

## Free to Choose: Can School Choice Reduce Student Achievement?<sup>†</sup>

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*A central argument for school choice is that parents can choose schools wisely. This principle may underlie why lottery-based school evaluations have almost always reported positive or zero achievement effects. This paper reports on a striking counterexample to these results. We use randomized lotteries to evaluate the Louisiana Scholarship Program, a voucher plan that provides public funds for disadvantaged students to attend private schools. LSP participation lowers math scores by 0.4 standard deviations and also reduces achievement in reading, science, and social studies. These effects may be due in part to selection of low-quality private schools into the program. (JEL H75, I21, I22, I28)*

The benefits and costs of increasing school choice in the United States education system are a matter of continuing debate. Choice advocates believe that increasing choice forces schools to compete for students, thereby boosting educational quality and promoting better matches between students and schools (Friedman 1962; Hoxby 2003). Proponents also cite surveys indicating that families are happier expressing choice, pointing to economic revealed preference considerations as a rationale for choice (Howell and Peterson 2002). The additional freedom to choose may be the reason that numerous lottery-based studies of school choice, possible only at schools where demand exceeds capacity, have found either positive or zero effects of choice programs on student achievement. For instance, charter school lottery studies have found some charters increase achievement markedly; impacts averaged over representative samples of charter schools are smaller but rarely negative (Abdulkadiroğlu et al. 2011; Angrist, Pathak, and Walters 2013; Dobbie and

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Fryer 2013; Chabrier, Cohodes, and Oreopoulos 2016).<sup>1</sup> Analyses of district-wide school choice plans show that attending a preferred public school yields limited test score impacts while improving college quality (Cullen, Jacob, and Levitt 2006; Hastings, Kane, and Staiger 2009; Deming et al. 2014). Randomized evaluations of voucher plans in New York, Washington D.C., and Dayton, Ohio show small average test score effects, with larger gains for some subgroups (Howell and Peterson 2002; Mayer et al. 2002; Howell et al. 2002; Krueger and Zhu 2004; Wolf et al. 2007, 2010). Together, these findings suggest that school choice programs generally produce zero or positive effects for participating students and almost never reduce student achievement.

This paper provides a striking contrast to the literature on lottery-based studies of school choice. We evaluate the Louisiana Scholarship Program (LSP), a school choice program that provides private school vouchers for disadvantaged Louisiana students attending low-performing public schools. Income-eligible students enrolled in public schools graded “C” or below on an achievement-based rating system may apply for an LSP voucher to cover tuition at an eligible private school. Private schools gain eligibility by applying to the Louisiana Board of Elementary and Secondary Education to host LSP students (Louisiana Department of Education 2015a). If the number of eligible applicants to a private school exceeds the available seats, LSP vouchers are distributed via stratified random lottery. We estimate causal effects of LSP vouchers by comparing outcomes for lottery winners and losers in 2013, the first year after the LSP expanded throughout Louisiana.

Lottery-based estimates show that LSP vouchers dramatically reduce academic achievement. Attending an LSP-eligible private school lowers math scores by an average of 0.41 standard deviations ( $\sigma$ ) and reduces reading, science, and social studies scores by  $0.08\sigma$ ,  $0.26\sigma$ , and  $0.33\sigma$  one year after the lottery. LSP participation shifts the distribution of scores downward in all four subjects, increasing the likelihood of a failing score by between 24 and 50 percent. These impacts are similar across family income levels and geographic locations. LSP voucher effects are more negative in earlier grades, though vouchers reduce achievement in later grades as well.

We find suggestive evidence that the negative effects of the LSP may be linked to selection of low-quality private schools into the program. LSP-eligible private schools charge lower tuition than nonparticipating schools, and the program’s negative math impacts are concentrated among the eligible schools with lowest tuition. Compared to nonparticipating schools, LSP-eligible private schools also experience sharp relative declines in enrollment prior to entering the program, though enrollment changes are unrelated to achievement effects among participants. We find no evidence for other candidate explanations for negative voucher impacts, including schools’ inexperience with the voucher-eligible population, transitional costs associated with the program’s statewide expansion, and the quality of fallback public schools available to LSP applicants. The LSP includes test-based accountability rules that aim to retrospectively identify and remove low-quality schools, but lottery

<sup>1</sup> An exception is Angrist, Pathak, and Walters (2013), a study that finds negative test score impacts for non-urban charter middle schools in Massachusetts.

estimates are similar for schools that were subsequently sanctioned for weak academic performance and for schools that were not sanctioned. This suggests that the program's accountability rules do not identify the low-quality schools that drive its negative achievement effects.

The rest of this article is organized as follows. The next section provides background on the Louisiana Scholarship Program and describes the data used to evaluate it. Section II outlines our empirical approach and reports lottery-based estimates of voucher effects. Section III documents the robustness of these estimates to adjustments for differential attrition between lottery winners and losers. Section IV explores mechanisms that might explain negative voucher impacts. Section V concludes.

## I. Data and Background

### A. *The Louisiana Scholarship Program*

School voucher programs are expanding quickly in the United States: the number of students using educational vouchers increased by 130 percent between 2009 and 2015 (Alliance for School Choice 2009, 2015). Paralleling this national trend, the Louisiana Scholarship Program launched in New Orleans in 2008. Legislation proposed by Governor Bobby Jindal authorized statewide expansion of the program in 2012, and it grew rapidly thereafter (Barrow 2012). This growth can be seen in Figure 1, which plots the numbers of LSP applicants, voucher recipients, and participating schools by year. Through the 2011–2012 school year the LSP awarded fewer than 2,000 vouchers annually for attendance at roughly 40 schools, mostly located in New Orleans. By 2014, 12,000 students applied for more than 6,000 LSP vouchers to attend 126 private schools, making the LSP the fifth-largest school voucher program in the United States (Louisiana Department of Education 2014a; Friedman Foundation for Educational Choice 2015).

Eligibility for LSP vouchers is limited to students from families earning below 250 percent of the federal poverty line. Applicants for grades 1 through 12 must also have attended public schools graded C, D, F, or T (turnaround) by the Louisiana School Performance Score (SPS) ratings system in the previous year. Rising kindergarteners have no previous school and so are exempt from this requirement. SPS ratings are based on a formula that combines test score levels, gains for low-achieving students, and (for high schools) graduation rates; most of the weight is placed on test score levels. In 2014, 54 percent of Louisiana Public Schools received SPS ratings low enough for enrolled students to qualify for LSP vouchers (Louisiana Department of Education 2015b).

Students apply for LSP vouchers to cover tuition at eligible private schools of their choice. LSP vouchers may also be used to attend public schools with SPS ratings of A or B, though few public schools participate in the program. An LSP voucher pays either the private school's tuition fee or the per pupil funding level of the student's home district, whichever is lower. LSP-eligible private schools typically charge less than public per pupil expenditure: in 2014, the average LSP voucher paid \$5,311, while students' sending districts spent \$8,605 (Louisiana Department of Education

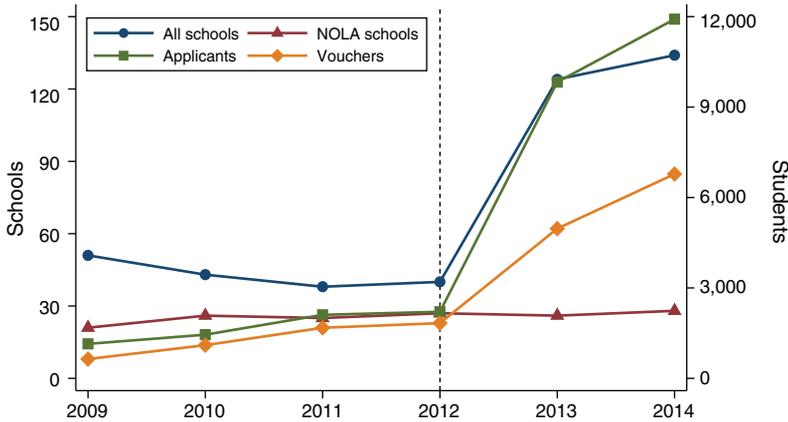


FIGURE 1. LOUISIANA SCHOLARSHIP PROGRAM STUDENTS AND SCHOOLS

*Notes:* This figure plots the number of schools participating in the Louisiana Scholarship Program (LSP; left axis) and the number of students applying for and receiving LSP vouchers (right axis). Circles indicate the total number of schools by year, and triangles show the number of schools in New Orleans. Squares display the number of applicants, and diamonds show the number of vouchers awarded. The vertical line indicates the 2011–2012 school year.

2014a). Private schools must accept the LSP voucher as full payment of tuition; charging “top-up” fees to LSP voucher recipients is prohibited.

Private schools become eligible to accept LSP voucher students by applying to the Louisiana Board of Elementary and Secondary Education (BESE). The application requests a maximum number of LSP seats. BESE reviews applications through site visits, financial audits, and health and safety assessments. If an application is accepted, BESE authorizes a number of seats that may be fewer than the requested number. Schools with more LSP voucher applicants than authorized seats must give priority to students with enrolled siblings, students living nearby, and students previously enrolled in D- or F-rated public schools.<sup>2</sup> Students may list multiple schools on their LSP applications, and seats at a school are allocated in order of student preference rankings, then by admissions priorities. Ties among equal-priority students are broken by random lottery (Louisiana Department of Education 2015a).

To maintain eligibility, private schools must undergo annual financial audits and administer Louisiana state achievement tests to LSP students. Non-LSP students enrolled at participating schools are not required to take these tests. Schools with more than 40 total voucher students or 10 voucher students per grade receive a public Scholarship Cohort Index (SCI) score, an SPS-like rating based on voucher student achievement. Schools with SCI scores lower than 50 (equivalent to an F on the SPS scale) in the second year of participation or any subsequent year are not eligible to enroll new voucher students the next year, though the school may retain students already enrolled. Schools without enough students to qualify for an SCI score may also be barred from accepting new voucher students if less than 25 percent of their LSP enrollees earn “proficient” test scores. In 2013–2014, 28 private schools served

<sup>2</sup>Enrollees in the Nonpublic School Early Childhood Development Program (NSECD), continuing students in transitional grades, and transfers from ineligible private schools may also receive admission priority.

enough LSP students to receive SCI scores, and 15 were sanctioned for scores below 50. Eight additional schools were sanctioned for low proficiency rates (Louisiana Department of Education 2014a).

The LSP has generated controversy since its inception. In response to a 2012 lawsuit filed by Louisiana's teachers' unions, the state Supreme Court ruled that funds earmarked for public schools cannot constitutionally be used to fund the LSP. In response, the state legislature approved the use of funds not designated for public education (Dreilinger 2013b). In 2013, the US Department of Justice filed a lawsuit alleging that the program interferes with federal desegregation orders by altering school racial composition. This lawsuit resulted in the requirement that applicant schools fill out "Brumfield-Dodd" reports documenting compliance with desegregation orders (Dreilinger 2013c). LSP detractors cite persistently low test scores among voucher students, while supporters note that the LSP serves very disadvantaged students and receives high scores on surveys of parental satisfaction (Dreilinger 2013a; Varney 2014). The LSP is also relevant to more general debates over school vouchers, serving as an example for similar proposed programs in other states (Ardon and Candal 2015). The expansion of voucher programs nationwide seems likely to be high on the agenda of US Education Secretary Betsy DeVos (Brown 2016).

