

LONG-TERM CONSEQUENCES OF SECONDARY SCHOOL VOUCHERS: EVIDENCE FROM ADMINISTRATIVE RECORDS IN COLOMBIA*

Joshua Angrist¹; Eric Bettinger²; and Michael Kremer³

ABSTRACT

Although the use of school vouchers is growing in the developing world, the impact of vouchers is an open question. Especially rare is any sort of long-term assessment. This paper estimates the long-term effect of Colombia's PACES program, which provided over 125,000 poor children with vouchers that covered half the cost of private secondary school. This program presents an unusual opportunity to assess the effect of demand-side education financing in a Latin American country where private schools educate a substantial fraction of pupils. The PACES program is of special interest because many vouchers were assigned by lottery, so program effects can be reliably assessed. We use administrative records to assess the long-term impact of PACES vouchers on high school graduation status and test scores. The principal advantage of administrative records is that there is no loss-to-follow-up and the data are much cheaper than a costly and potentially dangerous survey effort. On the other hand, individual ID numbers may be inaccurate, complicating record linkage, and selection bias contaminates the sample of test-takers. We discuss solutions to these problems. The results suggest the program increased secondary school completion rates, and that college-entrance test scores were higher for lottery winners than losers.

1. INTRODUCTION

Latin American and other developing countries are increasingly willing to experiment with demand-side subsidies for primary and secondary education. Examples include Chile and Columbia, which have both offered educational vouchers for private secondary schools and Brazil, India, Israel, and Mexico, which have introduced student stipends that reward attendance and performance. Interest in demand-side subsidies in developing countries parallels interest in the United States, where, for example, publicly-funded vouchers have been distributed in Milwaukee, Cleveland, and Florida.

Most of the evidence on vouchers to date focuses on short-run effects as measured through test scores in primary or secondary school. The evidence so far suggests some benefits for voucher recipients, though the extent of test score gains remains controversial.⁴ Missing from most studies of voucher effects is an assessment of impacts on longer-term outcomes – such as high school graduation rates – that are more clearly tied to economic success. Research on vouchers naturally focuses on short-run outcomes because participants are typically young. Moreover, follow-up long-run efforts are difficult and costly, especially in developing countries. Families move often, addresses are missing or inaccurate, and field conditions for survey work can be difficult.

This paper describes the results of an effort to evaluate the long-run effects of one of the largest voucher initiatives ever implemented, Colombia's PACES program.⁵ PACES presents an unusual opportunity to assess the effects of demand-side education financing in a Latin American country where private schools educate a substantial fraction of pupils. Between 1991 and 1997, PACES awarded nearly 125,000 vouchers to low-income high school students. In Bogota as well as a number of other large cities, demand exceeded supply, and PACES vouchers were awarded the

¹ MIT Department of Economics, 50 Memorial Drive, Cambridge, MA 02142

² Department of Economics, Case Western Reserve University, Cleveland, Ohio 44106-7206

³ Department of Economics, Littauer Center 207, Cambridge, MA 02138

⁴ See, e.g., Rouse (1998); Howell, et al (2000), and Krueger (2002)

⁵ PACES is an acronym for Programa de Ampliación de Cobertura de la Educación Secundaria.

vouchers by lottery. The random assignment of vouchers facilitates a natural-experiment research design where losers provide a control group for winners.

In previous research (Angrist, Bettinger, Bloom, King, and Kremer, 2002), we used the PACES voucher lotteries to estimate the impact on years of school completed between 6th and 8th grade, secondary school grade repetition, test scores, and the probability of working. These results strongly suggest that voucher winners devoted more time to school and learned more in school than they otherwise would have. Most of the results in our earlier study are for students observed about three years after winning the vouchers but still three years before high school graduation. While these results clearly suggest vouchers had a positive short-run effect on schooling and test scores, they do not tell us whether vouchers increased economically meaningful outcomes like high school graduation rates and achievement at the end of school.

Here we turn to a longer-term assessment that exploits administrative data from Colombia's centralized college entrance examinations, the ICFES test, taken by most high school graduates.⁶ To implement this strategy we used pupil ID numbers to match data from PACES applicant lists with ICFES exam records. As in our previous study, the random assignment of vouchers in a lottery is used to identify causal effects.

The principal advantage of administrative records over survey methods is that there is no loss to follow-up and administrative data are much cheaper than a costly (and potentially dangerous) survey effort. However, evaluation research with administrative data is not problem-free. While PACES applicant records provide a complete target population for matching, in practice, these records may be inaccurate. Inaccuracy complicates record linkage and can lead to a match rate that favors winners. Additionally, selection bias contaminates the sample of test-takers making it more difficult to compare test scores of lottery winners and losers. We discuss solutions to these problems. On balance the results of our follow-up effort point to lasting benefits for voucher winners, with substantially higher high school graduation rates and, after adjusting for selection bias, higher test scores among those who take the ICFES exam.

The next section provides additional background on the PACES program and the evaluation strategy. Following Section II, we outline the estimation framework. Section III presents results measuring the effect of PACES vouchers on high school graduation rates. Section IV discusses the problem of selection bias in an analysis of test scores and presents estimates of effects on scores using alternate approaches to the selection problem. Section V concludes the paper.

2. BACKGROUND

2.1 The PACES program

The Colombian government established PACES in late 1991 as part of a wider decentralization effort and in an attempt to quickly expand school capacity and raise secondary school enrollment rates (King, Orazem, and Wolgemuth, 1998). Although 89% of Colombia's primary-school age children were enrolled in 1993, only 75% of the eligible population was enrolled in secondary schools. Among eligible children in the poorest quintile of the population, 78% were enrolled in primary school, but only 55% were enrolled in secondary school (Sanchez and Mendez, 1995).

PACES targeted low-income families by offering vouchers to children in neighborhoods classified as falling into the two lowest socioeconomic strata (out of 6 possible strata). To qualify for a voucher, applicants must have been entering the Colombian secondary school cycle which begins with grade 6, and be aged 15 or under. Prior to applying, students must already have been admitted to a participating secondary school (i.e., one that would accept the voucher). Participating schools had to be located in participating towns, which included all of Colombia's largest cities. Just under half of private schools in the 10 largest cities accepted vouchers in 1993.

⁶ ICFES is an acronym for Colombia's college admissions testing service, the Instituto Colombiano Para El Fomento De La Educacion Superior.

PACES vouchers were worth about US\$190 by 1998, by which time the program was winding down and our original study began. The maximum voucher value was set initially to correspond to the average tuition of low-to-middle cost private schools in Colombia's three largest cities. Schools charging less than the vouchers' face value received only their usual tuition. PACES vouchers became less generous over time because they did not keep up with inflation. Our survey data show matriculation and monthly fees for private schools attended by voucher applicants in 1998 averaged about \$340, so most voucher recipients supplemented the voucher with private funds. By way of comparison, the average annual per-pupil public expenditure in Colombia's public secondary school system in 1995 was just over \$350 (DNP, 1999), and public school parents in our sample typically paid tuition or fees of roughly \$58. Per capita GNP in Colombia was then around \$2,280 (World Bank, 1999).

Participating schools tended to serve lower-income pupils, and to have lower tuition than nonparticipating private schools. Schools with a vocational curriculum were also over-represented among those in the program. Participating private schools included for-profit schools, religious-affiliated schools, and schools run by charitable foundations. Initially, vouchers could be used at both for-profit and non-profit schools, but after 1996, for-profit schools were excluded. The number of vouchers in use in any one year peaked at roughly 90,000 in 1994 and 1995. There were approximately 3.1 million secondary school pupils in Colombia in 1995, 37% of whom attended private schools. In Bogota, roughly 58% of 567,000 secondary school pupils attended private school.

Test score comparisons reported by King et al (1997) show achievement levels in participating private schools were close to those in public schools, though significantly below achievement levels in nonparticipating private schools. Pupil-teacher ratios and facilities were similar in public and participating private schools, and many of the teachers in the private schools most likely to participate in the PACES program are moonlighting or retired public school teachers. Non-participating private schools had lower pupil-teacher ratios and better facilities. Relatively elite private schools apparently opted-out of the PACES program. On the other hand, many private schools in Colombia serving low-income populations apparently welcomed PACES pupils.

