Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment

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Colombia used lotteries to distribute vouchers which partially covered the cost of private secondary school for students who maintained satisfactory academic progress. Three years after the lotteries, winners were about 10 percentage points more likely to have finished 8th grade, primarily because they were less likely to repeat grades, and scored 0.2 standard deviations higher on achievement tests. There is some evidence that winners worked less than losers and were less likely to marry or cohabit as teenagers. Benefits to participants likely exceeded the $24 per winner additional cost to the government of supplying vouchers instead of public-school places. (*JEL* I22, J13, I28)

While the academic controversy over school providers and school vouchers has raged most intensely in the United States, private schools account for only about 11 percent of U.S. enrollment (U.S. Department of Education, 1998). Moreover, over half of American parents report that they are very satisfied with the public schools their children attend. In the developing world, in contrast, private enrollment as a proportion of total enrollment is 2–3 times higher than in industrialized nations (Estelle James, 1993). Problems with public schools are usually more severe in low-income countries, since the quality and integrity of public sector service delivery is highly correlated with income levels (James E. Rauch and Peter B. Evans, 2000). In Indian schools, for example, a recent study found that one-third of headmasters were absent at the time of the researchers' visit (PROBE Team, 1999), while in Kenya, Paul Glewwe et al. (2000) found that teachers were absent 28 percent of the time. The view that private schools function better than public schools in the developing world has prompted calls for governments in poor countries to experiment with demand-side financing programs such as vouchers (e.g., George Psacharopolous et al., 1986).

This paper presents evidence on the impact of one of the largest school voucher programs to date, the Programa de Ampliación de Cobertura de la Educación Secundaria (PACES), a Colombian initiative that provided over 125,000 pupils with vouchers covering somewhat more than half the cost of private secondary school. Vouchers were renewable as long as students maintained satisfactory academic performance. Since many vouchers were awarded by lottery, we use a quasi-experimental research design comparing educational and other outcomes of lottery winners and losers. Subject to a variety

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of caveats, the resulting estimates provide evidence on program effects that are similar to those arising from a randomized trial. As far as we know, ours is the first study of a private-school voucher program in a developing country to take advantage of randomly assigned treatment.[1]

A survey of three applicant cohorts shows no significant differences between lottery winners and losers in enrollment three years after application, with most pupils in both the winner and loser groups still in school. But lottery winners were 15 percentage points more likely to attend private schools rather than public schools. Moreover, lottery winners had completed an additional 0.1 years of school and were about 10 percentage points more likely than losers to have completed eighth grade, primarily because they repeated fewer grades. Although high rates of grade repetition are a widely recognized problem in Latin America (see, e.g., Psacharopoulos and Eduardo Vélez, 1993; Hanan Jacoby, 1994), reduced repetition need not indicate greater learning. We therefore administered achievement tests to a subset of the pupils surveyed. The test results suggest that, on average, lottery winners scored about 0.2 standard deviations higher than losers, a large but only marginally significant difference. The effect on girls is larger and more precisely estimated than the effect on boys.

In addition to increased educational attainment and academic achievement, there is also some evidence that the voucher program affected noneducational outcomes. In particular, lottery winners were less likely to be married or cohabiting and worked about 1.2 fewer hours per week (again, mostly a difference for girls). Both of these results suggest an increased focus on schooling among lottery winners.

While comparisons between winners and losers provide a simple strategy for assessing program impact, our survey indicates that only about 90 percent of lottery winners had ever used the voucher or any other type of scholarship, while 24 percent of losers received scholarships from other sources. It therefore seems reasonable to think of lottery win/loss status as an instrument for scholarship receipt in a two-stage least-squares (2SLS) setup. There is a strong first stage here, though the relationship between voucher status and scholarship use is not deterministic. Instrumenting for scholarship use with lottery win/loss status suggests that scholarship use generated effects on grade completion and test scores that are roughly 50 percent larger than the reduced-form effect of winning the lottery.

The last part of the paper presents a fiscal and cost-benefit analysis of the voucher program. Most lottery winners would have attended private school anyway, at least for a few years, and therefore reduced their educational expenditure in response to the program. On the other hand, voucher winners who were induced to switch from public to private schools greatly increased their educational expenditure, since the voucher covered only about half the cost of private school. On balance, winners' gross school fees exceeded those of losers by about 70 percent of the amount they received from the voucher. Winners paid greater fees because they were more likely to go to private schools, and because some winners who would have gone to private schools anyway switched to more expensive private schools. Moreover, lottery winners worked less, so that, on balance, households winning the lottery actually devoted more resources to education than the voucher face value. We also estimate that the voucher program cost the government about $24 more per winner than the cost of creating a public school placement. These costs to participants and the

[1] U.S. studies in this mold include Jay Green et al. (1996) and Cecilia Elena Rouse (1998), who evaluated a voucher lottery in Milwaukee. Rouse's estimates, which control for attrition, show modest increases in math scores among voucher recipients. Other U.S. studies include William G. Howell et al. (2000), Bettinger (2001a), and Daniel Mayer et al. (2002), who evaluate various private scholarship programs. Chang-Tai Hsieh and Miguel Urquiola (2002) examine a large-scale voucher program in Chile but do not take advantage of random assignment. Rosemary Bellew and King (1993) assess a smaller program in Bangladesh. The literature on public/private comparisons in the United States is extensive. See, e.g., William N. Evans and Robert M. Schwab (1995) and Derek Neal (1997). Donald Cox and Emmanuel Jimenez (1990) compare public and private schools in Colombia and Tanzania, and Jimenez et al. (1991) summarize comparisons in five countries. See also the Harry A. Patrinos and David L. Ariasingham (1997) survey of demand-side financing in poor countries. Jere Behrman et al (2000) and Glewwe et al. (2000) use randomization to examine other educational interventions in developing countries.

government are likely to have been more than outweighed by the benefits of the voucher to participants—in the form of the economic return to increased educational attainment and test scores.

A number of channels could potentially account for the PACES program's effects on participants. The program clearly shifted some participants from public to private school, and pupils who shifted may have benefitted from the opportunity to attend private schools. There is also evidence that some pupils who would have attended private school anyway were able to attend more expensive private schools. Finally, voucher recipients may have had greater incentives to focus on school because vouchers could only be renewed for those pupils who did not repeat grades.

The paper is organized as follows. Section I provides background on education in Colombia and describes the PACES program in more detail. Section II discusses data and presents descriptive statistics from our survey. Section III discusses the effect of the program on school choice and basic educational outcomes. Section IV reports the effect of winning a voucher on test scores and noneducation outcomes. Section V discusses the use of lottery win/loss status as an instrument to identify the causal effect of receiving a scholarship. Finally, Section VI looks at the effect of the program on household and government expenditure, and compares program costs with the benefits to participants. Section VII concludes the paper.

I. Background

The Colombian government established the PACES program in late 1991 as part of a wider decentralization effort and in an attempt to expand private provision of public services (King et al., 1997). The program, which was partly funded by the World Bank, was also motivated as an effort to quickly expand school capacity and to raise secondary-school enrollment rates (King et al., 1998).[2] Although 89 percent of Colombia's primary-school age children were enrolled in 1993, only 75 percent of the eligible population was enrolled in secondary schools. Among children of eligible age in the poorest quintile of the population, 78 percent were enrolled in primary school, but only 55 percent were enrolled in secondary school (Fabio Sánchez and Jairo Méndez, 1995; note that secondary school covers grades 6–11 in Colombia).

The PACES program targeted low-income families by offering vouchers only to children residing in neighborhoods classified as falling into the two lowest socioeconomic strata (out of six possible strata). Applicants had to submit a utility bill to establish residential location and voucher eligibility. Targeting was enhanced by restricting vouchers to children who attended public primary schools. Almost half of children from the richest income quintile attended private primary schools. Studies by Patricia Morales-Cobo (1993) and Rocío Ribero and Jaime Tenjo (1997) suggests that the targeting was largely effective in Bogotá.

PACES vouchers were worth only about US$190 at the time of our survey. 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, and hence recipients had to supplement vouchers with additional payments to cover school fees. 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

[2] PACES was launched in November 1991 with advertisements in print and on radio soliciting applicants in participating cities (Alberto Calderón, 1996). A World Bank report (1993) on Colombian secondary schools notes that most schools operated two or three shifts and that some towns have little room for additional pupils in spite of projected enrollment growth. Other problems mentioned in the report include poor primary-school preparation, weak school management, lack of teacher preparedness, lack of textbooks, and shortages of other supplemental materials. The early 1990's was a general period of reform and liberalization in Colombia; see, for example, Adriana D. Kugler (1999).

(DNP, 1999), and public-school parents in our sample typically paid tuition or fees of roughly $58. Per capita GNP in Colombia is around $2,280 (World Bank, 1999).

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).[3] 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 ten largest cities accepted vouchers in 1993.

Participating schools tended to serve lower-income pupils, and to have lower tuition than nonparticipating private schools. Schools with a vocational curriculum were also overrepresented 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 nonprofit schools, but after 1996, for-profit schools were excluded.[4] 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 percent of whom attended private schools. In Bogotá, roughly 58 percent 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 very 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 were moonlighting or retired public-school teachers. Nonparticipating private schools had lower pupil–teacher ratios and better facilities. Clearly, then, relatively elite private schools opted out of the PACES program. Reasons for this may include delays in payment of voucher funds to schools and bureaucracy in the Colombian Institute for Education, Credit and Training Abroad (ICETEX), which ran the program. Moreover, vouchers were insufficient to cover much of the tuition at more expensive schools, and some school managers probably viewed the prospect of an influx of pupils from low-income backgrounds as undesirable. On the other hand, many private schools in Colombia serving low-income populations apparently welcomed the PACES program.