### B. Data Sources

The Louisiana Department of Education provided data covering voucher applications, background characteristics, lottery outcomes, and test scores for all students applying to the LSP between 2008 and 2012. As shown in Figure 1, the program was not heavily oversubscribed prior to 2012. Our analysis therefore focuses on students applying for LSP vouchers in Fall 2012, the first application cohort after the program expanded statewide. Follow-up scores on Integrated Louisiana Educational Assessment Program (iLEAP) or Louisiana Educational Assessment Program (LEAP) achievement tests are available for students in grades three through eight.<sup>3</sup> Primary outcomes are math, English Language Arts (ELA), science, and social studies LEAP and iLEAP scores in Spring 2013, the end of the academic year after LSP application. These scores are in standard deviation units, normed using means and standard deviations for students in the New Orleans Recovery School District (RSD) by grade and year.

The application data records students' rank-ordered choice lists of private schools, information for determining admission priorities, and voucher offers. We use this information to isolate random variation in voucher receipt. Vouchers are randomly assigned within "risk sets" defined by application year, grade, first-choice private school, and priority status. Our lottery analysis sample consists of first-time LSP voucher applicants for grades three through eight in 2012–2013, in risk sets in which some students were offered vouchers and others were not.

<sup>3</sup>LEAP exams are taken in fourth and eighth grade. iLEAP exams are taken in third, fifth, sixth, and seventh. The iLEAP includes items from nationally normed Iowa Tests of Basic Skills as well as items based on state testing criteria, while the LEAP includes only items based on state criteria.

TABLE 1—DESCRIPTIVE STATISTICS FOR STUDENTS

	Louisiana (1)	RSD (2)	Louisiana Scholarship Program		
			All applicants (3)	Randomized applicants (4)	Enrollees (5)
Female	0.487	0.473	0.489	0.487	0.539
Black	0.451	0.939	0.861	0.885	0.805
Hispanic	0.044	0.031	0.031	0.033	0.039
White	0.468	0.010	0.086	0.058	0.131
NSECD	—	—	0.004	0.005	0.006
Household income: mean	—	—	15,471	15,535	17,400
25th percentile			1,300	1,455	1,452
Median			12,000	12,840	15,000
75th percentile			24,781	24,864	28,032
Observations	715,012	14,689	3,723	1,412	1,019

*Notes:* Columns 1 and 2 show statistics for students enrolled in Louisiana and Recovery School District (RSD) public schools in grades 3–8 in the 2012–2013 school year. These statistics are obtained from the Louisiana Department of Education website. Column 3 shows statistics for first-time applicants to Louisiana Scholarship Program (LSP) schools in grades 3–8 for 2012–2013. Column 4 shows statistics for LSP applicants subject to first choice random assignment. Column 5 shows statistics for LSP enrollees.

We supplement data on LSP applicants with private school characteristics obtained from the Private School Universe Survey (PSS), along with tuition information gathered via internet searches and phone calls. The PSS, a biennial census of US private schools, collects data on enrollment by demographic group as well as class size, instructional time, religious affiliation, and geographic location. We matched the 2000–2012 PSS waves to voucher lottery data by school name and city, manually correcting small discrepancies for a few inexact matches (e.g., missing hyphens or apostrophes). This procedure yielded matches for 142 of 159 schools that participated in the LSP between 2008 and 2013. We searched for tuition for all Louisiana private schools in the 2012 PSS and successfully collected data on 94 percent of LSP schools and 92 percent of non-LSP schools. The online Appendix provides further details on data processing and sample construction.

### C. LSP Students and Schools

The LSP voucher applicant population is composed mostly of low-income minority students. Table 1 reports descriptive statistics for first-time voucher applicants, applicants subject to random assignment, and enrollees in the 2012–2013 school year, as well as for students enrolled in Louisiana public schools and the RSD. Eighty-six percent of LSP applicants are black, compared to 45 percent in Louisiana and 94 percent in the RSD. LSP voucher applicants come from families earning \$15,471, on average. As shown in column 4, randomized LSP applicants are very similar to the full applicant population. Column 5 shows that students who use LSP vouchers are slightly less disadvantaged than LSP applicants. Eighty-one percent of voucher recipients are black, and average family income is \$17,389 for this group. These income levels are well below 250 percent of the poverty line, which

TABLE 2—DESCRIPTIVE STATISTICS FOR PRIVATE SCHOOLS

	All Louisiana private schools			Matched city sample		
	LSP voucher schools (1)	Oversubscribed LSP schools (2)	Other private schools (3)	LSP voucher schools (4)	Oversubscribed LSP schools (5)	Other private schools (6)
Enrollment in 2012	311	243	323	323	239	349
Enrollment growth, 2000–2012	–12.4%	–16.1%	2.8%	–7.7%	–10.4%	1.9%
Tuition	\$4,898	\$4,653	\$5,760	\$5,115	\$4,740	\$6,430
Fraction black	0.327	0.433	0.158	0.387	0.517	0.188
Fraction Hispanic	0.020	0.021	0.037	0.021	0.021	0.041
Fraction white	0.622	0.517	0.752	0.564	0.433	0.714
Catholic school	0.645	0.679	0.391	0.594	0.619	0.367
Other religious affiliation	0.274	0.304	0.421	0.313	0.357	0.430
Student/teacher ratio	13.5	12.7	11.5	13.3	12.3	10.9
Days in school year	178.6	178.9	177.9	178.8	178.9	177.7
Hours in school day	6.8	6.8	6.7	6.8	6.7	6.7
Observations	124	56	235	96	42	158

*Notes:* This table reports characteristics of private schools in Louisiana using data from the Private School Universe Survey (PSS). Column 1 shows statistics for schools eligible for Louisiana Scholarship Program vouchers at any time through 2012–2013. Column 2 shows statistics for voucher schools with applicants subject to random assignment in 2012–2013. Column 3 shows statistics for non-LSP private schools. Columns 4, 5, and 6 report statistics for schools in cities with both LSP and non-LSP private schools. The second row reports average enrollment growth between 2000 and 2012 for schools with available data in both years. The third row measures tuition in the most recent available year, usually 2015–2016. Tuition is available for 94 percent of voucher schools and 92 percent of other private schools.

is the limit for LSP eligibility (i.e., \$37,825 for a family of two and \$57,625 for a family of four in 2012; see Department of Health and Human Services 2012).

Private schools participating in the LSP differ systematically from other Louisiana private schools. This can be seen in Table 2, which compares characteristics of LSP private schools versus other private schools in the state. LSP schools open in both 2000 and 2012 experienced an average enrollment loss of 13 percent over this time period, while other private schools grew 3 percent on average. LSP schools also charge lower prices: average tuition is \$4,898 for LSP schools and \$5,760 for non-LSP schools, a difference of roughly 15 percent. Most Louisiana private schools are associated with religious groups, but LSP schools are more likely to be affiliated with the Catholic church than other schools. LSP schools also serve more black students and have larger student/teacher ratios than do non-LSP schools. Instructional time per day and per year is comparable for these two groups.

Column 2 of Table 2 describes LSP schools that were oversubscribed and therefore admitted students by random lottery in Fall 2012. These schools are the basis for our analysis of LSP voucher effects. Oversubscribed schools are smaller and serve more black students than other LSP schools, but are otherwise generally similar. Columns 4 through 6 report corresponding statistics for schools in cities with at least one LSP school and one non-LSP school. Characteristics in this matched-city sample are similar to the broader sample in columns 1 through 3, suggesting that differences between LSP and non-LSP schools are not explained by geographic differences in private school markets.

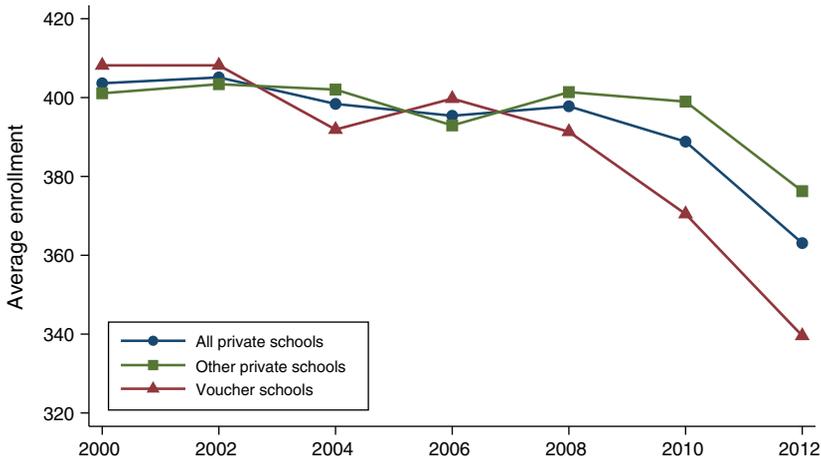


FIGURE 2. ENROLLMENT TRENDS IN LOUISIANA PRIVATE SCHOOLS

*Notes:* This figure plots average annual enrollment for private schools in Louisiana. Enrollment is measured using the Private School Universe Survey (PSS). Voucher schools are defined as schools eligible for the Louisiana Scholarship Program at any time through 2013–2014. Schools are included if they have available PSS data in both 2000 and 2012, which covers 93 of 159 voucher schools.

Figure 2 presents a more complete investigation of enrollment trends by plotting average annual enrollment for a balanced panel of private schools open in both 2000 and 2012. Schools are permanently categorized as LSP for this analysis if they received an LSP voucher student at any time through 2013–2014. The resulting sample covers 93 of the 159 schools that ever participated in the LSP. In 2000, enrollment levels were slightly higher in schools that eventually opted in to the voucher program than for other private schools. Mean enrollment began to decline for LSP schools around 2006, while enrollment was roughly constant for other schools until 2010. Both groups' enrollment fell after 2010, but this decline was sharper among LSP schools. As a result, LSP schools were roughly 10 percent smaller than non-LSP schools by the time the voucher program expanded statewide in 2012–2013.

## II. Lottery Estimates of Voucher Effects

### A. Empirical Framework

The primary equation of interest for our empirical analysis is

$$(1) \quad Y_i = \beta P_i + \sum_{\ell} \gamma_{\ell} d_{i\ell} + \mathbf{X}'_i \delta + \epsilon_i,$$

where  $Y_i$  is a test score for student  $i$ , and  $P_i$  is an indicator equal to one if this student uses an LSP voucher to attend a private school. The  $d_{i\ell}$  are a mutually exclusive and exhaustive set of lottery risk set dummies indicating combinations of first-choice school and priority status. The term  $\mathbf{X}_i$  is a vector of baseline covariates (gender, race, NSECD status, and family income quartiles) included to increase precision.