Voucher recipients were eligible for automatic renewal through eleventh grade, when Colombian high school ends, provided the recipient's academic performance warranted promotion to the next grade. Students who transferred from one participating private school to another could, in principle, transfer the voucher to the new school. In practice, however, our survey suggests many students who transferred schools after winning lost their vouchers. Cities and towns used lotteries to allocate vouchers when demand exceeded supply. In this study we concentrate on one of the largest applicant cohorts, pupils in Bogota who applied for vouchers in 1995. The 1995 applicant cohort actually applied in 1994 to enter private school in 6th grade in 1995. Assuming no grade repetition, students in this cohort would have been in 11th grade, their last year of high school, in 2000. Any 11th grader or high school graduates may take the ICFES exam, which is required for admission to Colombian colleges and universities.

2.2 Data and Descriptive Statistics

This study is based on a match of computerized records from the 1995 cohort of PACES applicants from Bogota with ICFES records showing whether these students registered for the exam and their scores if they took the exam. A detailed description of the matching procedure and test score data base is given in the data appendix. Table 1 reports descriptive statistics for the Bogota 1995 PACES cohort, which included 4,044 applicants. The demographic characteristics in the table were derived from PACES applications. About 59 percent of voucher applicants received the voucher. Applicants were almost 13 years old, on average, and about evenly split between boys and girls. Roughly 88 percent of applicants came from households with a telephone or access to a telephone.

We matched PACES applicants with their ICFES records using the Colombian national ID number, which consists of 11 digits. The first 6 digits show date of birth. The remaining five digits have no demographic content but can be checked for validity by verifying the final "check digit" in the ID number, which bears a mathematical relationship to the other digits. We used the embedded check digit and birth dates to determine whether ID numbers were valid. About 9.5% of applicants had invalid birth dates. Of the 3661 applicants with plausible birth dates, 97 percent reported valid ID numbers.

Voucher winners have demographic characteristics similar to those of losers, except possibly for a small age difference. This can be seen in the columns 3-6 of Table 1, which show differences by voucher status. The age differences by voucher status appear to be driven by a few outliers, probably due to incorrectly coded ID numbers

among losers, since applicant age is derived from IDs. The age gap is smaller when the sample is limited to those with valid ID numbers, though still marginally significant. We therefore control for age when estimating voucher effects.

The first outcome of interest is a dummy for whether a PACES applicant registered for the ICFES test. In addition to signaling an interest in higher education, ICFES registration is an excellent proxy for high school graduation status since 90 percent of high school graduates take the ICFES test (The World Bank, 1993) and all of those who are tested must be in 11th grade or have graduated high school (mostly the former). On the other hand, mis-measurement of ID numbers makes matching more difficult and complicates the interpretation of match-status. A particular concern in this context is the possibility that invalid winners' ID numbers may have been corrected through continued bureaucratic interaction with the PACES program, while inaccurate ID numbers of losers are less likely to be corrected. A difference in correction probabilities by win/loss status could generate a spurious difference in match rates that favors PACES winners.

In practice, there is little evidence in our data that differential accuracy by voucher status is a problem in the Bogota 1995 applicant cohort. This can be seen in Table 2, which reports differences by voucher status and linear probability estimates of the relationship between win/loss status ID validity. In particular, the table shows estimates of α_0 in the regression model:

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

where y_i is a dummy the validity for applicant i 's ID number, D_i is an indicator for whether applicant i won a voucher, and X_i is a vector of controls for age and sex. The standard errors reported in the table were corrected for heteroscedasticity.

Column 1 of Table 2 shows that about 88 percent of all applicants had a valid ID number. Voucher winners were 1 percentage point *less* likely to have a valid ID, but this difference is not significant. Restricting the sample to those with valid birth dates embedded in their ID numbers results in an even smaller voucher effect, with or without demographic controls. Results are also similar when estimated separately by sex. These results strongly suggest that voucher winners are not more likely to be matched solely because of more accurately recorded ID numbers.⁷

3. EFFECTS ON HIGH SCHOOL GRADUATION

Estimates of effects on high school graduation were constructed using an equation like (1), replacing the dependent variable with an indicator for ICFES registration. Students in the 1995 applicant cohort who were promoted on schedule should have registered to take the ICFES exam at one of two opportunities in the 2000 school year. Because some students may also have advanced or repeated grades, we searched for applicants among ICFES registrants for all exams offered in 2000 and 2001. If a student was found to have been tested more than once, we retained the first set of scores. In addition, we experimented with more restrictive matching strategies using city of residence and students' names for validation.⁸

Overall, about 35 percent of applicant ID numbers were matched to ICFES records. This is shown in Table 3, which reports match results for the sample with valid ID numbers as determined by the ID check digit and valid age data embedded in their ID numbers. A stricter definition that requires both ID numbers and city of residence to line up for a match generated an overall match rate of 34 percent, while matching on ID numbers and the first 7 letters of students' last names, leads to a match rate of 33 percent. Finally, requiring both city and a 7-letter match generates a match rate of 32 percent.

⁷ We did find some evidence of differential record-keeping in the 1992 cohort of Bogota applicants, the earliest Bogota cohort. Because of this and other data problems, the 1992 applicant cohort was omitted from the study.

⁸ City was considered matched if an applicant registered for the ICFES test in Bogota. Of course, in practice, some Bogota applicants may have been tested elsewhere. A few apparent matches from 1999 are included as well. See the data appendix for details.

The effect of winning a voucher on an ID match with no controls, reported in column 1 is about 7 percent. This falls to about 6 percent with demographic controls but returns to almost 7 percent when a dummy is added for telephone ownership. The estimated effects are slightly larger for boys than for girls, and the base rate is lower for boys. Using city of residence to validate matches leads to slightly smaller treatment effects for girls and overall, but the change is not substantial. Validation using name matches leads to a treatment effects almost identical to those without validation, and validation using both city and name generates estimates similar to those using city only. Overall, the estimated effects of voucher status on ICFES registration are remarkably robust to changes in sample and specification. Thus, it seems fair to say that PACES vouchers increased the likelihood of ICFES registration, and probably high school graduation, by 5-7 percentage points, with no consistent pattern of differences by sex.

Estimates of 5-7 percentage points in levels amount to an increase of 15-20 percent in the probability students take the ICFES exam. This seems like a large effect, but it is consistent with our earlier findings for the 1995 Bogota cohort, which showed voucher winners about 10 percentage points more likely to finish 8th grade in 1998. The increase of 5-7 percentage points in high school graduation rates among PACES winners could be explained by half to three-fourths of these additional 8th grade completers going on to finish high school three years later.

4. EFFECTS ON COLLEGE ENTRANCE EXAM SCORES

4.1 The Selection Problem

In this section we turn to an assessment of the effects of PACES vouchers on academic achievement as measured by ICFES test scores. The principal econometric challenge in an evaluation of effects on scores is caused by the fact that voucher winners were more likely to take the test than losers. Thus, the scores of winners and losers are no longer comparable. To see the likely consequences of this differential selection for comparisons of scores among test-takers, let Y_{1i} be the test score student i would obtain after winning the voucher and let Y_{0i} denote the test score student i would obtain otherwise. We assume that both of these potential outcomes are well-defined for all pupils, whether they actually took the test or not. The average causal effect of winning the voucher on the scores of all winners is $E[Y_{1i} - Y_{0i} | D_i = 1]$. Of course, in practice, we only get to observe scores for those who are tested. Moreover, among tested pupils, we only observe Y_{1i} for winners and Y_{0i} for losers.

Suppose we code the scores of non-takers as zero. Then the observed win/loss contrast in scores among those who were tested can be written

$$\begin{aligned} E[Y_i | Y_i > 0, D_i = 1] - E[Y_i | Y_i > 0, D_i = 0] &= E[Y_{1i} | Y_{1i} > 0, D_i = 1] - E[Y_{0i} | Y_{0i} > 0, D_i = 0] \\ &= E[Y_{1i} | Y_{1i} > 0] - E[Y_{0i} | Y_{0i} > 0], \end{aligned}$$

where the second equality is because D_i is randomly assigned. This contrast does not have a causal interpretation, however, since those with $Y_{1i} > 0$ and $Y_{0i} > 0$ are not drawn from the same population. In fact, we can expand this further to be

$$\begin{aligned} E[Y_i | Y_i > 0, D_i = 1] - E[Y_i | Y_i > 0, D_i = 0] &= E[Y_{1i} - Y_{0i} | Y_{0i} > 0] \\ &\quad + \{E[Y_{1i} | Y_{1i} > 0] - E[Y_{1i} | Y_{0i} > 0]\}. \end{aligned} \tag{2}$$

Thus, the win/loss contrast among test-takers is equal to the average causal effect on those who would have been tested anyway, $E[Y_{1i} - Y_{0i} | Y_{0i} > 0]$, plus a term that captures the selection bias due to the fact that we are conditioning on an endogenous registration outcome. The bias in equation (2) is likely to be negative if vouchers increase scores. Suppose, for example, that $Y_{1i} = Y_{0i} + \alpha$, where $\alpha > 0$. Then, the selection bias is

$$E[Y_{1i} | Y_{1i} > 0] - E[Y_{1i} | Y_{0i} > \alpha],$$

which is clearly negative.