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 failing a grade were supposed to be dropped from the PACES program. Figures from Calderón (1996) show that, on average, 77 percent of recipients renewed their vouchers, and estimates from our data are similar. By way of comparison, the national high-school promotion rate was about 70 percent. 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 after winning lost their vouchers.

Cities and towns used lotteries to allocate vouchers when demand exceeded supply. Municipal governments paid 20 percent of the voucher cost, while the central government paid 80 percent. Each municipality decided how many vouchers to fund, subject to a maximum allocated to towns by the central government. This allocation was determined by estimating the shortfall between primary-school enrollment and the available space in public secondary schools. Voucher award rates therefore varied considerably by city and year, depending on the ratio of applicants to available vouchers. Regional ICETEX offices worked with individual municipalities to determine the number of vouchers to be funded, to check school requirements for participation, and to monitor implementation of the program. The Bogotá ICETEX office provided software and instructions to regional offices for the purposes of random selec-

[3] Background information in this section is taken from Calderón (1996), King et al. (1997), and unpublished ICETEX documents.

[4] This was due largely to reported problems with low-quality for-profit schools created to exploit the vouchers. Calderón (1996) notes that even before the nonprofit restriction was imposed, only 15 percent of Bogotá's voucher students attended such institutions.

tion of applicants in cases of oversubscription. We obtained copies of lists of lottery winners and losers from ICETEX offices.[5]

II. The Applicant Survey

A. *Descriptive Statistics*

Beginning in the summer of 1998, we interviewed roughly 1,600 PACES applicants, stratifying to obtain approximately equal numbers of winners and losers. Interviewing was limited to the 1995 and 1997 applicant cohorts from Bogotá and the 1993 applicant cohort from Jamundi, a suburb of Cali. These years and cities were chosen for a combination of scientific and practical reasons. The largest and longest-running voucher program was in Bogotá, and our survey team was based there. Cali is Colombia's second-largest city and therefore also important, but almost no Cali applicants reported phone numbers, so we concentrated on a suburb, Jamundi. Telephones were used for the majority of interviews, primarily to reduce costs, but also because of interviewer safety and logistical considerations. In principle, the lottery was random *within* localities and conditional on whether households had access to a telephone. The results should therefore yield internally valid estimates of the causal effect of the program on voucher applicants with access to a telephone in surveyed cities. Over 80 percent of applicants had access to a phone, and in the Bogotá 1995 cohort, 88 percent had access to a phone, possibly via a neighbor.

Table 1 reports descriptive statistics for the sampling frame, attempted contacts, and completed interviews.[6] There were 6,156 applicants in the three applicant cohorts of interest. We attempted to interview almost 3,000 applicants, obtaining an overall response rate of 54 percent and a response rate of almost 61 percent for the 1997 Bogotá lottery. The higher response rate in the most recent lottery is not surprising since contact information for 1997 applicants is more recent. Interviews were completed with 55 percent of lottery winners and 53 percent of lottery losers. Although this response rate is far from ideal, the fact that winners and losers were almost equally likely to be interviewed is encouraging because the question of sample selection bias turns on whether voucher status is correlated with response probabilities (see, e.g., Angrist, 1997). Because response probabilities are virtually uncorrelated with voucher status, there should be little bias from our failure to interview all applicants.[7]

The typical applicant was about 13 years old at the time of application, while average age on the survey date varied from 13 for 1997 applicants to 17 for 1993 applicants. About half of the applicants were male. Roughly 85 percent of applicants were still in school, enrolled in grades ranging from sixth for the 1997 cohort to eighth or ninth for the 1993 cohort. Cohorts advance less than one grade per year because of repetition. The descriptive statistics also show that almost 90 percent of the applicants we interviewed started sixth grade in private school. This reflects the fact that eligibility for PACES vouchers was conditional on admission to a participating private school. Thus, most lottery losers went to private school anyway, at least for one year. On the other hand, only 63 percent of applicants were still in private school as of the survey date.

B. *Personal Characteristics and Voucher Status*

There is little evidence of any association between win/loss status and the individual

[5] In a few cities, the local ICETEX office assigned vouchers based on pupils' primary-school performance instead of randomly.

[6] The Data Appendix in our working paper (Angrist et al., 2001) provides additional information about the survey.

[7] The vast majority of nonresponders were people we could not reach by telephone, either because they had moved or because the telephone number we had no longer worked. Roughly 3 percent of families contacted refused to answer. The only significant difference in response rates by win/loss status is for the Jamundi cohort. In what follows, we present results for the Bogotá 1995 cohort and the combined cohorts separately. Complete follow-up is the holy grail of education research. Even careful evaluation studies using randomized and quasi-randomized designs (e.g., Rouse, 1998; Alan B. Krueger and Diane M. Whitmore, 2001) are based on samples with substantial loss to follow-up. Similarly, Howell et al. (2000) report follow-up rates similar to ours for U.S. voucher trials in three cities.

TABLE 1—SAMPLE DESIGN AND SURVEY RESPONSE DATA

Variable	Bogotá 1995	Bogotá 1997	Jamundi 1993	Combined sample	Test-takers
A. *Population:*					
N	4,044	1,770	342	6,156	—
Percentage awarded vouchers	58.8	84.7	50.0	65.8	—
B. *Attempted Interviews:*					
N	2,249	457	279	2,985	473
Percentage awarded vouchers	50.0	51.6	50.2	50.3	53.9
Response rate	0.523	0.606	0.591	0.542	0.598
Winner rate	0.528	0.619	0.650	0.553	0.624
Loser rate	0.518	0.593	0.532	0.531	0.571
C. *Completed Interviews:*					
N	1,176	277	165	1,618	283
Percentage awarded vouchers	50.4	52.7	55.2	51.3	55.6
Household visit	0.054	0.004	0.782	0.120	0.093
Age at time of application	12.6	12.4	12.5	12.6	12.6
	(1.3)	(1.4)	(1.9)	(1.4)	(1.2)
Age on survey date (from survey data)	15.0	13.1	16.9	14.9	15.6
	(1.3)	(1.4)	(1.5)	(1.7)	(1.2)
Male	0.510	0.495	0.424	0.499	0.511
Started 6th grade in private	0.910	0.880	0.669	0.880	0.832
Started 7th grade in private	0.763	0.731	0.626	0.744	0.731
Currently in private school	0.618	0.738	0.506	0.628	0.698
Highest grade completed	7.6	6.0	8.6	7.4	7.7
	(0.940)	(0.480)	(1.1)	(1.1)	(0.910)
Currently in school	0.836	0.957	0.778	0.851	0.841

Notes: Standard deviations for nonbinary variables are shown in parentheses. Sample sizes may differ across rows. Data are from 1998 household surveys. "Age at time of application" is imputed from the National Identification number reported on the application.

characteristics measured in our data from Bogotá, although winners and losers are less comparable in the 1993 Jamundi cohort. This can be seen in Table 2, which reports means and differences by win/loss status for all applicants in the study population, for sampled applicants, and for the sample of completed surveys. The sampling process began with lists showing applicant ID numbers, names, addresses, and phone numbers, separately for winners and losers. To obtain demographic characteristics for all applicants, whether surveyed or not, we coded sex from names and imputed age using ID numbers (which incorporate birthdays). Imputed age is subject to error since 13 percent of applicants have invalid ID numbers as determined by the ID number control digit. We excluded observations in which the applicant was younger than 9 or older than 25. In practice, this restriction affected only two observations. We used first names to assign sex for about 80 percent of the applicants. A final variable from the applicant record is a dummy for whether the applicant reported a phone number.

Winners and losers have similar telephone access, age, and sex mix in the 1995 and 1997 Bogotá data. As a further check on randomness, we compared win rates by school in schools with more than 20 applicants to city averages in the Bogotá data from 1995. No school had a win rate that differed significantly from the city average. In the Jamundi-93 sample, however, there are significant differences in average age and gender by win/loss status. Because the differences between winners and losers in the Jamundi lottery may indicate nonrandom assignment of vouchers, and because the 1997 Bogotá cohort is too recent for a good reading on some outcomes, we present results from the

