemented. The author finds that the policy slightly decreased the probability that students attended public schools and that it increased the probability that they attended private schools with better average characteristics. However, there is no evidence of a positive effect on test scores for students more likely to switch schools. Based on these results, and an observed increase in test scores for students most likely to stay in public schools, the author concludes that the effects of the policy on test scores were caused by a response from public schools. A major concern with previous results, however, is that date of birth is not truly random and that the existence of pre-trends in enrollment and educational outcomes could be biasing the results.[3]

This paper contributes to the discussion by providing a new identification strategy to look at how the policy affected choices and educational outcomes for eligible students. The regression discontinuity approach represents a more reliable approach to look at the direct impact that the policy had on its beneficiaries. Results allow me to discard that differentiated vouchers changed students' distribution across schools and that the program improved the educational outcomes of eligible as opposed to non-eligible students.

Second, this paper relates to the existing empirical literature on school vouchers and student stratification. Among the existing papers are Hsieh and Urquiola (2003, 2006) and McEwan et al. (2008) both of which find that the growth of the private sector in Chile increased stratification by socioeconomic status. Some evidence of sorting has also been found in Sweden (Sandström and Bergström, 2005; Böhlmark and Lindahl, 2007; Böhlmark et al., 2015). In India, Muralidharan and Sundararaman (2015) find that students from lower casts were less likely to accept vouchers if awarded. This paper contributes to previous studies by showing that introducing a targeted voucher may be ineffective in terms of reducing students' stratification by socioeconomic status.

Third, the paper also relates to the existing empirical literature looking at the impact of school vouchers on educational outcomes. From a theoretical point of view, school vouchers can affect educational outcomes by allowing students to migrate from public to private schools. Also, when a program is large in scale, vouchers can affect educational outcomes through a re-sorting of students across schools and a potential increase in competitive pressure among schools.

In general, evidence for small-scale voucher programs is mixed. In the US, studies have typically found no effect of receiving a voucher on test scores for non-African American students and some evidence, albeit not very robust, of a positive effect for African American students. In contrast, there is more robust evidence that voucher programs had a positive impact on graduation probabilities, particularly for African American students.[4] Worth mentioning is a more recent

[3]The author tries to address this by using RD estimates for pre-policy cohorts to account for biases. However, these trends could have changed over time biasing the results.

[4]see Wolf et al. (2010) for evidence on the D.C. Opportunity Scholarship Program. Peterson et al. (2003); Mayer

study by Abdulkadiroglu et al. (2015) that looks at the Louisiana Scholarship program and finds that vouchers reduced academic achievement for students, a result that the authors attribute to a selection of low-quality schools into the program.

Results for other countries are more positive than those for the U.S. In Colombia, for instance, studies find a positive impact of vouchers on test scores, as well as other long-term outcomes such as the probability of completing secondary school (Angrist et al., 2002, 2006).

Less empirical evidence can be found on the effects of large-scale voucher programs. Worth mentioning is an experimental study by Muralidharan and Sundararaman (2015) in India, where a large-scale voucher program was implemented. The authors find that four years after treatment, voucher lottery winners did not have higher test scores in Math, English, Science and Social Studies, although they did perform better in Hindi. However, the authors emphasize that private schools in India spend much less than public schools, implying a higher productivity of private schools. Interestingly, the authors find no evidence of spillover effects on public school students who do not apply for the voucher, or on private school students.Non-experimental studies looking at large-scale voucher programs, instead, typically do find positive effects of vouchers on public school students (see, among others, Figlio and Rouse, 2006; Gallego, 2013; Chakrabarti, 2008; Rouse et al., 2013; Figlio and Hart, 2014). [5]

The analysis I perform in this paper is somewhat different from that of previous studies as it focuses on the impact of increasing the voucher amount as opposed to giving students access to vouchers. Still, results contribute to the general literature by showing that increasing vouchers for low-income students and decreasing the prices that these students have to pay to attend private voucher schools, will not necessarily lead them to attend higher test score schools. Moreover, the analysis also shows that increasing the revenues that schools receive for serving low-income students will not necessarily improve results for these students over those of other non-eligible students. This could either be because the program may not improve outcomes for low-income students; or because the benefits may be captured by all students who are similar in terms of wealth, regardless of whether they are eligible or not.

Finally, the paper also relates to the existing literature looking at behavioral barriers that prevent individuals from making what could be optimal educational choices. There is a broad literature that has looked at the barriers that students face when trying to apply to a postsecondary institution, including lack of information or the complexity associated with evaluating a substantial number of options (Thaler and Sunstein, 2008). Both of which can drive students to make sub-optimal choices (Radford, 2013; Pallais, 2015; Smith et al., 2015; Thaler and Sunstein, 2008;

et al. (2002); Chingos and Peterson (2015) for evidence on the programs from the School Choice Scholarship Foundation implemented in New York City, Dayton, and Washington, D.C., and Rouse (1998); Witte et al. (2012) for evidence on the Milwaukee program

[5] see Epple et al., 2015 for a more extensive review of this literature

Ross et al., 2013). It has also been found that high-achieving low-income students tend to choose colleges that mimic the choices of their socioeconomically similar peers, despite being more academically advantaged (Hoxby and Avery, 2013). Less attention has been given to the role that these factors play in school choice, given that there are few places with broad school choice systems. Still, some studies have shown that parents of each race prefer schools where their own race is the clear majority, implying that minority parents face much larger tradeoffs between academics and social preferences when choosing schools (Hastings et al., 2009).

2 Institutional Background

2.1 Voucher System

Chile has had a universal voucher system since 1981. In the system there are three types of schools: public schools that are managed by local municipalities and that represent approximately 38 percent of total enrollment, private voucher schools that represent approximately 54 percent of total enrollment, and unsubsidized private schools scattered to upper-income households that represent approximately 8 percent of total enrollment. Up until 2008 both public and private voucher schools were paid a flat voucher per student based on attendance.

The Chilean voucher scheme is quite unique in that it imposes few restrictions to private voucher schools. These schools can receive voucher subsidies regardless of their religious status, can operate for-profit, are allowed to implement admission policies subject to few restrictions, and as of 1994, can also charge add-ons to parents. These add-ons are capped at about three times the voucher payment, but this constraint is rarely binding.[6] Instead, public schools face more restrictions; they are not allowed to charge add-ons to parents and cannot turn away students unless oversubscribed.

Based on the particularities of the Chilean voucher design, theoretical models such as Epple and Romano (1998) and MacLeod and Urquiola (2012, 2015) would suggest that the voucher system would lead to cream-skimming from the public sector, and stratification by income and/or ability within the private sector, which is exactly what is observed in practice.[7]

Although it is hard to isolate the causal effect of the voucher system on student sorting, existing research suggests that the voucher system has indeed led to increased socioeconomic stratification across schools. Among the relevant research, Hsieh and Urquiola (2003, 2006) implement a dif-

[6] According to Urquiola and Verhoogen (2009) most of elite unsubsidized private schools could take vouchers but choose not to.

[7] In the theoretical model developed by Epple and Romano (1998) stratification occurs as a consequence of positive peer effects. Instead, in the model developed by MacLeod and Urquiola (2012, 2015) stratification occurs because employers use an individual's school of origin as a signal of her skill.

ference in difference strategy where they compare stratification measures across municipalities with more or less growth in the private sector, finding that the voucher-induced growth in the private sector was associated with a middle class exodus from public schools. In a related work, McEwan et al. (2008) compare students sorting across towns of different population size. Based on the idea that there is a minimum size required for private school entry and that towns close to this threshold are comparable, the authors find that private entry is related to higher student stratification.

There is a broader cross sectional literature in Chile that documents the high levels of stratification by socioeconomic status. Valenzuela et al. (2010) suggest that Chile has one of the highest levels of school-level stratification by socioeconomic status in the OECD. Moreover, Mizala et al. (2007) show that stratification is particularly extensive in the private sector.[8]

2.2 Targeted Vouchers

The existing research on the high levels of segregation by socioeconomic status in the Chilean system was in part what led to the reforms that were implemented in 2008. The 2008 reform, *Ley de Subvención Escolar Preferencial*, introduced a targeted voucher for students belonging to the lowest 30 percent of the income distribution. Figure 1 shows how voucher amounts evolved in the period from 2006 to 2012 for eligible and non-eligible students. As can be seen, in 2012, eligible students were receiving approximately 65 USD extra per month, which represented roughly a 50 percent increase over the regular voucher amount.

The extra resources were made available to all public schools, as well as private voucher schools that signed up for the policy. Schools that chose not to sign-up for the policy could still receive the regular voucher amount for each student, but could not receive the extra resources from targeted vouchers. In what follows I describe the type of schools that chose to join the policy. This analysis is key to a better understanding of how the policy changed the choice sets for eligible parents. I then proceed to provide some information on how schools might have spent these extra resources. This second analysis is relevant to understand how the policy might or might not have impacted educational outcomes.

2.2.1 Schools that signed up for targeted vouchers

Schools that signed up to receive targeted vouchers had to agree to: provide detailed accounting of the use of targeted voucher funds, something that is not required for the regular voucher; present an educational improvement plan to the Ministry of Education, with detailed education reforms that the school would undertake to improve test scores; define anticipated test score gains for

[8]see Epple et al., 2015 for a more extensive review of the Chilean voucher system

future years, particularly for eligible students; eliminate screening of eligible students based on past academic performance and family background, although this was not enforced in practice; and not charge add-ons to eligible parents, although they could still charge add-ons to non-eligible parents.[9]

The policy, therefore, offered increased revenues for schools, but came at a cost. In practice by 2012, all public schools had chosen to join the policy, and approximately 95 percent of private vouchers schools that charged no add-ons to parents had chosen to join. However, among private voucher schools that charged add-ons to parents only about 50 percent had decided to join. Figure 2 shows the amount of private voucher schools that in 2012 were charging monthly copayments between 2 USD and 200 USD. Blue bars indicate the number of schools in each category that had chosen to join the policy by 2012 and grey bars show the number of schools in each category that had chosen not to join the policy. As can be seen, the great majority of schools that were charging between 2 USD and 50 USD had chosen to join the policy, but few schools with monthly add-ons above 50 USD had decided to join.

Table 1 presents average characteristics for public schools, private voucher schools that charge no add-ons to parents, and private voucher schools that charge add-ons to parents and had chosen either to join or not to join the policy. All measures are for 2011, the year when parents in my sample made their choices.[10] As can be seen, private voucher schools that charged high add-ons and/or served high SES students abstained from participating in the policy. Still, it can be seen from Table 1 that the policy gave low-income students free access to a subgroup of schools that in 2011 had higher test scores (their average academic performance was in the 66th percentile) and had higher SES students (the average SES status of their students was in the 78th percentile) than public schools and private voucher schools that charged no add-ons to parents.

2.2.2 Use of resources from targeted vouchers

To join the program, schools had to agree, among other things, to present an Educational Improvement Plan detailing the educational reforms that the school would undertake to improve academic results. The plan was also meant to set academic goals, particularly for eligible students. Educational Improvement Plans contained specific actions that schools would perform in

[9]Carrasco et al. (2014) provide qualitative evidence on private voucher schools' admission policies. According to the authors common admission practices include performing "game sessions" for prospective students aimed at measuring skills that can predict good behavior, development and adaptation, as well as interviews to prospective parents.

[10]Because the policy was implemented in 2008 these measures may already reflect some of the changes induced by the policy. Appendix C presents the average characteristics for these schools in 2007, the year before the policy was implemented. Data shows that in the period from 2007 to 2011, public schools and private voucher schools that had joined the policy increased their test scores. Still, it is always the case that private voucher schools with add ons that joined the policy are better than public schools and private voucher schools that charge no add-ons to parents.

areas such as: curriculum management, school leadership, student life, and resource management. Resources from targeted vouchers could be spent on, among other things, hiring teachers, educational assistants, and the necessary staff to meet the goals of the Educational Improvement Plans. However, schools were not allowed to use these resources for increased salaries, bonuses and other expenditure categories such as debt repayment or school celebrations (Feigenberg et al., 2017).

