Long-Term Educational Consequences of Secondary School Vouchers: Evidence from Administrative Records in Colombia[*]

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Abstract

Colombia's PACES program provided over 125,000 poor children with vouchers that covered half the cost of private secondary school. The vouchers were renewable annually conditional on adequate academic progress. Since many vouchers were assigned by lottery, program effects can reliably be assessed by comparing lottery winners and losers. Estimates using administrative records suggest the PACES program increased secondary school completion rates by 15-20 percent. Correcting for the greater percentage of lottery winners taking college admissions tests, the program increased test scores by two-tenths of a standard deviation in the distribution of potential test scores. (*JEL* I21, J13, I28)

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I. Introduction

Demand-side subsidies for education are increasingly common in developing countries. Chile and Colombia have both offered educational vouchers for private secondary schools, while Brazil, India, Israel, and Mexico have introduced student stipends that reward attendance and performance. Interest in demand-side subsidies in developing countries parallels interest in the United States, where publicly-funded vouchers for private schools have been distributed in a number of cities.

Previous research on primary and secondary school vouchers typically focuses on the short-run effects of vouchers on test scores. The results so far suggest that vouchers benefit some groups of recipients, though the extent of test score gains is disputed.[1] 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.

This paper examines the longer-run effects of Colombia's PACES program, one of the largest voucher initiatives ever implemented.[2] Between 1991 and 1997, PACES awarded nearly 125,000 vouchers to low-income high school students. Since vouchers were renewable annually conditional on satisfactory academic progress as indicated by scheduled grade promotion, the program provided incentives for students to work harder as well as widening their schooling options. PACES vouchers may therefore have effects similar to merit-based college scholarships and test-based achievement awards (see, e.g. Angrist and Lavy 2003, Kremer, Miguel and Thornton, 2003, Dynarski 2003).

In Bogotá as well as a number of other large cities, PACES vouchers were awarded by lottery. The random assignment of vouchers facilitates a natural-experiment research design in which losers provide a comparison group for winners. Our previous research (Angrist, Bettinger, Bloom, King, and Kremer, 2002) used the voucher lotteries to show that in the three years after random assignment, PACES winners completed more years of school, had lower grade repetition, higher test scores, and a lower probability of working than did losers.

[1]See, e.g., Rouse (1998); Howell and Peterson (2002), Angrist et al. (2002) and Krueger and Zhu (2003).

[2]PACES is an acronym for Programa de Amplicación de Cobertura de la Educación Secundaria.

This paper examines the impact of winning the lottery on outcomes seven years after the voucher lottery. In particular, we use administrative data from Colombia's centralized college entrance examinations, the ICFES, to obtain information on high school graduation status and academic achievement.[3] ICFES registration is a good proxy for high school graduation since 90 percent of all high school students take the exam (World Bank, 1993). The principle advantage of administrative records in this context, in addition to providing longer-term outcomes, are that our measure of high school graduation status suffers no loss to follow-up and that ICFES test score data are much less expensive to obtain than survey data or scores from a specialized testing program.

Our analysis shows that voucher winners have substantially higher high school graduation rates than losers. Since more lottery winners than losers took the ICFES exam, direct comparisons of test scores for winners and losers are subject to selection bias. We therefore discuss a number of solutions to this selection problem, including parametric methods and non-parametric quantile-specific bounds. After adjusting for selection bias, voucher winners appear to have learned more than losers. The fact that the program increased test scores even fairly high in the score distribution suggests that the program increased learning not only by increasing incentives for students at risk of repeating grades, but also through other mechanisms, such as increasing school choice.

The next section provides additional background on the PACES program and voucher lotteries. Section III presents estimates of the effect of PACES vouchers on high school graduation rates, as measured by ICFES registration rates. Section IV discusses the problem of selection bias in analyses of test scores and presents estimates of effects on scores using alternative approaches to the selection problem. Section V concludes the paper.

[3]ICFES is an acronym for Colombia's college admissions testing service, the Instituto Colombiano Para El Fomento De La Educación Superior.

II. Background

A. The PACES program

The PACES program, established in late 1991, offered vouchers to children in low-income neighborhoods. To qualify for a voucher, applicants must have been entering the Colombian secondary school cycle, which begins with grade 6, and have been aged 15 or less. Prior to applying, students must already have been admitted to a participating secondary school (i.e., one that would accept vouchers) in a participating town, which included all of Colombia's largest cities.[4]

PACES vouchers were worth about 190 US dollars in 1998. Our survey data show matriculation and tuition 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 non-participating private schools. Schools with a vocational curriculum were also over-represented among those participating in the program. Many elite private schools opted out, so just under half of private schools in the 10 largest cities accepted PACES vouchers in 1993. In 1995, there were approximately 3.1 million secondary school pupils in Colombia. Almost 40 percent attended private schools, and about 8% of these used PACES vouchers.

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[4]PACES was meant to increase secondary school enrollment and was motivated in large part by the fact that many secondary schools were clearly very crowded, especially in large cities. The average Colombian school day was four hours and many of the school buildings in Bogotá hosted multiple sessions or schools per day. In fact, according to data available from ICFES, less than 2 percent of public schools in Bogotá hosted only one session per day, while 17 percent hosted three sessions per day. Private schools that accepted the voucher, by contrast, hosted fewer sessions. Almost 15 percent of private voucher schools had only one session per day and only 4 percent had three sessions per day. This may suggest less crowded schools or that schools days were either formally or informally longer in private voucher schools. Additionally, due to the short school day and multiple sessions per day, a possible source of capacity in the school system was the opportunity for teachers to teach simultaneously at both public and private schools.

Pupil-teacher ratios and facilities were similar in public and participating private schools, and test scores in participating private schools were close to those in public schools, though significantly below those in non-participating private schools. Public schools and private voucher schools had similar access to technology (King, Rawlings, Gutierrez, Pardo, and Torres, 1997). Public schools and private voucher schools in Bogotá also perform similarly on the ICFES exam. The median ICFES score is similar across schools although public schools tend to have a larger number of students take the exams than private voucher schools. By contrast, the median score in private non-voucher schools in Bogotá is much higher than the median score at both public and vocuher-accepting private schools in Bogotá. Across the country, both private and public schools in Bogotá have higher ICFES scores than other areas of Colombia although since we can only compare voucher schools in Bogotá, we cannot compare voucher schools inside and outside of Bogotá.