TABLE 2—PERSONAL CHARACTERISTICS AND VOUCHER STATUS

	Bogotá 1995		Bogotá 1997		Jamundi 1993		Combined sample		Test-takers	
Dependent variable	Loser means	Won voucher	Loser means	Won voucher	Loser means	Won voucher	Loser means	Won voucher	Loser means	Won voucher
A. *Data from PACES Application:*										
Has phone	0.882	0.009	0.828	0.029	0.301	0.068	0.825	0.017	—	—
		(0.011)		(0.025)		(0.052)		(0.010)		
Age at time of application	12.7	−0.086	12.7	−0.227	12.7	−0.383	12.7	−0.133	—	—
	(1.3)	(0.045)	(1.5)	(0.102)	(1.5)	(0.162)	(1.4)	(0.040)		
Male	0.493	0.013	0.484	0.007	0.386	0.114	0.483	0.019	—	—
		(0.017)		(0.044)		(0.055)		(0.015)		
N	1,519	3,661	256	1,736	166	334	1,941	5,731	—	—
B. *Data for All Attempted Contacts:*										
Has phone	1	—	1	—	0.370	0.082	0.938	0.008	—	—
						(0.059)		(0.006)		
Age at time of application	12.8	−0.118	12.6	−0.193	12.8	−0.595	12.7	−0.177	—	—
	(1.3)	(0.060)	(1.5)	(0.136)	(1.6)	(0.183)	(1.4)	(0.052)		
Male	0.500	−0.007	0.488	−0.020	0.372	0.102	0.486	0.001	—	—
		(0.022)		(0.048)		(0.061)		(0.019)		
N	1,035	2,067	212	448	135	272	1,382	2,787	—	—
C. *Survey Data:*										
Age at time of survey	15.0	−0.013	13.2	−0.259	17.2	−0.375	14.9	−0.107	14.9	−0.160
	(1.4)	(0.078)	(1.4)	(0.171)	(1.4)	(0.217)	(1.7)	(0.068)	(1.4)	(0.162)
Male	0.501	0.004	0.527	−0.047	0.365	0.110	0.492	0.008	0.447	0.053
		(0.029)		(0.061)		(0.077)		(0.025)		(0.060)
Mother's highest grade completed	5.9	−0.079	5.9	0.654	4.4	1.46	5.8	0.183	5.5	−0.277
	(2.7)	(0.166)	(2.7)	(0.371)	(2.7)	(0.494)	(2.7)	(0.144)	(2.9)	(0.351)
Father's highest grade completed	5.9	−0.431	5.5	0.929	5.2	0.737	5.8	−0.042	4.0	−0.171
	(2.9)	(0.199)	(2.5)	(0.388)	(2.9)	(0.640)	(2.9)	(0.170)	(3.3)	(0.392)
Mother's age	40.7	−0.027	38.7	−0.146	43.6	−0.736	40.6	−0.076	40.3	0.459
	(7.3)	(0.426)	(6.6)	(0.808)	(8.8)	(1.42)	(7.4)	(0.362)	(6.6)	(0.811)
Father's age	44.4	0.567	41.9	0.265	45.5	1.92	44.1	0.537	43.5	1.18
	(8.1)	(0.533)	(7.3)	(0.973)	(9.1)	(1.61)	(8.1)	(0.453)	(7.7)	(1.06)
Father's wage (>2 min wage)	0.100	0.005	0.088	−0.008	0.133	−0.092	0.101	−0.003	0.052	0.083
		(0.021)		(0.043)		(0.056)		(0.018)	(0.222)	(0.039)
N	583	1,176	131	277	74	165	788	1,618	124	283

Notes: The table reports voucher losers' means and the estimated effect of winning a voucher. Numbers in parentheses are standard deviations in columns of means and standard errors in columns of estimated voucher effects. Models used for the estimates in Panels A and B include control for city and year of application; those for Panel C add controls for type of survey and instrument, neighborhood of residence, and month of interview. Sample size varies by row. The maximum sample size is shown in each panel. The sample for the outcome "Age at time of application" is restricted to applicants 9–25 years old.

Bogotá-95 sample separately from the results for the pooled sample including all three cohorts.

III. Impact on Scholarship Use, School Choice, and Schooling

Our estimates of lottery effects are based on the following regression model:

$$y_{ic} = \mathbf{X}_i'\boldsymbol{\beta}_0 + \alpha_0 Z_i + \delta_c + \varepsilon_{ic} \quad (1)$$

where y_{ic} is the dependent variable for child i from application cohort c (defined by city and year); $\mathbf{X}_i$ represents a vector of individual and survey characteristics like age, sex, and whether the survey was telephone or in person; Z_i is an indicator for whether child i won the voucher

TABLE 3—EDUCATIONAL OUTCOMES AND VOUCHER STATUS

	Bogotá 1995				Combined sample	
Dependent variable	Loser means (1)	No controls (2)	Basic controls (3)	Basic +19 barrio controls (4)	Basic controls (5)	Basic +19 barrio controls (6)
Using any scholarship in survey year	0.057 (0.232)	0.509 (0.023)	0.504 (0.023)	0.505 (0.023)	0.526 (0.019)	0.521 (0.019)
Ever used a scholarship	0.243 (0.430)	0.672 (0.021)	0.663 (0.022)	0.662 (0.022)	0.636 (0.019)	0.635 (0.019)
Started 6th grade in private	0.877 (0.328)	0.063 (0.017)	0.057 (0.017)	0.058 (0.017)	0.066 (0.016)	0.067 (0.016)
Started 7th grade in private	0.673 (0.470)	0.174 (0.025)	0.168 (0.025)	0.171 (0.024)	0.170 (0.021)	0.173 (0.021)
Currently in private school	0.539 (0.499)	0.160 (0.028)	0.153 (0.027)	0.156 (0.027)	0.152 (0.023)	0.154 (0.023)
Highest grade completed	7.5 (0.960)	0.164 (0.053)	0.130 (0.051)	0.120 (0.051)	0.085 (0.041)	0.078 (0.041)
Currently in school	0.831 (0.375)	0.019 (0.022)	0.007 (0.020)	0.007 (0.020)	−0.002 (0.016)	−0.002 (0.016)
Finished 6th grade	0.943 (0.232)	0.026 (0.012)	0.023 (0.012)	0.021 (0.011)	0.014 (0.011)	0.012 (0.010)
Finished 7th grade (excludes Bogotá 97)	0.847 (0.360)	0.040 (0.020)	0.031 (0.019)	0.029 (0.019)	0.027 (0.018)	0.025 (0.018)
Finished 8th grade (excludes Bogotá 97)	0.632 (0.483)	0.112 (0.027)	0.100 (0.027)	0.094 (0.027)	0.077 (0.024)	0.074 (0.024)
Repetitions of 6th grade	0.194 (0.454)	−0.066 (0.024)	−0.059 (0.024)	−0.059 (0.024)	−0.049 (0.019)	−0.049 (0.019)
Ever repeated after lottery	0.224 (0.417)	−0.060 (0.023)	−0.055 (0.023)	−0.051 (0.023)	−0.055 (0.019)	−0.053 (0.019)
Total repetitions since lottery	0.254 (0.508)	−0.073 (0.028)	−0.067 (0.027)	−0.064 (0.027)	−0.058 (0.022)	−0.057 (0.022)
Years in school since lottery	3.7 (0.951)	0.058 (0.052)	0.034 (0.050)	0.031 (0.050)	0.015 (0.044)	0.012 (0.043)
Sample size	562		1,147		1,577	

Notes: The table reports voucher losers' means and the estimated effect of winning a voucher. Numbers in parentheses are standard deviations in the column of means and standard errors in columns of estimated voucher effects. The samples used to estimate 7th- and 8th-grade completion effects exclude Bogotá 1997. The sample size for these outcomes is 1,304 in columns (5) and (6). The regression estimates are from models that include controls for city, year of application, phone access, age, type of survey and instrument, strata of residence, and month of interview.

lottery; and δ_c is an applicant cohort effect to control for the fact that the probability of winning varied by city and year. The coefficient of interest is α_0. We estimate (1) using three sets of control variables: "no controls," i.e., excluding the $\mathbf{X}_i$ variables; "basic controls" including the $\mathbf{X}_i$ variables; and "basic plus barrio controls" which includes the $\mathbf{X}_i$ variables plus 19 neighborhood dummies in the Bogotá-95 sample.[8]

[8] Neighborhoods in this case are large areas or districts.

A. *Effects on Scholarship Use and School Choice*

We begin with a simple analysis of the effect of winning the lottery on private-school scholarship receipt and the choice between public and private school. The most immediate effect of the lottery was to increase the likelihood of receiving a private-school scholarship. This can be seen in the first row of Table 3, which shows that at the time of our survey, voucher winners were 51 percentage points more likely than losers to have been using some kind of scholarship

(including non-PACES scholarships). Not all winners were using their PACES vouchers in the survey year. This is because 15 percent of winners were not in school at all, and another 16 percent were in public schools, and therefore ineligible for scholarships. Some lottery winners also lost their voucher after repeating a grade (7 percent), while 5 percent switched to nonparticipating private schools or failed to complete the paperwork for a transfer. Others attended schools that stopped accepting vouchers or lost their vouchers for unreported reasons. Just as not all winners were using a scholarship, some losers obtained scholarships from programs other than PACES and one loser was awarded a PACES voucher after reapplying the following year.

At the time of the survey, enrollment rates were 0.83 for losers and 0.85 for winners in the Bogotá-95 sample, an insignificant difference. The estimates in Table 3 also show that most PACES applicants entered sixth grade in a private secondary school, and most finished sixth grade whether or not they won a voucher. But lottery winners were 6–7 percentage points more likely than losers to have begun sixth grade in private school, and 15–16 percentage points more likely to be in private school at the time of our survey. The effect of winning the PACES lottery on the probability of private-school attendance was even larger in seventh grade, probably because losers were more likely to have left private school by then.

These results suggest the decision between public and private school was sensitive to variation in the price of private school induced by the program, while the decision whether to attend school was not.[9] This is consistent with a model in which those households most willing and able to pay for education attend private school; a middle group attends public school; and those least willing or able to pay do not attend at all. In this case, no one is on the private-school/no-school margin, and so small subsidies to private education do not directly increase overall enrollment.[10] However, since many public secondary schools in Colombia were turning away applicants due to overcrowding, PACES is likely to have opened up places in public school for other pupils by reducing public-school queuing.