The Ministry of Education classified schools in three categories according to their academic performance and the socioeconomic status of their students. Schools in higher categories had more freedom to define their Educational Improvement Plans and could decide how to allocate the resources from targeted vouchers. Instead, schools in the lowest category had to elaborate their Educational Improvement Plans with the assistance of the Ministry of Education. The sanction that schools could receive as a result of noncompliance with their Educational Improvement Plans depended on their classification. Schools in higher categories could be demoted to a lower category, and schools in the lowest category could eventually be closed. In practice, in 2008 when the policy was first implemented, no school was given the lowest category. By 2012, roughly 3 percent of schools had fallen to the lowest category, but none of them had been closed.

Even though the reform was meant to hold schools accountable for the extra resources from targeted vouchers, in practice a number of schools did not comply with the requirements, a point emphasized by Feigenberg et al. (2017). Information comes from an audit conducted by the Chilean Comptroller's Office in 2012 that compared the funding inflows from targeted vouchers for the 2008 to 2011 period to documented expenditures in 77 of 345 municipalities. According to Feigenberg et al. (2017), on average only 65 percent of received funds could be linked to validated expenditures during the audit report.

3 Data and Descriptive Statistics

In this study I use a unique national dataset containing detailed information at the individual level. What makes the data unique is information on students' socioeconomic ranking (*Ficha de Protección Social*) for 2012, which is the variable used to determine program eligibility. This variable was provided by the Ministry of Education and the Ministry of Social Development and was not available for previous studies.

Information on program eligibility is merged with administrative records on school enrollment that cover the whole population of students. To characterize schools, I use administrative data on schools' locations, prices, type of holder (i.e., private vs. public), size, and class size. Data from the SIMCE, a nationwide test for 2nd, 4th, 6th, and 8th graders, which is taken once a year, is used to measure schools' academic achievement as well as students' educational outcomes.

To characterize students, I use information from questionnaires given to parents in 2nd, 4th, 6th, and 8th grade together with the SIMCE. These questionnaires allow me to gather information about: mother's education, father's education, number of books in the house, monthly income, and childcare attendance. This information is complemented with detailed enrollment records to document students' age, gender and exact location. Data on students' exact addresses comes from enrollment records, where schools report parents' addresses. This information is not available for the entire sample, but I am able to get exact addresses for approximately 40 percent of students.

For simplicity I restrict my analysis to students entering 1st grade in 2012. The choice to focus on 1st grade students has to do with analyzing students who are relatively free to choose any school and who face no costs associated with changing schools. Still, results remain the same when looking at students in grades 1 to 8.[11] Because the socioeconomic ranking changes from year to year, and I only have data on this variable for 2012, I need to restrict my analysis to 2012, four years after the policy was first implemented. The benefit of focusing on such a period is that by then we expect that parents and students must have had a good sense of how the policy actually worked.

4 Empirical Strategy

In order to estimate the causal effect of the differentiated voucher I exploit the fact that the targeted vouchers' eligibility process generates large discontinuities in the relation between the socioeconomic status ranking and the probability of being eligible. Although there are other criteria that can determine eligibility, the socioeconomic ranking accounts for about 83 percent of all participants in the program (i.e., it is the binding threshold).[12] This allows me to implement a fuzzy regression discontinuity design. The estimating equations can be written as:

$$y_i = \alpha_0 + \rho D_i + f(r_i) + \delta' X + \epsilon_i \tag{1}$$

$$D_i = \gamma_0 + \pi 1[r_i \leq r_0] + g(r_i) + \nu' X + \mu_i \tag{2}$$

where y_i is the variable of interest, for example, academic performance of student i, D_i equals one if the student is eligible for targeted vouchers, r_i is the running variable (students socioeconomic ranking), $1[r_i \leq r_0]$ is an indicator function that equals one if the student is below the threshold for program eligibility, X are students' socioeconomic characteristics, and ϵ_i and μ_i are error terms.

[11] Results available upon request

[12] Students can also qualify as beneficiaries if: (i) they belong to the social program *Chile Solidario* which is the component of the Social Protection System in charge of serving families, people, and areas in social vulnerability condition, or (ii) they meet certain poverty requirements. However, under (ii) students will only be beneficiaries for a year and will have to be reevaluated under the government's socioeconomic ranking during the next year in order not to lose the benefit

Throughout the paper I use optimal bandwidths and robust confidence intervals proposed by Calonico et al. (2014). Because optimal bandwidths are estimated separately for each outcomes, the number of observations may vary depending on the outcome that is being studied.

The socioeconomic ranking used to determine eligibility is an instrument designed to measure the risk of being in poverty. It takes into account a household's ability to generate income based on education, experience, and county of residence, and a household's economic need based on, among other things, the number of children. The socioeconomic ranking is used to assign a number of other welfare programs and was created prior to the introduction of targeted vouchers. This ranking can change from year to year, either because the ages of household members change or because households choose to be reevaluated to apply to other welfare programs, which typically require up-to-date information on the socioeconomic ranking. If a student falls above the cutoff on a given year, he or she will no longer be eligible for targeted vouchers. Importantly, the threshold for targeted vouchers does not overlap with the threshold for any other program.

5 Results

5.1 First-Stage Estimates

Figure 3 plots targeted voucher assignment in 2012 as a function of the socioeconomic ranking in that same year. Black lines depict a fourth order polynomial fit for control and treatment units separately, and grey dots represent the sample average for each disjoint bin. Here, and in what follows, when talking about eligible students I will be referring to those students who were classified as eligible for targeted voucher by the Ministry of Education. In practice, not all of these students received the extra voucher amount because some of them chose to enroll in private schools or private voucher schools that didn't join the policy.

From Figure 3 it can be seen that students below the cutoff point are always eligible. However, the discontinuity is not sharp because students above the cutoff can still receive targeted vouchers if: (i) they belong to the social program *Chile Solidario* which is a component of the Social Protection System in charge of serving families, people, and areas in social vulnerability condition, or (ii) they meet certain poverty requirements. In theory, all students who belong to the program *Chile Solidario* will automatically be eligible, however parents who meet certain poverty requirements need to actively apply in order to become eligible.

In my sample, around 63% of students in the control group are not eligible, 15% of students in the control group are eligible because they belong to the program *Chile Solidario*, and 22% of students in the control group are eligible because they meet certain poverty requirements. In theory students in this latter group should only be eligible for a year and should be reevaluated according to the socioeconomic ranking during the next year in order not to lose the benefit. However, in practice, we observe that only 32% of students above the cutoff who met the poverty requirement lost their benefit in 2013, 18% were reevaluated and met the socioeconomic ranking threshold, and 50% continued to be eligible for meeting the poverty requirement.

Table 2 presents estimates of the change in the probability of being eligible for targeted vouchers for students who are below the cutoff, where $R \leq Cutoff$ is a dummy variable that equals one if the individuals' socioeconomic ranking is below the cutoff for program eligibility. As can be seen, being below the cutoff increases the probability of receiving a targeted voucher by 70 percentage points.

5.2 Balancing Checks

Next, I perform standard balancing checks to examine whether individuals just above and just below the cutoff are similar in terms of their observable characteristics. I look at a set of socioeconomic variables that should not be affected by the program. If the procedure is valid then RD estimates should be equal to zero.

Results are presented in Figure 4 and Table 3. Mother's and father's education equal total years of education; income equals monthly income in US dollars; books equal total number of books in the house; internet and computer are both dummies that equal one if the student had internet or computer in his or her house; and attended childcare is a dummy variable that equals one if the student attended childcare from 0 to 2 years old.

Figure 4 displays binned mean of observable characteristics of students by socioeconomic score relative to the cutoff. As can be seen, all of the studied characteristics change smoothly across the threshold. Regression estimates in Table 3 confirm the visual analysis; results are small in magnitude and precisely estimated, indicating that students around the cutoff are similar in terms of their socioeconomic characteristics. I do observe that students below the cutoff are 2 percentage points more likely to attend childcare from 0 to 2 years old. However, the difference is small in magnitude and only significant at the 10 percent level.[13]

To further check whether there is any sign of socioeconomic score manipulation I proceed to look at whether there is any evidence of a visible jump in the density around the discontinuity. Figure 5 (a) shows a histogram of scores relative to admission cutoff value, and Figure 5 (b) shows the result from the McCrary (2008) test. As can be seen, there is no sign of a visible jump in the density around the discontinuity. The McCrary (2008) test confirms this, showing no evidence of a statistically significant break.[14]

[13] Socioeconomic variables are available for approximately 70% of students in my sample. Missing rates are also smooth around the cutoff

[14] The log difference in height being 0.014 with standard error of 0.020

The fact that I do not observe any evidence of score manipulation near the cutoff is to be expected. Eligibility for targeted vouchers is determined by the Ministry of Education based on administrative data, and it does not require an application on behalf of parents. Also, information on cutoff scores is not made available to parents. Although there is some anecdotal evidence of socioeconomic ranking manipulation, this tends to occur at other cutoff points, where other welfare programs, such as housing, are assigned.

5.3 Targeted Vouchers and School Choice

I now turn to estimate the impact of being eligible for targeted vouchers on school choice. I expect that being eligible for a targeted voucher will: potentially expand the school choice sets for parents, and change the relative prices that parents have to pay for a school. Being eligible should expand choices because schools that join the program should now be more willing to accept eligible students as opposed to non-eligible students. It should also change relative prices because eligible students now do not have to pay add-ons to attend schools that joined the program.

The impact that targeted vouchers might have on enrollment decisions will ultimately depend on parents' preferences. In general, previous studies have found that when it comes to school choice, parents have a high valuation for proximity, and that preferences for test scores increase with students' income and own academic ability (Bayer et al., 2007; Hastings and Weinstein, 2008). Similar results have been found for Chile, were authors have documented that quality is a superior attribute and closeness to home an inferior attribute, suggesting that poor families tend to value more closeness to home than school quality (Gallego and Hernando, 2008; Chumacero et al., 2011).

In general, I expect that being eligible for targeted vouchers should lead parents to choose schools of higher test scores or higher socioeconomic status. However, because not all schools chose to join the policy, it is also possible that the change in relative prices leads parents to choose schools of lower test scores or lower socioeconomic status. This would be the case if, had they not been eligible, parents would have chosen a private school or a private voucher school that didn't join the policy. As a reference, in 2007, the year before the policy was implemented: approximately 57 percent of students entering 1st grade who met the requirement to be eligible for targeted vouchers enrolled in a public school; 16 percent enrolled in a private voucher school that charged no add-ons to parents; 18 percent enrolled in a private voucher school that charged add-ons to parents and that later joined the policy; and only 9 percent enrolled in a private or private voucher school that did not join the policy.[15] Therefore, I may not expect to see much of a negative effect in terms of having parents choose schools of lower test scores or lower socioeconomic status.

Results are presented in Figure 6 and Table 4. Chosen schools are characterized based on their observable characteristics in 2011, which is the year before parents made their choices. Private is a

[15] These numbers are similar for students closer to the threshold

dummy variable that equals one if the chosen school is private; test score equals the performance of the chosen school on 4th grade standardized test scores; socioeconomic status equals the average years of education of the mothers' of students attending the chosen school; add-on equals the monthly amount charged by the chosen school to non-eligible parents in USD; class size equals the average class size of the chosen school; and distance equals the distance traveled to school in miles.[16] Schools' test scores and socioeconomic status are both standardized by the mean and standard deviation of the variables in 2005.

