Private School Vouchers in Colombia

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In the early 1990's, secondary school enrollments amongst the poorest 20 percent of Colombia's population were only 55 percent. By contrast, 89 percent of the richest 20 percent of Colombia's population were attending secondary school and 75 percent of the overall population were enrolled (Sanchez and Mendes, 1995). As policymakers grappled with how to increase poor student enrollments and lessen the enrollment gap, they also faced an overburdened and overcrowded public school system. In Colombia, the average school day is four hours, and most public schools hosted multiple sessions of school per day. Only 2 percent of public secondary schools were hosting only one session per day, and almost 20 percent of public secondary schools were hosting three sessions per day. In lieu of these multiple sessions at each school, the World Bank (1993) noted that many schools could not facilitate additional enrollments despite projected enrollment growth.

In 1991, Colombia attempted to improve enrollment rates through a unique partnership between the public and private sectors (King, Orazem, and Wohlgemuth, 1998). The program, called the Plan de Ampliación de Cobertura de la Educación Secundaria (PACES), sought to take advantage of excess capacity in the private sector. The Colombian government issued private school vouchers for students entering 6th grade, the start of Colombian secondary school. The vouchers targeted the poorest third of the population and were renewable so long as the recipient made adequate progress towards secondary school graduation.

By 1997, PACES had grown into one of the world's largest private school voucher programs. Over 125,000 PACES vouchers had been awarded. While the program was large relative to other voucher programs, the program was small relative to the overall secondary school system. In 1995, approximately 3.1 million students

attended secondary schools in Colombia with about 37 percent of them in private schools.

One of the distinctive elements of PACES is its use of lotteries. From the beginning, the demand for PACES vouchers far exceeded the supply. PACES required the use of lotteries to allocate vouchers when there was excess demand. These lotteries created natural "control" and "treatment" groups similar to a randomized trial. Students who applied unsuccessfully to the voucher lottery form an unbiased comparison group for students who won the voucher lottery. Comparing the academic and non-academic outcomes of students involved in the voucher lottery can show the effects of the voucher program.

There have been two major studies utilizing these voucher lotteries to measure PACES' effects. The first study was conducted by Josh Angrist, Erik Bloom, Elizabeth King, Michael Kremer and me (Angrist et al. 2002). During 1998 and 1999, we attempted almost 3000 surveys of students who had applied for PACES' vouchers in selected cities throughout Colombia. The data from these surveys showed that after three years voucher lottery winners scored about 0.2 standard deviations higher on standardized exams, were 15 percentage points more likely to have attended private school, and were about five percentage points less likely to have repeated a grade in secondary school. Because of the reduced grade repetition, voucher winners had completed 0.1 more years of schooling. The vouchers, however, did not significantly affect dropout rates.

While the results of the first study were compelling, as I discuss below, there were reasons to doubt whether the voucher program had led to long-run differences in outcomes for voucher students. As a follow-up to the first study, Josh Angrist, Michael Kremer, and I pursued a longer-run follow up focusing on high school graduation and the

college entrance exam (Angrist et al forthcoming). In this study, we matched PACES application data to administrative records from Colombia's college entrance exam, the ICFES (Instituto Colombiano Para El Fomento De La Educación Superior). The results were striking. Voucher lottery winners were about 20 percent more likely to take college entrance exams than unsuccessful applicants. Not only were they more likely to take the exam, but they scored higher on the exam.

The present paper seeks to review these evidences from Colombia. Section 1 of the paper provides additional background information on the Colombia voucher program. Section 2 describes the data sources and methodologies used in these evaluations. Section 3 provides an overview of evidence on the vouchers' effectiveness after three years. Section 4 discusses the impact of the voucher on college entrance exams. Section 5 discusses possible mechanisms by which the voucher may have affected student outcomes, including possible puzzles raised in the evaluation of the Colombian voucher project. Section 6 concludes.

I. PACES Background

The Colombian government established the PACES program in November 1991. The program was part of a larger effort to decentralize public services and to expand private sector provision (King, *et al*, 1997). The Colombian government advertised the program in local newspapers and through radio ads, and the program immediately proved popular. In the first year of the program in Bogotá alone, 14,607 students applied for the program.

In order to improve enrollment rates among the poorest families in Colombia, PACES targeted low-income families (King, Orazem, and Wohlgemuth, 1998). To

qualify for the voucher, parents had to present a utility bill proving that they lived in one of the two lowest socioeconomic strata (out of 6 possible strata). Research by Morales-Cobo (1993) suggests that this targeting was effective in Bogotá.

To be eligible for the voucher, children had to be entering 6th grade, the start of Colombian secondary schools, and be under the age of 16. Children were also only eligible if they had been attending public school in the previous year and had already arranged admission at a participating private secondary school. Not all private schools participated in the program. Only about 40 percent of private schools actually accepted the voucher, and schools that typically participated were not elite schools but rather low tuition schools serving low income populations. King, Rawlings, Gutierrez, Pardo and Torres (1997) investigated differences between public secondary schools and participating private schools. They find that pupil-teacher ratios, test scores, and access to technology were similar across schools. The schools also had similar median scores on the ICFES exam.