We implement a number of strategies in an attempt to mitigate the selection bias inherent in conditional-on-registration comparisons. One simple approach is to code non-takers as having scored zero, or some other low value, say the score at the .01 or .1 quantile and then include these values in an analysis for the complete sample. This avoids selection bias, but may be misleading if some non-takers would have done better. Alternately, we can assume observations on test-takers represent the censored realization of underlying normally distributed score distribution. This leads to Tobit estimates. To make this more plausible and to test some of the underlying assumptions, we try different censoring points.

A second approach to selection problem exploits the fact that because selection bias is most likely negative, an analysis of outcomes conditional on registration conservative. In other words, selectioncontaminated estimates provide a lower bound on the likely impact of vouchers on achievement. Moreover, an upper bound on score effects, valid on weak non-parametric assumptions, can be obtained by adapting a theoretical result from our earlier paper (Angrist, *et al*, 2002). In particular, suppose we are prepared to assume that vouchers are never harmful, i.e., that $Y_{1i} \geq Y_{0i}$ for all i . This seems reasonable since voucher winners are, of course, free, to not use or stop using vouchers if they perceive adverse effects. Drop i subscripts to simplify notation and let $q_0(\theta)$ be the θ -quantile of the distribution of Y_0 and $q_1(\theta)$ be the θ -quantile of the distribution of Y_1 . Then, for any quantile, we have the following result on treatment effects with sample selection bias:

Proposition 1. Suppose that $Y_1 > Y_0$. Then

$$\begin{aligned} E[Y | D = 1, Y > q_1(\theta)] - E[Y | D = 0, Y > q_0(\theta)] &\geq E[Y_1 - Y_0 | Y_0 > q_0(\theta)] \\ &\geq E[Y | D = 1, Y > q_0(\theta)] - E[Y | D = 0, Y > q_0(\theta)] \end{aligned}$$

Moreover, if $Y_1 = h(Y_0)$ for any weakly increasing function $h(\bullet)$, then the left inequality is an equality.

Proof. $E[Y | D = 1, Y > q_1(\theta)] = E[Y_1 | Y_1 > q_1(\theta)]$ and $E[Y | D = 0, Y > q_0(\theta)] = E[Y_0 | Y_0 > q_0(\theta)]$ by random assignment. Furthermore,

$$\begin{aligned} E[Y_1 | Y_1 > q_1(\theta)] - E[Y_0 | Y_0 > q_0(\theta)] &= E[Y_1 - Y_0 | Y_0 > q_0(\theta)] + \{E[Y_1 | Y_1 > q_1(\theta)] - E[Y_1 | Y_0 > q_0(\theta)]\} \\ &\equiv E[Y_1 - Y_0 | Y_0 > q_0(\theta)] + \tau_0 \end{aligned}$$

If $Y_1 = h(Y_0)$, h is rank-preserving and $\tau_0 = 0$, so the second part is proved. Otherwise, we need to show that $\tau_0 > 0$. Note that

$$\begin{aligned} \tau_0 &= E[Y_1 I(Y_1 > q_1) - Y_1 I(Y_0 > q_0)] / P(Y_0 > q_0) \quad \text{since} \quad P(Y_1 > q_1) = P(Y_0 > q_0), \quad \text{so,} \\ \tau_0 &\geq E[Y_1 (I(Y_1 > q_1) - I(Y_0 > q_0))] = E[Y_1 | Y_1 > q_1, Y_0 < q_0] p_1 - E[Y_1 | Y_1 < q_1, Y_0 > q_0] p_0 \end{aligned}$$

where $p_1 \equiv \Pr[Y_1 > q_1, Y_0 < q_0]$ and $p_0 \equiv \Pr[Y_1 < q_1, Y_0 > q_0]$. Clearly $E[Y_1 | Y_1 > q_1, Y_0 < q_0] \geq E[Y_1 | Y_1 < q_1, Y_0 > q_0]$. Also, $p_1 = p_0$ because

$$\Pr[Y_1 > q_1, Y_0 < q_0] + \Pr[Y_1 > q_1, Y_0 > q_0] = \theta = \Pr[Y_1 > q_1, Y_0 < q_0] + (\theta - \Pr[Y_1 < q_1, Y_0 > q_0]).$$

This establishes the upper bound. The lower bound is a consequence of the fact that

$$\begin{aligned} E[Y | D = 1, Y > q_0(\theta)] - E[Y | D = 0, Y > q_0(\theta)] &= E[Y_1 - Y_0 | Y_0 > q_0(\theta)] + \{E[Y_1 | Y_1 > q_0(\theta)] - E[Y_1 | Y_0 > q_0(\theta)]\} \\ \text{and } E[Y_1 | Y_0 > q_0(\theta)] &= E[Y_1 | Y_1 \geq Y_0 > q_0(\theta)] \geq E[Y_1 | Y_1 > q_0(\theta)]. \end{aligned}$$

Note that we can choose a quantile, θ_0 , such that $q_0(\theta_0) = 0$, and then drop the lower θ_0 percent of the Y_1 distribution to obtain an upper bound on $E[Y_1 - Y_0 | Y_0 > 0]$. At the same time, the conditional-on-positive contrast in scores provides a lower bound. Moreover, if Y_1 and Y_0 are deterministically linked, the upper bound provides an

estimate of $E[Y_1 - Y_0 | Y_0 > 0]$.⁹ We exploit this simple strategy to estimate bounds for treatment effects at a number of points in the score distribution.

4.2 Estimates

Given the high validation rate for ID matches, it seems reasonable to focus on test scores for all students with an ID match. Language scores among all tested students averaged about 47 points, with a standard deviation of 5.6. The effect on the Language score of those who were tested is .7 with a standard error of .33. This can be seen in column 1 of Table 4a, which also reports estimated treatment effects using winsorized outcomes and Tobit censoring corrections. Coding non-takers' scores as zeros and including these in the analysis, so there is no sample selection bias, leads to an overall treatment effect of 3.1, with a standard error of .69. But this mostly comes from the increased probability of being tested among winners. An easier to-motivate variation on this approach rolls the scores of the bottom 1% up to the first percentile among takers, and adds non-takers to this group. This is equivalent to assuming non-takers would score in the first percentile, and analyzing impacts on the winsorized outcome. The result is a treatment effect of 1.1, with a standard error of .24, reported in column 3.

Columns 4-6 report Tobit coefficients estimated under alternative assumptions. A standard Tobit model in this case amounts to assuming non-takers would obtain a score less than or equal to 0 if tested; not surprisingly, this generates an implausibly large treatment effect of 9.8. To make the Tobit assumption more plausible, we artificially censor the data at points above zero. To see why this is valid, suppose that the observed outcome is generated by Tobit-type censoring above some positive threshold, c :

$$\begin{aligned} y_i(c) &= 1[y_i^* > c]y_i^*; \\ y_i^* &= X_i'\beta + \alpha D_i + \eta_i, \end{aligned} \tag{3}$$

where η_i is a Normally distributed error. Then it will also be true that

$$y_i(c + \delta) \equiv 1[y_i(c) > c + \delta]y_i(c) = 1[y_i^* > c + \delta]y_i^*, \tag{4}$$

so Tobit estimates of (3) for any positive δ are consistent for α as long as c is chosen so that untested student would score below this threshold if tested. Although $c=0$ is unlikely to work, censoring at higher quantiles seems plausible. Moreover, estimates of α can be compared under alternative choices of δ as an overidentification test for the combined assumptions of Tobit-type selection and Normal latent errors.

Changing the censoring point to the first percentile and then to the 10th percentile among takers, i.e., assuming non-takers would be located in these parts of the score distribution if tested, leads to significant estimates on the order of 2-4 points. Effects on Math scores, reported in Table 4b, are smaller than those on Language, though still significant after censoring at 1 percent and 10 percent. A separate analysis by sex leads to somewhat larger estimates for boys than girls.