Vouchers were renewed automatically through eleventh grade, when Colombian high school ends, provided the recipient's academic performance warranted promotion to the next grade. In practice, approximately 86 percent of voucher winners were promoted in 6th grade (as compared to 80 percent of voucher losers). It therefore seems likely that the incentive aspects of the PACES program were strongest for the weakest students, since these students were at greatest risk of failing a grade.

Our earlier results (Angrist et al, 2002) suggest that three years after entering the lottery, voucher winners were 16 percentage points more likely to be attending a private school. Fee payments for voucher winners were $52 higher than for losers, suggesting that some winners may have used their vouchers to trade up to higher priced schools. Although lottery winners and losers had similar enrollment rates, winners had completed .12 additional years of school, partly because they were 6 percentage points less likely to have repeated a grade. But winners also appear to have learned more. Among a sub-sample of lottery applicants who agreed to take a standardized test, winners scored .2 standard deviations more than losers, the equivalent of a full grade level. However, the sample of test takers was small (only 283), and hence these differences were only marginally significant at conventional levels. Moreover, since only 60 percent of those invited to take the test did so, sample selection issues remain a concern.

B. Data and Descriptive Statistics

Table 1 reports data from 4,044 application forms completed by applicants who applied in 1994 to enter private school in 6th grade in 1995 in Bogota. 59 percent of these applicants were awarded vouchers. 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 1999-2001 ICFES records using national ID numbers, an identification number consisting of 11 digits, the first 6 of which show date of birth.[5] A final "check digit" in the ID number 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 percent of applicants had invalid birth dates.[6] Among applicants with valid birth dates, 97 percent reported valid ID numbers.

There is no evidence that voucher winners are more likely to be matched with ICFES records because they have more accurately recorded ID numbers.[7] In fact, voucher winners were 1 percentage point *less* likely to have a valid ID, although this difference is not significantly different from zero, as can be seen in column 3, row 2 of Table 1. The results of restricting the sample to those with valid birth dates embedded in their ID numbers, reported in columns 4, show an even smaller voucher effect which is also statistically insignificant.

Voucher winners and losers had similar demographic characteristics, except possibly for a small age difference. These contrasts can be seen in the remaining rows of Table 1. The age differences by

[5]Angrist, Bettinger, and Kremer (2004) provide a detailed description of the matching procedure and the ICFES.

[6]Birth dates are considered valid when they imply applicants were aged 9-25.

[7]We found some evidence of differential record-keeping in the first cohort of Bogotá applicants from 1992, before the lottery process was computerized. Because of this and other data problems, the 1992 applicant cohort was omitted from this study.

voucher status appear to be driven by a few outlying observations, probably due to incorrectly coded ID numbers among losers. The age gap falls when the sample is limited to those with valid ID numbers, though it remains marginally significant. We therefore control for age when estimating voucher effects.

III. Effects on High School Graduation

As noted in the introduction, we use ICFES registration as a proxy for high school completion because 90 percent of graduating students take the ICFES exam. Estimates of voucher effects on high school graduation rates were constructed using the following regression model:

$$y_i = X_i'\beta_0 + \alpha_0 D_i + \epsilon_i, \tag{1}$$

where y_i is an indicator of ICFES registration, D_i is an indicator for whether applicant *i* won a voucher, and X_i is a vector of controls for age and sex. We also report estimates without covariates. 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 skipped or repeated grades, we also checked those registered for the exams offered in 1999 and 2001. If a student was found to have been tested more than once, we retained the first set of test scores.

About 35 percent of PACES applicants were matched to ICFES records using ID numbers, a result that can be seen in the first row of Table 2. This rate falls to 33-34 percent when city of residence or the first 7 letters of students' last names were also matched, and to 32 percent when both city of residence and the first 7 letters of students' last name were matched.

Results from models with no controls, reported in column 1, show that vouchers raised ICFES registration rates about 7 percentage points, a highly significant difference.[8] Because of the slight differences between voucher winners and losers reported in Table 1, the estimated effect falls to about 6 percentage points with demographic controls but remains significantly different from zero. There is no clear pattern of differences in voucher effects by sex, though the base rate is lower for boys. Using city of

[8]The standard errors reported in Table 2 and elsewhere are heteroscedasticity-consistent.

residence to validate matches leads to slightly smaller treatment effects for girls and overall, but the change is not substantial. Validation by matching on names as well as ID numbers leads to treatment effects almost identical to those without validation, and validation using both city and name generates estimates similar to those using city only.[9]

On balance, the estimated effects of voucher status on ICFES registration are remarkably robust to changes in sample, specification, and the definition of a match. 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 for both boys and girls. This amounts to an increase of 15-20 percent in the probability students took the ICFES exam.

IV. Effects on College Entrance Exam Scores

A. The Selection Problem

Because PACES voucher winners were more likely than losers to take the test, the test score distributions of winners and losers are not directly comparable. To see the likely consequences of differential test-taking rates for comparisons of scores among test-takers, let y_{1i} be the ICFES score student *i* would obtain after winning the voucher and let y_{0i} denote the 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, and whether they won the lottery 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 observe scores for those who were tested. Moreover, among tested pupils, we only observe y_{1i} for winners and y_{0i} for losers.

[9]Our previous estimates show voucher winners were less likely to have repeated a grade, and hence, as time goes on, we might expect the gap in ICFES registration rates between winners and losers to decline as more losers finish secondary school. But voucher winners who were had previously repeated a grade may have been more likely than losers to take the ICFES, so it is unclear whether the gap in ICFES registration rates should have increased or decreased over time. In practice, the gap in test-taking rates by win-loss status does not does not appear to have been declining over time.

Using a notation paralleling the notation for potential test scores, let T_{1i} and T_{0i} denote potential test-taking status. That is, T_{1i} is a dummy for whether a student would have taken the ICFES after winning the lottery and T_{0i} is a dummy for whether a student would have taken the ICFES after losing the lottery. By virtue of the random assignment of D_i, the vector of all potential outcomes $\{y_{1i}, y_{0i}, T_{1i}, T_{0i}\}$ is jointly independent of D_i, though the elements of this vector are probably correlated with each other. Observed test-taking status, the dependent variable in the previous section, is linked to these potential outcomes by the equation:

$$T_i = T_{0i} + (T_{1i} - T_{0i})D_i.$$

Similarly, the latent score variable (i.e., what we would observe if all students were tested) is

$$y_i = y_{0i} + (y_{1i} - y_{0i})D_i.$$

The observed win/loss contrast in test scores among those who were tested can be now written:

$$\begin{aligned} E[y_i | T_i=1, D_i=1] - E[y_i | T_i=1, D_i=0] &= E[y_{1i} | T_{1i}=1, D_i=1] - E[y_{0i} | T_{0i}=1, D_i=0] \\ &= E[y_{1i} | T_{1i}=1] - E[y_{0i} | T_{0i}=1], \end{aligned}$$

where the second equality is because D_i is randomly assigned. This contrast does not have a causal interpretation, since students with $T_{1i}=1$ and $T_{0i}=1$ are not drawn from the same population unless D_i has no effect on the probability of being tested. In fact, we can expand this further to write

$$\begin{aligned} E[y_i | T_i=1, D_i=1] - E[y_i | T_i=1, D_i=0] &= E[y_{1i} - y_{0i} | T_{0i}=1] \\ &\quad + \{E[y_{1i} | T_{1i}=1] - E[y_{1i} | T_{0i}=1]\}. \end{aligned} \tag{2}$$