Students could use the vouchers at both academic and vocational schools. Vocational schools, including some for-profit schools, were over-represented in the group of participating private schools although after 1996 for-profit were excluded from the voucher program. Because students were accepted at a private school prior to the lottery, we can actually dichotomize the lottery into two parts: students who had already been accepted at a vocational school and students who had already been accepted at an academic school. As I discuss below, in ongoing work with Michael Kremer and Juan Saavedra, we use this information to shed light on mechanisms by which the voucher may have affected student outcomes.

So long as students were promoted at the end of a grade, they could automatically

renew their voucher through eleventh grade, the end of Colombian high school. Students failing a grade were supposed to be dropped from the PACES program. Calderon (1996) shows that about 77 percent of recipients renewed their vouchers. Additionally, the rules of the voucher allowed students to transfer to other schools with the voucher; however, our data suggests that few students who transferred schools kept their vouchers.

The voucher initially covered full tuition in a participating private school, but the value did not keep pace with tuition. By 1998, the voucher covered a little over half of tuition fees. The funds for the voucher came from both the municipal (20 percent) and federal (80 percent) governments. Municipalities determined the appropriate number of vouchers, and each municipality conducted its own lottery if demand for the vouchers exceeded the supply. We obtained computerized or paper copies of lists of lottery winners and losers from local municipalities.

In the applicant lists, we observed the students' names, contact information, national identification number, and school of application. The most important piece of information was students' national identification numbers. An identification number consists of 11 digits, the first 6 of which show date of birth. The 11^{th} digit in the ID number has a mathematical relationship with the other 4 digits which we can check to verify that the ID number is valid. About 9.5 percent of applicants had invalid birth dates. This was the prevailing reason why ID's were invalid. If students reported valid birth dates, 97 percent of the time their ID number was valid.

Using the application data, we can verify if the lottery was indeed random. If the lottery was truly random, we should find few differences between the characteristics of voucher lottery winners and losers. Table 1 shows data from the applicant lists on age, gender, having a phone, and having a valid ID. We report data for multiple cohorts

including applicants from Bogotá who applied in 1992 and 1995. The first column of Table 1 shows the average characteristic amongst voucher lottery losers and its standard deviation. The second column shows the difference between voucher winners and losers and the corresponding standard error. In terms of gender and having a phone, we find no significant differences between voucher winners and losers in the 1995 cohort. When we look at age, we find that younger students are more likely to win the vouchers. This is significant and may suggest some type of nonrandomness.

Even if the lottery was random, however, there may be some reasons for this finding. First, in the Bogotá 1995 cohort, there are a couple of significant outliers (e.g. a reportedly 92-year old 5th grader) among voucher lottery losers. If we exclude these individuals or compare medians rather than means, the difference in ages is much smaller. Another possible reason for this difference involves the accuracy of the records. In most cases, we received two separate lists – one with all lottery losers and another with lottery winners. One of our worries was that information for lottery winners was updated while lottery losers' information was not. This could lead to more accurate ages, addresses, and ID numbers. In the Bogotá 1992 lottery, there appears to be some evidence of this. Voucher winners were about 5 percentage points more likely to have a valid ID than voucher losers and about 18 percentage points more likely to have reported a phone number. However, in the Bogotá 1995 cohort, we did not find differences in the likelihood that students had valid ID numbers.

As a final check on the data, we looked at the "win" rate of each of the schools. In theory schools should have had "win" rates among lottery applicants from their school that were similar to the overall lottery average. For the cohorts represented in Table 1, there were few outliers, and the existing outliers were typically the result of a low number

of overall applicants to that school. However, in one cohort not included in Table 1, we found significant outliers. In the excluded cohort, we found one school in particular where 100 percent of applicants had won the voucher. Given the school's reputation as a politically connected school and the large number of students who had applied, we could not rule out nonrandomness.

II. Data and Methodologies

A. Data Sources

There were three sources of data used in the analyses. First, as I explained above, our studies relied on information from the applicant list. Using contact information from the applicant list, we attempted to interview a random sample of voucher applicants from Bogotá in 1995.[1] Generally, we stratified this sample so that we were contacting equal numbers of lottery winners and losers. The resulting household surveys are the second source of data. The surveys included comprehensive details of students' schooling including a grade by grade summary of schools attended and grade promotion. The surveys also gathered information about students' parents and siblings. Our response rate in the surveys was 55 percent among voucher winners and 53 percent among voucher losers. The difference was not significant.

In conducting our interviews, few applicants actually refused to respond. The most frequent reason that individuals did not respond were bad addresses or moves. Our response rate is slightly lower than that in other voucher studies (e.g Mayer, Peterson, Myers, Tuttle, and Howell 2002). Although we would have liked the response rate to be higher, the symmetric response rates across winners and losers suggests that any bias

[1] In Angrist et al. (2002), we also interviewed cohorts of students who applied for a voucher in Bogotá in 1997 and in Jamundí in 1993. I only focus on the Bogotá 1995 cohort in this paper.

resulting from non-response is likely to be minimal (Angrist, 1995). Because response probabilities are uncorrelated with voucher win/loss status, there should be little bias from our failure to interview all applicants.

