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School Vouchers and Student Achievement: Evidence from the Louisiana Scholarship Program
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ABSTRACT

We evaluate the Louisiana Scholarship Program (LSP), a prominent school voucher plan. The LSP provides public funds for disadvantaged students at low-performing Louisiana public schools to attend private schools of their choice. LSP vouchers are allocated by random lottery at schools with more eligible applicants than available seats. We estimate causal effects of voucher receipt by comparing outcomes for lottery winners and losers. This comparison reveals that LSP participation substantially reduces academic achievement. Attendance at an LSP-eligible private school lowers math scores by 0.4 standard deviations and increases the likelihood of a failing score by 50 percent. Voucher effects for reading, science and social studies are also negative and large. An exploration of mechanisms suggests these effects are not due to private schools' inexperience with the program, disruption caused by school switching, or the quality of public school options available to LSP applicants. Negative voucher effects may be due in part to selection of low-quality private schools into the LSP: participating private schools charge lower tuition than other private schools and experience enrollment declines prior to entering the program. The program's negative math effects are concentrated among the eligible schools with lowest tuition.

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1 Introduction

The benefits and costs of injecting market forces into the US education system are a matter of continuing debate. School voucher programs, a paradigm of market-based education reform, allow students to direct public funds toward tuition payments at private schools of their choice. Advocates of such programs argue that vouchers force schools to compete for students, boosting educational quality and promoting better matches between students and schools (Friedman, 1962). On the other hand, vouchers may increase stratification by income and ability if richer, higher-ability students are induced to exit public schools (Epple and Romano, 1998; Ladd, 2002a). While they currently serve a small share of students, voucher programs are expanding quickly: the number of students using educational vouchers grew from 61,000 in 2009 to 141,000 in 2015, an increase of 130 percent. By 2014, 19 states operated at least one school voucher program (Alliance for School Choice, 2009, 2015).

Evidence on the effects of school vouchers is mixed. Much of the existing literature focuses on the causal effects of vouchers on outcomes for students who use them.[1] Rouse (1998) finds that the Milwaukee Parental Choice Program, the largest and oldest voucher system in the US, generates modest test score gains for voucher users. Randomized evaluations of privately-funded voucher plans in New York, Washington DC, and Dayton Ohio show negligible average test score impacts for students granted vouchers, though some subgroups may benefit (Howell and Peterson, 2002; Mayer et al., 2002; Howell et al., 2002; Krueger and Zhu, 2004). Similarly, Chingos and Peterson (2012) find no overall effect of the New York program on college enrollment. In an experimental evaluation of a publicly-funded voucher program in Washington DC, Wolf et al. (2007; 2010) show that vouchers do not affect test scores but boost high school graduation rates. Together, these findings show that voucher programs typically produce modest effects on outcomes for participating students.

This paper evaluates the Louisiana Scholarship Program (LSP), a school voucher program that provides targeted 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.

Our results show that LSP vouchers reduce academic achievement. Attendance at an LSP-eligible private school is estimated to lower math scores by an average of 0.41 standard deviations (σ) and reduce reading,

[1] Other studies look at the effects of voucher programs on public school performance (Hoxby, 2003; Figlio and Rouse, 2006; Chakrabarti, 2008, 2013a; Figlio and Hart, 2014) and student sorting (Epple et al., 2004; Campbell et al., 2005; Chakrabarti, 2013b). Several papers also study the effects of voucher programs outside the US (Angrist et al., 2002, 2006; Hsieh and Urquiola, 2006; Nielson, 2014; Muralidharan and Sundararaman, 2015). See Epple et al. (forthcoming), Barrow and Rouse (2009), Ladd (2002b) and Neal (2002) for reviews of research on school voucher programs.

science and social studies scores by 0.08σ, 0.26σ, and 0.33σ 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 investigate several candidate explanations for these effects. This investigation suggests that negative voucher impacts are not due to schools' inexperience with the program, school switching effects, or the quality of public school options available to LSP applicants. Impacts are equally negative for schools that participated in the program for multiple years and for schools enrolling more voucher recipients, indicating that the program's effects are not driven by private schools that are less experienced with voucher students. Likewise, typical estimates of mobility effects show that disruption caused by school switching is not a credible explanation for our large negative voucher estimates (Hanushek et al., 2004). Students lotteried out of the LSP earn scores similar to average students in disadvantaged urban districts and attend public schools typical for these areas.

We find evidence that the negative effects of the LSP may be linked to selection of low-quality private schools into the program. Participating schools charge below-average tuition and experience sharp relative declines in enrollment prior to entering the LSP compared to non-participating Louisiana private schools. This suggests the program attracts low-quality schools struggling to maintain enrollment. Moreover, the program's negative math effects are concentrated among the participating schools with lowest tuition. The LSP includes test-based accountability rules that aim to retrospectively identify and remove low-quality schools, but lottery estimates are similar for schools that were subsequently sanctioned for weak academic performance and for schools that were not sanctioned. This implies that current accountability rules do not identify the schools that drive negative voucher impacts.