Equation (2) shows that the win/loss contrast among test-takers is equal to the average causal effect on those who would have been tested anyway, $E[y_{1i} - y_{0i} | T_{0i}=1]$, plus a term that captures the selection bias due to the fact that we are conditioning on ICFES registration status, itself an outcome that is correlated with potential test scores.

The bias term in equation (2) is likely to be negative if PACES vouchers increased test scores. To illustrate this, suppose that $y_{1i} = y_{0i} + \alpha$, where $\alpha > 0$, and that students choose to be tested if their potential scores exceeded a constant threshold, μ. Then $T_{ji}=1[y_{ji}>\mu]$ for j=0,1; and the selection bias is

$$E[y_{0i}|\, y_{0i}>\mu-\alpha] - E[y_{0i}|\, y_{0i}>\mu],$$

which is clearly negative. Of course, this example presumes that vouchers are never harmful. If vouchers were harmful, selection bias could (by the same argument) mask a negative treatment effect.

To provide an empirical basis for the claim that selection bias in the sample of ICFES takers is likely to be negative, we used test scores from our earlier *random* sample of Bogota students, the sample used by Angrist, *et al* (2002) to assess the effect of vouchers in learning. Our sample of 259 tested students is somewhat more likely to have taken the ICFES test than the overall average test rate for the 1995 Bogota cohort (about 44% versus 35% overall). Importantly, however, and in contrast with the ICFES test, the likelihood of taking our test is the same for voucher winners and losers. Thus, the earlier sample of test takers is not be contaminated by self-selection bias of the sort affecting ICFES takers (though there is missing score data for other reasons).

A regression using stacked math and reading scores from the earlier testing sample generates a voucher effect of .186, with effects measured in standard deviation units (and with a standard error adjusted for student clustering of .105). Limiting this sample to the roughly 44% of tested students who also took the ICFES generates a voucher effect of .044 (s.e.=.157). The pattern of substantial (and usually marginally significant) positive effects in the full sample and considerably smaller and insignificant treatment effects when this simple is limited to those who also took the ICFES test appears for all dependent variables and specifications. This finding illustrates the fact that conditioning on ICFES testing status almost certainly drives positive treatment effects towards zero.

B. Parametric strategies

In a first attempt to adjust for selection bias, we used a modified Tobit procedure. In particular, we fit parametric models to artificially completed score data constructed by censoring observed scores at or above a particular value or quantile, with all those below this point *and non-takers* assigned the censoring point. Subject to the normality assumption, this provides consistent estimates of treatment effects on the latent scores of all students, assuming those not tested would have scored below the artificial censoring point. Moreover, a comparison of Tobit results using different censoring points provides a natural specification test for this procedure since, if correctly specified, results using different censoring points should be similar. A key drawback in this case is the need to assume normality of the uncensored latent score distribution. The quality of the normal approximation may be especially poor given the relatively discrete nature of the score data. We therefore discuss alternative approaches in the next section.

The idea behind the parametric approach is spelled out in more detail below. We assume that the causal effect of interest could be estimated by regressing latent scores, y_i, on D_i and covariates, X_i. That is, the regression of interest is

$$y_i = X_i'\beta + \alpha D_i + \eta_i, \tag{4}$$

where η_i is a normally distributed error. Now, construct an artificially censored dependent variable using

$$Y_i(\tau) \equiv 1[T_i y_i \geq \tau] y_i + 1[T_i y_i < \tau]\tau \tag{5}$$

for some positive threshold, τ. Assuming any untested student would have scored at or below this threshold if they had been tested, the parameters in (4) can be consistently estimated by applying Tobit to $Y_i(\tau)$. This is not realistic for τ=0 but it may be for, say, the 10^{th} percentile of the score distribution among test-takers. Finally, note that if Tobit using $Y_i(\tau_0)$ identifies α, then Tobit using $Y_i(\tau_1)$ will also work for any threshold value τ_1, such that $\tau_1 > \tau_0$.[10]

As a benchmark for this procedure, we again report estimates using the sample of test-takers, without adjusting for censoring. Among test-takers, winners scored about 0.7 points higher on the language exam, with similar though less precise effects in samples of boys and girls. These results are reported in column 1 of Table 3.[11] The estimated effects for math scores are smaller though still positive.

[10]As a partial check on this we compared the scores of ICFES takers and non-takers on our earlier achievement tests. Assuming percentile scores on the two tests are similar, this comparison is informative about the assumption invoked here. Indeed, the two test scores are highly correlated. Moreover, a comparison of earlier test results by ICFES-taker status shows markedly lower average scores and a clear distribution shift to the left for ICFES non-takers relative to ICFES takers.

[11]The same covariates and sample were used to construct the results in Tables 2-5.

Including all students and censoring both non-takers and low scorers at the first percentile of the test generates a voucher effect of 1.1 (s.e.= .24) for language and 0.79 (s.e.=.18) for math. This can be seen in column 2 of Table 3.

Tobit estimates using data censored at the first and 10th percentiles among test-takers suggest much larger effects than those that arise without correcting for selection bias. The Tobit estimates are on the order of 2-4 points for language and 2-3 points for math, in all cases significantly different from zero (reported in columns 3 and 4). Effects are at the lower end of this range, around 2 points, when the data are censored at the 10th percentile. The estimates using artificially censored data tend to be somewhat larger for boys than for girls.