Table 2 shows some basic student-level characteristics used in the analysis. We only report descriptive characteristics in this table and report student outcomes in other tables. As in Table 1, the first column shows the average characteristic of voucher lottery losers and the corresponding standard deviation. The second column of Table 2 shows the differences for voucher winners with the corresponding standard error. The third column shows the sample size for the specific variable. As before, we find few significant differences

The typical applicant was about 15. About half of the applicants were male. The average education of both the mothers and fathers in the sample was slightly less than 6 years. We detected some differences in the education levels of fathers in our data. We find that fathers of voucher winners had about 0.4 years less of schooling completed although we have a smaller sample size in these regressions. We also find that about 10 percent of the fathers in our data were earning more than 2 minimum wages. This does not vary across voucher status.

Our final source of data came from matching student applications to the ICFES exam. The ICFES is the national college entrance exam. Ninety percent of students graduating from Colombia's secondary school system take the ICFES exam. It is the most common exam used in college admissions and 75 percent of test-takers go on to college (World Bank 1993). Because of the high proportion of high school students who take the exam, the exam is likely a better proxy for high school graduation than for college entrance.

The primary variables used in the matching were the student's name and identification number. If applicant lists had been updated so that voucher winners had more correct names and ID numbers than voucher losers, then we may detect spurious effects of the voucher solely because winners have more accurate information. Because our analysis found that voucher winners and losers in the Bogotá 1995 lottery had similar likelihoods of having a valid national ID, we focus our matching solely on them. We also matched ICFES records for the 1992 cohort. Similar to the results I show below for the 1995 cohort, we found that voucher winners in the 1992 cohort were more likely to take the college entrance than voucher losers. However, because of the possibility that voucher winners' records for the 1992 cohort had been updated, our finding could be spurious.

ICFES exams are offered twice a year, and for the 1995 cohort, we searched for matches among all test-takers in 1999, 2000, and 2001. Assuming that students had not repeated, they should have taken the ICFES exam in 2000. The ICFES scores used here are from the redesigned scoring system introduced in March 2000. Our scores are for the Mathematics and Language components of the Common Core of Basic Competence (Nucleo Comun Competencias Basicas), which includes modules in Biology, Chemistry, Physics, Mathematics, Language, History, Geography, and a Foreign Language test chosen by the student. The ICFES is given over a two-day period with two morning sessions and an afternoon session on the first day. The Mathematics and Language components of the Common Core each take one hour and have 35 questions. Test scores are reported on a scale of 0-100, with the score distribution highly concentrated in the 30-70 range. The distributions of Mathematics and Language scores for all those tested in Bogotá in March 2000 appear in Figure 1 (for 6,868 examinees). We discuss the specific

variables from the ICFES and the matching strategy in Section 4 of this paper.

B. Empirical Methodologies

Because of randomization, simple t-tests comparing the outcomes of winners and losers can identify the effects of the voucher (Angrist and Kreuger 1999). We also use the following regression model to identify the effects of the voucher:

$$y_i = X_i'\beta_0 + \alpha_0 D_i + \varepsilon_i, \quad (1)$$

where y_i is a measure of some type of outcome for student i, D_i is an indicator for whether a student won the voucher lottery, and X_i includes covariates such as age, gender, and controls for neighborhood.

The parameter α shows the effect of winning the voucher lottery on student outcomes. This is often called the "intention to treat" parameter. It shows the effect of offering the voucher. The intention to treat reflects both the "effect of the treatment on the treated" (i.e. the effect of using a voucher) and the probability of being treated. If everyone who is offered the voucher uses it and no one in the control group does, then the "intention to treat" is equivalent to the "effect of the treatment on the treated." The randomization of the vouchers enables us to identify the "intention to treat." While we would like to identify the effect of using a voucher, we do not have a way of controlling for selection into using the voucher. While we can identify the people who were offered but declined the voucher, we cannot identify the individuals who were not offered the voucher and would have declined had they been offered.

Some have suggested that voucher experiments might facilitate identification of the effect of private schools (e.g. Rouse 1998). To be a good instrument for private schooling, the voucher lottery should be correlated with the likelihood that a student

attends private school but uncorrelated with student outcomes except through its influence on private schooling. It is the latter restriction that is likely not satisfied in the Colombian voucher experiment. In Colombia, there were a number of reasons that the voucher could have directly influenced test scores besides through private schooling. For example, the voucher could have been an income shock. As I show below, most of the voucher applicants attended private school in the year immediately after the lottery. The voucher could have just been a subsidy to families already committed to attending private school. Additionally, the voucher program may have changed student incentives. If a student failed a grade, they lost the voucher. This may have influenced voucher winners to try harder in school than they otherwise would have. I discuss possible mechanisms in Section 5 of this paper.

For most of the outcomes of interest, we can measure the effect using equation 1. However, when we look at test scores of voucher applicants on the ICFES exam, estimates based on equation 1 are likely biased. One of the outcomes we evaluate is whether or not students take the ICFES exam. Because the voucher may influence who takes the exams, it likely biases any comparisons of the average test scores of test takers. This is because we observe students who take the exam because of the voucher while we do not observe students who do not take the exam but would have had they received a voucher. We discuss some ways of dealing with this selection in Section 4 of the paper.

III. Effects after Three Years

Table 3 shows estimates of equation 1 for a variety of educational outcomes. The first column shows the average outcome for students who lost the voucher lottery. As we mentioned earlier, almost 90 percent of students who applied unsuccessfully for the

voucher still attended private school in sixth grade. Voucher winners were about 6 percentage points more likely to attend sixth grade in private school. By seventh grade the proportion of voucher lottery losers in private school drops to about 67 percent and voucher winners are about 17 percentage points more likely to be in private school. This difference in private school attendance rates persists up to the time of our survey.