Tobit estimates of α with c set at the first and 10th percentiles of the score distribution among takers are reported in columns 5 and 6. The Tobit estimates of α are 3.3 and 2.1, both reasonably precise. Assuming the Tobit model applies with censoring at the first percentile, the estimates should be the same at the 10th. The decline in estimates suggests censoring the first percentile is too low a threshold for non-takers. On the other hand, Tobit estimates of α are remarkably stable when the distribution is artificially censored with a cutoff that removes the lower 20-80% of scores. This can be seen in Figures 1a and 1b, which plot the estimate Tobit coefficients and confidence bands for alternative choices of c . The estimated treatment effects range from 1-2 points, turning down slightly when the lower 90% of scores among takers are censored. It should be noted, however, that the confidence intervals widen at this point. Moreover, Normality may be a poorer approximation for the score distribution at the upper tail. On balance, the model summarized in (2) and (3) seems to provide reasonably coherent account of the voucher impact on latent scores. Treatment effects in the 1-2 range amount to a score gain of .15-.33 standard deviations in the distribution

⁹ Krueger and Whitmore (2001) use this idea to estimate $E[Y_1 - Y_0 | Y_0 > 0]$ in a study of class size. See also Lee (2002).

for takers. Again, this is consistent with our earlier estimates of effects on achievement for a random sample of Bogota 8th graders in 1998.

Non-Parametric Estimates

A comparison of the entire distribution of test scores for winners and losers supports the notion that the voucher led to an increase in achievement by winners. Figures 2a and 2b, which plot kernel density estimates in the sample of all test takers, show slightly flattened and right-shifted distributions for winners. As with the comparisons of means, however, this contrast is contaminated by selection bias, in particular, the likely introduction of low-scorers into the sample of tested winners. Making the adjustment for sample selection suggested by Proposition 1 leads to a much clearer impression of a shift. This can be seen in Figures 3a and 3b, which plot score distributions after limited the distribution of winners to the top 28 percent of the score distribution (including zeros), the same proportion of the population as were tested among losers. Thus, Figure 3 plots scores conditional on $Y_0 > q_0(.28)$, where $q_0(.28) = 0$, and $Y_1 > q_1(.28)$. The adjusted figure shows a sharp rightward shift in the distribution for winners.

Non-parametric bounds on treatment effects at different quantiles, i.e., estimated bounds for $E[Y_1 - Y_0 | Y_0 > q_0(\theta)]$, are reported in Tables 5a and b for Language and Math scores, for θ_0 such that $q_0(\theta_0) = 0$, and for $\theta = .75, .85, .95$. The largest effects are at zero, i.e., effects on all pupils who would have been tested even if they had not won the lottery. The lower bound for effects on Language scores in this population is .68 (s.e.=.33), while the upper bound is 2.8 (s.e.=.31). The effects fall to an insignificant lower bound of .35 and an upper bound of 1.4 (s.e.=.34) for $\theta = .95$. Thus, the pattern of estimates by quantile suggests a larger shift in scores for pupils with Y_0 close the lower end of the score distribution among test takers than in the upper tail of the score distribution. The Tobit estimates with censoring between .2 and .8 are closer to the upper bound than the lower bound, suggesting these may be closer to the mark.

Separate estimates for boys and girls provide no evidence of a marked difference in treatment effects by sex. Effects conditional on $Y_0 > 0$, however, are larger for boys than girls. Finally, the effects on Math scores tend to be somewhat smaller than those on Language scores. Lower bounds on the estimated effects on Math scores are not significantly different from zero, though, given their similarity with the selection corrected estimates using other methods, the upper bound appears closer to the mark. An interesting reversal from the earlier parametric results are the slightly higher upper bounds for effects on Math scores relative to language scores, when looking at the upper tail of the score distribution.

The last non-parametric approach we used to estimate treatment effects on test scores free of selection bias is quantile regression. Assuming non-takers would score below the chosen quantile, differences in conditional quantiles by win/loss status are unaffected by selection bias and have a causal interpretation. The resulting estimates, reported in Table 6, show treatment effects of 5.5 points (s.e.=.67) on Language scores and 4.4 points (s.e.=.27) on Math scores at the .75 quantile, with much smaller though still significant effects at the .85 quantile. The effect on the .95 quantile is significant only for Language in the pooled sample, though effects in the upper tail are significant for both scores in the sample of boys. On balance, however, the quantile regression estimates also support the notion that the shift in distribution was concentrated at the bottom of the score distribution for test takers.

5. SUMMARY AND CONCLUSIONS

This paper presents evidence of the impact of educational vouchers on long-run outcomes for students who applied to the PACES program in Bogota. A study of PACES is of special interest because private secondary education accounts for over half of high school pupils in Bogota, and because the random assignment of vouchers facilitates causal comparisons. Administrative data on college entrance exams allow us to estimate the impact of vouchers on high school graduation and achievement. Two empirical problems in a study of this sort are elevated false match rates for winners and selection bias in comparisons among those who were tested. We show that the quality of record-keeping appears to have been similar for winners and losers so that false matches are probably not a problem. A variety of simply strategies are used to adjust for selection bias.

The results all point to an increase in high school graduation rates of as 5-7 percentage points, relative to a base rate of 25-30 percent. This is consistent with earlier results showing increased 8th grade completion among voucher winners on the order of 10 percentage points. The magnitude of estimated test scores gains turns partly on how selection bias is controlled. Tobit estimates with artificially censored data put the treatment effects between 1 and 2 points, roughly .15-.33F in the sample of those tested. Non-parametric bounds bracket this number, with a lower bound that is significantly different from zero for conditional-on-positive effects in the full sample. Since the upper bound is exact under the assumption of a monotone shift, and the pattern of Tobit estimates with artificial censoring is coherent, something close to a treatment effect of 2 points seems likely. Effects at higher quantiles of the score distribution and effects on Math scores are mostly somewhat smaller.

All the estimation methods used here point towards a substantial gain in both high school graduation rates and achievement as a result of the voucher program. The size and persistence of this impact suggests PACES was cost-effective. Although achievement the benefits of achievement gains per se are hard to quantify, there is a substantial economic return to high school graduation. At a minimum this suggests demand-side financing efforts along the lines of the PACES program warrant further study.

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DATA APPENDIX

ICFES-matching Details

The population to be matched consisted of 4,044 applicants from the 1995 PACES cohort in Bogota. Matching was carried out by searching computer files in the ICFES offices in Bogota. The search was implemented by our research assistants with assistance from ICFES staff members. ICFES tests are offered twice a year. We searched for matches among all test-takers in 1999, 2000, and 2001. A match is defined as an exact match of ID numbers. In addition, for some of the analyses in table 3, we validated matches by determining whether the test associated with a putative match was taken in Bogota, and by matching the first 7 letters of the registrants last name.

In the sample with an exact ID match, 27 registered but never took the exam. We code this event as a match but assigned a score of zero. Another 44 had scores recorded from tests offered in 1999, before the ICFES was a re-designed and a new score scale introduced. We assigned these individuals the mean score for their age/sex cell among takers in 2000 and 2001. Results are similar when the 1999 matches are treated as non-matches.

ICFES Subjects and Score Distribution

The ICFES scores used here are from the redesigned scoring system introduced in March 2000. The tests are from 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. In addition to these required tests, students choose to be tested in up to four areas of specialization. Our scores are for the Mathematics and Language components of the Common Core. 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 points. 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 Bogota in March 2000 are shown in the appendix figures (for 6,868 examinees).

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Table 1. Characteristics of ICFES Matching Sample by Voucher Status

	Means		Difference by Voucher Status			
	Full Sample	Sample W/ Valid Age	Full Sample	Sample w/ Valid Ages	Valid ID and Age	Valid ID and Age and Has Phone
	(1)	(2)	(3)	(4)	(5)	(6)
Won Voucher	.588	.585				
Valid ID	.876	.967				
Age at Time of Application	12.7 (1.8)	12.7 (1.3)	-.137 (.064)	-.086 (.045)	-.085 (.044)	-.091 (.047)
Male	.487	.493	.004 (.016)	.011 (.017)	.012 (.017)	.008 (.018)
Phone	.882	.886	.013 (.010)	.008 (.011)	.008 (.011)	---
N	4044	3661	4044	3661	3542	3139

Notes: Robust standard errors reported in parentheses. Regression estimates of differences by voucher status in column 4 are for sample with valid age data embedded in the National ID number. Column 5 limits the sample to those with a valid ID check digit and Column 6 further limits the sample to those with a phone. There are 1520 observations in Column 1 and 3664 in Column 2 for “Age at Time of Application.” Other sample sizes are as shown.