Assuming the Tobit model applies to data censored at the first percentile, the Tobit coefficient estimates should be the same when estimated using data censored at the 10th. The decline in estimates moving column 3 to column 4 of Table 3 therefore suggests the first percentile is too low a threshold for the Tobit model to apply. On the other hand, Tobit estimates of α are remarkably stable when the distribution is artificially censored with a cutoff that removes the lower 10-80 percent of scores. This can be seen in Figures 1a and 1b, which plot the estimated Tobit coefficients and confidence bands for alternative censoring points. The estimated treatment effects are fairly stable at around 2 points, turning down slightly when the lower 90 percent of scores among takers are censored. It should be noted, however, that the confidence intervals widen at this point. Moreover, normality may be a worse approximation for the upper tail of the score distribution.

On balance, the model described by equations (4) and (5) seems to provide reasonably coherent account of the voucher impact on latent scores. Effects in this range amount to a score gain of about $.2\sigma$ where σ is the standard deviation of the latent residual in equation (4). This is consistent with our earlier estimates of effects on achievement for a random sample of Bogotá 8th graders in 1998.[12]

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[12]Effect sizes calculated using the distribution of the latent Tobit residual seem like an appropriate standard of comparison since the testing strategy used in our earlier study can be thought of as providing estimates of effects on latent scores (i.e., scores when all applicants in the relevant sample were tested whether or not they registered for ICFES). While the positive treatment effect found here is consistent with our previous paper, it is unclear whether the magnitude of the voucher effect should have increased or decreased. On one hand, the private school attendance

C. Non-Parametric Bounds

The strategies discussed in the previous section rely on strong functional-form and distributional assumptions. This section builds on the discussion above to derive a set of non-parametric bounds on quantile-specific program impacts on the distribution of test scores. Because selection bias is most likely negative when treatment effects are positive, selection-contaminated comparisons provide a lower bound on the likely impact of vouchers on achievement. Here, we also develop an upper bound by adapting a theoretical result from our earlier paper (Angrist, *et al*, 2002). Related studies discussing non-parametric bounds on selection bias include Manski (1989) and Lee (2002).

Suppose we are prepared to assume that winning the lottery was never harmful, i.e., that $y_{1i} \geq y_{0i}$ for all *i*. The identifying power of this *monotone treatment response* assumption is discussed by Manski (1997). In this context, monotone treatment response seems reasonable since lottery winners were free to turn down their vouchers and attend public school if they felt continued voucher use was harmful.[13] It also seems reasonable to assume $T_{1i} \geq T_{0i}$ since test-taking status is probably determined by, among other things, expected scores. Finally, it is useful to define a score variable that equals zero for those not tested:

$$Y_{ji} = T_{ji} y_{ji} \text{ for } j=0,1. \qquad (3)$$

Note that given our "never harmful" assumptions, we also have $Y_{1i} \geq Y_{0i}$ for all i, and that

$$E[Y_{1i} - Y_{0i} | T_{0i}=1] = E[y_{1i} - y_{0i} | T_{0i}=1].$$

The observed outcome, Y_i, is linked to potential outcomes by

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i = T_{0i}y_{0i} + (T_{1i}y_{1i} - T_{0i}y_{0i})D_i.$$

gap was growing in 7th and 8th grades. On the other, within three years of the voucher lottery, 50 percent of voucher winners were no longer using the voucher, so effects may have waned.

[13]This condition need not hold if some students who chose to use vouchers anticipated gains that did not materialize, with a subset ending up having been harmed by voucher use. In practice, however, the "never harmful" assumption is made more plausible by the fact that vouchers typically did not cover the entire cost of private school. This means that students using vouchers presumably expected gains large enough to outweigh the financial costs of attending private school, with a low risk of adverse academic effects.

To simplify notation, drop subscripts for individuals and let $q_0(\theta)$ be the θ-quantile of the distribution of Y_0 and let $q_1(\theta)$ be the θ-quantile of the distribution of Y_1.

For the development that follows, it's useful to define a rank-preservation restriction on the joint distribution of (Y_0, Y_1):

Definition. The random variable Y_1 is said to be a *θ-quantile-preserving transformation* (θ-QPT) of the random variable Y_0 if $P(Y_1 \geq q_1(\theta) \mid Y_0 \geq q_0(\theta))=1$.

Note that Y_1 is a θ-QPT of Y_0 if the two potential outcomes are linking by a weakly increasing function, or if, for any two draws from the joint distributions of Y_1 and Y_0, the orderings of Y_1 and Y_0 are the same. The θ-QPT concept extends this idea of rank-preservation to quantile-specific comparisons.[14]

The following proposition establishes a set of quantile-specific bounds on average treatment effects in the presence of sample selection bias (the proof appears in the appendix):

Proposition 1. Suppose that $y_1 \geq y_0$ and $T_1 \geq T_0$. Choose $\theta \geq \theta_0$ where $q_0(\theta_0)=0$. Then

$$E[Y| D=1, Y>q_1(\theta)] - E[Y| D=0, Y>q_0(\theta)] \geq E[y_1 - y_0| y_0>q_0(\theta), T_0=1]$$

$$\geq E[Y| D=1, Y>q_0(\theta)] - E[Y| D=0, Y>q_0(\theta)].$$

Furthermore, if Y_1 is a θ-quantile-preserving transformation of Y_0, then the left inequality is an equality.

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 $E[y_1 - y_0| T_0=1]$. At the same time, the unadjusted conditional-on-positive contrast in test scores provides a lower bound. Moreover, if Y_1 and Y_0 are linked

[14]Athey and Imbens (2002), Blundell, Gosling, Ichimura, and Meghir (2004), and Chernozhukov and Hansen (2005) discuss the identifying power of similar assumptions. Note that θ-QPT does not amount to perfect rank correlation unless it holds for all quantiles. In practice, θ-QPT seems more plausible at upper quantiles of the latent score distribution since jumps or leap-frogging by non-takers who win vouchers is unlikely above high quantiles.

by a θ-QPT, the upper bound provides an estimate of $E[y_1 - y_0 | T_0=1]$.[15] We can use this fact to estimate or bound average treatment effects at a number of points in the score distribution.

Estimates of non-parametric bounds on treatment effects at different quantiles are reported in Table 4 for language and math scores, for θ_0 such that $q_0(\theta_0)=0$, and for θ=.75, .85., and .95. The largest effects are at $q_0(\theta_0)=0$, 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). For θ=.95, the bounds fall to an insignificant .35 on the low end and still-significant 1.4 (s.e.=.34) on the high end. The pattern of bounds by quantile is consistent with either a larger shift in scores for pupils with Y_0 close to the low end of the test-takers' score distribution, or with a tightening of the upper bound at higher quantiles, or both.