When we examine the highest grade completed, we find that voucher winners have completed about 0.1 years of schooling more than voucher losers within three years of the voucher lottery. This difference does not come from differences in drop-out rates. Voucher lottery winners and losers are equally likely to be enrolled in school at the time of our survey. The difference arises from grade repetitions. About 22 percent of voucher lottery losers had ever repeated a grade and voucher lottery winners were about 5.5 percentage points less likely to have repeated a grade.

This difference in repetitions is also manifested in looking at the likelihood of completion of 6^{th}, 7^{th}, and 8^{th} grades respectively. About 94 percent of lottery losers had finished 6^{th} grade but only 85 percent and 63 percent had finished 7^{th} and 8^{th} grades respectively. The difference between voucher winners also increases over time so by 8^{th} grade, voucher winners are about 10 percentage points more likely to have finished the grade.

While grade repetition is often used a measure of the quality of education in developing countries (e.g. Psacharopolous, Tan and Jimenez 1986), grade repetition may not fully signal academic achievement in the PACES setting. As part of the PACES program, students' vouchers were only renewed if students passed their grade. One explanation for lower repetition rates is that schools may have had an incentive to promote voucher students in an effort to keep tuition monies flowing to their schools.

To test whether the grade repetition result reflected higher academic achievement, we administered a standardized exam to a sample of ICFES applicants. On average, lottery losers scored about 0.2 standard deviations above the population mean in both math and writing while voucher winners scored about 0.2 standard deviations higher in writing. While voucher winners score higher in math and reading, the results are not statistically significant unless we combine the various test scores. Although the sample we tested was fairly small, the fact that voucher winners scored higher than voucher lottery losers suggests that the voucher had impacted student achievement within three years.

Finally, our results in Table 3 show that voucher students were less likely to be working at the time of survey. They were also less likely to be married or cohabiting.

IV. College Entrance Exams

After three years, we found that students had more years of school completed, less repetitions, and higher standardized exam scores, yet it was unclear if these effects after three years could turn into long-run effects. For example, by the third year after the lottery, more than half of the students were no longer using the voucher. Additionally, the group of students who took the exam we administered was small and may not have been fully representative of the population of lottery winners and losers since only 60 percent of the students we invited to the exam actually attended.

To test whether the voucher led to long-run educational differences between voucher winners and losers, we gathered additional data on PACES applicants' ICFES exams. From the ICFES records, we know whether a student took the ICFES exam and their test scores in math and language if they did take the test.

Table 4 shows estimates of the effects of vouchers on the likelihood that students take the ICFES exam. We report the coefficient on voucher from equation 1 when we include covariates for gender and age. We report estimates based on four different matching strategies. In the first strategy, we matched students' national identification numbers alone. On average we were able to match 35.4 percent of voucher applicants to their college entrance exam. Voucher winners, however, were 5.9 percentage points more likely to be matched than voucher losers.

We also report estimates based on matching both the identification number and the city of residence. Our match rate drops about 1.5 percentage points, but we still estimate a 5.6 percentage point effect of the voucher. In relative terms, this is about a 20 percent effect of the voucher on the likelihood of taking the college entrance exam. In the final row of Table 4, we report estimates based on the identification number, the city of residence, and the first seven letters of a student's last name. Our match rate drops to 31.8 percent with the more stringent match criteria. We still find a 5.6 percentage point difference between voucher winners and losers. Clearly, the voucher improved recipients' likelihood of taking the exam. Given that taking the exam is a good proxy for high school graduation, receiving the voucher dramatically improves the likelihood that students finish secondary school.

Having established the voucher's effect on test-taking, we now turn to the effect of the voucher on test performance. As mentioned earlier, measuring the effect on test performance is difficult since the voucher affect who takes the exam. To see this, consider the following example. Suppose that Juan is a student on the margin of dropping out of secondary school. If Juan received a voucher, it may have been enough to help him persist in secondary school and take the college entrance exam. If Juan is on

the margin of dropping out, he is likely not the top achieving student in his grade. Now suppose that the voucher has no effect on a student's ICFES exam scores. The average ICFES score of voucher winners is a weighted average of exam scores of students who would have taken the test in the absence of the voucher and also the students like Juan who would not have taken the ICFES exam without the voucher. Because Juan was a low achieving student, his score is likely less than the average exam scores who would have taken the ICFES even without a voucher, and hence, the average ICFES score of lottery winners will be less than the average ICFES score of voucher lottery losers (which is just the average of exam scores of students who would have taken the ICFES exam without the voucher). If Juan's story is typical, then comparisons of the average test scores of winners and losers will be biased downward.

Rows 1 and 5 of Table 5 make this comparison. When we compare the average test scores of voucher winners and losers who took the exam, we find that voucher winners score 0.70 points higher in language and 0.40 points higher in math. The estimated effect on language scores is statistically significant. If indeed the comparison of ICFES test scores is biased downward, then the estimated effects are smaller than the true effects of the voucher. In Angrist et al (forthcoming), we show that assuming that the voucher does not harm students (which seems reasonably given that students could quit using the voucher without penalty), then the estimated effects in Rows 1 and 5 of Table 5 are lower bounds for the true treatment effect.