Table 2. Effect of Voucher Status on the Probability of Having a Valid ID

	Full Sample	Sample with Valid Age Data		Sample with Valid Age and Phone	
	(1)	(2)	(3)	(4)	(5)
A. All Applicants					
Dependent Variable Mean	.876	.968		.968	
Voucher Winner	-.010 (.010)	.001 (.006)	-.0002 (.0060)	.001 (.006)	.0001 (.0063)
Age			-.010 (.002)		-.008 (.002)
Phone			-.0003 (.0092)		
N	4044	3661	3661	3244	3244
B. Female Applicants					
Dependent Variable Mean	.862	.963		.971	
Voucher Winner	-.023 (.015)	-.001 (.009)	-.002 (.009)	-.002 (.010)	-.003 (.009)
Age			-.010 (.003)		-.010 (.003)
Phone			.003 (.013)		
N	2076	1857	1857	1631	1631
C. Male Applicants					
Dependent Variable Mean	.891	.971		.971	
Voucher Winner	.004 (.014)	.003 (.008)	.002 (.008)	.004 (.008)	.003 (.008)
Age			-.011 (.003)		-.006 (.003)
Phone			.001 (.013)		
N	1968	1804	1804	1613	1613

Notes: The table reports estimates of equation (1) in the text. Robust standard errors are shown in parentheses.

Table 3. Voucher Status and the Probability of ICFES Match

	Exact ID Match			ID and City Match			ID and 7-letter Name Match			ID, City, and 7-letter Match		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All Applicants												
Dependent Var.	.354		.352	.339	.339	.337	.331	.330	.330	.318	.317	.317
Mean												
Voucher Winner	.072 (.016)	.059 (.015)	.066 (.015)	.069 (.016)	.056 (.014)	.063 (.015)	.072 (.016)	.059 (.014)	.067 (.015)	.068 (.016)	.056 (.014)	.064 (.015)
Male		-.052 (.014)	-.046 (.015)	-.053 (.014)	-.053 (.014)	-.045 (.002)	-.043 (.014)	-.043 (.014)	-.038 (.015)	-.038 (.014)	-.045 (.014)	-.039 (.015)
Age		-.160 (.005)	-.162 (.005)	-.156 (.005)	-.156 (.005)	-.158 (.002)	-.153 (.005)	-.153 (.005)	-.154 (.005)	-.149 (.005)	-.149 (.005)	-.150 (.005)
N	3542	3542	3139	3542	3542	3139	3542	3542	3139	3542	3542	3139
B. Female Applicants												
Dependent Var.	.387		.383	.372	.372	.368	.361	.357	.357	.348	.344	.344
Mean												
Voucher Winner	.067 (.023)	.056 (.021)	.060 (.022)	.069 (.023)	.057 (.021)	.060 (.022)	.071 (.023)	.060 (.021)	.070 (.022)	.073 (.023)	.062 (.021)	.070 (.022)
Age		-.168 (.006)	-.169 (.007)	-.164 (.006)	-.164 (.006)	-.165 (.007)	-.160 (.006)	-.160 (.006)	-.161 (.007)	-.156 (.006)	-.156 (.006)	-.156 (.007)
N	1789	1789	1571	1789	1789	1571	1789	1789	1571	1789	1789	1571
C. Male Applicants												
Dependent Var.	.320		.321	.304	.304	.306	.302	.304	.304	.288	.290	.290
Mean												
Voucher Winner	.079 (.022)	.063 (.020)	.073 (.021)	.071 (.022)	.055 (.020)	.066 (.021)	.074 (.022)	.059 (.020)	.065 (.021)	.065 (.022)	.050 (.020)	.058 (.021)
Age		-.153 (.007)	-.154 (.007)	-.148 (.007)	-.148 (.007)	-.150 (.007)	-.146 (.007)	-.146 (.007)	-.147 (.007)	-.141 (.006)	-.141 (.006)	-.143 (.007)
N	1752	1752	1568	1752	1752	1568	1753	1753	1568	1753	1568	1568

Notes. Robust standard errors are shown in parentheses. The sample includes all Bogota 95 applicants with valid ID numbers and valid age data. The sample is the same as in Table 1, Column 5.

Table 4a. Regression Estimates of the Effects of the Vouchers on **Language** Scores

	Ordinary Least Squares			Tobit		
	OLS with score>0 (1)	OLS censored at 0 (2)	OLS censored at 1% (3)	Tobit censored at 0 (4)	Tobit Censored at 1% (5)	Tobit Censored at 10% (6)
A. All Applicants						
Dependent Variable	47.4	16.4	37.3	16.4	37.3	42.7
Mean	(5.6)	(22.8)	(8.0)	(22.8)	(8.0)	(4.7)
Voucher Winner	.70	3.07	1.14	8.81	3.29	2.06
	(.33)	(.69)	(.24)	(1.99)	(.70)	(.46)
Male	.73	-1.90	-.44	-5.76	-1.67	-.72
	(.32)	(.69)	(.24)	(1.95)	(.69)	(.44)
Age	-.99	-7.66	-2.57	-25.7	-9.03	-5.50
	(.19)	(.22)	(.08)	(1.03)	(.37)	(.24)
N	1223	3541	3541	3541	3541	3541
B. Female Applicants						
Dependent Variable	47.0	17.7	37.6	17.7	37.6	42.8
Mean	(5.7)	(23.0)	(8.1)	(23.0)	(8.1)	(4.7)
Voucher Winner	.74	2.76	1.04	7.57	2.88	1.86
	(.44)	(.98)	(.34)	(2.60)	(.91)	(.59)
Age	-1.14	-7.95	-2.65	-24.45	-8.54	-5.30
	(.26)	(.31)	(.11)	(1.34)	(.47)	(.32)
N	672	1788	1788	1788	1788	1788
C. Male Applicants						
Dependent Variable	47.8	15.0	37.0	15.0	37.0	42.5
Mean	(5.5)	(22.4)	(7.9)	(22.4)	(7.9)	(4.6)
Voucher Winner	.66	3.41	1.25	10.30	3.77	2.29
	(.48)	(.97)	(.34)	(3.10)	(1.10)	(.71)
Age	-.80	-7.36	-2.49	-27.29	-9.63	-5.74
	(.29)	(.32)	(.12)	(1.61)	(.57)	(.37)
N	551	1753	1753	1753	1753	1753

Note: Robust standard errors are in parentheses. Censoring point is the indicated percentile of the test score distribution, conditional on taking the exam. Standard deviations are reported for the dependent variable means. One observation with a 1999 score is lost relative to Table 3 because of missing covariates for the imputation.

Table 4b. Regression Estimates of the Effects of the Vouchers on **Math** Scores

	Ordinary Least Squares			Tobit		
	OLS with score>0 (1)	OLS censored at 0 (2)	OLS censored at 1% (3)	Tobit censored at 0 (4)	Tobit Censored at 1% (5)	Tobit Censored at 10% (6)
A. All Applicants						
Dependent Variable	42.5	14.7	35.7	14.7	35.7	37.6
Mean	(4.9)	(20.4)	(5.8)	(20.4)	(5.8)	(4.6)
Voucher Winner	.40	2.74	.79	7.9	2.29	1.98
	(.29)	(.63)	(.18)	(1.8)	(.51)	(.45)
Male	.43	-1.80	-.32	-5.3	-1.26	-.85
	(.28)	(.62)	(.18)	(1.7)	(.49)	(.43)
Age	-.37	-6.82	-1.73	-23.0	-6.30	-5.13
	(.18)	(.24)	(.07)	(.93)	(.26)	(.23)
N	1225	3541	3541	3541	3541	3541
B. Female Applicants						
Dependent Variable	42.3	15.9	35.9	15.9	35.9	37.8
Mean	(4.8)	(20.7)	(5.8)	(20.7)	(5.8)	(4.6)
Voucher Winner	.18	2.37	.62	6.68	1.84	1.60
	(.38)	(.89)	(.25)	(2.34)	(.66)	(.58)
Age	-.50	-7.09	-1.79	-21.88	-6.06	-4.89
	(.23)	(.34)	(.10)	(1.20)	(.34)	(.30)
N	673	1788	1788	1788	1788	1788
C. Male Applicants						
Dependent Variable	42.8	13.5	34.8	13.5	34.8	38.9
Mean	(5.0)	(20.1)	(6.1)	(20.1)	(6.1)	(3.8)
Voucher Winner	.70	3.13	1.02	9.40	3.03	2.21
	(.44)	(.88)	(.27)	(2.78)	(.85)	(.62)
Age	-.18	-6.54	-1.80	-24.34	-7.21	-4.58
	(.27)	(.33)	(.10)	(1.45)	(.44)	(.32)
N	552	1753	1753	1753	1753	1753

Note: Robust standard errors are in parentheses. Censoring point is the indicated percentile of the test score distribution, conditional on taking the exam. Standard deviations are reported for the dependent variable means. One observation with a 1999 score is lost relative to Table 3 because of missing covariates for the imputation.