Figure 6 (a) to (f) display binned mean of observable characteristics of the schools chosen by parents by students' socioeconomic score relative to the cutoff. As can be seen, all of the studied school characteristics change smoothly across the threshold. Estimates in Table 4 Columns (1) to (6) confirm the visual analysis. Panel A contains reduced form estimates and Panel B contains instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher. All estimates include controls for mother's education, father's education, household income, and region. Results remain the same when using alternative bandwidths (see Table C.1). As can be seen, being below the cutoff has no impact on: the probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the average class size of the chosen school, or the distance traveled to school. Results do show that students below the cutoff choose schools that charge higher add-ons to non-eligible parents. However, these results are small in magnitude, indicating that eligible parents choose on average schools that charged 3 USD more per month from an average monthly cost of 15 USD for students in the control group.

5.4 Targeted Vouchers and Educational Outcomes

I next turn to estimate the impact of being eligible for targeted vouchers on students' educational outcomes. Although the program had no impact on parents' school choices, it is still possible that the program had a positive impact on educational outcomes. First of all, it is possible that the program allowed parents to choose schools that better meet their children's needs, albeit there are no significant differences in the observable characteristics of the chosen schools. Second, because schools received extra revenues as a result of the policy, they could have used these resources to improve the educational outcomes of eligible students.

Results for educational outcomes are presented in Figure 6 (g) to (i) and Table 4 Columns (7) to (9). Figure 6 (g) to (i) display binned mean of test scores by students' socioeconomic score relative to the cutoff, and Table 4 Columns (7) to (9) present estimates of students' performance in a language test that is applied nation-wide to students in 2nd and 4th grade, and a math test that

[16] I am only able to get exact location for approximately 40 percent of students in my sample which is why there are fewer observations in distance estimates.

is applied nation-wide to students in 4th grade. Test scores for 4th grade are standardized by the mean and standard deviation of these variables in 2005. Test scores for 2nd grade are standardized by the mean and standard deviation of the variable in that same year, because there is no measure of second grade test scores for previous years. As can be seen, results show no impact of the program on students' educational outcomes in the short and medium term. Coefficients are all negative and non-significant. Moreover, I can reject in all cases a positive impact on test scores above 0.04 standard deviations.

Because students in the control group may become eligible in subsequent years, I perform a second analysis where I use the socioeconomic ranking as an instrument for the number of years that the student has been eligible to look at educational outcomes in the short and mid-term. Results can be found on Table 5. As can be seen in Panel A, being above the threshold for targeted vouchers increases the number of years that students are eligible by approximately 0.7 years. In line with previous results coefficients are all negative and non-significant, with the exception of 4th grade math results that are negative and significant at the 10 percent level.

5.5 Understanding the Null Effects

To better understand whether there are barriers that could impede low-income students from attending higher socioeconomic status or higher test score schools, I proceed to look at heterogeneous effects across various margins. I begin by looking at whether effects differ by mothers' educational level. Previous literature has shown that parents' preferences for school characteristics tend to vary with socioeconomic status (Bayer et al., 2007; Hastings and Weinstein, 2008). Also, in the Chilean context where schools can implement admission policies subject to few restrictions, higher quality schools could choose to serve, among eligible students, those of higher socioeconomic status, leading to a differential impact of the program.[17]

Table 6 presents heterogeneous effects by mothers' education. In general results show no impact on the choices made by mothers with less than high school education, and mother's with high school education or tertiary education. No impact can be found on the test scores of the chosen school, the socioeconomic status of the chosen school, the average class size of the chosen school, or the distance traveled to school for any of these groups. Estimates suggest that the impact on the prices of the schools chosen by parents might be higher for students with more educated mothers. However, estimates are not precise enough to reject the hypothesis that the impact is the same for both groups of students. Results in columns (7) to (9) also show no impact on educational results for students with more or less educated mothers.

Next, I look at heterogeneous effects by distance to the nearest private voucher school that charges add-ons to parents and joined the policy. It is possible that the policy did not have an

[17] Robustness checks to supplement the heterogeneity analysis can be found in Appendix D.

effect on students because there were distance barriers that were preventing them from switching to private voucher schools with higher test scores or higher socioeconomic status. To explore whether there is any evidence in favor of this hypothesis, I run a regression for the subgroup of students who have at least one private voucher school that charges add-on to parents and joined the policy in less than 0.4 miles. I choose to look at heterogeneous effects by distance to this specific group of schools, because I believe that most of the policy's effect should come from giving low-income parents access to this group of private voucher schools that charge add-ons to non-eligible parents and that are now free for low-income students. As a point of reference, the distance travelled by 1st grade students in 2012 had a mean of 1.3 and a median of 0.73 miles. Because I do not have data on exact locations for all students in my sample, I have to restrict the analysis to the 40 percent of students for whom I do have information on exact addresses.[18]

Table 7 presents heterogeneous effects by distance to the nearest private voucher school that charges add-ons and joined the policy. As can be seen, there is no evidence of a differential impact of the program on the probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the average class of the chosen school, or the distance traveled to school for students who live closer to this sub-group of schools. I do observe that the program seems to have had a higher impact on the probability of choosing a school that charges higher add-ons to non-eligible parents, for students living closer to a private voucher school that charges add-ons and joined the policy, however, the difference between groups in non statistically significant.

In line with previous results, I observe that the program did not have a differential impact on educational outcomes for students living close to a private voucher school that charges add-ons to parents and joined the policy. Results for educational outcomes can be found in Table 7 in columns (7) to (9).

Finally, I proceed to look at whether results look different further away from the discontinuity. Extending results beyond the discontinuity is useful for the analysis for two main reasons. First, although the regression discontinuity design provides a credible identification strategy there might be concern that, because the socioeconomic status score can vary from year to year, uncertainty with respect to next years' eligibility status could be driving the null results. Uncertainty could be especially relevant for individuals close to the cutoff who are in the margin of becoming eligible or ineligible. In practice, data indicates that 95 percent of students in the sample who were close to the cutoff and who were eligible in 2012 continued to be eligible in 2013. However, 65 percent of students close to the cutoff in the sample who were ineligible in 2012 became eligible in 2013. A major concern, therefore, is that ineligible students close to the cutoff might foresee that they are likely to become eligible in the future. Extending results beyond the discontinuity allows me to

[18]The lack of data on exact addresses for the whole sample of students also prevents me from doing exercises with alternative distances

address this concern by including a broader control group that is unlikely to become eligible in the future. Only 17 percent of students who were ineligible in 2012 became eligible in the following year and only 2 percent of eligible students in 2012 lost their benefit in the following year.

Second, extending results beyond the discontinuity also allows me to determine whether results look any different for students who are further away from the cutoff. Students close to the cutoff are approximately in the 40th percentile in terms of socioeconomic status. Results could be different for students in the bottom of the distribution for a number of reasons: changes in relative prices could be more relevant for this group of students; they may face a different supply of schools in their neighborhoods; or they may have different preferences for school attributes. The same could be true for students who are further above the threshold.

To extend my results beyond the discontinuity I follow Angrist and Rokkanen (2015) and exploit the availability of dependent variable predictors other than the running variable, to estimate the causal impact of targeted vouchers for students who are away from the cutoff. The basic idea is that the link between the running variable and outcomes can be broken by conditioning on a relevant set of controls. The running variable r_i can be thought of as a function of two parts $g(x_i, \epsilon_i)$, where x_i is observed and ϵ_i is not. Conditional on x_i the only source of variation in r_i, and consequently in eligibility, is ϵ_i. Thus, the conditional independence assumption requires that, conditional on the observed x_i, potential outcomes are mean-independent of unobserved determinants of the running variable.

The strategy used is similar to a conventional matching strategy, where the conditional independence assumption helps to break the link between treatment status and potential outcomes. However, the approach represents an improvement over the conventional matching strategy because it uses the information inherent in the regression discontinuity design to guide the choice of the conditional vector x_i and to test the veracity of the conditional independence assumption.

Following Angrist and Rokkanen (2015), I construct a conditional vector that includes controls for mothers' education, fathers' education, household income, municipality of residence, and whether the household had access to internet and computer. I choose a conditional vector that breaks the link between the running variable and outcomes while preserving the common support required for the matching strategy. In practice this can be done by choosing a vector such that, once I control for this vector, the running variable does not predict changes in the outcome variables at either side of the cutoff.

Using this set of control variables, I am able to break the link between the running variable and outcome variables for students who are in a window of -3000 to 0 points around the cutoff, and significantly reduce the link for students who are in a window of 0 to 8000 points around the cutoff. This represents approximately 54 percent of students in my sample. I choose not to include student beyond this window because there is evidence of socioeconomic score manipulation for students who are further away (see Figure 5). For households who are outside of my window, the running variable might be reflecting things aside from socioeconomic indicators, such as parents' ability to manipulate the socioeconomic score. This strategy helps to extend results for a larger sample of students. Regression discontinuity estimates typically include 13 percent to 20 percent of students in the sample.

Results for the conditional independence test can be found in Table 8. Panel A and Panel C show the relationship between the running variable and the outcomes of interest for students who are below and above the cutoff. The running variable is divided by 1000, meaning that, for example, a 1000 increase in the socioeconomic score predicts a 0.014 standard deviation increase in the test scores of schools chosen by parents for students who are below the threshold. Next, Panels B and D show this relationship once I control by the set of socioeconomic indicators. As can be seen in Panel B, including the set of controls significantly reduces the relationship between the running variable and the outcomes of interest for students who are below the cutoff. Aside from distance and class size, all the other coefficients are significantly reduced and are no longer significant. Estimates for students below the cutoff are used to determine what the outcomes would have looked like for students above the threshold had they been treated.[19]

Results are less encouraging for students who are above the cutoff. Although the relationship between the running variable and the outcomes of interest is significantly reduced once I include controls, the estimates are still statistically significant. Estimates for students above the threshold are used to predict what the outcomes for students below the threshold would have looked like had they not been treated. Thus, the estimates for students below the cutoff may be slightly downward biased. This shouldn't be much cause for concern given that the coefficients are small in magnitude.

I further complement this formal conditional independence assumption testing with a graphical tool that looks at the relationship between outcome residuals -after regressing outcomes on the conditional vector- and the running variable (Angrist and Rokkanen, 2015). If the conditional independence assumption is correct, then the relationship between outcome residuals and the running variable should be flat, except possibly for a jump at the cutoff. Results can be found in Figure 7, black lines depict a fourth order polynomial fit for control and treatment units separately, and grey dots represent the sample average for each disjoint bin. Consistent with the results reported in Table 8, the relationship between outcome residuals and running variable is essentially flat below the cutoff, except for distance. However, for individuals above the threshold, there is a small positive relationship between the running variable and residuals.

[19] Because the set of controls does not completely eliminate the relationship between class size, distance and the running variable, these estimates should be interpreted a little more carefully. Estimates for class size may be downward biased for students above the threshold, and estimates for distance may be upward biased for students above the threshold.

Results in Table 8 and Figure 7 indicate that the strategy will allow me to credibly determine what the outcomes would have been for students above the threshold, had they been treated. However, results on what the outcomes would have been for student below the threshold had they not been treated may be slightly upward biased, leading me to underestimate the impact of the treatment. This is because the conditioning vector is not able to fully remove the relationship between the running variable and the outcome of interest for individuals who are above the threshold.

Having evaluated the robustness of the conditional independence assumption, I proceed to estimate my results using linear reweighting and propensity score weighting. Results for the linear reweighing estimator are based on Kline (2011). Kline's reweighing estimator begins with linear models for conditional means, which can be written:

$$
\begin{aligned}
E[y_i|x_i, D_i = 0] &= x_i'\beta_0 \\
E[y_i|x_i, D_i = 1] &= x_i'\beta_1
\end{aligned}
\tag{3}
$$

This leads to the following matching style estimator at specific running variable values:

$$
E[Y_{1i} - Y_{0i}|r_i = c] = (\beta_1 - \beta_0)'E[x_i|r_i = c] \tag{4}
$$

Table 9 reports linear reweighing estimates of average treatment effects. I estimate both the average treatment effect on the treated (Panel A) and the average treatment effect on the un-treated (Panel B). Consistent with previous results, coefficients are very small in magnitude indicating that being below the cutoff for targeted vouchers didn't have an impact on this broader sample of students. In line with previous finding, results do show that targeted vouchers can lead parents to choose schools that charge higher add-ons to non-eligible parents. I also observe a negative effect on class size, and students' performance on language second grade tests. However, although significant, these coefficients are very small in magnitude indicating that access to targeted vouchers could have led parent to choose schools that are 1 percent smaller, and could have decreased test scores for students in 0.02 standard deviations. For students above the threshold, I observe that if they had been below the cutoff for targeted vouchers they would have chosen schools with somewhat higher add-ons, and done worse on second grade language tests by 0.03 standard deviations.