Decisions to participate in the LSP may be related to potential academic achievement, so ordinary least squares (OLS) estimation of equation (1) may not recover causal effects of voucher use. We therefore employ a lottery-based instrumental variables (IV) strategy to estimate voucher effects. Let  $Z_i$  denote an indicator equal to one if student  $i$  was offered an LSP voucher. We estimate equation (1) by two-stage least squares (2SLS), with first-stage equation

$$(2) \quad P_i = \pi Z_i + \sum_{\ell} \rho_{\ell} d_{i\ell} + \mathbf{X}_i' \theta + \eta_i.$$

Two-stage least squares estimates are obtained via OLS estimation of (1) after substituting  $\hat{P}_i$ , the predicted value from (2), for  $P_i$ . The voucher offer instrument  $Z_i$  is randomly assigned within risk sets and therefore independent of family background and other determinants of potential achievement. Assuming that voucher offers only influence test scores through LSP participation and weakly increase the likelihood of participation for all students, the 2SLS estimate of  $\beta$  may be interpreted as a local average treatment effect (LATE), that is, an average causal effect of participation for “compliers” induced to attend private schools by LSP vouchers (Imbens and Angrist 1994; Angrist, Imbens, and Rubin 1996).

### B. Covariate Balance

Within lottery risk sets, students offered LSP vouchers should look much like students not offered vouchers. Table 3 presents a check on this by comparing baseline characteristics for voucher lottery winners and losers. These calculations are restricted to our lottery analysis sample, which includes 1,412 first-time applicants for grades three through eight in risk sets subject to random assignment in Fall 2012. Column 1 displays mean characteristics for lottery losers, while column 2 reports coefficients from regressions of baseline variables on the voucher offer indicator  $Z_i$ , controlling for risk set indicators. Column 3 shows corresponding coefficients for the 88 percent of applicants with follow-up test score data. Demographic characteristics and income distributions are similar for lottery winners and losers, indicating that random assignment was successful. Mean differences for individual characteristics are small, and  $p$ -values for joint tests of balance across all baseline characteristics give no cause for concern.

### C. IV Estimates

Lottery estimates show that LSP vouchers reduce academic achievement. Table 4 reports results for Spring 2013 math, ELA, science, and social studies LEAP/iLEAP scores. As shown in column 1, lottery offers boost the probability of voucher use by 68 percentage points in the subsequent year. This estimate corresponds to the first-stage coefficient  $\pi$  in equation (2). Column 2 shows reduced form differences in test scores between lottery winners and losers, obtained by substituting  $Y_i$  for  $P_i$  on the left-hand side of (2). Voucher lottery losers outscore winners by  $0.28\sigma$  in math,  $0.06\sigma$  in ELA,  $0.18\sigma$  in science, and  $0.23\sigma$  in social studies.

TABLE 3—COVARIATE BALANCE

	Non-offered mean (1)	Offer differential	
		Full sample (2)	With follow-up (3)
Female	0.474	0.012 (0.033)	0.008 (0.035)
Black	0.900	-0.034 (0.021)	-0.028 (0.022)
Hispanic	0.030	0.003 (0.012)	0.001 (0.013)
White	0.050	0.019 (0.015)	0.018 (0.016)
NSECD	0.004	-0.001 (0.006)	-0.002 (0.006)
Household income	15,410	1,636 (1,097)	1,025 (1,118)
Income below $p_{25}$	0.254	-0.007 (0.029)	0.000 (0.030)
Income below $p_{50}$	0.503	-0.030 (0.035)	-0.017 (0.036)
Income below $p_{75}$	0.753	-0.048 (0.034)	-0.028 (0.035)
Joint $p$ -value	—	0.659	0.932
Observations		1,412	1,248

*Notes:* This table compares characteristics of offered and non-offered applicants to Louisiana Scholarship Program schools for grades 3–8 in the 2012–2013 school year. The sample is restricted to first-time applicants subject to first choice random assignment. Column 1 reports mean characteristics for applicants not offered a seat, while columns 2 and 3 report differences between offered and non-offered applicants. These differences come from regressions that control for risk set indicators. The sample in column 3 is restricted to applicants with follow-up test scores.  $p_{25}$ ,  $p_{50}$ , and  $p_{75}$  refer to the twenty-fifth, fiftieth, and seventy-fifth percentiles of household income in the non-offered group. The last row shows  $p$ -values from tests that all differentials equal zero. Standard errors, clustered by risk set, are in parentheses.

Because the IV models estimated here are just-identified, 2SLS estimates of  $\beta$  in equation (1) equal ratios of corresponding reduced-form and first-stage estimates. These estimates appear in column 3. The 2SLS coefficients show that LSP participation lowers math scores by  $0.41\sigma$  one year after the lottery and reduces ELA, science, and social studies scores by  $0.08\sigma$ ,  $0.26\sigma$ , and  $0.33\sigma$ , respectively. Estimates for math, science, and social studies are highly statistically significant, though the estimate for ELA is insignificant at conventional levels. Here and elsewhere, standard errors are clustered by risk set.<sup>4</sup> Column 4 shows corresponding OLS estimates. OLS and 2SLS estimates are very similar, suggesting little selection into voucher use within lottery risk sets. The OLS estimates are negative and statistically significant in all four subjects.

Together, the estimates in Table 4 clearly demonstrate that attending LSP-eligible private schools reduces voucher recipients' test scores. It's worth benchmarking

<sup>4</sup>Clustering by risk set accounts for negative dependence between voucher offers for students in the same lottery. With a fixed number of offers available, an offer for one student reduces the likelihood of offers for other students in the same risk set.

TABLE 4—TWO-STAGE LEAST SQUARES ESTIMATES OF VOUCHER EFFECTS ON TEST SCORES

Subject	First stage (1)	Reduced form (2)	2SLS (3)	OLS (4)
Math	0.679 (0.029)	-0.281 (0.061)	-0.413 (0.091)	-0.386 (0.066)
Observations			1,247	
ELA	0.679 (0.029)	-0.055 (0.053)	-0.081 (0.079)	-0.120 (0.056)
Observations			1,248	
Science	0.689 (0.030)	-0.181 (0.066)	-0.263 (0.095)	-0.282 (0.065)
Observations			1,221	
Social studies	0.690 (0.030)	-0.229 (0.060)	-0.331 (0.089)	-0.270 (0.059)
Observations			1,220	

*Notes:* This table reports estimates of the effects of attendance at Louisiana Scholarship Program (LSP) voucher schools on LEAP/iLEAP test scores. The sample includes first-time voucher applicants subject to first choice random assignment applying to grades 3–8 in 2012–2013. Column 1 reports first-stage effects of voucher offers on attendance at an LSP school, while column 2 reports reduced form effects of offers on test scores. Column 3 reports two-stage least squares estimates of the effects of LSP participation, and column 4 reports corresponding ordinary least squares estimates. All models control for risk set indicators and baseline demographics (sex, race, NSECD, and indicators for household income quartiles). Standard errors, clustered by risk set, are in parentheses.

these effect sizes against the impacts of important educational interventions evaluated in the recent literature. Rouse (1998) estimates that participating in the Milwaukee Parental Choice Program boosts math scores by 0.08–0.12 $\sigma$  per year. Evidence from the Tennessee STAR experiment indicates that cutting class size by one third increases achievement by roughly 0.2 $\sigma$  (Krueger 1999; Chetty et al. 2011), while estimated standard deviations of achievement impacts across teachers and schools range from 0.1–0.2 $\sigma$  (Chetty, Friedman, and Rockoff 2014a; Angrist et al. 2017). Studies of effective charter schools show annual score gains between 0.2 $\sigma$  and 0.4 $\sigma$  (Abdulkadiroğlu et al. 2011; Dobbie and Fryer 2011; Angrist et al. 2012; Curto and Fryer 2014). The negative impacts of LSP vouchers, on the order of 0.3–0.4 $\sigma$  in math, science, and social studies, are therefore comparable in magnitude to some of the largest effects documented in recent studies of education programs.

#### D. Effects on Performance Categories

Louisiana’s educational accountability system groups LEAP and iLEAP scores into five performance categories: Unsatisfactory, Approaching Basic, Basic, Mastery, or Advanced. These categorizations carry high stakes for both students and schools. Fourth and eighth grade students must score Approaching Basic or above in math and ELA, and Basic or above in at least one subject, to be promoted to the next grade (Louisiana Board of Elementary and Secondary Education 2015). The SPS school rating system awards points for each student scoring at least Basic; scores below Basic are considered failures and awarded no points (Louisiana Department of Education 2015b).

TABLE 5—VOUCHER EFFECTS ON TEST SCORE PERFORMANCE CATEGORIES

Subject	Approaching basic or above (1)	Basic or above (2)	Mastery or above (3)	Advanced (4)
Math	−0.156 (0.045)	−0.216 (0.047)	−0.067 (0.024)	−0.012 (0.011)
CCM	[0.802]	[0.567]	[0.090]	[0.017]
Observations		1,214		
ELA	−0.022 (0.034)	−0.107 (0.047)	−0.032 (0.031)	0.002 (0.011)
CCM	[0.844]	[0.563]	[0.100]	[0.009]
Observations		1,222		
Science	−0.035 (0.047)	−0.153 (0.049)	−0.040 (0.018)	−0.001 (0.004)
CCM	[0.810]	[0.468]	[0.062]	[0.003]
Observations		1,211		
Social studies	−0.096 (0.041)	−0.160 (0.045)	−0.026 (0.020)	−0.004 (0.003)
CCM	[0.759]	[0.513]	[0.044]	[0.004]
Observations		1,209		
Qualify for promotion (4th and 8th grade)		−0.284 (0.086)		
CCM		[0.786]		
Observations		347		

*Notes:* This table reports 2SLS estimates of how attendance at Louisiana Scholarship Program (LSP) schools affects LEAP/iLEAP score categories. The dependent variable in each column is an indicator for scoring in the relevant performance category or higher. The last row shows effects on passing LEAP exams for fourth and eighth graders. Passing requires scores of Approaching Basic or above in math and ELA and Basic or above in at least one subject. See notes to Table 4 for a description of the 2SLS model specification. Control complier means (CCM); mean outcomes for non-offered compliers are shown in brackets. Standard errors, clustered by risk set, are in parentheses.

We investigate LSP vouchers' effects on high-stakes performance categories in Table 5. Specifically, this table reports 2SLS estimates of equation (1) for a series of outcomes equal to one if a student scores at or above each performance category. To benchmark these effects, we also report control complier means (CCMs), which are average non-LSP outcomes for voucher lottery compliers. Appendix A provides the details of CCM estimation and other methods for characterizing compliers employed in the analysis to follow.

LSP vouchers shift students into lower performance categories and increase the likelihood of failing scores. Attending an LSP-eligible private school reduces the probability of scoring at least Approaching Basic in math by 16 percentage points from a base of 80 percentage points, a result that can be seen in column 1 of Table 5. This implies an 80 percent increase in Unsatisfactory math scores (16 points on a base of 20). Vouchers also increase the probabilities of Unsatisfactory scores in the other three subjects, though these effects are smaller in magnitude. Column 2 shows that voucher use substantially boosts the likelihood of failing tests in every subject: impacts on the probability of scoring at least Basic are negative and statistically

significant for all four tests. LSP participation reduces the probability that compliers earn passing math scores by 21.6 percentage points from a base of 56.7, implying a 50 percent increase in failures (21.6/43.3). Corresponding increases for ELA, science, and social studies are 24, 29, and 33 percent, respectively.

Effects on higher performance categories are smaller in absolute magnitude, but some imply large proportionate impacts. As shown in column 3, vouchers cut the probability of qualifying for Mastery or above in math by 6.7 percentage points from a base of 9.0, a 74 percent reduction. The corresponding decrease in science is 65 percent (4.0/6.2). Fewer than 2 percent of compliers earn Advanced scores in each subject, and impacts on this category are small.