Table 5a. Bounds on Voucher Effects on **Language** Scores, for Applicants Who Would Have Taken the ICSES Exam Anyway

Score Cutoff	Percentile	Loser's Avg Score Above Quantile (1)	No Controls		With Controls	
			Lower Bound (2)	Upper Bound (3)	Lower Bound (4)	Upper Bound (5)
A. All Applicants						
0	72 nd Percentile	46.92 (5.52)	.68 (.33)	2.81 (.31)	.70 (.33)	2.80 (.31)
41	75 th Percentile	48.68 (3.94)	.46 (.26)	2.47 (.26)	.49 (.26)	2.46 (.26)
47	85 th Percentile	51.23 (3.00)	.49 (.27)	2.39 (.28)	.50 (.27)	2.37 (.28)
52	95 th Percentile	55.60 (1.73)	.35 (.31)	1.38 (.34)	.36 (.31)	1.39 (.34)
B. Female Applicants						
0	67 th Percentile	46.61 (5.66)	.65 (.45)	2.29 (.42)	.74 (.45)	2.33 (.42)
43	75 th Percentile	49.18 (3.66)	.46 (.35)	1.86 (.35)	.52 (.35)	1.87 (.35)
47	85 th Percentile	51.49 (3.08)	.31 (.37)	2.32 (.40)	.32 (.37)	2.31 (.40)
52	95 th Percentile	55.73 (1.82)	.11 (.45)	1.32 (.52)	.11 (.45)	1.34 (.53)
C. Male Applicants						
0	74 th Percentile	47.33 (5.31)	.67 (.48)	3.19 (.45)	.66 (.48)	3.18 (.45)
36	75 th Percentile	47.91 (4.63)	.54 (.44)	2.61 (.42)	.54 (.43)	2.62 (.41)
47	85 th Percentile	51.57 (2.92)	.69 (.39)	1.36 (.39)	.69 (.38)	1.35 (.38)
51	95 th Percentile	54.58 (2.08)	.20 (.44)	2.33 (.46)	.27 (.45)	2.35 (.47)

Notes: The table reports bounds computed using the formulas in Proposition 1 in the text. Means and standard deviations are shown in column 1. Estimated bounds and standard errors are shown in columns 2-4.

Table 5b. Bounds on Voucher Effects on **Math** Scores, for Applicants Who Would Have Taken the ICFES Exam Anyway

Score Cutoff	Percentile	Loser's Avg Score Above Quantile	No Controls		With Controls	
			Lower Bound	Upper Bound	Lower Bound	Upper Bound
		(1)	(2)	(3)	(4)	(5)
A. All Applicants						
0	70 th Percentile	42.27 (4.75)	.40 (.29)	2.40 (.27)	.40 (.29)	2.41 (.27)
37	75 th Percentile	43.74 (3.75)	.35 (.25)	1.76 (.25)	.34 (.25)	1.76 (.25)
42	85 th Percentile	46.23 (3.18)	.24 (.28)	1.44 (.28)	.27 (.28)	1.48 (.28)
47	95 th Percentile	50.29 (2.40)	-.09 (.39)	1.85 (.42)	-.11 (.39)	1.80 (.43)
B. Female Applicants						
0	68 th Percentile	42.25 (4.67)	.14 (.38)	1.63 (.36)	.18 (.38)	1.66 (.36)
39	75 th Percentile	44.33 (3.34)	.24 (.32)	1.08 (.32)	.27 (.32)	1.09 (.32)
42	85 th Percentile	46.22 (2.79)	.28 (.34)	1.53 (.34)	.32 (.34)	1.55 (.34)
47	95 th Percentile	49.88 (1.56)	.06 (.40)	2.19 (.42)	.06 (.40)	2.18 (.42)
C. Male Applicants						
0	74 th Percentile	42.31 (4.86)	.70 (.44)	3.27 (.42)	.70 (.44)	3.28 (.42)
35	75 th Percentile	43.19 (4.26)	.66 (.40)	2.39 (.40)	.65 (.40)	2.39 (.39)
42	85 th Percentile	46.30 (3.66)	.27 (.46)	1.31 (.47)	.32 (.46)	1.37 (.47)
45	95 th Percentile	49.75 (3.16)	-.60 (.60)	2.45 (.70)	-.61 (.60)	2.44 (.71)

Notes: The table reports bounds computed using the formulas in Proposition 1 in the text. Means and standard deviations are shown in column 1. Estimated bounds and standard errors are shown in columns 2-4.

Table 6. Quantile Regression Results

Language Cutoff	Math Cutoff	Coeff on Voucher at	Language Scores		Math Scores	
			No Covs	Covs	No Covs	Covs
A. All Applicants						
44	40	75 th Percentile	3.00 (1.09)	5.50 (.67)	2.00 (.74)	4.40 (.27)
48	43	85 th Percentile	3.00 (.41)	3.00 (.44)	1.00 (.45)	1.00 (.72)
54	48	95 th Percentile	2.00 (.40)	1.50 (.65)	1.00 (.39)	1.00 (.50)
N			3541	3541	3541	3541
B. Female Applicants						
45	40	75 th Percentile	3.00 (1.33)	4.80 (1.25)	1.00 (1.34)	3.00 (.88)
48	43	85 th Percentile	3.00 (.27)	3.00 (.71)	0.00 (.77)	1.61 (.99)
54	48	95 th Percentile	2.00 (.80)	1.50 (.76)	1.00 (.60)	1.00 (.86)
N			1788	1788	1788	1788
C. Male Applicants						
44	39	75 th Percentile	8.00 (1.76)	4.00 (.44)	5.00 (1.32)	1.75 (.76)
48	43	85 th Percentile	3.00 (1.02)	2.00 (1.07)	1.00 (.44)	1.00 (.72)
54	47	95 th Percentile	2.00 (.82)	2.22 (.93)	1.00 (.58)	2.00 (.86)
N			1753	1753	1753	1753

Notes: The table reports quantile regression coefficients at the indicated quantiles. Quantile score cutoffs are derived from score distributions where non-takers' test scores are set to zero.

Figure 1a. Tobit Coefficients by Censoring Percentile in Language Score Distribution