Figures 9 and 10 provide a visual evaluation of previous results by plotting linear reweighting estimates of $E[Y_{0i}|r_i = c]$ and $E[Y_{1i}|r_i = c]$ for all values of c. In Figure 9 the estimates of $E[Y_{1i}|r_i = c]$ to the left side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{0i}|r_i = c]$ (blue line) are an extrapolation based on equation 3. Instead, in Figure 10 the estimates of $E[Y_{0i}|r_i = c]$ to the right side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{1i}|r_i = c]$ (blue line) are an extrapolation.

The conditional means in the figures were constructed by plugging individual values of x_i into Equation 3 and smoothing the results using local linear regression. The figures present a picture consistent with that suggested by the estimates in Table 9, that is, small effects along all measured outcomes.

Finally, I complement previous results with a propensity score estimate approach. The logit model for the propensity score incorporates the control variables and parametrization used to construct tests in Table 8. The estimated propensity score distribution for individuals above and below the cutoff exhibits a substantial degree of overlap. This is documented in Figure 8, which plots the histogram of propensity score fitted values for treated and control observations above and below a common horizontal axis. The propensity-score-weighted estimates reported in the bottom half of Table 9 (Panels A and B), are consistent with the linear reweighting estimates shown in the first row of the Table.

6 Conclusion

Much of the debate over school vouchers revolves around the idea that voucher systems may lead to high levels of socioeconomic stratification. This is undesirable from a public perspective because socioeconomic segregation typically conveys school inequities and a loss in social cohesion. School inequities arise because schools with higher socioeconomic status students benefit from positive peer effects, higher quality teachers, more involved parents and more economic resources. At the same time, a loss in social cohesion occurs because segregation prevents students from different socioeconomic status from sharing a common experience in schools.

A question that remains open in the literature is whether, and to what extent, alternative voucher designs can help to overcome the socioeconomic segregation that is typically associated with voucher programs. In this paper I am able to address this question by looking at a reform in Chile where voucher amounts were increased by 50% for students in the lowest 40% of the income distribution. Using a unique dataset, I am able to exploit the fact that eligibility for targeted vouchers in Chile is a discontinuous function of a socioeconomic ranking and implement a regression discontinuity design. This allows me to estimate the impact that being eligible for a targeted voucher had on parents' school choices, and their consequential distribution across schools, as well as on eligible students' educational results.

Results show that being eligible for a targeted voucher had no impact on the observed characteristics of the schools chosen by parents. It had no impact on parents' probability of choosing a private school, the test scores of the chosen school, the socioeconomic status of the chosen school, the size of the chosen school, the average class size of the chosen school, or the distance traveled to school. Although I do observe that eligible parents choose schools that charge higher add-ons to non-eligible parents, the magnitude of this effect is negligible. There is also no evidence that eligible students are doing better than non-eligible students on a language test that is applied to second and fourth graders or a math test that is applied to fourth graders.

Two important conclusion can be drawn from this paper. First, eligible parents did not respond to the policy by choosing schools with significantly better observable characteristics. A result I argue is driven by both demand and supply side mechanisms. On the supply side, I observe that high test score private voucher schools abstained from participating in this policy. On the demand side I argue that low-income parents face other barriers, aside from costs and distance, that prevent them from attending higher socioeconomic status or higher test score schools. Barriers could include lack of information, complexities associated with evaluating a substantial number of school options, or issues of social belonging that lead parents to choose schools where their own social class is majority. Second, educational results did not improve in the eligibility margin, suggesting that schools did not respond to the policy by devoting more resources to eligible as opposed to non-eligible students.

Previous results contribute greatly to the empirical discussion on the role that targeted vouchers played in improving the educational outcomes of low-income students in Chile (MINEDUC, 2012; Correa et al., 2014; Villarroel, 2012; Mizala and Torche, 2013; Neilson, 2013; Navarro-Palau, 2017; Feigenberg et al., 2017). Using an improved identification strategy this paper is able to show that differentiated vouchers did not change students' distribution across schools and that they did not lead to an improvement in educational outcomes for eligible as opposed to non-eligible students. Further research is needed to determine whether the extra resources from this program did or did not contribute to the general increase in test scores for low-income students experienced during this period. However, this paper provides new evidence on the mechanisms that could be at work.

Findings are also extremely relevant from a policy perspective. The Chilean 2008 reform is one of the most important educational reforms to have been implemented in Chile in the last years, and efforts continue to be made to reduce the potential barriers that could be keeping low-income students from attending better performing private voucher schools. Previous results speak about the need for further intervention in order for progressive vouchers to help reduce socioeconomic stratification. In particular, it stands out the importance of enhancing private voucher school participation, either through mandatory regulation or others. It also stands out the need to implement information campaigns that can reduce some of the barriers that parents face when choosing a school, as it has been shown that even the more educated parents who have good private voucher schools within a reasonable distance, did not respond to the price decrease by choosing better schools. Other details that would be worth considering have to do with the the level of understanding that parents have about the program and how parents weigh the risk of loosing the benefit. As it is possible that parents in Chile might have a poor understanding of how the policy actually works, or that they might be afraid of loosing their benefit from one year to the next.

The Chilean experience is also highly relevant for a number of other countries considering an expansion of school choice systems. Empirical studies in Chile have shown how voucher systems can lead to increased segregation by socioeconomic status (Hsieh and Urquiola, 2003, 2006; McEwan et al., 2008). This study takes a step forward and is able to show that, without further intervention, alternative voucher designs, such as progressive vouchers, may prove to be ineffective in terms of addressing the high levels of socioeconomic segregation that may come as a result of voucher systems.

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7 Figures and Tables

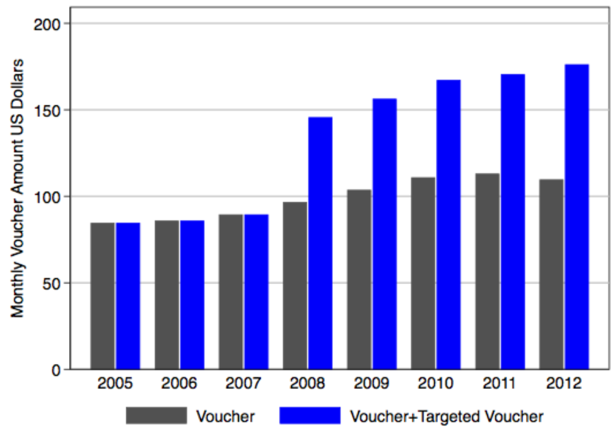


Figure 1: Voucher amounts over time

This figure shows how the voucher amount evolved over time for students who were non -eligible for targeted vouchers and students who were eligible for targeted vouchers. All amounts are in 2012 US dollars and represent a month of transfer. The voucher presented is for students in first grade at schools with full shift. Source: Ministry of Education.

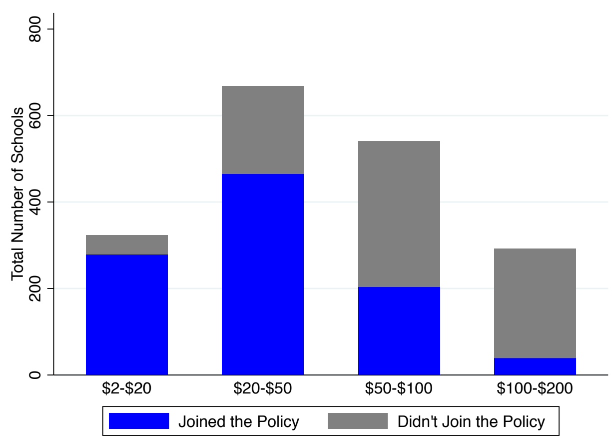


Figure 2: Private voucher schools that had and had not joined the policy in 2012 by monthly add-ons charged to parents

This figure shows the number of private voucher schools serving primary education that had and had not joined the policy in 2012 depending on the monthly voucher that they charged to parents. Source: Ministry of Education.

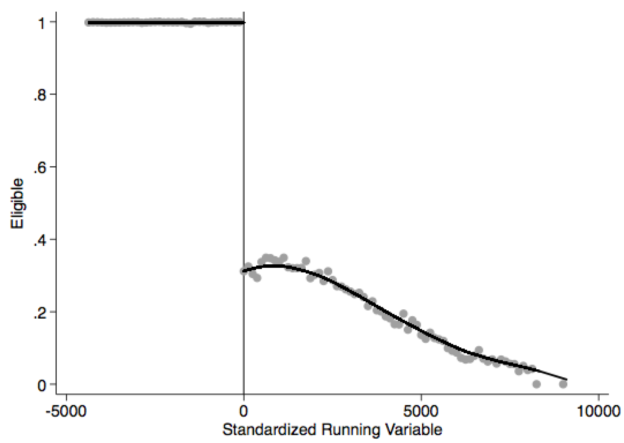


Figure 3: First Stage

Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

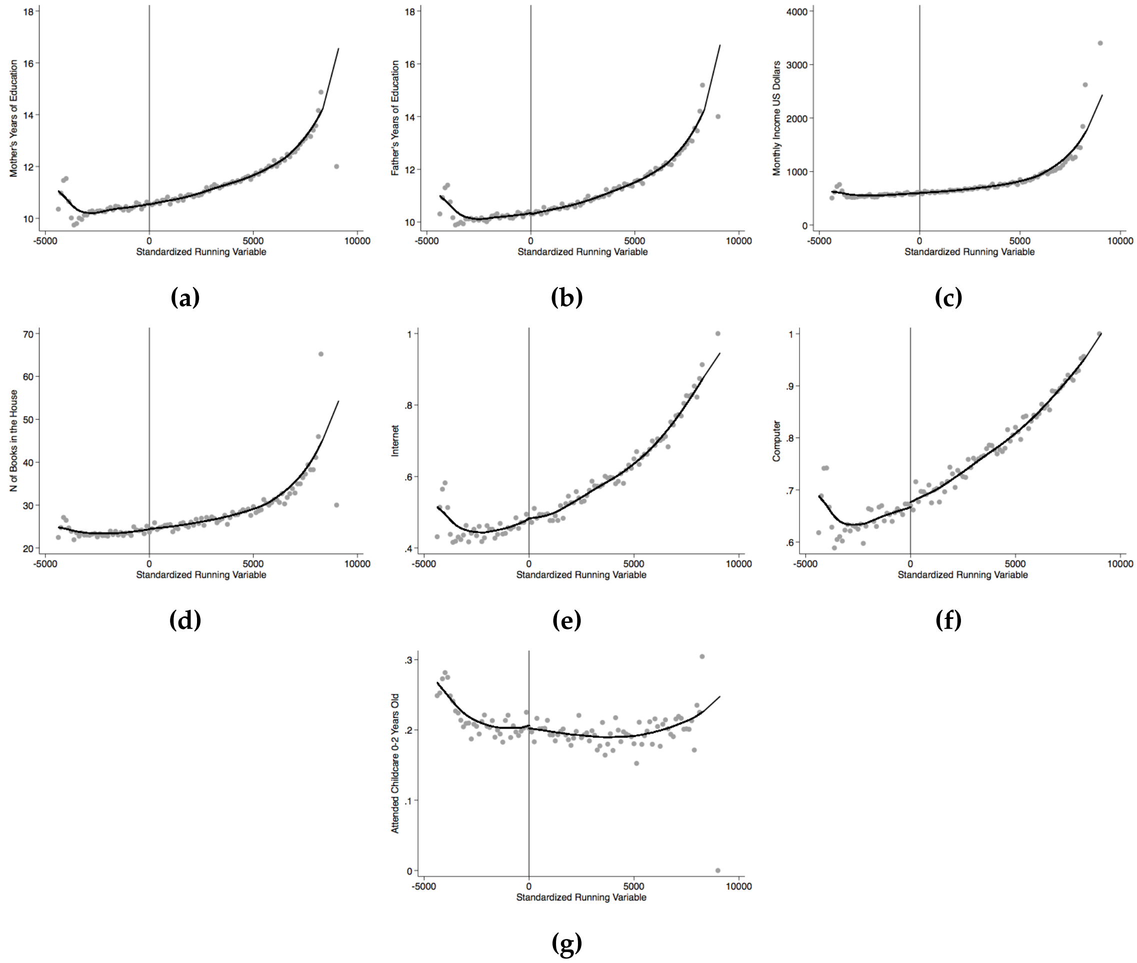
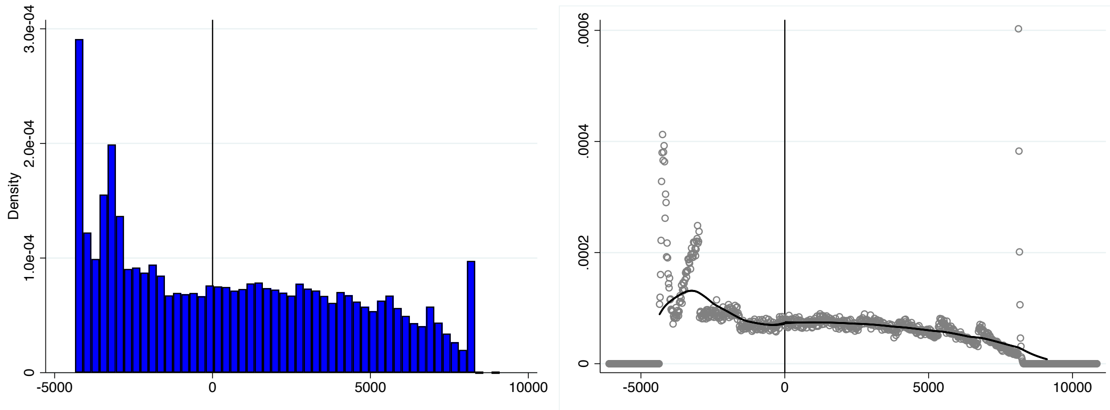