In Table 5, we also employ two other strategies to estimate the effect of the voucher. In the remaining rows, we censor the sample by assigning values to students who did not take the exam. The motivation for this specification is simple. Suppose that students' latent (or expected) ICFES exam is related to the probability of taking the exam.

Students who expect to score low will likely not take the exam. If non-takers have low exam scores, we may be able to estimate the effect of the program by assigning them a test score. In Rows 2, 3, 6, and 7, we assign non-test-takers the test score of the 1st percentile of test takers. In Rows 4 and 8, we assign non-test-takers the test score of the 10th percentile. With the censoring, we use both OLS and Tobit models. In the Tobit models, we make the additional assumption that the underlying distribution of test scores is normally distributed.

When we censor, the estimated effects of the voucher on test scores is much larger than the raw comparisons of means. In the censored OLS models, we find estimated effects of 1.14 in language and 0.79 in math. Both estimates are statistically significant. In the Tobit models, we find estimated effects of about 2 points in both language and math. The estimates are also statistically significant.

It is not surprising that the censoring leads to larger and significant effects of the voucher. We are essentially giving low test scores to non-takers who are disproportionately voucher lottery losers. One might wonder how sensitive the results are to the censoring point. In Figure 2, we show the estimated effects using Tobit when we move the censoring to different percentiles of the test score distribution. Consistently, regardless of the censoring point, we find effects of the voucher near 2 points. The standard error bands show that these estimates remain significant.

One of the surprising results in Figure 2 is the fact that even at high censoring points (e.g. the 80th percentile) we still find that the voucher led to improvements in students' test scores. This may imply that even among high achieving voucher applicants, voucher winners test scores have improved. In Angrist et al (forthcoming), we explored this in greater detail to test this hypothesis. Using nonparametric strategies, we

demonstrated that even at the top of the distribution of test scores voucher winners scored higher than voucher lottery losers.

V. Mechanisms for the Voucher Effect

The results thus far suggest that voucher winners had higher academic achievement after three years and through the end of secondary school. The result reflects the effect of winning the voucher and not the effect of using a voucher. As we mentioned above, the voucher could have improved student outcomes for a variety of reasons. It could have been an income shock. The voucher could have strengthened the incentives for students to work hard. The voucher could have also changed the type of schools and peers that students had.

To shed light on some of the possible mechanisms, Bettinger, Kremer, and Saavedra (2004) consider the effects of the voucher on students who applied to vocational schools. As part of the PACES lottery, students had to be accepted at a participating private school *before* applying for the voucher. Many students applied to vocational schools. These vocational schools were of inferior quality to the other private schools. They had a smaller proportion of students take the ICFES exam and their students typically scored worse on the ICFES exam than non-vocational schools.

However, among the students who applied to vocational schools, the voucher seemed to have odd effects on the types of schools that students attended. Voucher winners used their voucher to attend vocational schools. Voucher lottery losers, by contrast, changed schools and went to academic schools instead. After three years, voucher winners were 18 percentage points more likely to be attending a vocational school, and as a result, they were more likely to attend schools of inferior quality as

measured by academic performance on the ICFES.

Despite this fact, voucher winners who had initially applied to vocational schools had better academic outcomes than voucher lottery losers who had applied to the same schools. Table 6 shows these results. Voucher winners were 4-5 percentage points more likely to take the ICFES exam and had higher reading scores on the exam (which is likely a lower bound for the true effect on reading scores as discussed in Section 4).

The fact that voucher winners attended inferior schools and yet had more positive outcomes suggests that the schools and peers may not have been the operative channel by which the voucher affected student outcomes. Ongoing work by Michael Kremer, Juan Saavedra, and me seeks to identify other characteristics of these schools which may provide some hint as to why the voucher winners who applied to vocational schools were so successful.

VI. Conclusions

The Colombian voucher program was one of the largest voucher programs in the world, and the program seems to have had a positive effect on student outcomes after three years and through the end of secondary school. After three years, students winning the voucher had higher test scores, less grade repetition, and more years of schooling completed than students who had lost the voucher lottery. Additionally, voucher winners were more likely to attend private school, less likely to be working, and less likely to be married or cohabiting. By the end of high school, voucher winners were more likely than voucher lottery losers to have taken the college entrance exam and voucher winners had higher college entrance exam scores.

The voucher program was a unique partnership between the public and private

sectors in Colombia. Thousands of students in Bogotá and about 125,000 students nationwide took advantage of the program. As Angrist et al (2002) shows, public expenditure increased only slightly in funding the program, yet the benefits accrued to voucher winners more than justified the costs.

One remaining puzzle in the Colombian voucher experience is how the voucher program affected the students. If the voucher program affected students solely through private schools, then the voucher program may have different policy implications than a voucher program which affected students by changing their incentives. The preliminary evidence, at least for the subset of voucher winners who applied to vocational schools, suggests that the academic quality of the schools may not have been the mechanism by which the voucher affected students' outcomes. The voucher winners who had applied to vocational schools attended schools with inferior academic quality but yet they had better academic outcomes than voucher lottery losers. Future research will hopefully identify the specific channel(s) by which vouchers affect students and hence provide a clearer picture of why the private-public partnership in the case of Colombian vouchers generated such dramatic improvements in students' academic performance.