The bottom row of Table 5 looks specifically at the effects of LSP participation on the probability that fourth and eighth grade students earn LEAP scores sufficient for grade promotion in the public school accountability system. The outcome here is an indicator equal to one if a student scores at least Approaching Basic in both math and ELA, and Basic or above in at least one subject. LSP participation more than doubles the likelihood that students fail to qualify for grade promotion. Voucher use reduces the probability of passing by 28.4 percentage points from a base of 78.6, implying a 133-percent increase in failures (28.4/21.4). Private schools are not required to promote or retain students on the basis of state achievement test scores, of course, but this result shows that LSP vouchers have substantial effects on an outcome used for high-stakes decisions elsewhere.

### E. Effects on Score Distributions

To develop a more complete picture of LSP vouchers' distributional effects, we estimate marginal test score densities for compliers lotteried into the program and compliers who did not receive LSP vouchers. Let  $Y_i(1)$  and  $Y_i(0)$  denote potential scores for student  $i$  as a function of the LSP participation "treatment"  $P_i$ . We characterize distributions of these potential outcomes by estimating equations of the form

$$(3) \quad \frac{1}{h} K\left(\frac{Y_i - y}{h}\right) \times P_i = \tau_y P_i + \sum_{\ell} \kappa_{\ell y} d_{i\ell} + \mathbf{X}'_i \lambda_y + v_{iy},$$

instrumenting  $P_i$  with the voucher offer indicator  $Z_i$  as before. Here,  $K(u)$  is a symmetric kernel function maximized at  $u = 0$ , and  $h$  is a bandwidth. Under standard regularity conditions, the 2SLS estimate of  $\tau_y$  is a consistent estimate of the density function of  $Y_i(1)$  for voucher lottery compliers evaluated at  $y$  (Angrist et al. 2016; Walters 2014). Estimates of the density of  $Y_i(0)$  for compliers are obtained by substituting  $(1 - P_i)$  for  $P_i$  on both sides of (3). Our implementation evaluates complier densities at a grid of 100 points using a Gaussian kernel and Silverman's (1986) rule-of-thumb bandwidth.

Figure 3 reveals that LSP participation shifts the entire achievement distribution downward for all four subjects. This results in lower treated densities at high test score levels and higher treated densities at low levels relative to distributions for non-treated compliers lotteried out of the program. Figure 3 also reports Kolmogorov-Smirnov test statistics equal to maximum differences in estimated complier CDFs,

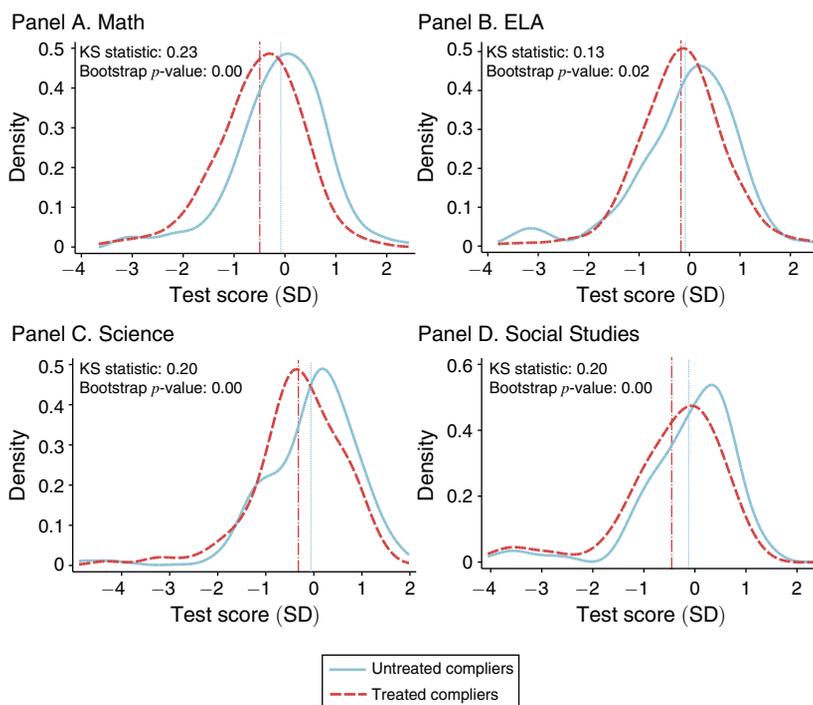


FIGURE 3. TEST SCORE DISTRIBUTIONS FOR VOUCHER COMPLIERS

*Notes:* This figure plots marginal potential test score distributions for Louisiana Scholarship Program voucher lottery compliers. Treated densities are estimated using 2SLS regressions of the interaction of a kernel density function and an LSP participation indicator on the participation indicator, instrumented by a random offer indicator and controlling for risk set dummies and baseline demographics. Untreated densities are estimated by replacing participation with one minus participation in this 2SLS procedure. All models use a Gaussian kernel and the Silverman (1986) rule of thumb bandwidth. Vertical dotted lines indicate mean untreated outcomes, and dashed/dotted lines indicate mean treated outcomes. KS statistics are maximum differences in complier CDFs. The bootstrap procedure used to test distributional equality is described in the Appendix.

along with bootstrap  $p$ -values from tests of distributional equality (see Appendix A), which result in rejections of distributional equality at conventional levels for all four subjects ( $p \leq 0.02$ ).

#### F. Effects on Subgroups

Previous studies of voucher programs and Catholic private schools have emphasized effect heterogeneity across demographic groups, particularly by race (Neal 1997; Howell and Peterson 2002). Because 86 percent of LSP applicants are black, there is insufficient power to split our sample by race. We instead investigate heterogeneity by family income and location, which may capture differences in resources and schooling opportunities. Columns 1 and 2 of Table 6 report estimates from 2SLS models that interact LSP participation with family income and add the interaction of income with the lottery offer as a second instrument, controlling for a main effect of income. The income interaction is insignificant in all subjects,

TABLE 6—VOUCHER EFFECTS BY SUBGROUP

Subject	By family income (\$1,000s)		By location		By Catholic affiliation		By grade	
	Main effect (1)	Interaction (2)	New Orleans/ Baton Rouge (3)	Other (4)	Catholic (5)	Not Catholic (6)	3rd–5th (7)	6th–8th (8)
Math	−0.413 (0.093)	−0.002 (0.005)	−0.276 (0.284)	−0.436 (0.095)	−0.462 (0.144)	−0.286 (0.104)	−0.631 (0.140)	−0.207 (0.110)
Observations	1,247		133	1,114	643	471	664	583
<i>p</i> -value	0.636		0.593		0.319		0.016	
ELA	−0.078 (0.082)	−0.001 (0.004)	−0.034 (0.259)	−0.086 (0.083)	−0.090 (0.119)	−0.034 (0.121)	−0.301 (0.119)	0.135 (0.080)
Observations	1,248		133	1,115	643	472	664	584
<i>p</i> -value	0.787		0.847		0.747		0.002	
Science	−0.266 (0.096)	0.002 (0.005)	−0.412 (0.298)	−0.242 (0.099)	−0.222 (0.135)	−0.238 (0.148)	−0.396 (0.119)	−0.132 (0.137)
Observations	1,221		132	1,089	630	463	656	565
<i>p</i> -value	0.708		0.588		0.936		0.145	
Social studies	−0.338 (0.091)	0.003 (0.005)	−0.542 (0.268)	−0.301 (0.092)	−0.470 (0.135)	−0.105 (0.106)	−0.387 (0.131)	−0.276 (0.122)
Observations	1,220		132	1,088	629	463	656	564
<i>p</i> -value	0.473		0.394		0.035		0.542	

*Notes:* This table reports estimates from 2SLS models that interact Louisiana Scholarship Program (LSP) participation with observed student and school characteristics. Columns 1 and 2 interact LSP participation with family income. Income is demeaned in the estimation sample, so that main effects are at the mean. Column 3 shows effects for students in New Orleans and Baton Rouge, while column 4 shows effects for students in other places. Columns 5 and 6 report effects for Catholic schools and schools with other or no religious affiliation. Column 7 shows effects for students applying in third through fifth grade, while column 8 shows effects for students applying in sixth through eighth. See notes to Table 4 for a description of the 2SLS model specification. *p*-values are from tests of the hypothesis that interaction effects or subgroup differences are zero. Standard errors, clustered by risk set, are in parentheses.

implying similar effects for richer and poorer students. Columns 3 and 4 compare effects for students in New Orleans and Baton Rouge, Louisiana's two largest urban centers, or elsewhere. These estimates show similar effects for urban centers and other locations, though estimates for New Orleans and Baton Rouge are imprecise due to small samples.

A large literature evaluates the effects of Catholic private schools on student outcomes (Neal 1997; Altonji, Elder, and Taber 2005). Columns 5 and 6 of Table 6 report LSP voucher impacts by Catholic affiliation. Effects are similar for Catholic and non-Catholic schools. The estimated effect for social studies is more negative for Catholic schools, but this difference is only marginally significant and may be a chance finding given the large number of splits examined. These estimates indicate that Catholic LSP schools do not improve test scores for voucher applicants.

Columns 7 and 8 of Table 6 report effects by grade, which are relevant for understanding LSP vouchers' effects on human capital accumulation. Results here suggest that impacts of LSP participation are more negative for younger children. Students in grades three through five lose  $0.62\sigma$  in math, an effect three times as large as the loss for students in grades six through eight ( $0.21\sigma$ ). Similarly, vouchers reduce

TABLE 7—ROBUSTNESS TO ADJUSTMENTS FOR DIFFERENTIAL ATTRITION

Subject	Full sample			Without imbalanced risk sets			Bounds	
	Non-offered follow-up rate (1)	Offer differential (2)	2SLS estimate (3)	Non-offered follow-up rate (4)	Offer differential (5)	2SLS estimate (6)	Lower bound (7)	Upper bound (8)
Math	0.856	0.079 (0.015)	-0.413 (0.091)	0.908	0.017 (0.013)	-0.397 (0.099)	-0.494 (0.091)	-0.178 (0.091)
Observations		1,412	1,247		1,059	962	1,412	
ELA	0.857	0.078 (0.015)	-0.081 (0.079)	0.905	0.019 (0.013)	-0.098 (0.095)	-0.208 (0.080)	0.101 (0.087)
Observations		1,412	1,248		1,059	958	1,412	
Science	0.836	0.078 (0.016)	-0.263 (0.095)	0.890	0.006 (0.015)	-0.272 (0.104)	-0.362 (0.096)	-0.016 (0.097)
Observations		1,412	1,220		1,059	942	1,412	
Social studies	0.835	0.079 (0.016)	-0.331 (0.089)	0.888	0.008 (0.015)	-0.362 (0.112)	-0.404 (0.104)	-0.032 (0.102)
Observations		1,412	1,221		1,059	941	1,412	

*Notes:* This table explores the robustness of estimated voucher effects to adjustments for differential attrition between offered and non-offered students. Column 1 shows the fraction of non-offered applicants with follow-up test scores. Column 2 shows coefficients from regressions of a follow-up indicator on an offer indicator, controlling for sex, race, NSECD status, income quartiles, and risk set dummies. Column 3 shows the full-sample 2SLS estimates from Table 4. Columns 4 through 6 order the sample by risk-set specific attrition differentials and drop the 25 percent of students from risk sets with the largest differentials. Column 4 shows follow-up rates in the trimmed sample, column 5 shows offered/non-offered attrition differentials, and column 6 shows 2SLS estimates. Columns 7 and 8 report nonparametric bounds on local average treatment effects of LSP participation, estimated via the method described in the Appendix. Standard errors, clustered by risk set, are in parentheses.