Figure 4: Visual Evaluation of Robustness Checks I

Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.



(a) **(b)**

Figure 5: Visual Evaluation of Robustness Checks

Notes: McCrary r(bandwidth) = 1742.61 r(binsize) = 17.373 r(se) = .020256 r(theta) = .01413

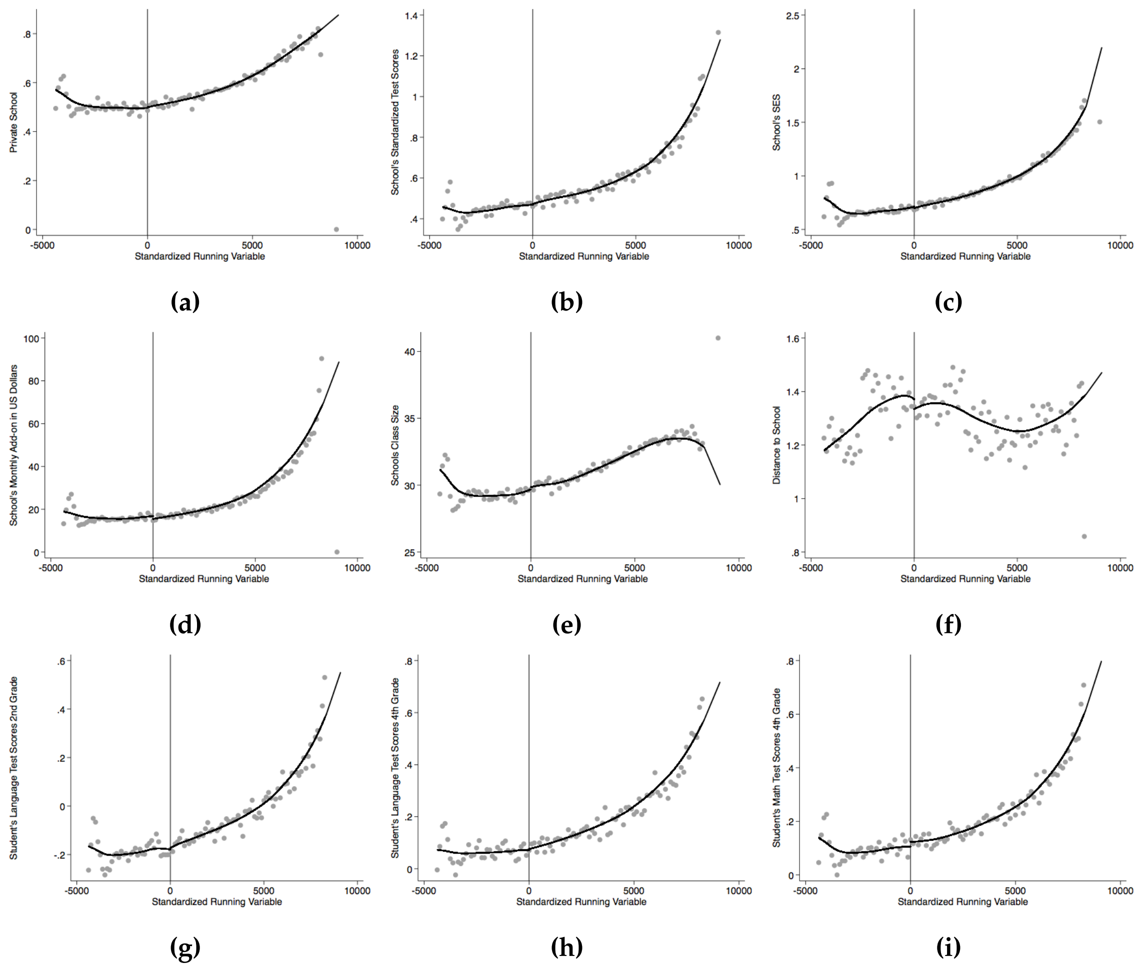


Figure 6: Visual Evaluation of Results

Grey dots present the average in the outcome variable for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

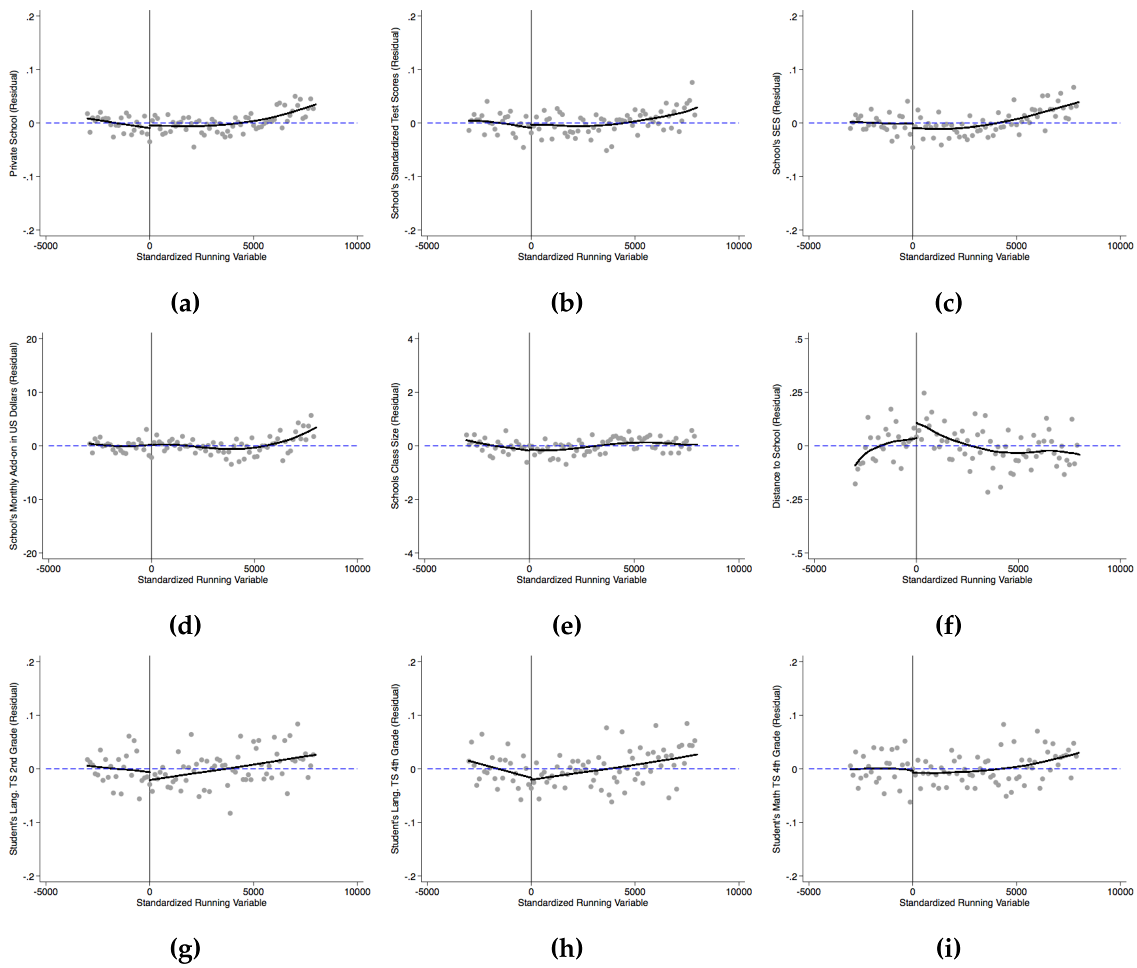


Figure 7: CIA test

Grey dots present the average in the outcome residuals (after regressing outcomes on the conditional vector) for individuals in equally spaced disjoint bins. Black lines depict a fourth order polynomial fit for control and treatment units separately.

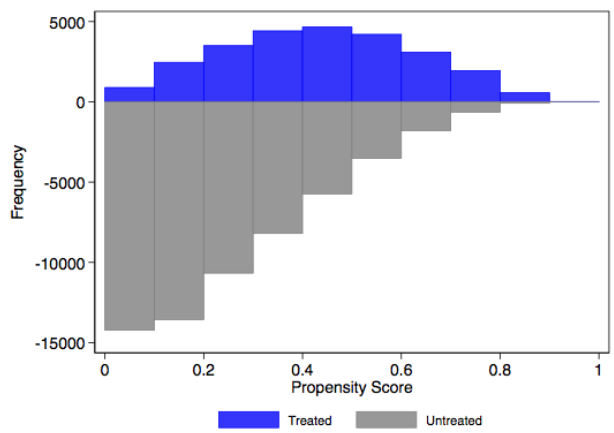


Figure 8: Histogram of estimated propensity score in the window [-3000,8000]