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Table 1. Student Characteristics by Voucher Status

	Losers' Mean (1)	Difference by Voucher Status (2)
A. Bogotá 1995		
Age	12.78 (2.22)	-.137 (.064)
Male	.484	.004 (.016)
Has Phone	.874	.013 (.010)
Has Valid ID	.882	-.010 (.010)
B. Bogotá 1992		
Age	12.83 (1.23)	.093 (.029)
Male	.533	-.042 (.010)
Has Phone	.397	.184 (.009)
Has Valid ID	.681	.053 (.009)

Notes: The table reports voucher losers' mean and difference for voucher winners. Standard deviations are in the first column for non-binary variables. Standard errors are included in the second column.

Table 2. Descriptive Statistics by Voucher Status

	Losers' Mean (1)	Difference by Voucher Status (2)	N (3)
A. Bogotá 1995			
Age at time of survey	15.0 (1.4)	-0.013 (0.078)	1,172
Male	0.501	0.004 (0.029)	1,139
Mother's highest grade completed	5.9 (2.7)	-0.079 (0.166)	1, 075
Father's highest grade completed	5.9 (2.9)	-0.431 (0.199)	911
Mother's age	40.7 (7.3)	-0.027 (0.426)	1,123
Father's age	44.4 (8.1)	0.567 (0.533)	940
Father's wage (>2 min wage)	0.100	0.005 (0.021)	861

Notes: The table reports voucher losers' mean and difference for voucher winners. Standard deviations are in the first column for non-binary variables. Standard errors are included in the second column.

Table 3. Descriptive Statistics and Estimates of the Voucher Effect

Dependent Variable	Bogotá 1995	
	Losers' Means (1)	Coefficient on Voucher Status Basic Controls (2)
Started 6th Grade in Private	0.877	0.057
	(0.328)	(0.017)
Started 7th Grade in Private	0.673	0.168
	(0.470)	(0.025)
Currently in Private	0.539	0.153
	(0.499)	(0.027)
Highest Grade Completed	7.5	0.130
	(0.960)	(0.051)
Currently in School	0.831	0.007
	(0.375)	(0.020)
Finished 6th Grade	0.943	0.023
	(0.232)	(0.012)
Finished 7th Grade	0.847	0.031
	(0.360)	(0.019)
Finished 8th Grade	0.632	0.100
	(0.483)	(0.027)
Ever Repeated a Grade	0.224	-0.055
	(0.417)	(0.023)
Number of Repetitions of 6th Grade	0.194	-0.059
	(0.454)	(0.024)
Math Scores [n=282]	0.178	0.153
	(0.120)	(0.114)
Reading Scores [n=283]	0.204	0.203
	(0.115)	(0.114)
Writing Scores [n=283]	0.126	0.128
	(0.116)	(0.105)
Total Test Scores [n=282]	0.217	0.205
	(0.116)	(0.108)
Applicant is Working	0.1690	-0.0297
	(0.3751)	(0.0205)
Married or living with companion	0.0160	-0.0087
	(0.1256)	(0.0059)
N	562	1,147

Notes: The table reports voucher losers' means and the estimated effect of winning a voucher. Numbers in parentheses are standard deviations in the column of means and standard errors in columns of estimated voucher effects. The regression estimates are from models that include controls for phone access, age, type of survey and instrument, strata of residence, and month of interview.

Table 4. Voucher Status and the Probability of ICFES Match

Matching Strategy	Dep. Var. Mean	Coefficient on Voucher Status Basic Controls
	(1)	(2)
Exact ID Match	.354	.059
		(.015)
ID and City Match	.339	.056
		(.014)
ID and 7-letter Name Match	.331	.059
		(.014)
ID, City, and 7-letter Match	.318	.056
		(.014)

Notes: Robust standard errors are shown in parentheses. The sample includes all Bogotá 95 applicants with valid ID numbers and valid age data (i.e. ages 9 to 25 at application).

Table 5. OLS and Tobit Estimates of Voucher Effect on ICFES Exams

Specification	Dep. Var. Mean	Coefficient on Voucher Status Basic Controls
	(1)	(2)
Language Scores		
OLS with score>0	47.4	.70
	(5.6)	(.33)
OLS censored at 1%	37.3	1.14
	(8.0)	(.24)
Tobit censored at 1%	37.3	3.29
	(8.0)	(.70)
Tobit censored at 10%	42.7	2.06
	(4.7)	(.46)
Math Scores		
OLS with score>0	42.5	.40
	(4.9)	(.29)
OLS censored at 1%	35.7	.79
	(5.8)	(.18)
Tobit censored at 1%	35.7	2.29
	(5.8)	(.51)
Tobit censored at 10%	37.6	1.98
	(4.6)	(.45)