ELA scores by  $0.3\sigma$  for younger children, while the ELA estimate for older children is positive and marginally significant. These cross-grade differences are statistically significant at conventional levels ( $p \leq 0.02$ ). Estimates of science and social studies effects are also more negative for younger applicants, though the differences for these subjects are not statistically significant.

### III. Attrition

Even when LSP vouchers are randomly assigned, non-random attrition from the sample may compromise the comparability of lottery winners and losers, possibly generating selection bias. Column 1 of Table 7 shows high follow-up rates for the lottery sample: test scores in each subject are observed for at least 83 percent of lottery losers. As shown in column 2, however, follow-up scores are more likely to be observed for lottery winners than for losers. Specifically, the probability of an observed score is 8 percentage points higher for lottery winners, conditional on risk sets and baseline demographics. This difference is likely due to the fact that LSP participants are tested for accountability purposes, while non-participants who exit the public school system are not followed.

The differential attrition process would have to be extremely pathological to explain the large negative estimates reported in Table 4. For example, if all students

without test scores among those offered vouchers scored at the eighty-fifth percentile of the offered distribution and all those missing scores from the non-offered group scored at the fifteenth percentile of the non-offered distribution, the reduced-form estimate for math would equal  $-0.12\sigma$  with a standard error of  $0.05\sigma$ , a statistically significant effect. The math reduced form would be approximately zero if missing offered students scored at the ninety-fifth percentile and non-offered students scored at the fifth percentile of their respective distributions. This degree of imbalance seems very implausible in view of column 3 of Table 3, which shows that observed characteristics remain balanced in the sample of students with followup scores. Nonetheless, we cannot be assured of balance on unobserved characteristics.

We conduct two additional analyses to formally assess the robustness of our results to selective attrition. The first drops lottery risk sets with large attrition differentials and reports estimates for the remaining sample. The second constructs nonparametric bounds on local average treatment effects under a monotonicity assumption on the attrition process. The latter approach is in the spirit of Lee (2009), who derives sharp bounds on treatment effects in randomized experiments with monotonicity. Engberg et al. (2014) apply similar methods in a lottery-based research design with imperfect compliance, an approach we follow here. Intuitively, if a voucher offer weakly reduces the likelihood of attrition for all students, the usual LATE framework must be augmented with an additional set of “at risk” compliers who exit the sample when denied an offer. This prevents identification of the mean treated outcome for the subgroup of compliers who remain in the sample, but this mean can be bounded using observed response probabilities and quantiles of the outcome distribution. Appendix B formalizes this argument and details the methods we use to construct bounds for LATE.

Adjustments for differential attrition do not overturn the conclusion that LSP participation reduces achievement. Columns 4 through 6 of Table 7 report results after dropping risk sets with the largest attrition differentials. This trimmed sample is constructed by computing risk set-specific differential attrition rates, ordering students according to the rate for their risk set, and dropping the 25 percent of students with the largest differentials. Column 4 shows that follow-up rates in the remaining sample are roughly 90 percent, and column 5 shows that differences in attrition between lottery winners and losers are small enough to be no longer statistically significant. As can be seen in column 6, 2SLS estimates of voucher effects are essentially unchanged by the trimming procedure. Combined with the observation that baseline characteristics remain balanced in the sample with follow-up scores, these results suggest that the attrition process is not very selective. Our full sample lottery estimates are therefore unlikely to be compromised by attrition.

Columns 7 and 8 display estimated bounds on local average treatment effects for compliers. These bounds are relatively wide because of the large difference in attrition rates between lottery winners and losers. Upper bounds for math, science, and social studies are negative, however, and the associated confidence intervals rule out small positive effects. The estimated upper bound for math is  $-0.18\sigma$ , and this estimate is statistically significant at the five-percent level. The conclusion that LSP vouchers reduce math scores is therefore robust to this conservative adjustment for differential attrition.

TABLE 8—VOUCHER EFFECTS AND PROGRAM CHARACTERISTICS

Program	Study (1)	Math effect (2)	Funding (3)	Eligibility (4)	Voucher amount (5)	Top-up allowed (6)	Schools opt in (7)	Religious schools (8)
Louisiana Scholarship Program (LA)	Authors' estimates	$-0.41\sigma$	Public	Income $< 2.5 \times$ FPL, low-performing school	Min. of tuition and public PPE	No	Yes	Yes
DC opportunity Scholarship Program (Washington, DC)	Wolf et al. (2007)	$0.13\sigma^a$	Public	Income $< 1.85 \times$ FPL	Min. of tuition and \$7,500 (2004)	Yes <sup>b</sup>	Yes	Yes
Parents Advancing Choice in Education (Dayton, OH)	Howell et al. (2002)	$0.08\sigma^c$	Private	Income $< 2 \times$ FPL	Min. of $0.6 \times$ tuition and \$1,200 (1998)	Yes	No	Yes
School Choice Scholarships Foundation (New York, NY)	Howell et al. (2002)	$0.08\sigma^c$	Private	Income $< 1.3 \times$ FPL	\$1,400 (1997)	Yes	No	Yes
Washington Scholarship Fund (Washington, DC)	Howell et al. (2002)	$-0.02\sigma^c$	Private	Income $< 2.7 \times$ FPL	Min. of $0.6 \times$ tuition and \$1,700 (1998)	Yes	No	Yes
Milwaukee Parental Choice Program (Milwaukee, WI)	Rouse (1998)	$0.12\sigma^d$	Public	Income $< 1.75 \times$ FPL	Public PPE <sup>e</sup>	No <sup>e</sup>	Yes	No <sup>e</sup>

*Notes:* This table compares school voucher programs' characteristics and achievement effects. Column 1 lists the article evaluating each program, and column 2 reports estimated effects on first-year math achievement in standard deviation units. Estimates from studies that report intent-to-treat (ITT) estimates are rescaled by first-stage effects on private school participation. Column 3 indicates whether a program is publicly or privately funded. Column 4 lists eligibility criteria, with income limits reported as a fraction of the federal poverty line (FPL). Column 5 reports the maximum amount of the voucher at the time of the evaluation. PPE refers to per pupil expenditure. Column 6 indicates whether a program allows parents to "top-up" the voucher by paying additional tuition beyond the maximum voucher amount. Column 7 indicates whether schools must opt in to the program to become eligible for voucher payments. Column 8 indicates whether the voucher can be used to pay tuition at religious schools.

<sup>a</sup>ITT estimate from table 4-1 is scaled by first-stage effect from table 2-5.

<sup>b</sup>Footnote 4 suggests that families rarely paid out-of-pocket when tuition exceeded the voucher amount.

<sup>c</sup>ITT estimates from table 4 are scaled by baseline math standard deviations from table 3 and first-stage effects from table 6.

<sup>d</sup>This is an annual gain estimate from a student fixed effects specification pooling data for four years (table VI, column 2).

<sup>e</sup>Since Rouse's (1998) study, the program rules have changed to reduce the maximum voucher below public per pupil expenditure, permit a limited amount of top-up, and allow participation by religious schools.

#### IV. Mechanisms

The negative effects of the LSP are surprising since many studies of oversubscribed school choice programs find positive or zero effects. Table 8 compares math achievement effects and program rules for the LSP versus several other voucher programs evaluated in the recent literature. Other programs use roughly similar income eligibility limits and rules for determining maximum voucher payments. Like the LSP, most other programs also allow vouchers to be used for tuition at religious schools, and some require private schools to opt into participation. The LSP is fairly unusual in prohibiting families from topping up the voucher payment when it falls short of private school tuition, a rule that may limit incentives for expensive, high-quality private schools to opt in. At the same time, the Milwaukee Parental Choice Program also prohibited top-up payments at the time of Rouse's (1998) evaluation, and this program increased achievement.

Overall, Table 8 shows that there is nothing distinctive about the LSP's basic structure that would be expected to yield negative achievement effects. We next assess several potential mechanisms that might explain the negative effects of LSP vouchers: lack of private school experience with state tests and the LSP-eligible population, problems associated with statewide expansion, disruption effects due to school switching, the quality of public schools attended by LSP lottery losers, and negative selection of private schools into the program. While this investigation is necessarily more speculative than our lottery-based analysis of program impacts, we find suggestive evidence that negative voucher effects are linked to lower quality private school participation in the LSP.

#### *A. Experience with the LSP Program*

Our estimates capture effects for LSP voucher applicants for 2012–2013, the year in which the LSP expanded statewide. Private schools may have been inexperienced with standardized tests and unfamiliar with the needs of LSP students during this transitional period. Newly participating schools also had little time to adapt their curricula to match state exam content. This lack of experience with LSP students and the program in general may have contributed to the LSP's negative effects.

Table 9 presents the results of three analyses that shed light on this hypothesis. Columns 1 and 2 compare effects for private schools that entered the LSP in 2012–2013 with schools that entered in prior years. Earlier entrants had more time to adjust to state assessments and were more experienced with the program before statewide expansion. Estimated effects for early and late entrants are negative and similar in all four subjects. Evidently, the negative effects of the LSP are not driven by private schools new to the program.

Along similar lines, columns 3 and 4 of Table 9 investigate differences in effects between the transitional 2012–2013 cohort and earlier applicant waves. Lack of oversubscription in the program's early years prevents a lottery-based analysis for earlier cohorts. As shown in Table 4, however, 2SLS and OLS estimates for 2012–2013 are very similar, thereby suggesting modest unobserved differences between applicants who accept and decline vouchers. We therefore report OLS estimates for applicant cohorts prior to 2012, with the caveat that these estimates may be affected by selection bias. OLS estimates for students applying from 2008 to 2011 are negative and similar to corresponding estimates for the 2012 cohort. This suggests that the negative effects of LSP participation were present before expansion and are not a temporary artifact of the effort to scale up the program statewide.<sup>5</sup>

Finally, to explore the role of mismatch between private school curricula and state exams, columns 5 and 6 of Table 9 report estimates from 2SLS models that interact LSP participation with the share of students at a school receiving LSP vouchers. The voucher share is jackknifed to remove the influence of a student's own enrollment

<sup>5</sup>Consistent with this evidence, a recent followup analysis by Mills and Wolf (2016, 2017) documents that the LSP's negative effects persist into the second year of participation for the 2012–2013 cohort. However, their results show a large baseline imbalance in the number of schools listed by lottery winners and losers along with significantly smaller first-stage impacts on LSP participation than we find in Table 4. This suggests that their data are not adequate to reconstruct the LSP voucher assignment process.