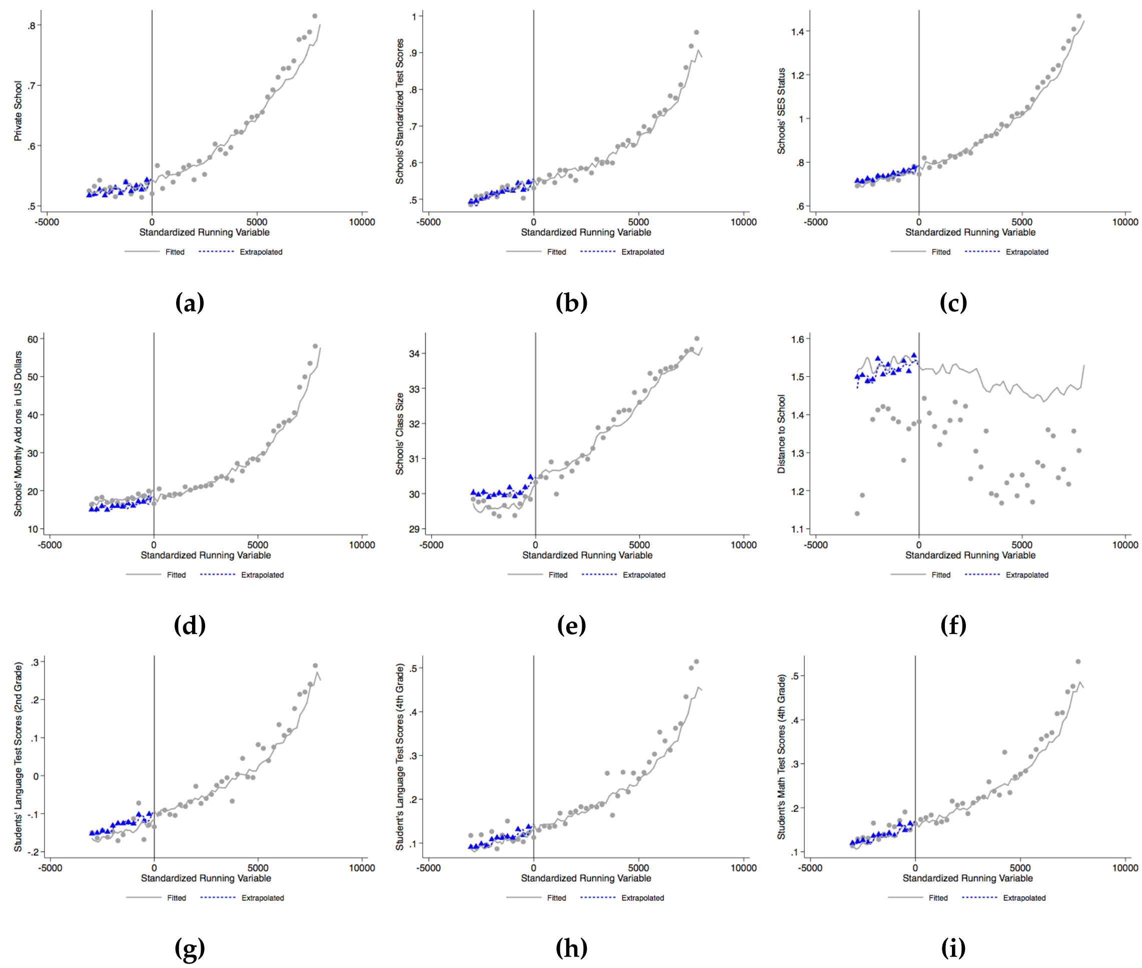


Figure 9: CIA-based Estimates Below the Cutoff

Conditional Independent Assumption based estimates of $E[Y_{0i}|r_i = c]$ and $E[Y_{1i}|r_i = c]$ for all values of c. Estimates of $E[Y_{1i}|r_i = c]$ to the left side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{0i}|r_i = c]$ (blue line) are an extrapolation.

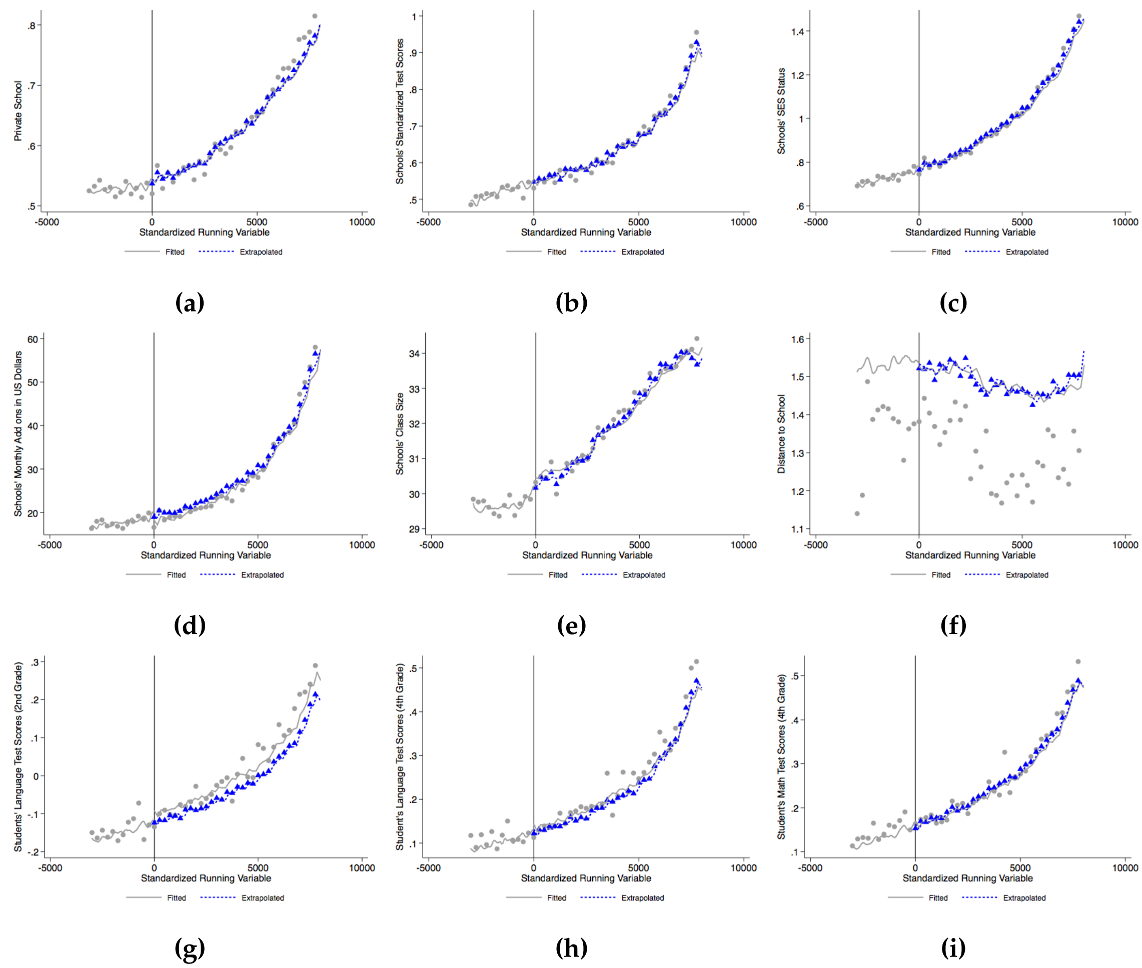


Figure 10: CIA-based Estimates Above the Cutoff

Conditional Independent Assumption based estimates of $E[Y_{0i}|r_i = c]$ and $E[Y_{1i}|r_i = c]$ for all values of c. Estimates of $E[Y_{0i}|r_i = c]$ to the right side of the cutoff (grey line) are fitted values from regression models for observed outcomes, while the estimates of $E[Y_{1i}|r_i = c]$ (blue line) are an extrapolation.

Table 1: Schools' Characteristics

	(1) Test Scores	(2) Test Scores Language	(3) Test Scores Math	(4) SES	(5) Add-on	(6) Size	(7) Class Size
Public (57%)	249.8	256.2	242.8	9.0	0.0	23.3	16.5
	(27.7)	(28.3)	(31.1)	(1.8)	(0.0)	(25.7)	(12.6)
Private Voucher w/No Add-On that joined the policy (19%)	247.4	256.0	238.4	9.1	0.0	25.1	18.0
	(28.1)	(27.6)	(32.5)	(2.2)	(0.0)	(28.3)	(14.2)
Private Voucher w/Add-On that joined the policy (13%)	261.6	266.0	256.8	11.6	41.0	53.4	31.6
	(21.3)	(20.4)	(23.7)	(1.1)	(31.3)	(38.5)	(9.9)
Private Voucher w/Add-On that didn't join the policy (11%)	272.9	276.8	268.8	12.9	86.0	55.7	30.5
	(19.6)	(18.6)	(22.0)	(1.1)	(47.0)	(39.5)	(9.5)

Includes all subsidized primary schools in 2012. Test score equals the average result of the schools on the 4th grade standardized test in 2011, SES equals the average years of education of mothers' of students attending those schools, add-on equals the total amount charged to non-eligible parents in those schools, school size equals the cohort size at those schools, and class size equals the average class size at those school.

Table 2: First Stage 2012

	Eligible
$R \leq Cutoff$	0.695
	(0.00843)
Mean Control	0.304
Observations	28,119

Results from rdrobust (Calonico et al., 2014).

Table 3: Robustness Check

	(1) Mother's Education	(2) Father's Education	(3) Income	(4) Books	(5) Internet	(6) Computer	(7) Attended Childcare (0-2)
$R \leq Cutoff$	0.0473	0.0644	-10.79	0.266	0.00378	0.00343	0.0235
	(0.0956)	(0.0964)	(15.23)	(0.663)	(0.0157)	(0.0158)	(0.0130)
Mean Control	10.51	10.31	605.1	24.56	0.484	0.672	0.198
Observations	17,360	18,167	21,572	24,796	19,066	16,612	18,366

Results from rdrobust (Calonico et al., 2014). Standard errors in parenthesis.

Table 4: School Choice and Educational Outcomes

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: Impact of Being Below the Cutoff (Reduced Form)									
$R \leq Cutoff$	0.00655	0.00919	0.0241	2.060	0.0599	0.0379	-0.0242	-0.0163	-0.0308
	(0.0128)	(0.0180)	(0.0169)	(0.758)	(0.283)	(0.0702)	(0.0236)	(0.0239)	(0.0219)
Mean Control	0.497	0.467	0.693	15.31	29.72	1.356	-0.170	0.0827	0.135
Observations	26,335	27,695	23,519	30,426	23,339	10,927	32,625	26,748	27,064
Panel B: Impact of Being Eligible for a Targeted Voucher (IV)									
Eligible	0.00629	0.00553	0.0339	2.978	0.0542	0.0502	-0.0373	-0.0252	-0.0393
	(0.0173)	(0.0238)	(0.0263)	(1.104)	(0.369)	(0.0995)	(0.0349)	(0.0323)	(0.0292)
Mean Control	0.497	0.467	0.693	15.31	29.72	1.356	-0.170	0.0827	0.135
Observations	30029	32964	20515	29794	28532	10264	30691	29870	31048

Results from rdrobust (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. Panel A contains reduced from estimates and Panel B contains instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher.

Table 5: Educational Outcomes

	(1) Student Language 2nd Grade	(2) Student Language 4th Grade	(3) Student Math 4th Grade
Panel A: First Stage-Years Eligible			
$R \leq Cutoff$	0.643	0.748	0.748
	(0.0147)	(0.0346)	(0.0346)
Mean Control	1.329	2.932	2.932
Observations	15,507	10,866	10,866
Panel B: Impact of Being Eligible for an extra year of Targeted Voucher (IV)			
Years Eligible	-0.0432	-0.00955	-0.0634
	(0.0412)	(0.0362)	(0.0380)
Mean Control	-0.170	0.0827	0.135
Observations	24948	18512	15076

Results from rdrobust (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. Panel A contains first stage estimates and Panel B contains instrumental variable estimates, where the discontinuity is used as an instrument for number of years eligible for a targeted voucher.

Table 6: School Choice and Educational Outcomes: Heterogeneous effects by mothers' education

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Mother has less than High School Education									
Eligible	-0.0439	0.0586	-0.00493	0.448	-0.281	0.119	-0.0310	0.0461	-0.0262
	(0.0297)	(0.0435)	(0.0493)	(1.302)	(0.693)	(0.214)	(0.0620)	(0.0610)	(0.0506)
Mean Control	0.380	0.332	0.455	6.233	27.42	1.297	-0.330	-0.0892	-0.0373
Observations	11,683	11,976	6,489	7,452	11,379	2,825	11,551	9,137	12,899
Mother has High School Education or Tertiary Education									
Eligible	0.0288	-0.0279	0.0493	4.339	-0.329	-0.0751	-0.0484	-0.0664	-0.0319
	(0.0200)	(0.0337)	(0.0283)	(1.533)	(0.456)	(0.114)	(0.0442)	(0.0448)	(0.0410)
Mean	0.623	0.658	0.943	24.46	32.03	1.468	0.00357	0.226	0.291
Observations	19,432	13,387	14,748	21,484	14,422	6,585	16,431	14,728	14,178

Results from rdrobust (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. All columns contain instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher.