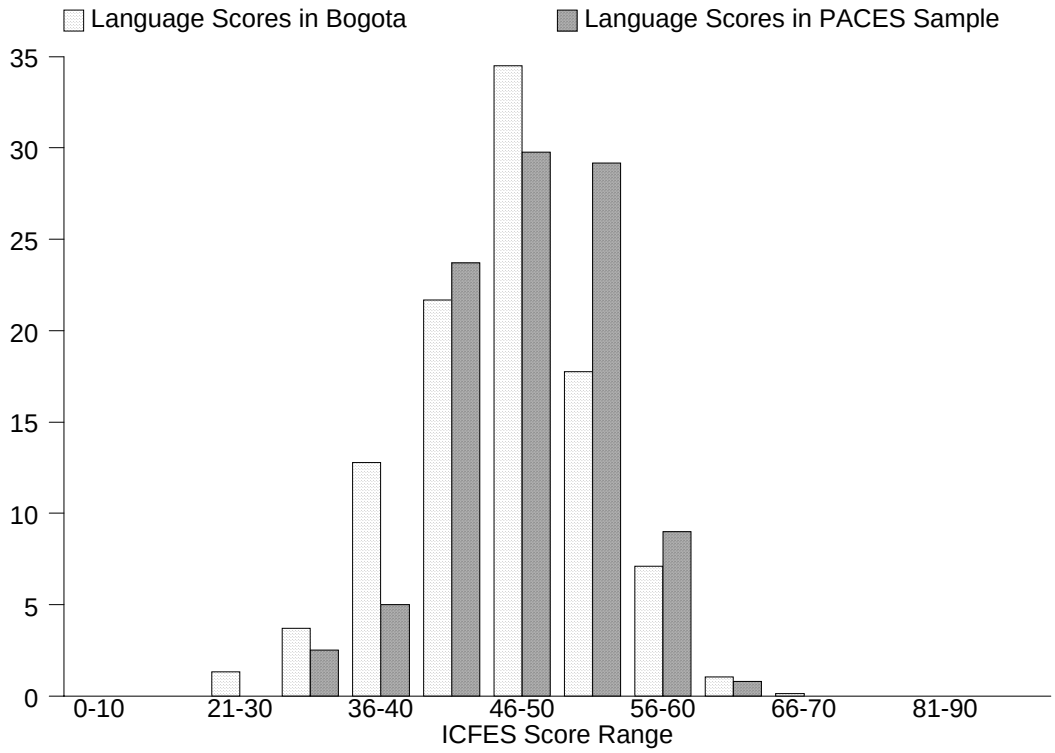
Notes: The table reports voucher losers' means and the estimated effect of winning a voucher. Numbers in parentheses are standard deviations in the column of means and standard errors in columns of estimated voucher effects. The regression estimates are from models that include controls for age and gender. Censoring point is the indicated percentile of the test score distribution, conditional on taking the exam.

Table 6. OLS Estimates of Voucher Effect on ICFES Exams for Vocational Students

Dependent Variable	Coefficient on Voucher Status			
	Vocational		Non-vocational	
	(1) Losers' Means	(2) Basic Controls	(3) Losers' Means	(4) Basic Controls
A. Probability of Taking ICFES				
ID Match	.274	.056	.318	.057
		(.030)	(.466)	(.017)
ID & City Match	.265	.052	.301	.061
		(.030)	(.459)	(.017)
ID & Name Match	.202	.048	.235	.033
		(.028)	(.424)	(.016)
N	336	802	1077	2578
B. Performance Outcomes on the ICFES				
Math Score cond'l on taking	41.47	.844	42.37	.420
	(4.811)	(.621)	(4.655)	(.332)
Reading Score cond'l on taking	45.73	2.19	47.04	.487
	(5.890)	(.768)	(5.373)	(.373)
N	89	254	334	891

Notes: The table reports voucher losers' means and the estimated effect of winning a voucher. Numbers in parentheses are standard deviations in the column of means and standard errors in columns of estimated voucher effects. The regression estimates are from models that include controls for phone access, age, type of survey and instrument, strata of residence, and month of interview. Columns 1 and 2 report estimates for students who had applied to and been accepted at a vocational school prior to the voucher lottery.