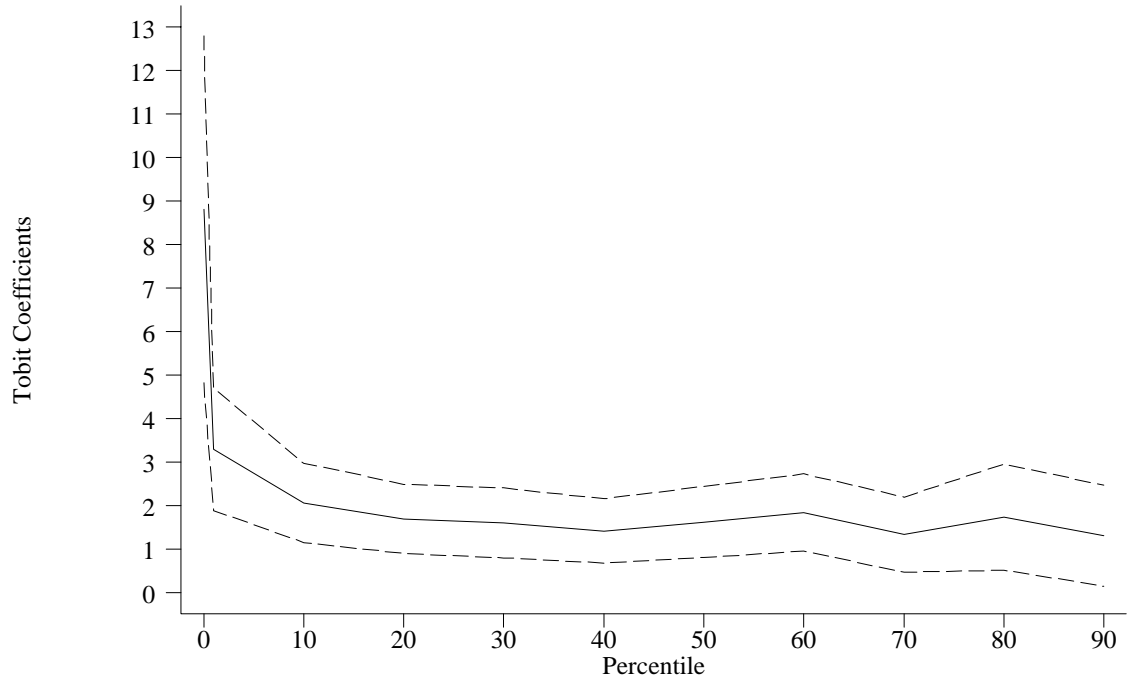


Figure 1b. Tobit Coefficients by Censoring Percentile in Math Score Distribution

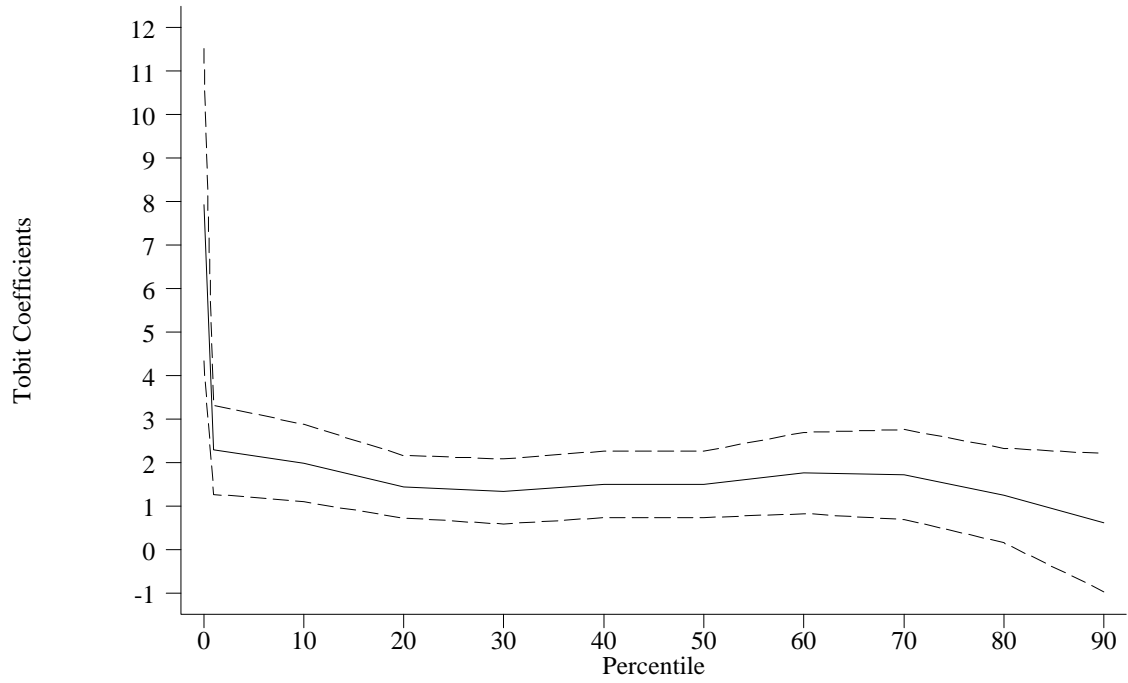


Figure 2a. Language Scores by Voucher Status (No Correction for Selection Bias)

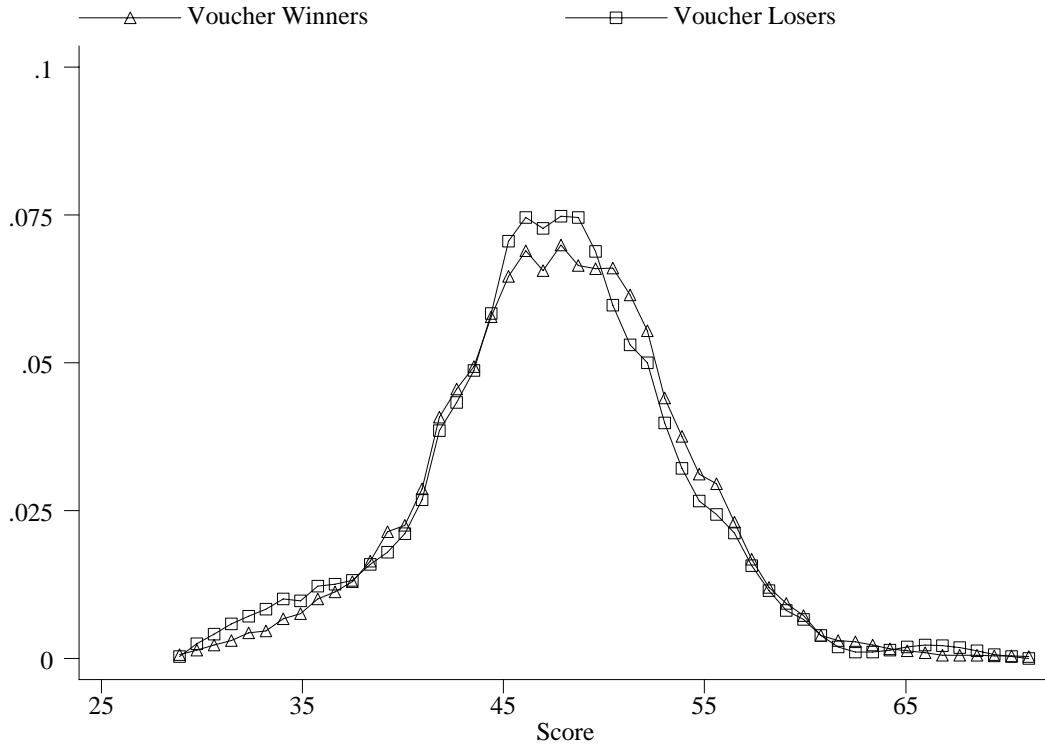


Figure 2b. Math Scores by Voucher Status (No Correction for Selection Bias)

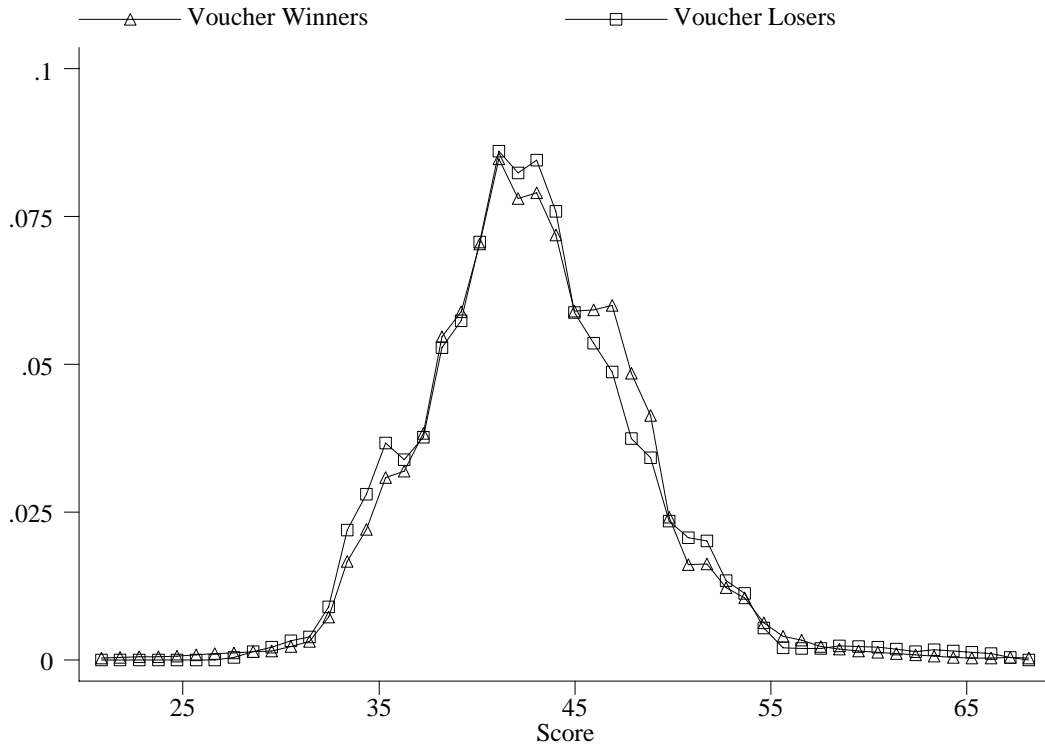


Figure 3a. Language Score Distribution by Voucher Status for Equal Proportions of Winners and Losers