Table 7: School Choice and Educational Outcomes: Heterogeneous effects by distance to nearest private voucher school with add-ons that joined the policy

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Nearest P. Voucher School with Add-ons is less than 0.4 miles away									
Eligible	0.0140	0.0187	0.0244	4.086	-0.256	0.0625	-0.0964	-0.111	-0.0310
	(0.0331)	(0.0556)	(0.0347)	(2.092)	(0.533)	(0.132)	(0.0703)	(0.0781)	(0.0656)
Mean Control	0.636	0.438	0.893	22.89	33.86	1.200	-0.127	0.153	0.176
Observations	6,969	5,543	8,488	9,280	8,520	4,933	7,136	5,121	5,855
Nearest P. Voucher School with Add-ons is more than 0.4 miles away									
Eligible	0.0533	0.0482	-0.0188	2.519	0.213	-0.0180	0.0898	0.0984	-0.00310
	(0.0373)	(0.0421)	(0.0479)	(2.341)	(0.913)	(0.114)	(0.0701)	(0.0780)	(0.0642)
Mean Control	0.527	0.435	0.816	17.38	31.47	1.589	-0.179	0.0600	0.156
Observations	5,852	9,983	5,293	6,957	3,779	8,866	6,966	4,201	5,478

Results from rdrobust (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. All columns contain instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher.

Table 8: Conditional Independence Test

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: Below the cutoff without Controls									
Running Variable	-0.0014 (0.0035)	0.0141 (0.0051)	0.0238 (0.0049)	0.7383 (0.2525)	0.0170 (0.0767)	0.0282 (0.0183)	0.0135 (0.0073)	0.0037 (0.0071)	0.0173 (0.0066)
Mean	0.528	0.517	0.727	17.728	29.686	1.363	-0.143	0.112	0.145
Observations	25,728	24,991	24,950	25,600	25,325	10,208	23,451	21,377	21,420
Panel B: Below the cutoff with Controls									
Running Variable	-0.0063 (0.0032)	-0.0045 (0.0047)	-0.0012 (0.0039)	-0.0811 (0.2147)	-0.1372 (0.0647)	0.0413 (0.0182)	-0.0041 (0.0072)	-0.0110 (0.0071)	-0.0009 (0.0065)
Mean	0.528	0.517	0.727	17.728	29.686	1.363	-0.143	0.112	0.145
Observations	25,728	24,991	24,950	25,600	25,325	10,208	23,451	21,377	21,420
Panel C: Above the cutoff without Controls									
Running Variable	0.0332 (0.0009)	0.0404 (0.0013)	0.0776 (0.0012)	3.8766 (0.0835)	0.5601 (0.0181)	-0.0240 (0.0046)	0.0432 (0.0019)	0.0386 (0.0018)	0.0376 (0.0017)
Mean	0.619	0.640	0.967	26.958	31.937	1.304	0.004	0.229	0.260
Observations	58,450	56,792	56,713	58,207	57,682	22,123	54,885	51,044	51,260
Panel D: Above the cutoff with Controls									
Running Variable	0.0058 (0.0010)	0.0049 (0.0014)	0.0089 (0.0012)	0.1970 (0.0861)	0.0760 (0.0189)	-0.0217 (0.0053)	0.0085 (0.0022)	0.0085 (0.0021)	0.0058 (0.0020)
Mean	0.619	0.640	0.967	26.958	31.937	1.304	0.004	0.229	0.260
Observations	58,450	56,792	56,713	58,207	57,682	22,123	54,885	51,044	51,260

This table reports regression-based tests of the conditional independence assumption described in the text. Panels B and C show the coefficient on the running variable in models that control for mothers' education, fathers' education, household income, municipality of residence and child's gender. Estimates use only observations to the left or right of the cutoff as indicated in column headings. Robust standard errors are reported in parentheses.

Table 9: Conditional Independence Results

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: Below the cutoff									
Linear Reweighting	0.0021	0.0015	-0.0108	1.6627	-0.383	0.0179	-0.0186	-0.0071	-0.0046
	(0.0039)	(0.0054)	(0.0047)	(0.2395)	(0.0787)	(0.0398)	(0.0088)	(0.0085)	(0.0077)
N untreated	58450	56792	56713	58207	57682	22123	54885	51044	51260
N treated	25728	24991	24950	25600	25325	10208	23451	21377	21420
Reweighting	-0.0103	-0.0091	-0.0293	0.3893	-0.5018	0.0054	-0.0281	-0.0153	-0.0103
	(0.0042)	(0.0056)	(0.0052)	(0.2405)	(0.0883)	(0.0265)	(0.0091)	(0.0089)	(0.0078)
N untreated	58434	56774	56695	58191	57666	22095	54862	51031	51246
N treated	25726	24989	24948	25598	25323	10163	23450	21374	21417
Panel B: Above the cutoff									
Linear Reweighting	-0.0004	0.0006	0.0047	1.0031	-0.0230	0.0017	-0.0329	-0.0108	0.0033
	(0.0046)	(0.0064)	(0.0060)	(0.4513)	(0.0927)	(0.0362)	(0.0105)	(0.0109)	(0.0101)
N untreated	58450	56792	56713	58207	57682	22123	54885	51044	51260
N treated	25728	24991	24950	25600	25325	10208	23451	21377	21420
Reweighting	-0.0010	-0.0022	-0.0036	1.1981	-0.1813	0.0461	-0.0339	-0.0160	-0.0002
	(0.0049)	(0.0070)	(0.0072)	(0.5473)	(0.1042)	(0.0240)	(0.0111)	(0.0113)	(0.0109)
N untreated	58434	56774	56695	58191	57666	22095	54862	51031	51246
N treated	25726	24989	24948	25598	25323	10163	23450	21374	21417

This table reports CIA estimates of the effect of being eligible for targeted vouchers on school choice and educational outcomes. Panel A reports results for students below the cutoff and Panel B reports results for student below the cutoff. In each panel the first row reports results from a linear reweighting estimator, and the second row reports results from inverse propensity score weighting, as described in the text. Controls are the same as used to construct the test statistic reported in Table 8. Standard errors (shown in parentheses) were computed using a nonparametric bootstrap with 500 replications. The number of treated and untreated (above and below the cutoff) observations in the relevant outcome samples appear below standard errors.

A For Online Publication: Test Scores Before and After Targeted Vouchers

This section reviews how test scores evolved during the 2005-2012 period for eligible versus ineligible students. Estimates indicate that by 2012, the gap in test scores between these two groups had decreased by roughly 0.08 standard deviations. Results can be found in Figure A.1 and Table A.1. Previous studies that have looked at this result include Neilson (2013) and Feigenberg et al. (2017). In both cases, the authors have to make some assumption about which students would have been eligible in the past, because data on eligibility is only available as of 2008. Given that I have data on the socioeconomic ranking for the whole population of students in 2012, I can easily construct a measure of eligibility for previous cohorts by characterizing as eligible those students who in 2012 were below the threshold for eligibility. Estimates using this improved measure of eligibility for previous cohorts indicate that the difference in test scores between eligible and ineligible students decreased in this period, but that changes are below those reported by previous studies that are of the order of 0.2 standard deviations.

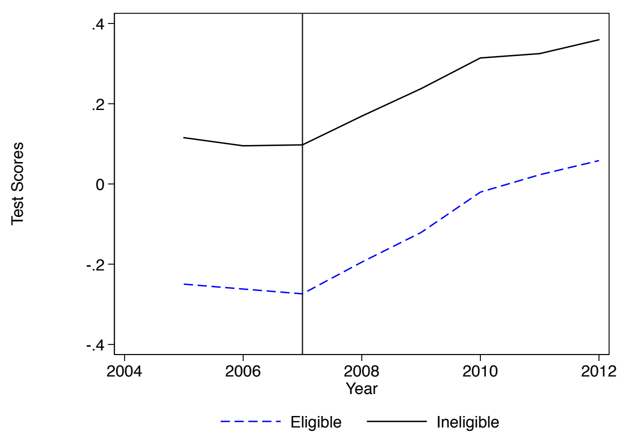


Figure A.1: Test Scores Before and After Targeted Vouchers

Test scores equal the students' performance on standardized Math and Language tests that are applied nationwide to students in 4th grade.

Table A.1: Test Scores Before and After Targeted Vouchers

	(1) Test Scores
Student Eligible	-0.104
	(0.00612)
Student Eligible x 2006	0.0311
	(0.00743)
Student Eligible x 2007	0.0189
	(0.00788)
Student Eligible x 2008	0.0306
	(0.00761)
Student Eligible x 2009	0.0208
	(0.00797)
Student Eligible x 2010	0.0548
	(0.00767)
Student Eligible x 2011	0.0932
	(0.00772)
Student Eligible x 2012	0.0843
	(0.00794)

Estimates are at the student level. Test scores equal the students' performance on standardized math and language tests that are applied nationwide to students in 4th grade. Estimates include year fixed effects and controls for mother's education, father's education and income. Standard errors are clustered at the school level.

B Appendix: School characteristics in 2007

This section presents average school characteristics in 2007 for schools that in 2012: where public; where private voucher and not charging add-ons to parents; where private voucher, charging add-ons to parents, and had joined the policy; where private voucher, charging add-ons to parents, and had not joined the policy.

Table B.1: Schools' Characteristics in 2007

	(1) Test Scores	(2) Test Scores Language	(3) Test Scores Math	(4) SES	(5) Add-on	(6) Size	(7) Class Size
Public (57%)	235.8	242.6	228.5	9.4	0.0	26.5	17.4
	(27.5)	(27.5)	(30.5)	(1.8)	(0.0)	(30.0)	(13.3)
Private Voucher w/No Add-On	231.7	240.4	222.7	9.4	0.0	25.9	18.3
that joined the policy (19%)	(29.0)	(28.7)	(32.1)	(2.2)	(0.0)	(29.8)	(14.6)
Private Voucher w/Add-On	256.0	259.5	252.1	12.3	41.8	54.5	32.7
that joined the policy (13%)	(22.9)	(22.2)	(24.7)	(1.2)	(34.2)	(37.9)	(9.9)
Private Voucher w/Add-On	264.3	267.3	260.9	13.1	69.4	57.2	31.0
that didn?t join the policy (11%)	(22.7)	(21.8)	(24.7)	(1.2)	(45.9)	(43.0)	(9.3)

Includes all subsidized primary schools in 2012.Test score equals the average result of the schools on the 4th grade standardized test in 2007, SES equals the average years of education of mothers' of students attending those school, add-on equal the total amount charged to non-eligible parents in those school, school size equals the cohort size at those school, class size equals the average class size at those school.

C Appendix: Estimates with Alternative Bandwidths

This section presents the main results form this study using alternative bandwidths. Results from Table 4 use optimal bandwidths computed using Calonico et al. (2014). These optimal bandwidths range between 800 to 1500 points. In this section I present estimates using a 500, 1000, 1500 and 2000 bandwidths. All estimates are for the effect of being eligible for a targeted voucher, where the discontinuity is used as an instrument for eligibility.