TABLE 9—VOUCHER EFFECTS BY EXPERIENCE WITH THE PROGRAM

Subject	By year school entered program		By student application year (OLS)		By voucher enrollment share	
	In 2012 (1)	Before 2012 (2)	2008–2011 (3)	2012 (4)	Below median (5)	Above median (6)
Math	–0.410 (0.103)	–0.425 (0.174)	–0.350 (0.095)	–0.442 (0.050)	–0.347 (0.158)	–0.434 (0.100)
Observations	757	490	615	3,261	540	572
<i>p</i> -value	0.942		0.389		0.641	
ELA	–0.078 (0.100)	–0.083 (0.131)	–0.185 (0.110)	–0.165 (0.040)	–0.100 (0.127)	–0.030 (0.114)
Observations	758	490	616	3,259	540	573
<i>p</i> -value	0.978		0.865		0.682	
Science	–0.291 (0.114)	–0.217 (0.174)	–0.515 (0.115)	–0.286 (0.041)	–0.249 (0.131)	–0.219 (0.153)
Observations	739	482	613	3,189	533	558
<i>p</i> -value	0.723		0.060		0.882	
Social studies	–0.354 (0.110)	–0.291 (0.157)	–0.423 (0.128)	–0.295 (0.041)	–0.290 (0.124)	–0.338 (0.150)
Observations	738	482	613	3,189	532	558
<i>p</i> -value	0.745		0.339		0.805	

*Notes:* This table reports estimates from models interacting Louisiana Scholarship Program (LSP) participation with measures of schools' experience with the program. Column 1 shows 2SLS estimates for schools that entered the program in 2012, while column 2 reports estimates for schools that participated in the program before 2012. Columns 3 and 4 report OLS estimates for students applying in 2008–2011 and 2012. The OLS sample includes first-time applicants to LSP schools for grades 3–8 from the 2008–2009 school year through the 2012–2013 school year. OLS models interact LSP participation with an indicator for applying before 2012 and control for first choice-year-grade indicators as well as sex, race, NSECD status, and family income quartile. Columns 5 and 6 show 2SLS estimates for schools above and below the sample median voucher enrollment share. See notes to Table 4 for a description of the 2SLS model specification. *p*-values are from tests of the hypothesis that subgroup differences are zero. Standard errors, clustered by risk set, are in parentheses.

choice. The average voucher enrollment share above the median of this measure is 0.42. This implies that some participating private schools administer tests to a large fraction of their students and therefore have a strong incentive to tailor instruction to state exam content. Results here show that if anything, schools serving more voucher students appear to generate larger achievement losses. The estimates are negative for schools both above and below the median voucher share, with slightly more negative math and social studies effects for schools above the median. Together, the results in Table 9 provide no evidence that either lack of experience with the LSP or temporary problems due to the statewide expansion are responsible for the program's negative effects.

### B. School Switching and Disruption Effects

LSP participants switch from public schools to private schools. School switching may account for the negative effects of LSP vouchers if moving between schools disrupts student learning. Yet, this explanation is implausible for two reasons. First, the disruptive effects of school switching are typically estimated to be small. For example, Hanushek, Kain, and Rivkin (2004) estimate that switching reduces math

achievement by roughly  $0.03\sigma$  on average. Second, school switching is a feature of all lottery-based evaluations of school choice programs, and many of these studies (including the other voucher programs in Table 8) show zero or positive effects in the first post-lottery year (Abdulkadiroğlu et al. 2011; Cullen, Jacob, and Levitt 2006; Howell and Peterson 2002; Wolf et al. 2010). School switching alone is therefore insufficient to explain negative voucher impacts.

### C. Public School Fallbacks

Lottery-based estimates capture causal effects of LSP participation relative to the schools that applicants would otherwise attend. Recent research demonstrates that some public charter schools in New Orleans generate very large test score gains (Abdulkadiroğlu et al. 2016). If voucher lottery losers attend these or other high-performing schools, the negative effects of LSP participation may be due to high scores in public school fallbacks rather than low performance at private schools. To some extent this issue is addressed by the distributional estimates in Figure 3, which show that mean untreated scores for compliers are below mean scores in the New Orleans RSD. This indicates that complier scores are not especially high at fallback public schools. Nevertheless, a complete interpretation of LSP effects requires understanding the mix of schools that define the voucher complier counterfactual.

We estimate characteristics of complier fallback schools with the equation

$$(4) \quad C_{s(i)} \times (1 - P_i) = \psi(1 - P_i) + \sum_{\ell} \mu_{\ell} d_{i\ell} + \mathbf{X}'_i \alpha + \xi_i,$$

instrumenting  $(1 - P_i)$  with the voucher offer  $Z_i$ . Here,  $s(i)$  indicates the school attended by student  $i$ , and  $C_{s(i)}$  is a characteristic of this school. By the same logic underlying the density estimation procedure based on equation (3), the 2SLS coefficient  $\psi$  captures the average of  $C_{s(i)}$  for compliers denied the opportunity to use LSP vouchers (Abadie 2002).

Table 10 describes counterfactual schools for voucher compliers. Columns 1 and 2 report mean school characteristics for offered and non-offered students, and column 4 reports 2SLS estimates of equation (4). A voucher offer reduces the probability of attending a charter school from 0.14 to 0.04 and lowers the probability of attending another public school from 0.77 to 0.22. As shown in column 4, these changes imply that 14 percent of compliers attend charter schools when denied an offer, and 82 percent attend other public schools. The remaining 4 percent attend schools of unknown type, possibly other private schools.

The last two rows of column 4 report fractions of students passing math and ELA tests at fallback schools. These results come from estimation of (4) setting  $C_s$  equal to the fraction of students at school  $s$  scoring Basic or above. Sixty-one percent of compliers' peers earn passing scores in math, and 57 percent pass ELA. These rates are well below the Louisiana state average (roughly 70 percent in each subject) and slightly below the RSD average (66 and 60 percent in math and ELA; Louisiana Department of Education 2014b). This investigation of counterfactuals shows that the negative effects of LSP participation are not due to atypical fallback

TABLE 10—CHARACTERISTICS OF TREATMENT AND FALLBACK SCHOOLS FOR VOUCHER APPLICANTS

	All applicants		Voucher compliers	
	Offered (1)	Not offered (2)	Offered (3)	Not offered (4)
Voucher school	0.730	0.051	1.000	0.000
Charter school	0.044	0.140	0.000	0.141
Other public school	0.216	0.772	0.000	0.819
Unknown school type	0.010	0.037	0.000	0.040
Fraction Basic or above: math	0.540	0.590	0.436	0.611
ELA	0.561	0.586	0.497	0.565

*Notes:* This table describes characteristics of schools attended by offered and non-offered applicants to the Louisiana Scholarship Program. The sample includes first-time voucher applicants, subject to first-choice random assignment, applying to grades 3–8 in 2012–2013. Columns 1 and 2 compare characteristics of the schools attended by offered and non-offered students. Columns 3 and 4 compare school characteristics for compliers who enroll in voucher schools in response to random offers. Fractions scoring Basic or above in math and ELA cover all students attending public schools, including non-applicants; for students attending voucher schools, these fractions include only voucher applicants.

schools: compliers denied vouchers score below the RSD average and attend mostly traditional public schools with achievement comparable to schools in disadvantaged urban districts. The negative impacts of LSP vouchers are due instead to extremely low scores for compliers in private schools.

#### D. Private School Selection

The descriptive statistics in Table 2 show that the LSP attracts private schools with low tuition and declining enrollment. This suggests that low-quality private schools may be disproportionately likely to opt into the LSP. To investigate whether negative selection of private schools can explain the program's negative achievement impacts, Table 11 reports relationships between voucher effects and school quality measures among participating schools.

Columns 1 and 2 show estimates from 2SLS models interacting LSP participation with a school's change in log enrollment between the two PSS waves prior to entering the LSP. The interaction coefficients for changes in log enrollment are close to zero and statistically insignificant, implying that effects are not especially negative for private schools experiencing the fastest enrollment losses. Estimates of this interaction effect are reasonably precise: we can reject that an additional 10 percent annual decline in enrollment is associated with a  $0.08\sigma$  decrease in a school's math effect.<sup>6</sup>

On the other hand, math achievement effects are significantly more negative for schools with lower tuition. Columns 3 and 4 report results from models that interact LSP participation with tuition. The estimates show that a \$1,000 increase in tuition is associated with a  $0.26\sigma$  increase in a school's math effect. The interaction model

<sup>6</sup>The upper bound of a 95 percent confidence interval for the additional achievement impact associated with a 100 percent increase in enrollment is  $-0.09\sigma + 1.96 \times 0.22\sigma = 0.34\sigma$ . Enrollment changes are computed over a two-year period, so this corresponds to a 50 percent annual change. The upper bound of a 95 percent confidence interval for a 10 percent annual change is therefore  $0.34\sigma \times 0.2 = 0.07\sigma$ .

TABLE 11—VOUCHER EFFECTS BY MEASURES OF SCHOOL QUALITY

Subject	By change in log enrollment		By tuition (\$1,000s)		By performance sanction	
	Main effect (1)	Interaction (2)	Main effect (3)	Interaction (4)	Sanctioned (5)	Not sanctioned (6)
Math	-0.352 (0.098)	-0.092 (0.223)	-0.355 (0.091)	0.263 (0.121)	-0.384 (0.118)	-0.452 (0.139)
Observations		938		1,050	672	575
<i>p</i> -value		0.679		0.030		0.709
ELA	-0.039 (0.091)	-0.015 (0.332)	-0.037 (0.087)	0.167 (0.106)	-0.129 (0.113)	-0.023 (0.111)
Observations		939		1,051	673	575
<i>p</i> -value		0.963		0.114		0.501
Science	-0.214 (0.111)	-0.397 (0.276)	-0.196 (0.100)	0.118 (0.113)	-0.277 (0.149)	-0.248 (0.113)
Observations		918		1,031	653	568
<i>p</i> -value		0.150		0.299		0.876
Social studies	-0.273 (0.104)	0.186 (0.313)	-0.265 (0.090)	0.170 (0.121)	-0.322 (0.125)	-0.341 (0.129)
Observations		917		1,030	653	567
<i>p</i> -value		0.552		0.158		0.919

*Notes:* This table reports estimates from 2SLS models interacting Louisiana Scholarship Program (LSP) participation with measures of the quality of the private schools to which students applied. Columns 1 and 2 show 2SLS estimates from a model interacting LSP participation with the change in log enrollment between the two most recent PSS surveys prior to entering the program, instrumenting with the interaction of the change in log enrollment and the lottery offer. The sample in these columns is restricted to schools for which PSS data are available. Columns 3 and 4 display 2SLS estimates interacting LSP participation with tuition. The sample in these columns is restricted to schools with available tuition data. Column 5 reports effects for schools that were sanctioned for academic performance in 2013–2014, and column 6 reports effects for schools that were not sanctioned. Interacting variables are demeaned in the estimation sample, so that main effects are at the mean. See notes to Table 4 for a description of the 2SLS model specification. *p*-values are from tests of the hypothesis that interaction effects or subgroup differences are zero. Standard errors, clustered by risk set, are in parentheses.

predicts a math effect of  $-0.06\sigma$  for a private school with average tuition compared to  $-0.36\sigma$  for an average oversubscribed LSP school.<sup>7</sup> Tuition interaction estimates for the other three subjects are also positive, though somewhat smaller and statistically insignificant.