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. Panel A of Figure 2, which plots 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. Adjusting for sample selection bias as suggested by Proposition 1 leads to a clearer impression of a shift. This can be seen in Panel B, which plots score distributions after limiting the distribution of winners to the top 28 percent of the score distribution (including zeros). In other words, Panel B plots scores conditional on $Y_0 > q_0(.72)$, where $q_0(.72)=0$, and $Y_1 > q_1(.72)$. The adjusted figure shows a clearer rightward shift in the distribution for winners, especially in the middle of the density.

The density plots and differences in average treatment effects reported for different quantiles in Table 4 suggest the PACES program had an impact on the distribution of test scores beyond a simple "location shift." As a final exploration of distribution effects (and to quantify the impression left by the

[15]The latter result can also be understood as follows. Angrist (1997) shows that monotonicity in selection status ($T_{1i} \geq T_{0i}$), combined with a constant-effects link between y_1 and y_0, implies that controlling the probability of sample selection eliminates selection bias. Symmetric truncation is equivalent to fixing the probability of sample selection. The proposition generalizes this result to models with a non-constant but still rank-preserving link between potential outcomes. Krueger and Whitmore (2001) used a similar idea to estimate $E[y_1 - y_0 | T_0=1]$ in a study of class size.

figures), we estimated the impact of winning a voucher at different points in the cumulative distribution of test scores. In particular, we estimated voucher effects in equations analogous to equation (1), with the dependent variable given by $1[Y_i>c]$, where c is a quantile in the score distribution among test takers. This procedure uses a sample where test scores for non-takers are coded as zero. Therefore, assuming that those not tested would have scored below c, the resulting estimates are unaffected by selection bias.[16]

Estimates of effects on the distribution of test scores, reported in Table 5, show the largest impact on the probability test scores exceeded the lowest decile in the score distribution (among test-takers). For example, the probit marginal effects of the impact of a voucher on the probability of crossing the first decile are .063 (s.e.=.015) for the language score distribution and .068 (s.e.=.016) for the math score distribution. These estimates appear in columns 3 and 6 of Table 5. The corresponding estimates fall to a bit over .04 at the median, and then to around .025 at the 75th percentile. It seems unlikely that many students at the 75th percentile of the distribution of test scores among test takers were in danger of having to repeat a grade. The substantial impact of the program on the likelihood of scoring in the upper quartile among test takers therefore suggests the program operated through channels other than simply reducing the risk of grade repetition. Moreover, while the estimated distribution shifts at the upper decile in this specification are only .01 and .003 for language and math, the former effect is still significantly different from zero.

V. Summary and Conclusions

This paper presents evidence on the impact of PACES vouchers on relatively long-run educational outcomes for applicants to the Bogota voucher lottery. PACES vouchers subsidized private school attendance, and were renewable annually, conditional on grade advancement. The random assignment of vouchers facilitates causal comparisons between those who did and not receive vouchers.

[16]An alternative and perhaps more conventional procedure that captures effects on distribution while avoiding selection bias under the same assumptions is quantile regression (QR). In this case, however, quantile regression is made less attractive by the almost-discrete nature of the test scores. About 80 percent of the mass of the distribution of scores among takers falls into a range of 15 points or less. This near-discreteness causes QR estimates to behave poorly and invalidates standard asymptotic theory for QR since a regularity assumption for quantile regression is continuity of the dependent variable.

Administrative data on college entrance exams allow us to estimate the impact of vouchers on high school graduation rates and scholastic achievement.

The empirical results point to an increase in (proxy) high school graduation rates of 5-7 percentage points, relative to a base rate of 25-30 percent. This is consistent with our earlier results showing a 10 percentage point increase in 8^{th} grade completion rates among voucher winners, as well as gains on a standardized test we had administered to small sample of applicants. The magnitude of the test score gains in our follow-up study turn partly on how selection bias is controlled. Tobit estimates with artificially censored data put the treatment effects at around 2 points, or roughly $.2\sigma$ relative to the standard deviation of latent scores. Non-parametric bounds bracket this number, with a lower bound that is significantly different from zero. Since the upper bound is tight under the assumption of a rank-preserving treatment effect and the Tobit estimates satisfy a simple over-identification test, something close to the Tobit estimate of 2 points seems like a good summary estimate.

For the most part, bounds on average treatment effects at higher quantiles of the score distribution are smaller than those at the lower end. This may be because program effects were actually greatest at the bottom of the distribution, perhaps due to the incentive effects generated by PACES vouchers. However, it also seems likely that the upper bounds are tighter higher in the distribution for technical reasons having to do with the relationship between potential outcomes in the treated and non-treated states. In any case, the fact that lottery winners were substantially more likely to score in the top quartile on the national university entrance exam suggests that the program probably improved learning not only by increasing financial incentives, but also by expanding school choice.

On balance, our results suggest a substantial gain in both high school graduation rates and achievement as a result of the voucher program. Although the benefits of achievement gains *per se* are hard to quantify, there is a substantial economic return to high school graduation in Colombia. At a minimum this suggests demand-side financing efforts similar to the PACES program warrant further study. An unresolved question, however, is how to reconcile the consistently positive voucher effects for

Colombia reported here with more mixed results for the U.S. (See, e.g.., Rouse, 1998; Howell and Peterson 2002). One possibility is that PACES is a better experiment. Among U.S. voucher studies, even the randomized trials were comprised by complex research designs and substantial attrition. As it turns, the U.S. results are sensitive to how these problems are handled (Barnard, Frangakis, Hill and Rubin 2003; Krueger and Zhu 2003). Another possible explanation for divergent effects is a larger gap in the quality of public and private schools in Colombia. Finally, PACES included features not necessarily shared by other voucher programs, such as incentives for academic advancement and the opportunity for those who would have gone to private school anyway to use vouchers to attend more expensive schools.