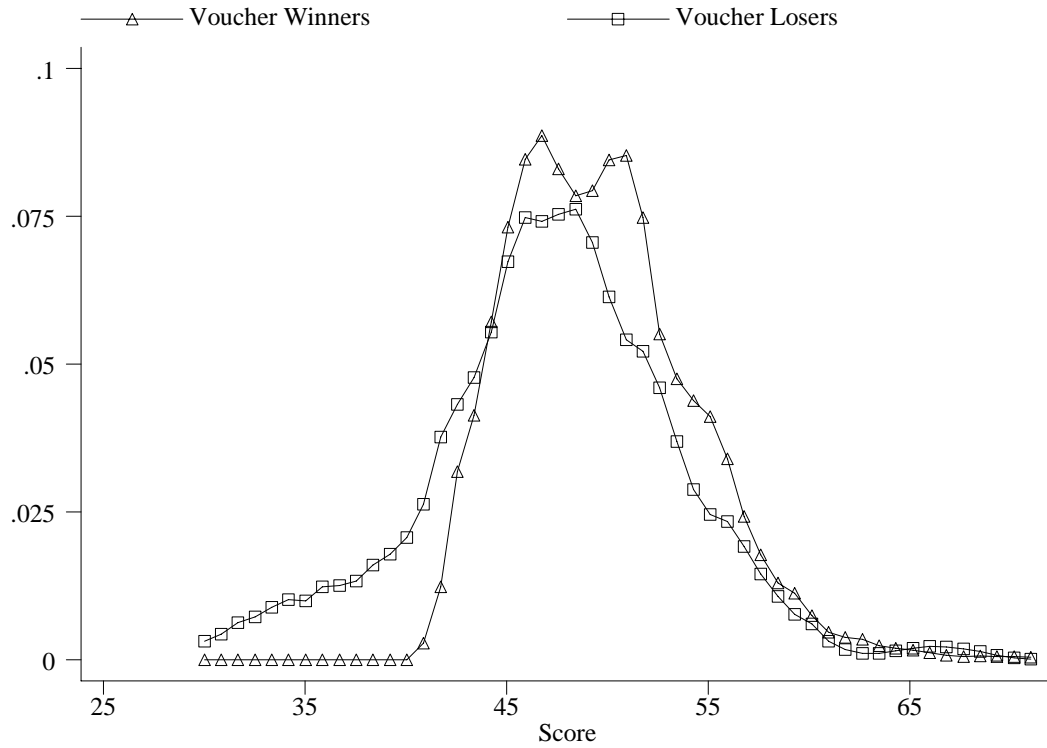
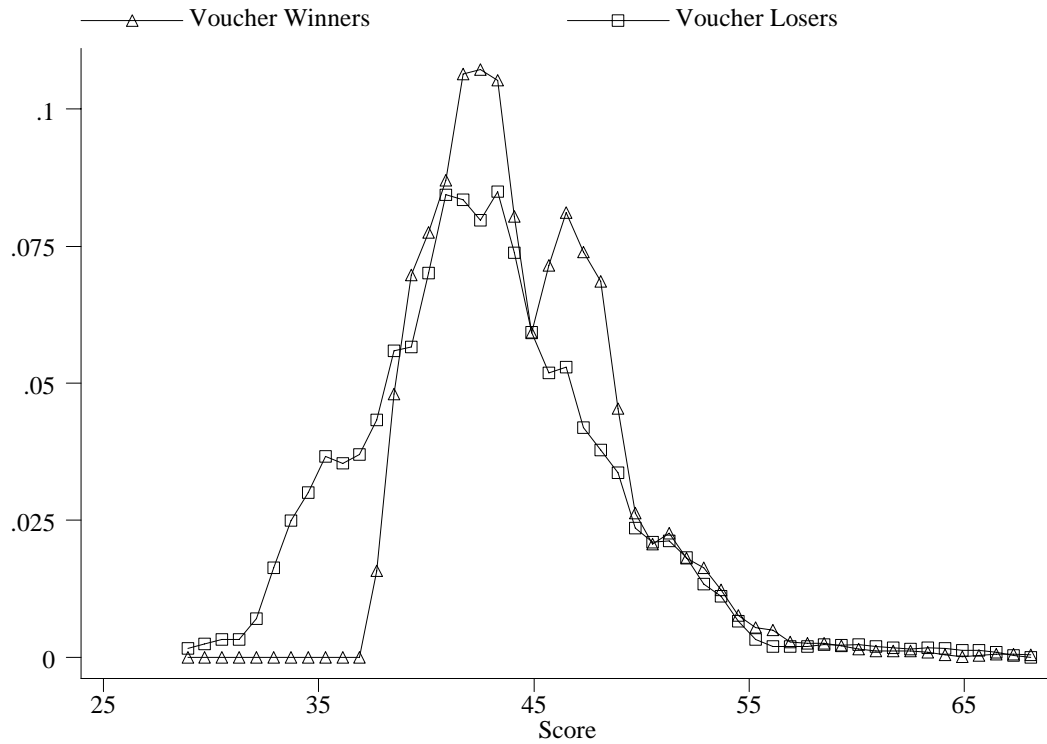
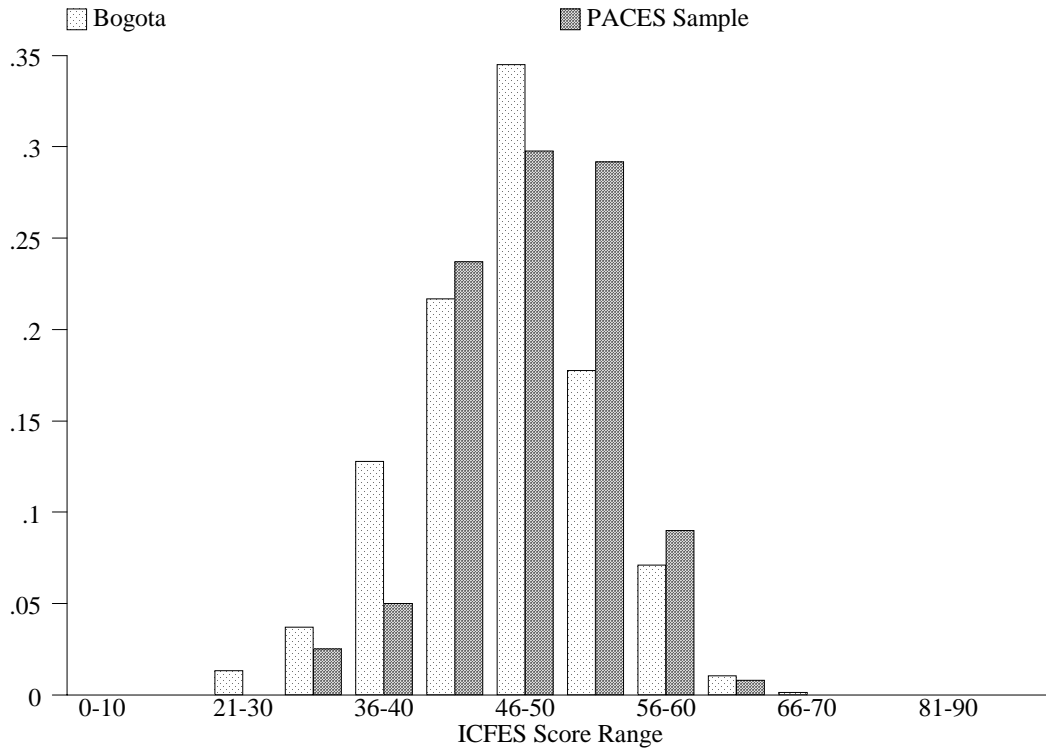


Figure 3b. Math Score Distribution by Voucher Status for Equal Proportions of Winners and Losers



Appendix Figure Ia. Distribution of Language Scores in Bogota versus PACES Sample



Appendix Figure Ib. Distribution of Math Scores in Bogota versus PACES Sample