A. Language



B. Math

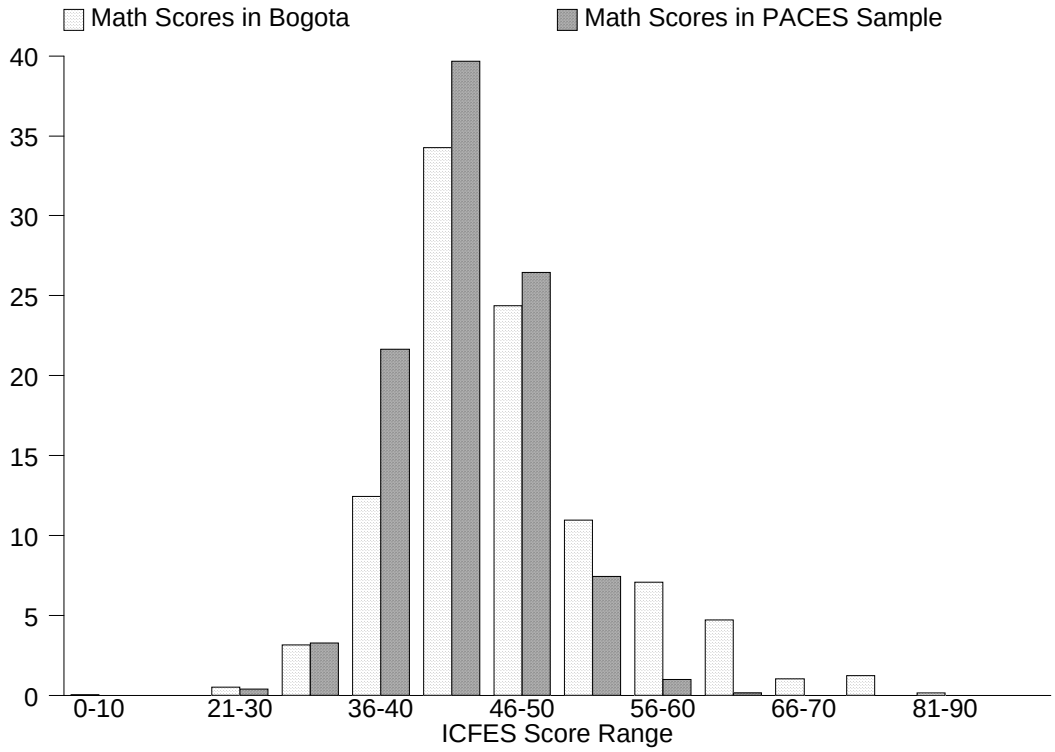


Figure 1. Distribution of Test Scores in Bogotá versus PACES Sample.

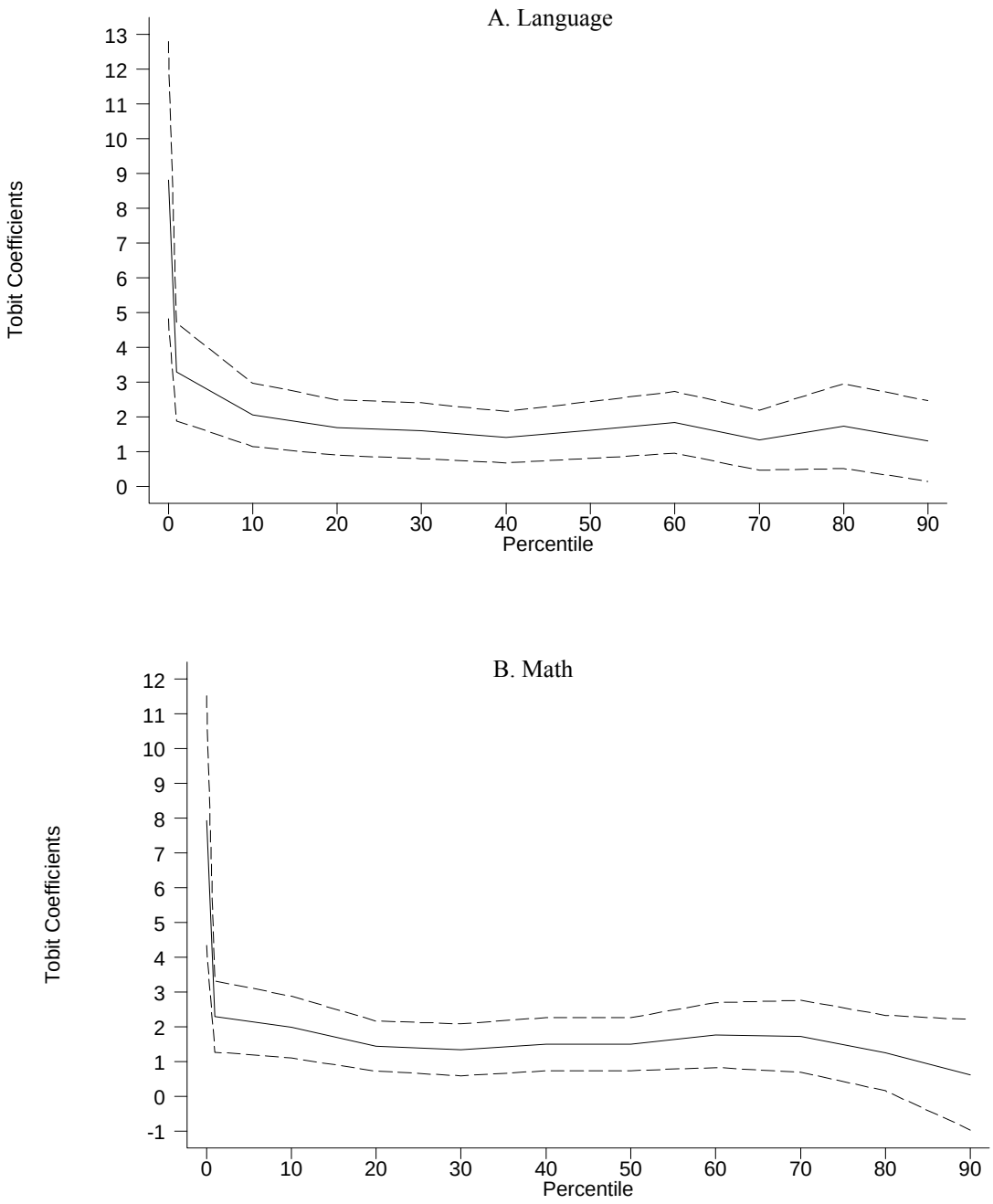


Figure 2. Tobit Coefficients by Censoring Percentile in Score Distribution. The figure plots Tobit estimates of the effect of vouchers on test scores, using data censored at the point indicated on the X-axis (i.e., values below the indicated percentile are assigned a value of zero). For the purposes of this exercise, non-takers are also coded as having a score of zero.