This paper contributes to a large literature using randomized admission lotteries to estimate the causal effects of schools on student outcomes (Cullen et al., 2006; Abdulkadiroğlu et al., 2011; Angrist et al., 2013; Dobbie and Fryer, 2013; Deming et al., 2014). Ours is the first study to demonstrate large negative effects of a school choice program. A recent followup analysis by Mills and Wolf (2016) investigates the achievement impacts of the LSP in the second year of participation for the 2012 voucher applicant cohort. Their estimates show that the negative impacts of LSP vouchers persist into the second year, a result that complements the findings reported here.

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 3 outlines our empirical approach and reports lottery-based estimates of voucher effects. Section 4 documents the robustness of these estimates to adjustments for differential attrition between lottery winners and losers. Section 5 explores mechanisms that might explain negative voucher impacts. Section 6 concludes.

2 Data and Background

2.1 The Louisiana Scholarship Program

The Louisiana Scholarship Program, also known as the Student Scholarships for Educational Excellence 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 can be seen in Figure 1, which plots the number 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 US (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 to qualify students 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 the lesser of the private school's tuition fee and the per-pupil funding level of the student's home district. 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, 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 gain eligibility 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 less than the requested number. Schools with more LSP voucher applicants than authorized seats must give priority to students with enrolled siblings, those living nearby and those previously enrolled in D or F-rated public schools.[2] 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).

[2]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.

To maintain eligibility private schools must undergo annual financial audits and administer Louisiana state achievement tests to LSP students. Non-LSP students enrolled at the 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 the achievement of voucher students. Schools with SCI scores less than 50 (equivalent to an F on the SPS scale) in the second year of participation or a subsequent year are not eligible to enroll new voucher students the next year, though they may retain students already enrolled. Schools without enough students to qualify for an SCI 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 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).

2.2 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. Followup 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.[3] 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.

[3]LEAP exams are taken in 4th and 8th grade; iLEAP exams are taken in 3rd, 5th, 6th and 7th. 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.

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.

Data on LSP applicants are supplemented 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 along with class size, instructional time, religious affiliation and geographic location. We matched the 2000-2012 waves of the PSS 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. Appendix A provides further details on data processing and sample construction.

2.3 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 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), students who use LSP vouchers are slightly less disadvantaged than the general population of applicants. Eighty-one percent of voucher recipients are Black and average family income equals \$17,389 for this group. These income levels are well below 250 percent of the poverty line, the limit for LSP eligibility (\$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 to characteristics of 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 presents a more complete investigation of enrollment trendsby 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. Enrollment levels were slightly higher in 2000 for 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. Enrollment fell in both groups 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.

3 Lottery Estimates of Voucher Effects

3.1 Empirical Framework

The primary equation of interest for our empirical analysis is

$$Y_i = \beta P_i + \sum_{\ell} \gamma_\ell d_{i\ell} + X_i'\delta + \epsilon_i, \tag{1}$$

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 application school and priority status. X_i is a vector of baseline covariates included to increase precision (gender, race, NSECD status, and family income quartiles).

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

$$P_i = \pi Z_i + \sum_{\ell} \rho_\ell d_{i\ell} + X_i'\theta + \eta_i. \tag{2}$$

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 β may be interpreted as a local average treatment effect (LATE), an average causal effect of participation for "compliers" induced to attend private schools by LSP vouchers (Imbens and Angrist, 1994; Angrist et al., 1996).

3.2 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 followup 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.

3.3 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 year following the lottery. This estimate corresponds to the first-stage coefficient π 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 0.28σ in math, 0.06σ in ELA, 0.18σ in science, and 0.23σ in social studies.

Because the IV models estimated here are just-identified, 2SLS estimates of β 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 0.41σ one year after the lottery, and reduces ELA, science and social studies scores by 0.08σ, 0.26σ and 0.33σ, 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.[4] 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

[4]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.

subjects.

Together, the estimates in Table 4 clearly demonstrate that attendance at LSP-eligible private schools reduces test scores for voucher recipients. It's worth benchmarking the sizes of these effects against the impacts of important educational interventions evaluated in the recent literature. Rouse (1998) estimates that participation 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 0.2σ (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 et al., 2014; Angrist et al., 2015). Studies of effective charter schools show annual score gains between 0.2σ and 0.4σ (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.

3.4 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. High stakes are attached to these categories 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).

We investigate the effects of LSP vouchers 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), average non-LSP outcomes for voucher lottery compliers. Appendix B 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 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.

Effects on higher score 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.