B. *Effects on Schooling*

Lottery winners completed more schooling than losers, and were less likely to repeat grades. For example, lottery losers had completed 7.5 years of schooling at the time of our survey, but winners in the 1995 Bogotá sample completed an additional 0.12–0.16 years (0.8 years in the full sample). As noted earlier, there was no statistically significant effect on enrollment. The effect on years of schooling and the lack of an effect on enrollment is primarily the result of a reduced probability of grade repetition for winners. This is reflected in a sharp increase in the likelihood lottery winners had finished eighth grade as of the survey date, with a smaller impact on seventh-grade completion. In the Bogotá-95 sample, over 20 percent of losers had repeated a grade since beginning sixth grade, and almost 20 percent repeated sixth grade. But the probability of grade repetition was reduced by 5–6 percentage points for lottery winners.

The estimates of α_0 change little as the list of control variables changes, a result to be expected since the voucher lottery was random. The estimation results are also similar in the Bogotá-95 and full samples, and are largely invariant to the inclusion of neighborhood effects. Estimates and standard errors for the Bogotá-95 sample also change little in models with school effects.

Separate results by sex, reported in Table 4, show moderately larger effects on educational attainment for girls, though the pattern of sex differences in the effects on private-school enrollment are not clear-cut. Results for the Bogotá-95 sample show male lottery winners

[9] We can convert the private-school enrollment effects to an elasticity as follows. PACES vouchers reduced the marginal cost of private-school attendance by about 50 percent while vouchers increased private-school enrollment in seventh grade by about 17 percent. The implied elasticity of private enrollment with respect to marginal cost is therefore 0.34.

[10] PACES subsidies were initially large enough to cover the entire cost of private school, and may have shifted recipients from no school to private school when the program started. However, the voucher value was later eroded by inflation.

TABLE 4—EDUCATIONAL OUTCOMES AND VOUCHER STATUS, BY GENDER

	Coefficient on voucher status					
	Bogotá 1995				Combined sample	
	Male		Female		Male	Female
Dependent variable	Loser means	Basic controls	Loser means	Basic controls	Basic controls	Basic controls
Started 6th grade in private	0.857	0.082	0.897	0.027	0.058	0.077
	(0.351)	(0.025)	(0.304)	(0.021)	(0.023)	(0.021)
Started 7th grade in private	0.646	0.187	0.699	0.143	0.166	0.177
	(0.479)	(0.035)	(0.460)	(0.033)	(0.031)	(0.029)
Currently in private school	0.543	0.136	0.535	0.171	0.124	0.182
	(0.499)	(0.039)	(0.500)	(0.039)	(0.033)	(0.033)
Highest grade completed	7.4	0.124	7.6	0.140	0.056	0.122
	(0.990)	(0.076)	(0.934)	(0.065)	(0.062)	(0.052)
Currently in school	0.843	−0.020	0.819	0.035	−0.026	0.029
	(0.365)	(0.029)	(0.386)	(0.027)	(0.024)	(0.022)
Finished 6th grade	0.932	0.014	0.954	0.032	0.003	0.027
	(0.252)	(0.018)	(0.210)	(0.013)	(0.017)	(0.012)
Finished 7th grade	0.825	0.026	0.869	0.041	−0.003	0.022
	(0.380)	(0.029)	(0.338)	(0.025)	(0.024)	(0.020)
Finished 8th grade	0.589	0.095	0.674	0.105	0.066	0.078
	(0.493)	(0.039)	(0.470)	(0.036)	(0.030)	(0.027)
Repetitions of 6th grade	0.229	−0.087	0.160	−0.036	−0.070	−0.033
	(0.506)	(0.037)	(0.395)	(0.030)	(0.031)	(0.023)
Ever repeated after lottery	0.254	−0.083	0.195	−0.029	−0.076	−0.035
	(0.436)	(0.034)	(0.370)	(0.031)	(0.028)	(0.025)
Total repetitions since lottery	0.296	−0.101	0.213	−0.031	−0.079	−0.037
	(0.550)	(0.042)	(0.459)	(0.033)	(0.035)	(0.026)
Calendar years in school since lottery	3.7	−0.029	3.6	0.091	−0.041	0.081
	(0.962)	(0.077)	(0.941)	(0.063)	(0.067)	(0.055)
Sample size	280	575	282	572	779	798

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

with an insignificant 0.12 more years of schooling while female lottery winners obtained 0.14 years more of schooling, a statistically significant effect. Differences by sex are more pronounced in the full sample, with an insignificant 0.06 more years of schooling for boys, and a statistically significant 0.12 more years of schooling for girls. It should also be noted that while effects for boys are almost entirely due to grade repetition, the effects for girls appear to come from both reduced grade repetition and additional time spent in school.[11]

The greater probability of eighth-grade completion and lower repetition rates for lottery winners seem like desirable outcomes. In fact, high rates of grade repetition in Latin America are widely seen as symptomatic of poorly functioning public schools.[12] But the interpretation of these effects is complicated by the fact that pupils who failed a grade were supposed to forfeit PACES vouchers. Private schools may therefore have had an incentive to promote pupils with vouchers even if their performance did not meet normal promotional standards. To ex-

[11] There is little evidence that the effect of winning the voucher varied with applicants' socioeconomic strata of residence or parents' education. However, estimates for subgroups are imprecise.

[12] For example, R. W. Harbison and Eric A. Hanushek (1992) and Psacharopolous and Vélez (1993) use repetition rates as a measure of school quality in Colombia and Brazil.

plore this possibility, we look at effects on test scores and noneducational outcomes in the next section.

IV. Effects on Test Scores and Noneducation Outcomes

A. *Effects on Test Scores*

We tested children from the 1995 applicant cohort in three Bogotá neighborhoods. These neighborhoods were chosen because they had relatively large numbers of winners and losers, and because of the availability of suitable (and safe) testing sites. The tests were administered in 1999, approximately one year after our household survey and three years after the children applied for the program. The test sample was drawn from applicants for whom we had survey data. Participants were solicited by telephone, followed by hand delivery of letters describing the purpose of the test and inviting pupils to be tested. Those who failed to appear on the test day were invited again for a second testing, except at the last sitting. To encourage participation, refreshments were provided at each site, and each test concluded with the raffle of a bicycle and other prizes. Pupils were also given 5,000 or 10,000 pesos (U.S. $3.23 or $6.45) to cover travel costs. The invitation letter noted the offer of refreshments, travel reimbursement, and raffle. See the Data Appendix for additional details on the testing, available at ⟨http://www.aeaweb.org/aer/contents/⟩.

Our evaluation used *La Prueba de Realización*, a grade-specific multiple-choice achievement test for native Spanish speakers, published by Riverside. We administered only the mathematics, reading, and writing subtests, each taking about 30 minutes. This test was chosen because Colombian educators participated in test development and the test had been used previously in Colombia (Nancy S. Cole et al., 1993). The Appendix to our working paper (Angrist et al., 2001) compares test results from the Hispanic-American test-norming populations for grades 9 and 10 with the results from our test. Colombian ninth-graders in our sample scored lower than American pupils in mathematics, but they had reading skills slightly better than American tenth-graders. The average Colombian writing score was close to the average score for American tenth graders.

The Test Sample.—Of the 1,176 Bogotá 1995 applicants surveyed, 473 were invited for testing. Statistics for pupils invited and tested appear in the last column of Table 1. Of the 473 invited, 283 were tested, an overall response rate of about 60 percent. The test-response rate is about 5 percent higher for winners, but the difference in response rates by voucher status is not statistically significant. The personal characteristics of those tested are generally similar to those of the full Bogotá-95 sample. Also encouraging is the fact that, conditional on taking the test, there is little evidence of differences in personal characteristics between voucher winners and losers. This comparison can be seen in the last column of Table 2.

Test Results.—Table 5 reports estimates of the effect of winning the voucher lottery on test scores. Columns (1) and (2) of Table 5 show results from models with and without covariates.[13] Columns (3) and (4) present the results of estimating a single voucher coefficient for stacked subject results, in models with a pupil random effect. That is, we estimated

$$(2) \qquad y_{is} = \mathbf{X}_i'\boldsymbol{\beta}_0 + \alpha_0 Z_i + \delta_i + \varepsilon_{is}$$

where y_{is} is pupil i's score in subject s, and δ_i is a random effect used to adjust standard errors for the fact that there is likely to be within-pupil correlation across subjects. Note that test-score results are reported in standard deviation units.

Lottery winners scored just over 0.2 standard deviations more than lottery losers, though this difference is (not surprisingly, given the small test sample) only marginally significant. According to U.S. norms for *La Prueba*, two-tenths of a standard deviation is roughly the score gain associated with one additional school year (Cole et al., 1993). This effect should probably

[13] The results in columns (1) and (3) are from models that include site dummies only. The results in columns (2) and (4) are from models that include controls for age, sex, parents' schooling, strata of residence, type of interview, and survey form.