APPENDIX

Proof of Proposition 1. $E[Y|\, D{=}j, Y>q_j(\theta)] = E[Y_j|\, Y_j>q_j(\theta)]$ by random assignment. Also,

$$E[Y_1|\, Y_1>q_1(\theta)] - E[Y_0|\, Y_0>q_0(\theta)] = E[Y_1-Y_0|\, Y_0>q_0(\theta)] + \{E[Y_1|\, Y_1>q_1(\theta)]-E[Y_1|\, Y_0>q_0(\theta)]\}$$
$$\geq E[y_1-y_0|\, y_0>q_0(\theta), T_0=1] + b_0.$$

If Y_1 is a θ-QPT of Y_0 then $b_0=0$, so the second part is proved. Otherwise, we need to show that $b_0 \geq 0$. Note that

$$b_0 = E[Y_1 1(Y_1>q_1) - Y_1 1(Y_0>q_0)]/P(Y_0>q_0)$$

since $P(Y_1>q_1) = P(Y_0>q_0)$, so,

$$b_0 \geq E[Y_1(1(Y_1>q_1) - Y_1 1(Y_0>q_0))]$$
$$= E[Y_1(1(Y_1>q_1) - 1(Y_0>q_0))] = E[Y_1(1(Y_1>q_1, Y_0<q_0)-1(Y_1<q_1, Y_0>q_0))],$$

where the second equality above is a consequence of the facts that $1(Y_1>q_1)=1(Y_1>q_1, Y_0<q_0)+1(Y_1>q_1, Y_0>q_0)$ and $1(Y_0>q_0)=1(Y_1<q_1, Y_0>q_0)+1(Y_1>q_1, Y_0>q_0)$. Further simplifying, we have

$$E[Y_1(1(Y_1>q_1, Y_0<q_0)-1(Y_1<q_1, Y_0>q_0))] = E[Y_1|\, Y_1>q_1, Y_0<q_0]p_1 - E[Y_1|\, Y_1<q_1,Y_0>q_0]p_0,$$

where $p_1=Pr[Y_1>q_1, Y_0<q_0]$ and $p_0=Pr[Y_1<q_1, Y_0>q_0]$. 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

$$P(Y_1>q_1) = p_1 + Pr[Y_1>q_1, Y_0>q_0] = \theta = Pr[Y_1>q_1, Y_0>q_0] + p_0 = P(Y_0>q_0).$$

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

$$E[Y|\quad D{=}1,Y>q_0(\theta)]-E[Y|\quad D{=}0\quad Y>q_0(\theta)]=E[Y_1-Y_0|Y_0>q_0(\theta)]+\{E[Y_1|Y_1>q_0(\theta)]-E[Y_1|\, Y_0>q_0(\theta)]\}$$

and

$$E[Y_1|\, Y_0>q_0(\theta)] = E[Y_1|\, Y_1 \geq Y_0>q_0(\theta)] \geq E[Y_1|\, Y_1>q_0(\theta)].$$

We can use this proof to get a sense of when the upper bound is likely to be tight. The bias of the upper bound is

$$b_0 \equiv E[Y_1|\, Y_1>q_1(\theta)]-E[Y_1|\, Y_0>q_0(\theta)],$$

which equals zero when Y_1 preserves the θ-quantile of Y_0. Since some of the applicants induced to take the test by winning the lottery presumably scored above the minimum score achieved by applicants who took the test after losing the lottery, the bound is unlikely to be perfectly tight. However, because few of these "leap-frogging" applicants are likely to have scored in the upper quantiles of the distribution, the likelihood that the θ-QPT assumption holds probably increases with θ. We should therefore expect upper bounds estimated using the proposition to be tighter at the top of the distribution than the bottom.

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Table 1. Characteristics of ICFES Matching Sample by Voucher Status

	Means		Difference by Voucher Status (winners vs. losers)			
	Full Sample (1)	Sample W/ Valid Age (2)	Full Sample (3)	Sample w/ Valid Ages (4)	Valid ID and Age (5)	Valid ID and Age and Has Phone (6)
Won Voucher	.588	.585				
Valid ID	.876	.967	-.010	.001	--	--
			(.010)	(.006)		
Age at Time of Application	12.7	12.7	-.137	-.086	-.085	-.091
	(1.8)	(1.3)	(.064)	(.045)	(.044)	(.047)
Male	.487	.493	.004	.011	.012	.008
			(.016)	(.017)	(.017)	(.018)
Phone	.882	.886	.013	.008	.008	---
			(.010)	(.011)	(.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. A valid age must be between 9 and 25. 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. 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)
A. All Applicants (N=3542)								
Dependent Var. Mean	.354		.339		.331		.318	
Voucher Winner	.072	.059	.069	.056	.072	.059	.068	.056
	(.016)	(.015)	(.016)	(.014)	(.016)	(.014)	(.016)	(.014)
Male		-.052		-.053		-.043		-.045
		(.014)		(.014)		(.014)		(.014)
Age		-.160		-.156		-.153		-.149
		(.005)		(.005)		(.005)		(.005)
B. Female Applicants (N=1789)								
Dependent Var. Mean	.387		.372		.361		.348	
Voucher Winner	.067	.056	.069	.057	.071	.060	.073	.062
	(.023)	(.021)	(.023)	(.021)	(.023)	(.021)	(.023)	(.021)
Age		-.168		-.164		-.160		-.156
		(.006)		(.006)		(.006)		(.006)
C. Male Applicants (N=1752)								
Dependent Var. Mean	.320		.304		.302		.288	
Voucher Winner	.079	.063	.071	.055	.074	.059	.065	.050
	(.022)	(.020)	(.022)	(.020)	(.022)	(.020)	(.022)	(.020)
Age		-.153		-.148		-.146		-.141
		(.007)		(.007)		(.007)		(.006)

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). The sample is the same as in Table 1, Column 5.

Table 3. OLS and Tobit Estimates of the Effects of the Vouchers on ICFES Scores

	OLS with score>0 (1)	OLS censored at 1% (2)	Tobit Censored at 1% (3)	Tobit Censored at 10% (4)
A. Language Scores				
Full Sample				
Dep Var Mean	47.4	37.3	37.3	42.7
	(5.6)	(8.0)	(8.0)	(4.7)
Voucher Effect	.70	1.14	3.29	2.06
	(.33)	(.24)	(.70)	(.46)
Female Applicants				
Dep Var Mean	47.0	37.6	37.6	42.8
	(5.7)	(8.1)	(8.1)	(4.7)
Voucher Effect	.74	1.04	2.88	1.86
	(.45)	(.34)	(.91)	(.59)
Male Applicants				
Dep Var Mean	47.8	37.0	37.0	42.5
	(5.5)	(7.9)	(7.9)	(4.6)
Voucher Effect	.66	1.25	3.77	2.29
	(.48)	(.34)	(1.10)	(.71)
B. Math Scores				
Full Sample				
Dep Var Mean	42.5	35.7	35.7	37.6
	(4.9)	(5.8)	(5.8)	(4.6)
Voucher Effect	.40	.79	2.29	1.98
	(.29)	(.18)	(.51)	(.45)
Female Applicants				
Dep Var Mean	42.3	35.9	35.9	37.8
	(4.8)	(5.8)	(5.8)	(4.6)
Voucher Effect	.18	.62	1.84	1.60
	(.38)	(.25)	(.66)	(.58)
Male Applicants				
Dep Var Mean	42.8	34.8	34.8	38.9
	(5.0)	(6.1)	(6.1)	(3.8)
Voucher Effect	.70	1.02	3.03	2.21
	(.44)	(.27)	(.85)	(.62)

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. Sample sizes in Column 1, Panel A are 1223 for the whole sample, 672 for females, and 551 for males. Sample sizes are similar in Column 1, Panel B with an additional male and female. The samples in the other columns are 3541 overall, 1788 women, and 1753 men. Covariates include age and gender.