The tuition interaction estimates suggest that selection of low-quality schools into LSP participation can account for a substantial portion of the program's negative math effects. The LSP's strict test-based accountability sanctions aim to mitigate this type of selection by removing low-performing schools. Similar sanctions appear to be effective at improving achievement in other contexts (Chiang 2009; Rockoff and Turner 2010; Rouse et al. 2013; Deming et al. 2016); we might expect the LSP to improve over time if its sanctions successfully identify the participating schools with most negative achievement effects. Columns 5 and 6 of Table 11 assess the efficacy of the program's accountability rules by comparing effects for

<sup>7</sup>Using the statistics in Table 2, the predicted effect for an average school is  $-0.36\sigma + 0.26\sigma \times \left( \frac{\$5,760 - \$4,653}{\$1,000} \right) = -0.06\sigma$ .

the 23 schools sanctioned for low scores in 2013–2014 to effects for unsanctioned schools. Estimates for these two groups are similar and not statistically distinguishable. This implies that the unadjusted test score levels used to determine LSP sanctions are not a reliable guide to causal achievement effects: voucher impacts are equally negative for schools not sanctioned for low scores. In other words, the existing accountability rules do not appear to identify the low-quality schools that drive the negative effects of the LSP.

## V. Conclusion

This paper shows that the expansion of school choice can reduce student achievement. The Louisiana Scholarship Program, a large school choice program providing private school vouchers to poor students attending low-performing public schools, reduces academic achievement one year after program entry, lowering mean test scores and increasing the likelihood of failure in math, reading, science, and social studies. These impacts are consistent across subgroups and geographic locations and are robust to adjustments for differential attrition between lottery winners and losers.

Private schools must apply for eligibility to enroll LSP voucher students. Survey data indicate that LSP-eligible schools charge lower tuition and experience rapid enrollment declines relative to other nearby private schools before entering the program. In addition, tuition is inversely related to math achievement effects among participating schools. These facts suggest that the LSP attracts a negatively selected group of private schools with substantial negative achievement effects. A further question is why this form of selection occurs for the LSP, but not for other similarly structured voucher programs evaluated in the existing literature. The links between the effects of school choice, program design, and market characteristics are an important direction for future research.

The estimates reported here capture causal impacts of oversubscribed private schools. Evidently, many parents wish to enroll their children in these schools despite their negative test score impacts. This may reflect either lack of knowledge about achievement effects or demand for school characteristics other than academic quality, such as religious instruction or a change in peer environment. Existing estimates of the link between achievement gains and adult earnings suggest that the perceived value of these other amenities would have to be extraordinarily large to explain the choice to participate in the LSP. For example, Chetty, Friedman, and Rockoff (2014b) estimate that a 1 standard deviation increase in math scores due to improved teacher quality boosts the present discounted value of lifetime earnings by about \$42,000 at age 12. This implies that the test score losses suffered by LSP participants in one year may be worth as much as \$17,000 per student.<sup>8</sup>

<sup>8</sup>Chetty, Friedman, and Rockoff (2014b) calculate that the average present discounted value of earnings at age 12 in the United States equals \$522,000 in 2010 dollars. They estimate that a one standard deviation increase in teacher value-added in a single grade boosts adult earnings by 1.3 percent. The standard deviation of teacher math value-added in student test score units equals  $0.16\sigma$ , implying that a one standard deviation improvement in test scores is worth  $\$522,000 \times 0.013/0.16 = \$42,413$ . If the link between test score effects and earnings effects is similar for the LSP, the math estimate in Table 4 translates into an earnings impact of  $-0.41 \times \$42,413 = -\$17,389$ .

Parent knowledge and program effectiveness may change over time as low-performing schools face accountability sanctions and information about school quality is revealed. Our estimates show that schools not sanctioned for low achievement perform just as poorly as sanctioned schools, indicating that level-based accountability standards may not be sufficient to identify and remove unproductive schools unless the threat of sanctions induces significant changes in future years. The evolution of choice behavior and program effects for future cohorts is another key question for future work.

#### APPENDIX A. COMPLIER CHARACTERISTICS

This Appendix describes the methods used to compute characteristics and potential outcome distributions for LSP voucher lottery compliers. As in the local average treatment effect (LATE) framework of Imbens and Angrist (1994), let  $Y_i(1)$  and  $Y_i(0)$  denote potential test scores as a function of the LSP treatment indicator  $P_i$ , and let  $P_i(1)$  and  $P_i(0)$  denote potential treatment choices as a function of the voucher lottery offer  $Z_i$ . Observed treatment is  $P_i = P_i(Z_i)$ , and the observed outcome is  $Y_i = Y_i(P_i)$ . The term  $X_i$  denotes a vector of baseline covariates.

Assume the vector  $(Y_i(1), Y_i(0), P_i(1), P_i(0), X_i)$  is independent of  $Z_i$  and that  $P_i(1) \geq P_i(0)$  for all  $i$ , with strict inequality for a positive measure of students. Then, for any measurable function  $g(Y_i, X_i)$ , Lemma 2.1 in Abadie (2002) implies

$$(A1) \quad \frac{E[g(Y_i, X_i)P_i|Z_i = 1] - E[g(Y_i, X_i)P_i|Z_i = 0]}{E[P_i|Z_i = 1] - E[P_i|Z_i = 0]} \\ = E[g(Y_i(1), X_i)|P_i(1) > P_i(0)],$$

$$(A2) \quad \frac{E[g(Y_i, X_i)(1 - P_i)|Z_i = 1] - E[g(Y_i, X_i)(1 - P_i)|Z_i = 0]}{E[1 - P_i|Z_i = 1] - E[1 - P_i|Z_i = 0]} \\ = E[g(Y_i(0), X_i)|P_i(1) > P_i(0)].$$

The left-hand side of (A1) is the Wald (1940) instrumental variables estimand using  $Z_i$  as an instrument for  $P_i$  in an equation for  $g(Y_i, X_i)P_i$ . Likewise, the left-hand side of (A2) is the IV estimand using  $Z_i$  as an instrument for  $(1 - P_i)$  in an equation for  $g(Y_i, X_i)(1 - P_i)$ . Equations (A1) and (A2) imply that these IV procedures yield mean values of  $g(Y_i, X_i)$  for compliers in the treated and untreated states.

We apply these results to estimate complier characteristics and potential outcome distributions. In practice, our IV models control for lottery risk-set indicators; the arguments in Angrist and Imbens (1995) imply the resulting 2SLS estimates are weighted averages of within-risk-set complier means. Control complier means in Table 5 are obtained by setting  $g(Y_i, X_i) = Y_i$  in equation (A2). Counterfactual school characteristics in Table 10 are obtained by setting  $g(Y_i, X_i) = C_{s(i)}$ . (The school characteristic  $C_{s(i)}$  may be viewed as an additional outcome variable.)

Treated and untreated complier densities in Figure 3 are obtained by setting  $g(Y_i, X_i) = \frac{1}{h}K\left(\frac{Y_i - y}{h}\right)$  in (A1) and (A2). Density estimation also requires selecting the bandwidth  $h$ . We use Silverman's (1986) rule-of-thumb bandwidth for the Gaussian kernel function, given by

$$h = 1.06 \sigma_y n^{-1/5},$$

where  $\sigma_y$  is the standard deviation of the outcome and  $n$  is the sample size. A complication arises in using this rule for complier density estimation because both standard deviations of complier outcomes and the number of compliers in the data are unobserved. We estimate standard deviations of complier potential outcomes by setting  $g(Y_i, X_i)$  equal to  $Y_i$  and  $Y_i^2$  in (A1) and (A2). This yields complier estimates of the first two noncentral moments of  $Y_i(1)$  and  $Y_i(0)$ , which are then used to construct an estimate of  $\sigma_y$  for each potential outcome. The expected number of treated compliers in the sample is  $n_c^1 = p_z \cdot \pi \cdot n$ , where  $p_z = \Pr[Z_i = 1]$ . The number of treated compliers is the fraction of lottery winners times the population share of compliers (equal to the first stage coefficient  $\pi$ ) times total sample size. Likewise, the expected non-treated complier sample size is  $n_c^0 = (1 - p_z) \cdot \pi \cdot n$ . We plug the empirical lottery offer probability and first stage coefficient into these formulas to construct rule-of-thumb bandwidths appropriate for complier density estimation.

Figure 3 also reports bootstrap  $p$ -values from tests of the null hypothesis that treated and untreated complier distributions are equal. The underlying tests are based on methods from Abadie (2002), who notes that treated and untreated complier distributions are equal if and only if the distribution of  $Y_i$  does not depend on  $Z_i$ . A test statistic for this hypothesis is the maximum difference in CDFs for the  $Z_i = 1$  and  $Z_i = 0$  samples. Differences in CDFs are estimated by regressing  $1\{Y_i \leq y\}$  on  $Z_i$  for 100 equally spaced values of  $y$  covering the support of  $Y_i$ , controlling for risk-set indicators. The Kolmogorov-Smirnov (KS) statistic is the maximum of absolute values of the coefficients across these regressions.

A bootstrap distribution for the KS statistic is constructed by first drawing samples with replacement stratified by risk set and then randomly assigning simulated lottery offers to match the full-sample proportions offered within each risk set. The KS statistic is then recomputed in each bootstrap sample. The bootstrap  $p$ -value for a test of equality of treated and untreated complier distributions is the fraction of bootstrap KS statistics greater than the full-sample KS statistic. We implement this procedure in Figure 3 using 250 bootstrap trials.

Finally, to aid interpretation of the magnitudes of differences in distributions, the reported KS statistics in Figure 3 are maximum differences in complier CDFs rather than maximum differences in offered and non-offered CDFs. Complier CDFs are estimated by plugging  $1\{Y_i \leq y\}$  into (A1) and (A2) at the same 100 points used in the bootstrap tests for distributional equality.

#### APPENDIX B. BOUNDS ON VOUCHER EFFECTS

We next describe methods for bounding local average treatment effects in the presence of differential attrition between lottery winners and losers. The arguments

here follow those in Engberg et al. (2014), adapted to the notation used in our analysis. As in Appendix A, define potential outcomes  $Y_i(p)$  and potential treatments  $P_i(z)$  and assume these are independent of  $Z_i$ . Now, however, let the treatment variable  $P_i$  take three values:  $P_i \in \{0, 1, a\}$ . When  $P_i = a$ , student  $i$  attrits from the sample, and her outcome is not observed.

We make the following monotonicity assumption on responses to voucher offers:

$$P_i(1) \neq P_i(0) \Rightarrow P_i(1) = 1.$$

This restriction implies that any student who changes behavior in response to a voucher offer does so to participate in the LSP program. In other words, no one exits LSP in response to an offer, and no one exits the sample in response to an offer.

Under this assumption the population can be partitioned into the following groups:

- (i) Always takers:  $P_i(1) = P_i(0) = 1$ .
- (ii) Never takers:  $P_i(1) = P_i(0) = 0$ .
- (iii) Always attriters:  $P_i(1) = P_i(0) = a$ .
- (iv) Compliers:  $P_i(1) = 1, P_i(0) = 0$ .
- (v) At-risk:  $P_i(1) = 1, P_i(0) = a$ .

This classification scheme is a version of the principal stratification framework of Frangakis and Rubin (2002), which divides an experimental population into groups defined by responses to random assignment. The twist here relative to the usual LATE model is the presence of at-risk students. Without such students, IV estimates of voucher effects are consistent for local average treatment effects. With these students, LATE is not identified, and we must bound it.