TABLE 5—TEST RESULTS

Variable	OLS results (1)	OLS results with covariates (2)	RE (3)	RE with covariates (4)	Sample size (5)
A. *All Applicants:*					
Total points	0.217	0.205			282
	(0.116)	(0.108)			
Math scores	0.178	0.153			282
	(0.120)	(0.114)			
Reading scores	0.204	0.203			283
	(0.115)	(0.114)			
Writing scores	0.126	0.128			283
	(0.116)	(0.105)			
Pooled test scores			0.170	0.148	846
			(0.095)	(0.088)	
Math and reading scores			0.192	0.162	568
			(0.101)	(0.096)	
B. *Female Applicants:*					
Total points	0.199	0.263			146
	(0.162)	(0.126)			
Math scores	0.292	0.346			146
	(0.145)	(0.141)			
Reading scores	0.117	0.152			147
	(0.158)	(0.136)			
Math and reading scores			0.204	0.235	293
			(0.130)	(0.117)	
C. *Male Applicants:*					
Total points	0.204	0.170			134
	(0.169)	(0.189)			
Math scores	0.010	0.004			134
	(0.178)	(0.187)			
Reading scores	0.276	0.220			134
	(0.183)	(0.190)			
Math and reading scores			0.143	0.087	268
			(0.160)	(0.160)	

Notes: Robust standard errors are reported in parentheses. Standard errors in columns (1) and (2) are corrected for within-school-of-application clustering. Test scores are in standard deviation units. The estimates in columns (2) and (4) are from models that include controls for applicant's age, gender, parents' schooling, strata of residence, and type of survey and instrument. Columns (3) and (4) models include random effects (RE) for each test subject. The sample for "Pooled test scores" includes three observations per student (one for each subject) while "Math and reading scores" includes two observations per student.

be seen as large, since subjects were tested three years after applying to the program. Lottery winners also scored higher on all subtests, though the only significant difference is for reading scores ($t = 1.8$). The results for the stacked subjects, reported in columns (3) and (4), also show marginally significantly higher scores for lottery winners, with the largest effects for models that stack math and reading scores only.

Models estimated separately for boys and girls generate larger and more precise effects for girls than boys. For example, the estimated effect on total points for girls, reported in column

(2) of Panel B for models with covariates, is 0.26 (SE = 0.13). The corresponding estimate for boys, reported in Panel C, is 0.17 (SE = 0.19). The finding of a stronger effect on girls echoes some of the survey results.

Earlier we noted that reduced grade repetition among lottery winners could theoretically have been caused by a reduction in promotion standards for lottery winners, as well as by increased learning or a change in school quality. Comparing the test scores of winners and losers who were promoted provides evidence that the grade-repetition results are not due solely to schools' lowering the bar for promotion of winners. If the program itself did not affect achievement, but did lead schools to relax promotions standards for winners, then average test scores for lottery winners who were promoted should be lower than average test scores for lottery losers who were promoted.[14] In fact, the composite test scores of winners who were promoted are about 0.14 standard deviations *greater* than the scores of promoted losers, although the difference is not significant.

Another possible channel through which the program could have reduced grade repetition is increased effort by voucher recipients in order to avoid failing a grade and losing their vouchers. In this scenario, the program would have been just as successful if it had made payment to students conditional on satisfactory academic performance, with no element of school choice. This would imply that the primary incentive effect should be on those who are near the margin for passing on to the next grade. However, quantile regression estimates (not reported here) suggest that the increase in test scores is not confined to low quantiles of the score distribution. For reading and writing, there is no strong pattern of differential effects across quantiles, while for math, the effects are, if anything, larger at the top of the distribution. Standard errors for these estimate are, of course, large, given the small sample.

[14] Suppose schools promote if a random variable x, representing the school's internal assessment of the student, is greater than a cutoff c, which takes on two values, c_W for winners and c_L for losers. Suppose $c_W < c_L$, but test scores, T, and the variable x are unaffected by winning the lottery. Then the expected test score for lottery losers who are promoted is $E(T|x > c_L)$. The expected score for winners will be a weighted average of this and $E(T|c_W < x < c_L)$. Average scores for promoted losers will therefore exceed average scores for promoted winners as long as $E(T|x)$ is increasing in x.

B. *Other Outcomes*

Table 6 reports estimates of the effect of winning the lottery on noneducational outcomes. Approximately 1.6 percent of lottery losers from Bogotá were married or living with a companion, a low proportion consistent with the fact that the average age of survey respondents was about 15. Since this outcome is rare, we estimated probit models as well as linear probability models.

Both probit marginal effects and ordinary least-squares (OLS) estimates suggest that marriage and cohabitation were reduced for lottery winners, a marginally significant effect. There is some evidence from the pooled sample that lottery winners were less likely to be working than losers, with the largest effects in Bogotá. There is also a significant difference in hours worked. In particular, lottery winners worked 1.2 fewer hours per week than losers. This effect is larger and more precisely estimated for girls. The reduction in work may be due to income effects for the household, the greater time demands of private school relative to public school, or increased incentives for lottery winners to spend time studying so as to avoid failing a grade and losing the PACES voucher.

V. Instrumental Variables Estimates of Scholarship Effects

The analysis so far focuses on reduced-form effects of winning the lottery. In the discussion of Table 3, however, we noted that some lottery losers were awarded other scholarships, while some winners failed to use or retain their PACES scholarships. This section discusses two-stage least-square (2SLS) estimates of the effect of ever receiving any scholarship using voucher win/loss status as an instrumental variable (IV). While only 6 percent of lottery losers used a scholarship at the time of the survey, 24 percent had used a scholarship at some point. In contrast, 90 percent of winners used a scholarship at some time. The 2SLS estimates based on this difference are necessarily larger than the reduced-form effects of winning the lottery

TABLE 6—NONEDUCATIONAL OUTCOMES AND VOUCHER STATUS

	Coefficient on voucher status					
	Bogotá 1995			Combined sample		
Dependent variable	Loser means (1)	Basic controls (2)	Probit with basic controls (3)	Loser means (4)	Basic controls (5)	Probit with basic controls (6)
A. *Male and Female:*						
Married or living with companion	0.0160 (0.1256)	−0.0087 (0.0059)	−0.0066 (0.0038)	0.0171 (0.1297)	−0.0094 (0.0056)	−0.0065 (0.0034)
Has child	0.0338 (0.1809)	−0.0103 (0.0096)	−0.0079 (0.0075)	0.0303 (0.1714)	−0.0069 (0.0079)	−0.0055 (0.0062)
Applicant is working	0.1690 (0.3751)	−0.0297 (0.0205)	−0.0299 (0.0184)	0.1616 (0.3684)	−0.02651 (0.0171)	−0.0254 (0.0153)
Number of hours working	4.881 (12.3)	−1.222 (0.6441)	— —	4.417 (11.60)	−0.8699 (0.5235)	— —
Sample size	562	1,147	1,147	760	1,577	1,577
B. *Male:*						
Married or living with companion	0.0036 (0.0598)	−0.0039 (0.0038)	— —	0.0027 (0.0518)	−0.0027 (0.0026)	— —
Applicant is working	0.2321 (0.4230)	−0.0366 (0.0331)	−0.0336 (0.0324)	0.2252 (0.4183)	−0.0283 (0.0278)	−0.0294 (0.0261)
Number of hours working	6.421 (13.69)	−0.6376 (1.072)	— —	6.198 (13.31)	−0.6231 (0.8859)	— —
Sample size	280	575	575	373	779	778
C. *Female:*						
Married or living with companion	0.0284 (0.1663)	−0.0100 (0.0113)	−0.0112 (0.0078)	0.0309 (0.1733)	−0.0113 (0.0109)	−0.0114 (0.0068)
Pregnant or has child	0.0603 (0.2384)	−0.0151 (0.0179)	−0.0155 (0.0138)	0.0541 (0.2266)	−0.0092 (0.0147)	−0.0119 (0.0110)
Applicant is working	0.1064 (0.3089)	−0.0314 (0.0235)	−0.0262 (0.0189)	0.1005 (0.3011)	−0.0317 (0.0196)	−0.0240 (0.0156)
Number of hours working	3.351 (10.57)	−2.116 (0.6527)	— —	2.704 (9.36)	−1.499 (0.5240)	— —
Sample size	282	572	572	388	798	798

Notes: Numbers in parentheses are standard deviations in columns of means and standard errors in columns of estimated voucher effects. Results are for samples with nonmissing noneducational and educational outcomes. Columns (2) and (3) show results from models that control for whether applicants had access to a phone, age, gender, type of survey and instrument, strata of residence, and month of interview. Columns (5) and (6) also include controls for city and year of application.

since winning the lottery is only imperfectly correlated with receiving a scholarship.

The assumption that a *scholarship use* dummy satisfies the exclusion restriction in an instrumental variables (IV) setup motivates 2SLS estimation of the equation:

$$y_i = \mathbf{X}_i'\boldsymbol{\beta}_1 + \alpha_1 s_i + \xi_i \tag{3}$$

where s_i is a dummy for scholarship use, and $\mathbf{X}_i$ is the vector of "basic controls" used in previous tables. The associated first-stage relationship using Z_i as an instrument is

$$s_i = \mathbf{X}_i'\boldsymbol{\gamma} + \pi Z_i + \eta_i. \tag{4}$$

The estimate of π is about 0.66 (SE = 0.021) so the second-stage estimates can be expected to be about 50 percent larger than the corresponding reduced-form estimates. The interpretation

TABLE 7—OLS AND 2SLS ESTIMATES OF THE EFFECT OF EVER USING A PRIVATE SCHOOL SCHOLARSHIP

	Coefficient on "Ever used a private-school scholarship"				
	Bogotá 1995			Combined sample	
Dependent variable	Loser means	OLS	2SLS	OLS	2SLS
Highest grade completed	7.5	0.167	0.196	0.141	0.134
	(0.965)	(0.053)	(0.078)	(0.042)	(0.065)
In school	0.831	0.021	0.010	0.033	−0.003
	(0.375)	(0.021)	(0.031)	(0.017)	(0.026)
Total repetitions since lottery	0.254	−0.077	−0.100	−0.069	−0.091
	(0.508)	(0.029)	(0.042)	(0.023)	(0.035)
Finished 8th grade	0.632	0.114	0.151	0.108	0.127
	(0.483)	(0.028)	(0.041)	(0.025)	(0.038)
Test scores (total points)	−0.099	0.379	0.291	—	—
	(1.0)	(0.111)	(0.153)		
Married or living with companion	0.016	−0.009	−0.013	−0.010	−0.014
	(0.126)	(0.006)	(0.009)	(0.006)	(0.009)
N	562	1,147		1,577	