Table 4. Bounds on Voucher Effects

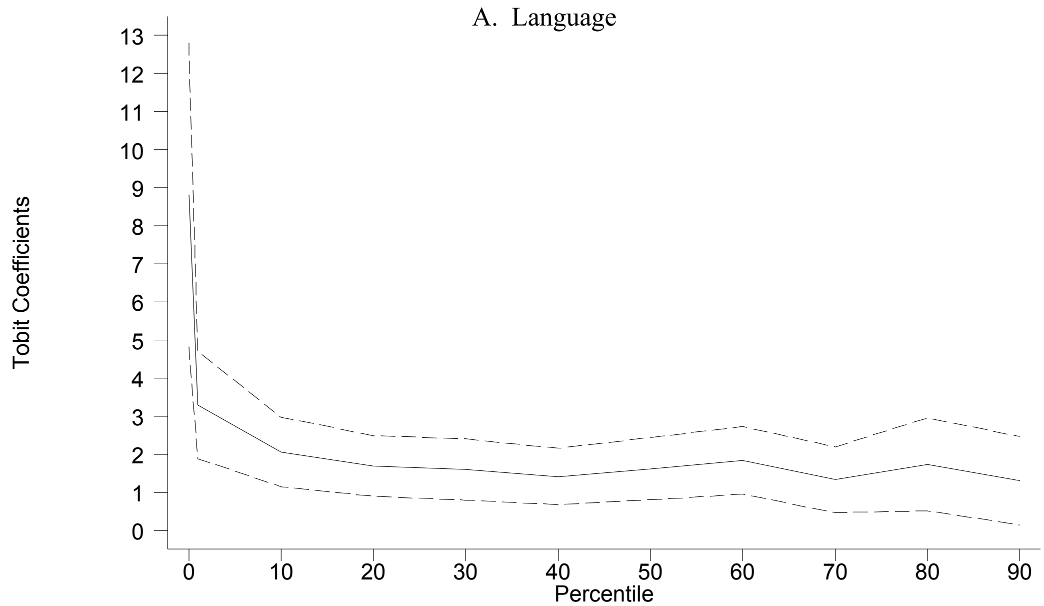
Loser's Value at Percentile	Percentile of Loser's Distribution	Loser's Avg Score Above Quantile (1)	w/o Covs Lower Bound (2)	w/o Covs Upper Bound (3)	w/ Covs Lower Bound (4)	w/ Covs Upper Bound (5)
		A. Language Scores				
0	72^{nd} Percentile	46.9	.68	2.81	.70	2.80
		(5.5)	(.33)	(.31)	(.33)	(.31)
41	75^{th} Percentile	48.7	.46	2.47	.49	2.46
		(3.9)	(.26)	(.26)	(.26)	(.26)
47	85^{th} Percentile	51.2	.49	2.39	.50	2.37
		(3.0)	(.27)	(.28)	(.27)	(.28)
52	95^{th} Percentile	55.6	.35	1.38	.36	1.39
		(1.7)	(.31)	(.34)	(.31)	(.34)
		B. Math Scores				
0	70^{th} Percentile	42.3	.40	2.40	.40	2.41
		(4.8)	(.29)	(.27)	(.29)	(.27)
37	75^{th} Percentile	43.7	.35	1.76	.34	1.76
		(3.8)	(.25)	(.25)	(.25)	(.25)
42	85^{th} Percentile	46.2	.24	1.44	.27	1.48
		(3.2)	(.28)	(.28)	(.28)	(.28)
47	95^{th} Percentile	50.3	-.09	1.85	-.11	1.80
		(2.4)	(.39)	(.42)	(.39)	(.43)

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 2-5. Columns 4-5 include controls for age and gender

Table 5. Effects on the Probability of Exceeding Test Score Percentiles

Test Score: Language	Test Score: Math	Score Threshold	Language Scores: OLS No Covs (1)	Language Scores: OLS Covs (2)	Language Scores: Probit Covs (3)	Math Scores: OLS No Covs (4)	Math Scores: OLS Covs (5)	Math Scores: Probit Covs (6)
40	35	10^{th}	.069	.057	.063	.073	.061	.068
		Percentile	(.016)	(.014)	(.015)	(.016)	(.014)	(.016)
44	39	25^{th}	.055	.045	.047	.062	.052	.054
		Percentile	(.014)	(.013)	(.014)	(.015)	(.014)	(.014)
47	42	50^{th}	.050	.043	.041	.051	.044	.044
		Percentile	(.012)	(.012)	(.011)	(.012)	(.012)	(.011)
51	45	75^{th}	.033	.030	.025	.034	.031	.027
		Percentile	(.009)	(.008)	(.007)	(.009)	(.009)	(.008)
54	48	90^{th}	.015	.013	.010	.005	.003	.003
		Percentile	(.006)	(.006)	(.004)	(.006)	(.006)	(.005)

Notes: The dependent variable indicates whether students exceeded various percentiles in the relevant score distribution for test-takers. Marginal effects are reported for probit estimates. Standard errors are reported in parentheses. Sample size is 3,541.