Table C.1: School Choice and Educational Outcomes with Alternative Bandwidths

	(1) School Private	(2) School Test Scores	(3) School SES	(4) School Add-on	(5) School Class Size	(6) School Distance (Miles)	(7) Student Language 2nd Grade	(8) Student Language 4th Grade	(9) Student Math 4th Grade
Panel A: 500 Bandwidth									
Eligible	0.0299	0.0476	0.0462	4.037	0.165	0.0141	-0.0280	-0.0251	-0.107
	(0.0263)	(0.0387)	(0.0337)	(1.680)	(0.551)	(0.143)	(0.0606)	(0.0557)	(0.0512)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	6569	6370	6358	6540	6467	2489	5114	4815	4845
Obs right	6879	6669	6659	6846	6770	2627	5421	5125	5148
Panel A: 1000 Bandwidth									
Eligible	0.00921	0.0148	0.0338	3.092	0.0892	0.0540	-0.0366	-0.0106	-0.0526
	(0.0184)	(0.0270)	(0.0234)	(1.175)	(0.387)	(0.0969)	(0.0421)	(0.0392)	(0.0361)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	12865	12464	12439	12811	12653	4761	10034	9493	9525
Obs right	13470	13056	13040	13408	13258	5174	10640	10125	10178
Panel C: 1500 Bandwidth									
Eligible	0.00125	0.00161	0.0223	2.903	-0.219	0.0487	-0.0365	-0.0251	-0.0403
	(0.0151)	(0.0221)	(0.0191)	(0.968)	(0.318)	(0.0785)	(0.0346)	(0.0324)	(0.0298)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	19126	18528	18497	19043	18821	7046	14871	14094	14110
Obs right	20640	19991	19965	20542	20314	7882	16370	15580	15640
Panel D: 2000 Bandwidth									
RD_Estimate	-0.00260	-0.00294	0.0140	2.698	-0.318	0.0365	-0.0225	-0.0203	-0.0332
	(0.0132)	(0.0193)	(0.0166)	(0.844)	(0.277)	(0.0670)	(0.0302)	(0.0284)	(0.0261)
Mean	0.616	0.666	1.019	38.558	30.615	1.313	-0.097	0.236	0.273
Obs left	27274	26394	26350	27158	26816	10917	21142	20027	20031
Obs right	27390	26519	26480	27261	26950	11159	21795	20800	20853

Results from rdrobust (Calonico et al., 2014). All estimates include controls for mother's education, father's education and region. All cells contains instrumental variable estimates, where the discontinuity is used as an instrument for being eligible for a targeted voucher in 2012.

D Appendix: Robustness Checks for heterogeneous results

Table D.1: Robustness Check:Heterogeneous effects by mothers' education

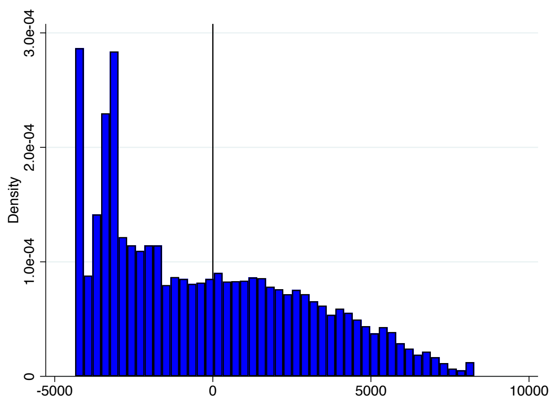
	(1) Mother's Education	(2) Father's Education	(3) Income	(4) Books	(5) Internet	(6) Computer	(7) Attended Childcare (0-2)
Mother has less than High School Education							
$R \leq Cutoff$	-0.0249	0.00177	-0.192	1.326	0.00904	-0.0164	0.0488
	(0.0960)	(0.115)	(13.23)	(0.922)	(0.0242)	(0.0241)	(0.0198)
Mean Control	7.877	8.758	421.1	17.14	0.326	0.524	0.157
Observations	10,217	11,702	9,267	7,995	7,110	7,927	7,037
Mother has High School Education or Tertiary Education							
$R \leq Cutoff$	-0.0471	0.0279	-28.41	-0.510	-0.00464	0.00976	0.00802
	(0.0406)	(0.0901)	(21.99)	(0.963)	(0.0174)	(0.0137)	(0.0145)
Mean Control	12.66	11.53	753.0	29.63	0.599	0.791	0.224
Observations	13,671	14,824	13,773	13,653	15,154	16,708	15,870

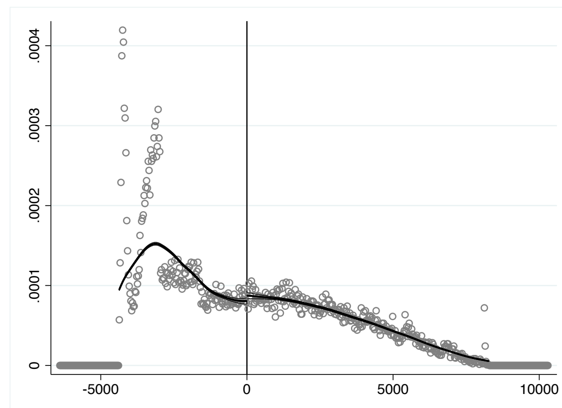
Results from rdrobust (Calonico et al., 2014). Mother's education and father's education equal total years of education. Books equal the total amount of books in the house and Internet and Computer are dummy variables that equal one if the family has internet and/or a computer. Attended Childcare is a dummy variable that equals one if the child attended childcare when 0-2 years old and when 3-4 years old. Standard errors in parenthesis.

Table D.2: Robustness Check:Heterogeneous effects by distance to nearest private voucher school with add-ons that joined the policy

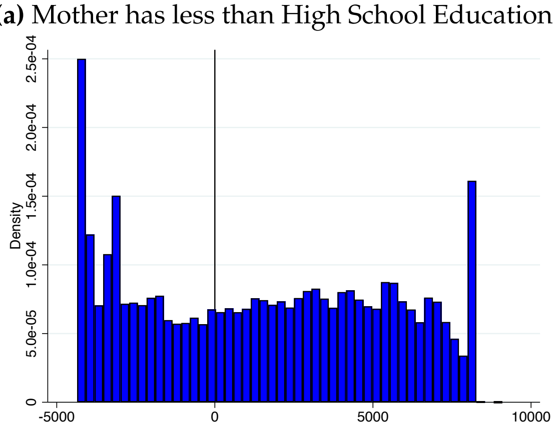
	(1) Mother's Education	(2) Father's Education	(3) Income	(4) Books	(5) Internet	(6) Computer	(7) Attended Childcare (0-2)
Nearest P. Voucher School with Add-ons is less than 0.4 miles away							
$R \leq Cutoff$	0.0993	0.0664	-14.74	-0.122	0.0287	0.0192	-0.00345
	(0.165)	(0.170)	(27.43)	(1.519)	(0.0276)	(0.0207)	(0.0234)
Mean Control	11.01	10.72	682.3	27.70	0.572	0.728	0.221
Observations	4,766	5,717	7,347	5,268	6,060	8,817	5,674
Nearest P. Voucher School with Add-ons is more than 0.4 miles away							
$R \leq Cutoff$	-0.0971	-0.136	-22.45	-0.666	-0.0341	-0.0420	0.0219
	(0.165)	(0.189)	(35.21)	(1.444)	(0.0319)	(0.0283)	(0.0238)
Mean Control	10.78	10.59	668.9	27.02	0.575	0.738	0.215
Observations	5,787	4,560	4,738	5,671	4,421	4,700	5,692

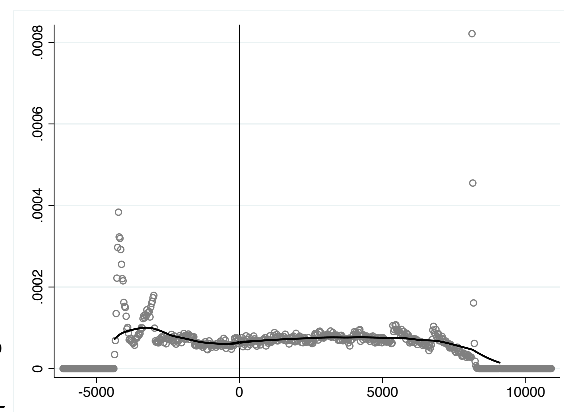
Note: Results from rdrobust (Calonico et al., 2014). Mother's education and father's education equal total years of education. Books equal the total amount of books in the house and Internet and Computer are dummy variables that equal one if the family has internet and/or a computer. Attended Childcare is a dummy variable that equals one if the child attended childcare when 0-2 years old and when 3-4 years old. Standard errors in parenthesis.





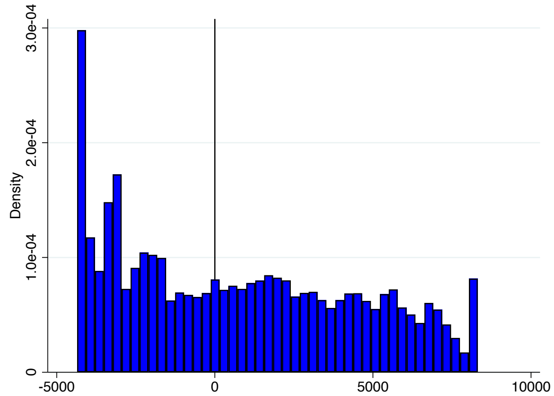
(a) Mother has less than High School Education

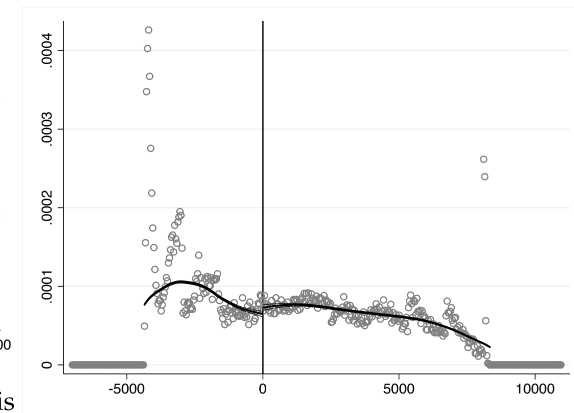




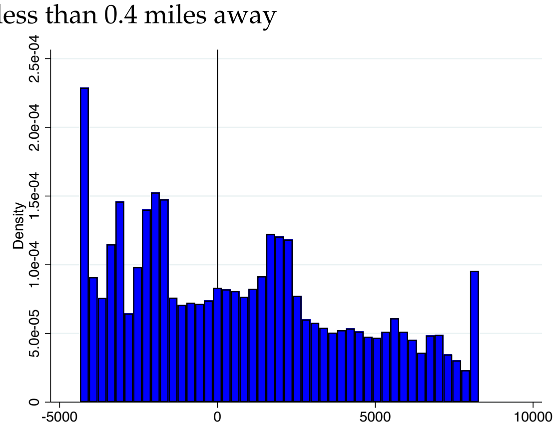
(b) Mother has High School Education or Tertiary Education

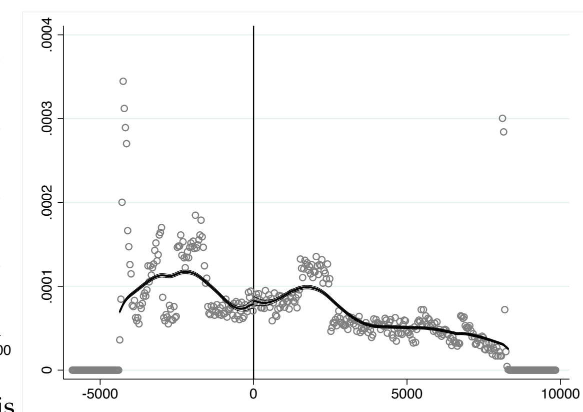
Figure D.1: Visual Evaluation of Robustness Checks:Heterogeneous effects by mothers' education





(a) Nearest P. Voucher School with Add-ons is less than 0.4 miles away





(b) Nearest P. Voucher School with Add-ons is more than 0.4 miles away

Figure D.2: Visual Evaluation of Robustness Checks: Heterogeneous effects by distance to nearest private voucher school with add-ons that joined the policy