Notes: The table reports loser means and OLS and 2SLS estimates of the effect of ever having used a private-school scholarship. Results are from models that control for city, year of application, whether applicant had access to a phone, age, type of survey and instrument, strata of residence, and month of interview. Robust standard errors are reported in parentheses. Data for the outcome "Finished 8th grade" in the combined sample does not include applicants to the Bogotá-1997 voucher lottery. For the outcome "Test scores (total points)," the sample is restricted to those individuals who took the exam. In the 2SLS specification, the endogenous regressor *used scholarship* is instrumented with voucher status.

of α_1 in this case is as an approximate effect of treatment on the subset of scholarship users who would not have used a scholarship without PACES (Guido W. Imbens and Angrist, 1994).[15]

The 2SLS estimate of the effect of scholarship use on highest grade completed is about 0.2 in the Bogotá-95 sample and 0.13 in the full sample. These estimates are reported in Table 7. Two-stage least-squares estimates of voucher effects on the probability of finishing eighth grade are 13–15 percentage points, nearly a 25-percent increase in completion rates. This seems to be in the ballpark of Susan M. Dynarski's (2001) estimated completion elasticities with respect to U.S. financial aid for college students, though obviously not directly comparable. Another interesting result is the 2SLS estimate of the effect on test scores, 0.29, somewhat smaller than the corresponding OLS estimate. The 2SLS estimates are likely to be more useful for predicting the impact of scholarship programs on new scholarship recipients than are the reduced-form effects, which are diluted by take-up rates less than one and the availability of alternative financing.

VI. Impact on Household and Government Expenditure

This section discusses the impact of the program on household and government budgets. We begin by showing that approximately 70 percent of voucher funds flowed to increased education expenditures, with the remainder going to educational spending that households would have made without the voucher. Taking into account the reduction in work by lottery winners suggests that winning the lottery induced

[15] At first blush, *private-school attendance* might appear to be the appropriate endogenous regressor for a 2SLS setup. But this seems unlikely to satisfy the required exclusion restriction since increased effort and increased school quality probably also mediate the effects of the voucher. Consistent with this, in practice, 2SLS estimates treating *private-school attendance* as an endogenous regressor generate estimates that are implausibly large.

TABLE 8—MATRICULATION, TUITION FEES, AND VOUCHER STATUS FOR BOGOTÁ 1995 APPLICANTS

	Full sample			Conditional on private-school attendance		
Variable	Loser means (1)	No controls (2)	Basic controls (3)	Loser means (4)	No controls (5)	Basic controls (6)
Using any scholarship in survey year	0.054 (0.227)	0.497 (0.023)	0.494 (0.023)	0.085 (0.279)	0.709 (0.027)	0.696 (0.028)
Currently in private school	0.530 (0.500)	0.156 (0.029)	0.152 (0.028)	1	—	—
Currently in public school	0.290 (0.454)	−0.140 (0.025)	−0.146 (0.025)	0	—	—
Scholarship value	16.0 (64.2)	74.3 (5.4)	72.6 (5.4)	29.8 (85.8)	101.8 (7.6)	98.1 (7.9)
Scholarship value (conditional on >0)	199.0 (122.4)	−13.3 (19.1)	−11.3 (18.5)	211.1 (118.2)	−25.5 (19.1)	−22.0 (18.5)
Gross school fees	191.5 (188.3)	52.3 (10.2)	48.1 (11.0)	332.2 (133.6)	11.0 (10.7)	9.8 (10.7)
Net school fees	175.9 (185.6)	−22.0 (11.5)	−24.5 (9.9)	302.4 (154.3)	−90.7 (11.6)	−88.2 (11.7)
Gross school fees for public schools	54.6 (109.4)	3.2 (13.4)	1.0 (13.8)	—	—	—
Currently using scholarship from the private school	0.031 (0.173)	−0.008 (0.011)	−0.010 (0.012)	0.059 (0.235)	−0.026 (0.019)	−0.031 (0.020)
Sample size	534	1,085	1,085	283	661	664

Notes: The table reports voucher losers' means and the estimated effect of winning a voucher. Numbers in parentheses are standard deviations in columns of means and standard errors in columns of estimated voucher effects. Sample sizes differ from other tables because of missing fee data. The sample sizes differ slightly across rows because of missing data for "Currently using scholarship from the private school" and the outcomes with the more comprehensive fee measure. The regression estimates in columns (3) and (6) are from models that include controls for whether applicants had access to a phone, age, type of survey and instrument, strata of residence, and month of interview.

households to devote more net resources to education. The higher fees paid by voucher winners are due primarily to winners' greater likelihood of attending private school. However, there is also some evidence that applicants who would have attended private schools anyway traded up to more expensive private schools in response to winning the voucher. Since the voucher did not reduce the cost of private school at the margin, this result weighs against the simplest models of education as human-capital investment without credit constraints.

The results in subsection B suggest that it cost the government about $24 more per lottery winner to provide school places through PACES than through the public system. Finally, subsection C aggregates the impact on households, schools, and the government budget, arguing that the total social costs of providing additional school places through the PACES voucher system were small, and therefore dwarfed by the benefits of the program to participants. The analysis in this section uses data for the 1995 Bogotá applicant cohort only.

A. *Impact on Household Educational Expenditure*

Three years after the 1995 lottery in Bogotá, about 55 percent of winners and 5 percent of losers were still receiving scholarships (a result from Table 3 repeated in the first row of Table 8 for the sample of observations with usable fee data). In this sample, 53 percent of losers were still in private school in the survey year, with the private-school enrollment rate 15.2 percent higher for winners after control for covariates.

Among applicants to the Bogotá-95 lottery, winners received an average of $74 more in scholarship aid than losers, a result reported in the fourth row of Table 8. Conditional on receiving a scholarship, scholarship amounts were similar for winners and losers, at roughly $200. The estimates in Table 8 also show gross matriculation and tuition fees were $52 greater for lottery winners than losers. Thus, the 1998 voucher expenditures of $74 per winner caused an increase of $52 in gross fees for winners, about 70 percent of the extra amount received by winners on average. The remaining $22 of voucher funds presumably increased noneducational expenditures by lottery winners.[16]

Aside from paying for school fees, households bear the opportunity cost of the effort students devote to education. The estimates for noneducational outcomes in Table 6 suggest lottery winners spent 1.2 fewer hours working each week. According to our survey data, the average hourly wage was 71 cents.[17] Assuming that students work 48 weeks per year, this reduction corresponds to an opportunity cost of $1.2 \times \$.71 \times 48$ weeks, approximately $41. Combining the increase of $52 in expenditures on fees and the $41 of lost earnings, we estimate that PACES lottery winners devoted $93 more to education than losers in the survey year, or 126 percent of the $74 in extra scholarship assistance they received.

Disaggregating Effects on Fees.—While winning households spent about $52 more on school fees, this average conceals important heterogeneity. Since vouchers covered only part of the cost of private school, families with children who were induced to switch to private school increased their educational expenditure sharply. However, most of the applicants who lost the lottery started private school in sixth grade anyway, and over half were still in private school in the survey year. So most vouchers were received by applicants who would have attended private school without the vouchers.

Simple models of education as human-capital investment with perfect credit markets suggest that since PACES vouchers were worth only $190 per year, while most private schools cost over $300 per year, vouchers were inframarginal. In other words, vouchers were not large enough to have caused households to increase educational spending by choosing a more expensive private school.[18] On the other hand, winning the voucher could have led households to choose more expensive private schools if educational spending is limited by credit constraints or if education has consumption value as well as investment value.

In a noncausal, purely accounting sense, the $52 of increased expenditure by winners on school fees can be decomposed into the effects of increased private-school enrollment, and a switch to more expensive private schools by winners. Let Z be a dummy for lottery win/loss status as before, except we now drop "i" subscripts to simplify notation. Also, let R denote type of school attended (1 for private, 0 for public) and let F denote education expenditure. Gross school fees conditional on lottery win/loss status (i.e., fees paid by pupils without subtracting voucher amounts) are equal to

$$E[F|Z] = E[F|Z, R = 1]P[R = 1|Z] + E[F|Z, R = 0]P[R = 0|Z].$$

The overall change in fees is a linear combination of changes in public/private enrollment and changes in fees charged by school type. We can simplify the fee contrast between winners and losers using the fact that public-school fees changed little and overall school enrollment was also affected little, so that $P[R = 1|Z = 1] - P[R = 1|Z = 0] \approx -\{P[R = 0|Z = 1] - P[R = 0|Z = 0]\}$. Then we have the following accounting relationship:

[16] The estimated displacement of private expenditure is even lower when a more comprehensive expenditure measure is used. Lottery winners report an estimated $84 more in comprehensive scholarship assistance (i.e., including expenditure on uniforms and textbooks) and an extra $74 more in comprehensive educational expenditure.

[17] Conditional on working, the average daily wage in our sample was $5.71. We estimated the hourly wage assuming a seven-hour work day.