Let  $\pi^g$  denote population shares of the five groups for  $g \in \{at, nt, aa, c, ar\}$ . Likewise, let  $\mu_p^g$  denote the mean of  $Y_i(p)$  for group  $g$  and  $p \in \{0, 1\}$ . The average causal effect of voucher receipt for compliers is  $LATE \equiv \mu_1^c - \mu_0^c$ . To bound this quantity, first note that the population shares of each group are identified, since

$$\Pr[P_i = 1 | Z_i = 0] = \pi^{at},$$

$$\Pr[P_i = 0 | Z_i = 1] = \pi^{nt},$$

$$\Pr[P_i = a | Z_i = 1] = \pi^{aa},$$

$$\Pr[P_i = 0 | Z_i = 0] - \Pr[P_i = 0 | Z_i = 1] = \pi^c,$$

$$\Pr[P_i = a | Z_i = 0] - \Pr[P_i = a | Z_i = 1] = \pi^{ar}.$$

Mean observed outcomes for non-treated students by offer status are

$$E[Y_i | P_i = 0, Z_i = 1] = \mu_0^{nt},$$

$$E[Y_i | P_i = Z_i = 0] = \left( \frac{\pi^{nt}}{\pi^c + \pi^{nt}} \right) \mu_0^{nt} + \left( \frac{\pi^c}{\pi^c + \pi^{nt}} \right) \mu_0^c.$$

These expressions show that the never taker mean is observed among students who decline offers, and the group of non-offered, non-treated students is a mixture of never takers and compliers. The non-treated complier mean can then be backed out as

$$\mu_0^c = \frac{(\pi^c + \pi^{nt})E[Y_i | P_i = Z_i = 0] - \pi^{nt}E[Y_i | P_i = 0, Z_i = 1]}{\pi^c}.$$

It is straightforward to show that the moments in this equation are equivalent to those used in equation (A2) when  $g(Y_i, X_i) = Y_i$ , substituting  $1\{P_i = 0\}$  for  $(1 - P_i)$  since  $P_i$  is now an unordered treatment.

The presence of at-risk students prevents us from backing out  $\mu_1^c$  in similar fashion. To bound it, note that we can identify the distribution of  $Y_i(1)$  for the pooled population of compliers and at-risk students. Specifically, we have

$$\begin{aligned} \text{(B1)} \quad & \frac{E[1\{Y_i \leq y\}1\{P_i = 1\}|Z_i = 1] - E[1\{Y_i \leq y\}1\{P_i = 1\}|Z_i = 0]}{E[1\{P_i = 1\}|Z_i = 1] - E[1\{P_i = 1\}|Z_i = 0]} \\ &= \Pr[Y_i(1) \leq y | P_i(1) \neq P_i(0)] \\ &\equiv F_1(y). \end{aligned}$$

This result follows by applying equation (A1).

The minimum possible value of  $\mu_1^c$  occurs when compliers occupy the entire lower tail of this mixture distribution. The complier share in the mixture is  $\pi^c/(\pi^c + \pi^{ar})$ . Then,

$$\begin{aligned} \mu_1^c &\geq E\left[Y_i(1) | Y_i(1) \leq F_1^{-1}\left(\frac{\pi^c}{\pi^c + \pi^{ar}}\right), P_i(1) \neq P_i(0)\right] \\ &= \frac{E\left[Y_i 1\left\{Y_i \leq F_1^{-1}\left(\frac{\pi^c}{\pi^c + \pi^{ar}}\right)\right\} 1\{P_i = 1\} | Z_i = 1\right]}{E[1\{P_i = 0\} | Z_i = 0] - E[1\{P_i = 0\} | Z_i = 1]} \\ &\quad - \frac{E\left[Y_i 1\left\{Y_i \leq F_1^{-1}\left(\frac{\pi^c}{\pi^c + \pi^{ar}}\right)\right\} 1\{P_i = 1\} | Z_i = 0\right]}{E[1\{P_i = 0\} | Z_i = 0] - E[1\{P_i = 0\} | Z_i = 1]} \\ &\equiv \mu_{\min}, \end{aligned}$$

where the first equality follows from another application of equation (A1), rescaling appropriately by the probability that the event  $\left\{ Y_i \leq F_1^{-1}\left(\frac{\pi^c}{\pi^c + \pi^{ar}}\right) \right\}$  occurs in the mixture of treated compliers and at-risk students. Similarly, an upper bound for the treated complier mean is

$$\begin{aligned} \mu_1^c &\leq E\left[ Y_i(1) \mid Y_i(1) \geq F_1^{-1}\left(\frac{\pi^{ar}}{\pi^c + \pi^{ar}}\right), P_i(1) \neq P_i(0) \right] \\ &= \frac{E\left[ Y_i 1\left\{ Y_i \geq F_1^{-1}\left(\frac{\pi^{ar}}{\pi^c + \pi^{ar}}\right) \right\} 1\{P_i = 1\} \mid Z_i = 1 \right]}{E\left[ 1\{P_i = 0\} \mid Z_i = 0 \right] - E\left[ 1\{P_i = 0\} \mid Z_i = 1 \right]} \\ &\quad - \frac{E\left[ Y_i 1\left\{ Y_i \geq F_1^{-1}\left(\frac{\pi^{ar}}{\pi^c + \pi^{ar}}\right) \right\} 1\{P_i = 1\} \mid Z_i = 0 \right]}{E\left[ 1\{P_i = 0\} \mid Z_i = 0 \right] - E\left[ 1\{P_i = 0\} \mid Z_i = 1 \right]} \\ &\equiv \mu_{\max}. \end{aligned}$$

Bounds on LATE are then

$$\mu_{\min} - \mu_0^c \leq LATE \leq \mu_{\max} - \mu_0^c.$$

Estimation of these bounds is implemented with the following steps:

- (i) Estimate the probabilities  $\pi^{ar}$  and  $\pi^c$  as minus the shifts in the probability of attrition and non-participation induced by the lottery offer.
- (ii) Estimate the CDF of  $Y_i(1)$  for the mixture of compliers and at-risk students using equation (B1).
- (iii) Use the estimated CDF to find  $F_1^{-1}\left(\frac{\pi^c}{\pi^c + \pi^{ar}}\right)$  and  $F_1^{-1}\left(\frac{\pi^{ar}}{\pi^c + \pi^{ar}}\right)$ . This can be done by searching over values of  $y$  to find the point that yields the appropriate value of  $F_1(y)$ .
- (iv) Use the expressions above to estimate  $\mu_{\max}$  and  $\mu_{\min}$ .
- (v) Estimate  $\mu_0^c$  using equation (A2), setting  $g(Y_i, X_i) = Y_i$  and substituting  $1\{P_i = 0\}$  for  $(1 - P_i)$ .
- (vi) Construct bounds for LATE using the estimates of  $\mu_{\max}$ ,  $\mu_{\min}$ , and  $\mu_0^c$ .

After estimating the bounds we obtain standard errors by conducting 100 bootstrap replications of the entire procedure. In practice, risk set indicators and baseline covariates are included in all regressions used to estimate group shares, CDFs, and mean potential outcomes.

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## Online Appendix

This project combines Louisiana Scholarship Program application and test score data provided by the Louisiana Department of Education with private school characteristics from the Private School Universe Survey. This appendix describes each data file used in the analysis and details the procedures used to clean and match them.

### OA.1 Application data

The Louisiana Department of Education provided data on all LSP voucher applications submitted between Fall 2008 and Fall 2012. The raw data include 16,739 application records with ranked lists of at least one and up to five private school choices. Additional variables code application grade and year, admission priorities based on sibling status, NSECD status, geographic proximity, previous school, and the school for which a student was offered a voucher (if any). The application data also include basic demographics (race and sex) along with family income used to determine LSP eligibility.

From the raw data, we extract first-time 2012-2013 applicants to grades 3-8 from the raw data. Next, from this subsample we select students in first-choice priority classes within which there is variation in first-choice voucher offers. This leaves 1,412 students subject to random assignment at first-choice schools. Within this sample the lottery offer is coded as indicating a voucher offer at the first-choice school, and risk sets are coded as interactions of application grade and first choice school.

### OA.2 Test score data

Our second data source is a database of 7,187 LEAP and iLEAP scores on tests taken by LSP applicants between Spring 2009 and Spring 2013. The test score file was meant to follow LSP applicants in grades 3-8 for one year after application. Each record includes a set of variables tracking scaled versions of math, ELA, science, and social studies scores along with performance category codes for these scores. We standardize the scaled scores using means and standard deviations for RSD students in each subject, grade, and year. Students in the test score file are distinguished by a scrambled identifier called the *randomid*. The file also includes a school identifier, called the *sitedcd*, that codes the testing location; this identifier is used in most public data files provided by the Louisiana Department of Education. We use the *sitedcd* field to merge school names, LSP private school status, charter school and public school status, and SPS scores for public schools into the data file. LSP participation is coded as an indicator equal to one if a student is tested at an LSP-participating private school.

We drop records with no test score information. Two percent of student identifiers have multiple test score records in the same year. Among these duplicates we give preference to (a) tests taken at LSP private schools; and (b) complete records with no missing scores for any test. If a student has two sets of incomplete scores and neither test was taken at an LSP school, we combine observations into one record with the most

complete possible set of scores. This leaves less than 0.5 percent of students with multiple conflicting records; among these, we pick one record at random. Finally, the test score file is merged with the LSP applicant file using the *randomid*, which appears in both files. Eighty-nine percent of LSP applicants in grades 3-8 have matching records in the test score file.

### **OA.3 Private School Universe Survey**

Characteristics for Louisiana private schools are measured by the Private School Universe Survey (PSS). The PSS is a census conducted every two years by the National Center for Education Statistic, of all US private schools. PSS data files are available from 1990 through 2012. Key variables include total enrollment and enrollment shares by race, number of teachers, religious affiliation, instructional time (length of school day and year), geographic identifiers (state, county, zip code, city name, and exact address), and a school identifier that is constant over time.

We used school names and cities to match a list, provided by the Louisiana Department of Education, of participating LSP private schools to PSS data from 2012, 2010, and 2008. Many LSP schools had exact matches in the PSS, and others produced close inexact matches because of differences in punctuation or naming conventions between the two data sets. For example, the PSS name field often included the modifier “school” whereas the LSP database did not (as in “ST JOSEPH ELEMENTARY” vs. “ST JOSEPH ELEMENTARY SCHOOL”). We manually matched these cases when school names and cities clearly made sense. This resulted in matches for 142 of 159 LSP participant private schools. The remaining 17 schools either had no close match in the PSS or multiple close matches with ambiguity regarding the correct school.

### **OA.4 Tuition data**

We searched for tuition data for the 359 Louisiana private schools present in the 2012 PSS. The search took place in two phases. First, we checked school websites for 2015-2016 tuition information, converting monthly or annual rates to 10-month tuition for all schools. Some schools listed discounts for certain student categories, such as church members; we used the undiscounted rate in these cases. Second, when no information was listed on a school’s website, we contacted the main office by phone. The combination of online searches and phone calls yielded tuition information for 116 of 124 voucher schools (94 percent) and 216 of 235 non-voucher schools (92 percent).