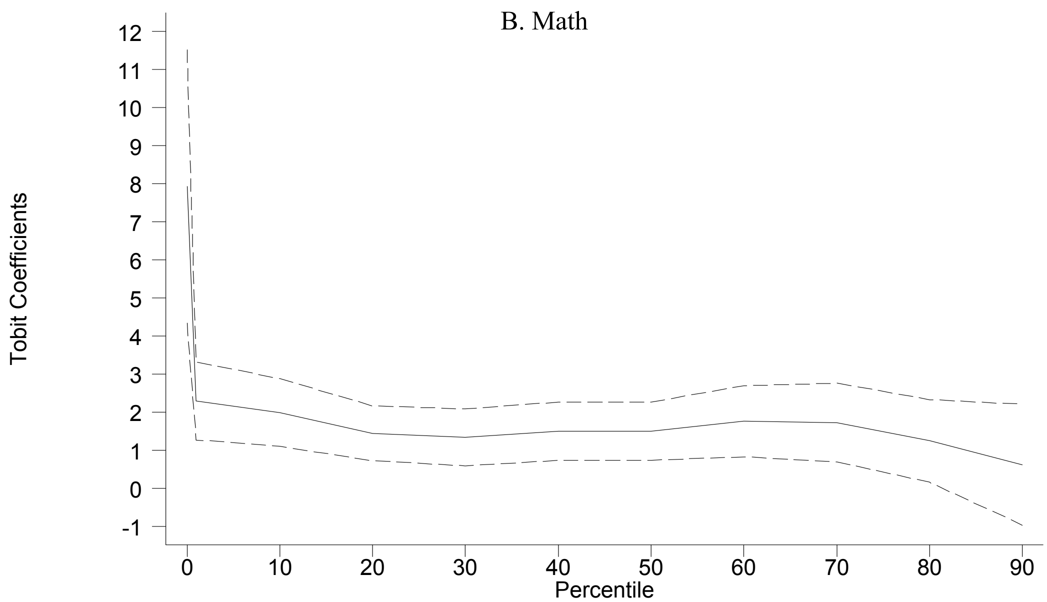
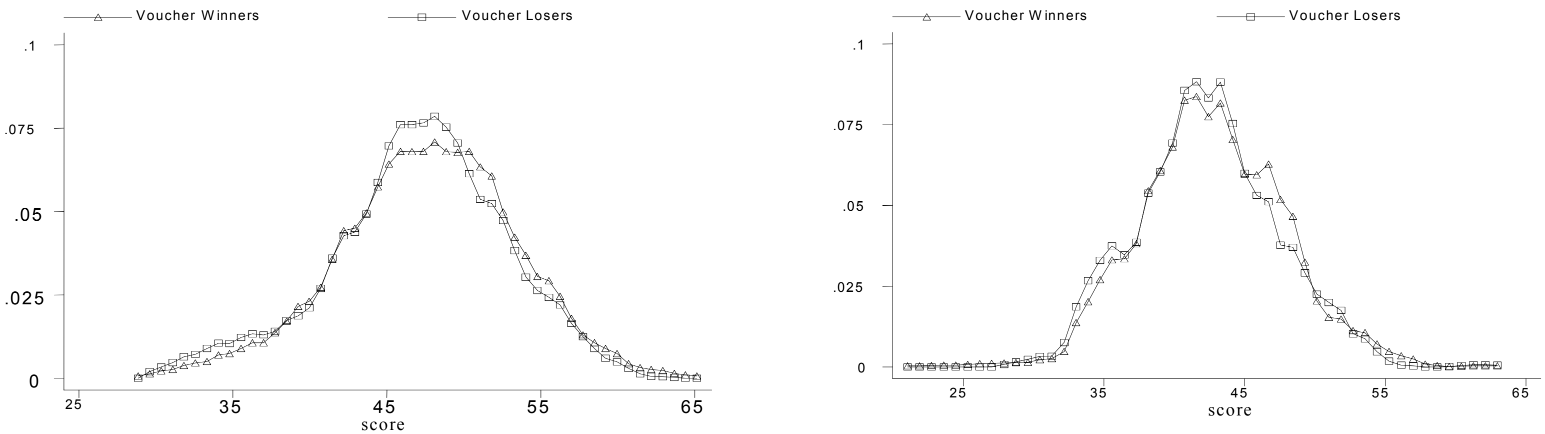


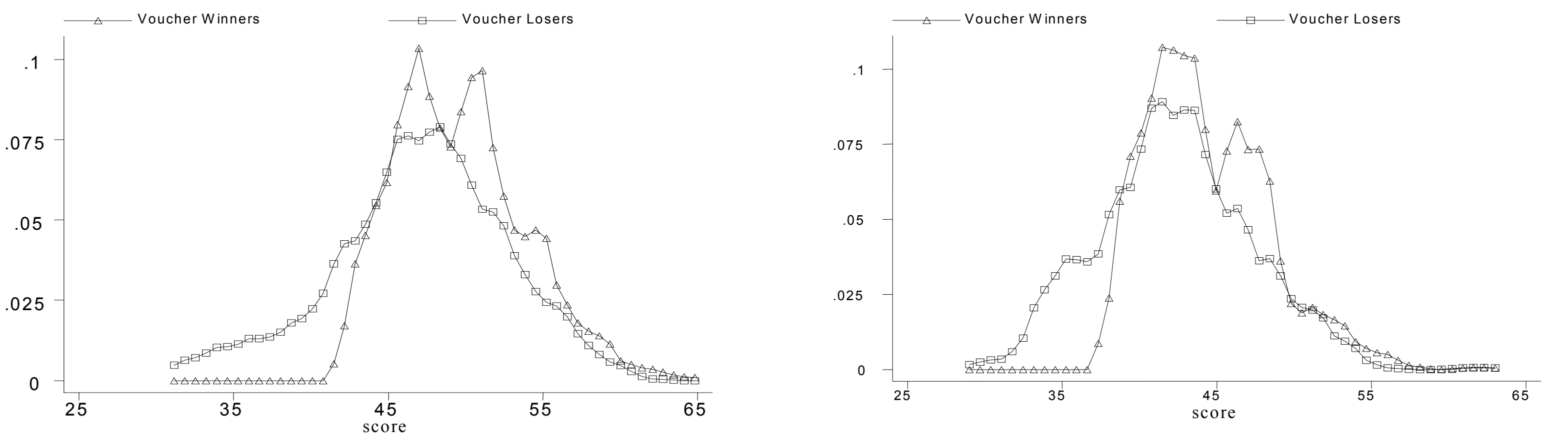
Figure 1. Tobit Coefficients by Censoring Percentile in Score Distribution. The figure plots Tobit estimates of the effect of vouchers on test scores, using data censored at the point indicated on the X-axis (i.e., values below the indicated percentile are assigned a value of zero). For the purposes of this exercise, non-takers are also coded as having a score of zero.

A. Uncorrected Distributions



Language Math

B. Distributions Using Equal Proportions of Winners and Losers



Language Math

Figure 2. Test Score Distributions. Panel A shows uncorrected score distributions for both winners and losers. Panel B similarly shows the uncorrected distributions of scores for voucher losers. Suppose π% of losers were tested, while $\pi+\kappa$% of winners were tested. Panel B shows the distribution of scores for winners for the upper π% of the winners' score distribution only.