[18] To see this, note that if education is pure human-capital investment, people choose schools so that a school costing one dollar more generates exactly one more dollar of present discounted earnings. For people who would in any case have attended a school costing more than $190, the voucher does not affect this first-order condition.

$$
\begin{aligned}
(5)\quad & E[F|Z=1]-E[F|Z=0] \\
&= \{E[F|Z=1,\,R=1]-E[F|Z=1,\,R=0]\} \\
&\quad \times \{P[R=1|Z=1]-P[R=1|Z=0]\} \\
&\quad + P[R=1|Z=0]\{E[F|Z=1,\,R=1] \\
&\quad - E[F|Z=0,\,R=1]\}.
\end{aligned}
$$

In words, the overall fee increase is caused by the private–public fee difference for winners, times private-school enrollment effects of the program, plus the win/loss contrast in fees for private-school pupils.[19] The right-hand-side components of (5) are as follows:

$$P[R=1|Z=1]-P[R=1|Z=0]=0.15$$

$$
\begin{aligned}
&E[F|Z=1,\,R=1]-E[F|Z=1,\,R=0] \\
&\quad = 343-58=285
\end{aligned}
$$

$$
\begin{aligned}
&E[F|Z=1,\,R=1] \\
&\quad - E[F|Z=0,\,R=1]=11
\end{aligned}
$$

$$P[R=1|Z=0]=0.53.$$

This implies a total effect of \$49, which is less than \$52 because of the approximation used to simplify (5), with \$43 due to school switching. Thus, in an accounting sense, the bulk of the change in household expenditure can be attributed to increased private-school enrollment. However, for reasons discussed below, this decomposition provides an incomplete picture of the causal effect of the program on the fee distribution.

Causal effects on fees for families who would have sent their children to private school anyway are difficult to measure since we do not know who these families are. Simply comparing fees by win/loss status conditional on private-school attendance [the second term in (5), above] leads to a biased estimate that is almost certainly too low. To see this, let F_0 be the public or private fee a pupil would pay if he or she loses the lottery and let F_1 be the public or private fee he or she would pay if he or she wins, and let R_0 and R_1 denote private-school attendance if a pupil loses or wins the lottery respectively. Similarly, let f_0 be the private-school fee a pupil would pay if he or she loses the lottery and let f_1 be the private-school fee a pupil would pay if he or she wins. Thus $f_0 = F_0R_0$ and $f_1 = F_1R_1$. We imagine that these variables are defined for every pupil, though in practice, we can only observe F_0, R_0, and f_0 for losers and F_1, R_1, and f_1 for winners. $E[f_1 - f_0|f_0 > 0]$ is the effect on fees for those who would attend private school even if they were to lose the lottery. The observed contrast in fees for private-school pupils can be written as follows:

$$
\begin{aligned}
(6)\quad & E[F|Z=1,\,R=1]-E[F|Z=0,\,R=1] \\
&= E[f_1-f_0|f_0>0] \\
&\quad + \{E[f_1|f_1>0]-E[f_1|f_0>0]\}.
\end{aligned}
$$

The term in braces reflects selection bias in the conditional-on-positive contrast. Under mild assumptions, this term is negative.[20] Assuming, as

[19] Without simplification the comparison is

$$
\begin{aligned}
&E[F|Z=1]-E[F|Z=0] \\
&\quad = P[R=1|Z=0] \\
&\qquad \times \{E[F|Z=1,\,R=1]-E[F|Z=0,\,R=1]\} \\
&\qquad + E[F|Z=1,\,R=1] \\
&\qquad \times \{P[R=1|Z=1]-P[R=1|Z=0]\} \\
&\qquad + P[R=0|Z=0] \\
&\qquad \times \{E[F|Z=1,\,R=0]-E[F|Z=0,\,R=0]\} \\
&\qquad + E[F|Z=1,\,R=0] \\
&\qquad \times \{P[R=0|Z=1]-P[R=0|Z=0]\}.
\end{aligned}
$$

[20] This is easy to show in a model where $f_1 = h(f_0)$ for any increasing transformation. More generally, winners who attend private school only if they win probably attend cheaper private schools than those who attend regardless. Suppose, for example, that school quality complements ability and higher-quality schools are more expensive. Then low-ability children attend public schools if they lose the lottery and attend cheap private schools if they win the lottery, while high-ability children attend expensive private schools whether or not they win a voucher. Alternatively, consider a model with credit constraints in which the poor attend public school; the

seems likely, that $f_1 \geq f_0$, the left-hand side therefore is a lower bound on the causal effect, $E[f_1 - f_0 | f_0 > 0]$.

The parameter $E[f_1 - f_0 | f_0 > 0]$ is not identified without further assumptions (see e.g., Gary Chamberlain, 1986), though the previous discussion suggests we can treat the observed contrast in fees for private-school students as a lower bound. Under mild assumptions, we can obtain a reasonably tight upper bound on this and a related family of parameters: $E[f_1 - f_0 | f_0 > m_0(\theta)]$ where $m_0(\theta)$ is the θ-quantile of the losers' fee distribution. By choosing $m_0(\theta) = 0$, we bound $E[f_1 - f_0 | f_0 > 0]$, while picking points at higher quantiles, we measure the effect on those who would have spent more on private schooling in the absence of the lottery. For example, we can bound the effect of winning the lottery on private-school fees for those who would have spent more than the voucher amount ($190) on private-school fees in the absence of the lottery. This result is stated formally below.

PROPOSITION: *Suppose that $F_1 \geq F_0$. Let $m_0(\theta)$ be the θ quantile of the distribution of F for losers, with $m_1(\theta)$ defined similarly for winners. Then*

$$(7)\quad E[f_1 - f_0 | f_0 > m_0(\theta)] \leq \{E[F|Z = 1, F > m_1(\theta)] - E[F|Z = 0, F > m_0(\theta)]\} \div P[R = 1|Z = 0, F > m_0(\theta)].$$

PROOF:

Define $F_M = F_0 1(F_0 \leq m_0(\theta)) + F_0 1(F_0 > m_0(\theta), R_0 = 0) + F_1 1(F_0 > m_0(\theta), R_0 = 1)$. Note that $F_M = F_1$ for households above the quantile who would have attended private schools anyway. Otherwise, $F_M = F_0$. Thus, $F_1 \geq F_M \geq F_0$. Let $p_\theta = P[R_0 = 1 | F_0 > m_0(\theta)]$. Then

$$\begin{aligned} &E[F_M|F_0 > m_0(\theta)] - E[F_0|F_0 > m_0(\theta)] \\ &\quad = \{p_\theta E[F_1|F_0 > m_0(\theta), R_0 = 1] \\ &\qquad + (1 - p_\theta)E[F_0|F_0 > m_0(\theta), R_0 = 0]\} \\ &\qquad - E[F_0|F_0 > m_0(\theta)]. \end{aligned}$$

Note that

$$\begin{aligned} &E[F_0|F_0 > m_0(\theta)] \\ &\quad = p_\theta E[F_0|F_0 > m_0(\theta), R_0 = 1] \\ &\qquad + (1 - p_\theta)E[F_0|F_0 > m_0(\theta), R_0 = 0]. \end{aligned}$$

Therefore,

$$\begin{aligned} &E[F_M|F_0 > m_0(\theta)] - E[F_0|F_0 > m_0(\theta)] \\ &\quad = p_\theta E[F_1 - F_0|F_0 > m_0(\theta), R_0 = 1]. \end{aligned}$$

Since $f_0 = R_0 F_0$, this implies

$$\begin{aligned} &\{E[F_M|F_0 > m_0(\theta)] - E[F_0|F_0 > m_0(\theta)\}/p_\theta \\ &\quad = E[f_1 - f_0 | f_0 > m_0(\theta)] \end{aligned}$$

which is the quantity we seek to bound. Also, since $F_1 \geq F_M \geq F_0$ for all applicants, $E[F_1|F_1 > m_1(\theta)] \geq E[F_1|F_0 > m_0(\theta)] \geq E[F_M|F_0 > m_0(\theta)]$, and we have

$$\begin{aligned} &E[F_1|F_1 > m_1(\theta)] - E[F_0|F_0 > m_0(\theta)] \\ &\quad \geq E[F_M|F_0 > m_0(\theta)] - E[F_0|F_0 > m_0(\theta)]. \end{aligned}$$

By randomization, $E[F_1|F_1 > m_1(\theta)] = E[F|Z = 1, F > m_1(\theta)]$ and $E[F_0|F_0 > m_0(\theta)] = E[F|Z = 0, F > m_0(\theta)]$ and $p_\theta = P[R = 1|Z = 0, F > m_0(\theta)]$, which implies (7) and completes the proof.

To see why the upper bound works in the case where $\theta = 0$, note that $E(F_M - F_0)$ is the average difference between winners' and losers' fees due to households who would have attended private schools in any case trading up to more expensive private schools. This is less than the observed difference in total fee payments by win/loss status, $E(F_1 - F_0)$. Econometric intuition for this result comes from the fact that in parametric sample selection models,

rich attend private school; and the very rich attend expensive private schools. Then lottery winners who would have attended public school if they lost the lottery will attend cheaper schools than lottery winners who would have attended private school even if they lost.

TABLE 9—LOWER/UPPER BOUNDS OF VOUCHER EFFECTS ON MATRICULATION AND TUITION FEES FOR BOGOTÁ 1995 APPLICANTS WHO WOULD HAVE ATTENDED PRIVATE SCHOOL AND PAID MORE THAN CUTOFF FEES

Dollar cutoff	Corresponding quantile of loser fee distribution	Loser average fee above quantile	Loser probability of private-school attendance	Loser average private-school fee above cutoff	Upper bound	Lower bound
190.0	52nd percentile	370.5	0.973	368.1	38.4	2.9
		(104.7)	(0.163)	(95.1)	(9.5)	(8.9)
283.2	60th percentile	396.7	0.981	392.5	31.4	9.2
		(94.4)	(0.136)	(83.0)	(9.7)	(8.8)
335.8	70th percentile	424.8	0.981	419.3	32.1	17.7
		(92.7)	(0.136)	(78.7)	(11.3)	(10.0)
370.1	80th percentile	460.2	0.972	452.8	36.5	14.0
		(96.0)	(0.167)	(78.3)	(9.3)	(12.3)
427.0	90th percentile	523.3	0.943	511.9	48.3	27.7
		(101.0)	(0.233)	(74.5)	(16.8)	(18.3)

Notes: The upper bound above a given percentile is computed in a two-step process. First, we estimate the difference between winners' and losers' matriculation fees conditional on being greater than the given quantile in their respective distributions. Second, we divide this difference by the probability that losers above the given percentile attend private school. The lower bound above a cutoff is computed by differencing winners' and losers' fees conditional on private-school attendance and conditional on being greater than the cutoff. See text for further details.

controlling for the probability of sample selection eliminates selection bias. Comparing winners and losers at the same quantiles equalizes the "probability of selection" if $F_1 = h(F_0)$ for some monotone increasing transformation, h. In fact, with no public-school fees, the bound is exact when $F_1 = h(F_0)$. More generally, dividing by $P[R = 1|Z = 0, F > m_0(\theta)]$ corrects for the fact that some of those with positive fees were attending public school, and the bound applies even without a deterministic link between F_0 and F_1.

Estimates of the right-hand size of (7) are reported in Table 9, along with a lower bound using the biased comparison for quantiles analogous to (6); this is, $E[F|Z = 1, R = 1, F > m_0(\theta)] - E[F|Z = 0, R = 1, F > m_0(\theta)]$. As noted earlier, this is a plausible lower bound because of negative selection bias. Note also that any reasonable behavioral model would predict that a family that spent less than the voucher amount on private school without a voucher would spend more after the voucher. We therefore focus on bounding effects that are conditional on paying pre-voucher fees equal to at least the voucher amount, roughly \$190.

Among losers paying at least \$190 in fees, the average fee was \$371. Almost all of these pupils were in private school (in fact, some reports of public-school fees above \$190 are probably in error; others refer to a handful of elite public schools that charge significant fees). The lower bound on $E[f_1 - f_0|f_0 > 190]$ is about \$3, but the upper bound is \$38. The voucher amount of \$190 is the 0.52 quantile of the fee distribution. Above this amount, the bounds are tighter. The estimated upper bounds above 0.6 range from \$31 to \$48, while the lower bounds range from \$9 to \$28. The lower-bound estimates are not significantly different from zero at the 5-percent level. In some cases, however, the lower bounds are close to a 10-percent significance level, while the upper bounds allow for effects on the order of 10 percent of fee costs and 20 percent of the voucher value. Thus, it seems likely that winners in the upper half of the fee distribution spent 5–10 percent more on private schools than they otherwise would have. This implies that the marginal propensity to spend voucher income on more expensive private schools was nontrivial, counter to a simple model of human-capital investment without credit constraints.

Price Discrimination by Private Schools.—Another potential source of increased expenditure on fees by winners is price discrimina-

tion. There is little evidence that private schools discriminated by charging more to applicants with PACES scholarships. The easiest way for schools to price discriminate was to offer scholarships to those less likely to be able to afford education at the full price (i.e., applicants without PACES vouchers). In practice, however, we found little evidence of price discrimination. Our survey indicates that roughly 6 percent of losers in private school received a school scholarship, while 3 percent of winners in private school received a school scholarship. This is a small and insignificant difference. Per lottery winner (i.e., without conditioning on attending private school), the difference is only about 1 percent.

B. *Impact on the Government Budget*

The PACES program was established in part to expand secondary-school enrollment without using the public system. We estimate that the program increased public educational expenditure by about $24 per lottery winner, relative to the cost of accommodating these pupils in public school. As discussed in Section III, winners were no more likely than losers to attend school, but the program probably did expand overall school enrollment by freeing up places in public schools as lottery winners transferred to private schools.

To see where the $24 figure comes from, note that the probability of attending public school, reported in Table 8, fell by 0.14 for lottery winners. The average per-pupil cost of a public secondary-school slot was about $350, excluding implicit rental for school facilities. In the short run, the marginal cost of public-school slots may differ from the average cost, but in the long run, it seems reasonable to assume marginal and average costs will be similar. Assuming the marginal cost of providing public-school places equals the average cost, adding school spaces through PACES reduced long-run expenditure on public schools by 14 percent of $350 or roughly $50 per winner, so the extra public educational expenditure per lottery winner is about $74 (to pupils) −$50 (in reduced public-school costs) = $24. Moreover, allowing the marginal cost to differ from average cost by $100 either way still leads to voucher program costs in the $10–$40 range.

C. *Overall Cost and Benefits*

The extra society-wide educational resource cost per lottery winner differs from the roughly $24 of extra public-education expenditure, since households used part of the voucher funds to offset education costs they would have incurred privately, and lost income from their children's work. The average lottery winner received $74 more than the average loser in scholarship assistance, but spent only $52 more on gross school fees. Lottery winners earned $41 less than losers through work. Winning households' net resource contribution was therefore $52 (additional school fees) + $41 (reduced earnings) − $74 (voucher) = $19. This implies that the society-wide additional educational resource cost per lottery winner was approximately $24 (government) + $19 (households) = $43.

The comparison of costs and benefits should take account of the fact that three years of costs were incurred prior to our survey. The total cost of the program can therefore be estimated by multiplying the annual resource cost times the roughly three years winners received vouchers, for a total of about $3 \times 24 = \$72$ in additional public-educational expenditure and $3 \times \$43 = \$129$ in total societal resource cost. Actual costs are probably somewhat higher, however, since voucher take-up rates declined over time, with 88 percent of winners having ever used a voucher, and only 49 percent using it in the survey year. Multiplying costs by 88 percent/49 percent for the first and second years yields an upper bound on the three-year cost of the program of about $195 using the $43/year figure for social costs.

These costs are likely to have been small relative to the benefits for participants. Although lottery winners gave up current earnings, they completed an additional 0.12 to 0.16 grades and scored approximately 0.2 standard deviations higher on tests. Among U.S. Hispanic students who took the same test, the difference in test scores between seventh- and eighth-graders, or between eighth- and ninth-graders, was also about 0.2 standard deviations, so the achievement gain from winning the lottery may be as large as that associated with a full year of schooling. Our estimates using a recent Colombian labor-force survey show returns to a year of schooling of about 10 percent.

If the gain from the program is solely the economic return to an additional 0.12 years of schooling, the program raised winners' wages by 1.2 percent per year, whereas if it is equal to that from a full year of schooling it raised wages by 10 percent. Annual earnings of parents in our sample were about $2,400 per worker, and PACES applicants should be able to earn more, since the average parent had only 5.9 years of education while the average applicant had already completed 7.5 years and was still in school at the time of our survey. We therefore assume the expected earnings of applicants are $3,000. Thus, PACES seems very likely to raise lottery winners' wages by $36 per year, and might raise wages by as much as $300 per year if higher test scores have a grade-equivalent payoff. Discounted over applicants working lives, these benefits easily outweigh the social costs of the voucher program, which are probably no more than $195.

A more complete cost-benefit analysis would take into account the program's effects on nonparticipants. Pupils left behind in public schools may have been hurt by the departure of motivated classmates for private schools, as argued by Hsieh and Urqiola (2001), or alternatively, public schools may have responded positively to increased competition, a possibility considered by Caroline M. Hoxby (2000) and Bettinger (2001b). Such general-equilibrium effects cannot be assessed by comparing lottery winners and losers. But since the partial-equilibrium cost-benefit analysis is clear-cut, and since only 15 percent of winners moved from public to private schools, any negative external effects on nonparticipants would have to have been extraordinarily large to outweigh program benefits.

VII. Summary and Conclusions

Governments in many developing countries are increasingly willing to experiment with demand-side subsidies and public–private partnerships to meet basic education needs. The impact of these programs and policy innovations is an open question. Colombia's PACES program provides 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. Our results suggest that lottery winners benefitted from higher educational attainment, primarily as a consequence of reduced grade repetition, as well as from higher test scores and a lower probability of teen cohabitation or employment. Our estimates of the economic benefits to participants far exceed the estimated costs. Most of the results suggest PACES vouchers had a stronger effect on the education of girls than on the education of boys.

Our findings suggest that demand-side programs like PACES can be a cost-effective way to increase educational attainment and academic achievement, at least in countries like Colombia with a weak public-school infrastructure and a well-developed private-education sector. A number of channels could account for the impact of PACES vouchers. First, lottery winners were more likely to have attended participating private schools, and these schools may be better than public schools. Second, vouchers allowed some pupils who would have attended private schools anyway to attend more expensive schools. Finally, because voucher recipients who failed a grade risked losing vouchers, lottery winners had an incentive to devote more effort to school. The net effect is such that the benefit of voucher awards were more than enough to offset the costs. In work in progress, we are assessing longer-term consequences of voucher receipt. Preliminary results indicate that the program increased secondary-school completion rates, and that college-entrance test scores were higher for lottery winners than losers. These results are indicative of greater learning and seem unlikely to be due solely to greater incentives for PACES recipients to avoid grade repetition.

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