3.5 Effects on Score Distributions

To develop a more complete picture of the distributional effects of LSP vouchers we estimate marginal test score densities for compliers lotteried into and out of the program. 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

$$\tfrac{1}{h}K\left(\tfrac{Y_i-y}{h}\right)\cdot P_i = \tau_y P_i + \sum_{\ell}\kappa_{\ell y}d_{i\ell} + X_i'\lambda_y + v_{iy}, \tag{3}$$

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 τ_y is a consistent estimate of the density function of $Y_i(1)$ for voucher lottery compliers evaluated at y (Angrist et al., forthcoming; 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. The Figure also reports Kolmogorov-Smirnov test statistics equal to maximum differences in estimated complier CDFs, along with bootstrap p-values from tests of distributional equality (see Appendix B). These tests result in rejections of distributional equality at conventional levels for all four subjects ($p \leq 0.02$).

3.6 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). Eighty-six percent of LSP applicants are Black so 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 interacts 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, implying similar effects for richer and poorer students. Columns (3) and (4) display estimates from a model interacting participation with whether applicants reside in New Orleans and Baton Rogue, 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 Rogue are imprecise due to small samples.

A large literature evalutes the effects of Catholic private schools on student outcomes (Neal, 1997; Altonji et al., 2005). Columns (5) and (6) of Table 7 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 the effects of LSP vouchers 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 0.62σ in math, an effect three times as large as the loss for students in grades six through eight (0.21σ). Similarly, vouchers reduce ELA scores by 0.3σ for younger children, while the ELA estimate for older children is positive and marginally significant. These cross-grade differences in effects are statistically significant at conventional levels ($p \leq 0.01$). Estimates of science and social studies effects are also more negative for younger applicants, though differences for these subjects are not statistically significant.

4 Attrition

Even with random assignment of LSP vouchers, 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 that followup rates for the lottery sample are high: test scores are observed for at least 83 percent of lottery losers in each subject. As shown in column (2), however, followup scores are more likely to be observed for lottery winners than for losers. 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 in private schools for accountability purposes, while non-participants who exit the public school system are not followed. It's worth noting that baseline characteristics remain balanced between winners and losers in the sample with followup scores, which can be seen in column (3) of Table 3. Nonetheless, we cannot be assured of balance on unobserved characteristics.

We conduct two analyses to assess the robustness of our results to selective attrition. The first drops lottery risk sets with large attrition differentials and reports voucher effects in the remaining sample. The second constructs nonparametric bounds on local average treatment effects. The latter approach is in the spirit of Lee (2009), who derives sharp bounds on treatment effects in randomized experiments under a monotonicity assumption on the attrition process. 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 C 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 largest differentials. Column (4) shows that followup rates in the remaining sample are roughly 90 percent, and column (5) shows that differences in attrition between lottery winners and losers are small and 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 followup scores, these results suggest that the attrition process is not very selective. Our full sample lottery estimates are therefore likely to be reliable.

Columns (7) and (8) display estimated bounds on local average treatment effects for compliers. These bounds are relatively wide owing to 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 -0.18σ, and this estimate is statistically significant at the 5-percent level. The conclusion that LSP vouchers reduce math scores is therefore robust to this conservative adjustment for differential attrition.

5 Mechanisms

The negative effects of the LSP are surprising in view of the small positive or zero effects found in studies of other similar school voucher programs. Table 8 compares math achievement effects and program rules for the LSP and several other voucher plans 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 in to participation. The LSP is fairly unusual in prohibiting families from topping up the voucher payment when it falls short of private school tuition, which 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 basic structure of the LSP that would be expected to yield negative achievement effects. We next assess four other potential mechanisms that might explain the negative effects of LSP vouchers: lack of private school experience with state tests and the LSP, 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 selection of lower-quality private schools into LSP participation.

5.1 Lack of Experience with State Tests and LSP Students

Our estimates capture effects for students applying for LSP vouchers for 2012-2013, a year during 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. New participating schools also had little time to adapt their curricula to match the content of state exams. This lack of experience with LSP students and state tests may have contributed to the program'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 to 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 waves of applicants. 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, suggesting modest unobserved differences between applicants that 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 the program up statewide.[5]

[5]Consistent with this evidence, Mills and Wolf document that the negative effects of the LSP persist into the second year of participation for the 2012-2013 cohort.

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 interacts 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 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 the content of state exams. Results here indicate that effects do not vary with the share of students receiving LSP vouchers. Together, the results in Table 9 provide no evidence that lack of experience with the LSP, temporary features of the statewide expansion, or mismatch between private school curricula and state exams are responsible for the program's negative effects.

5.2 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. This explanation is implausible for two reasons, however. First, typical estimates of the disruptive effect of school switching are small. For example, Hanushek et al. (2004) estimate that switching reduces math achievement by roughly 0.03σ 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 et al., 2006; Howell and Peterson, 2002; Wolf et al., 2010). School switching alone is therefore insufficient to explain negative voucher impacts.

5.3 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., 2015). 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 proper interpretation of the effects of the LSP requires understanding the mix of schools that define the voucher complier counterfactual.

We estimate characteristics of complier fallback schools with the equation

$$C_{s(i)} \cdot (1 - P_i) = \psi(1 - P_i) + \sum_{\ell} \mu_\ell d_{i\ell} + X_i'\alpha + \xi_i, \tag{4}$$

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 ψ 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 negative effects of LSP participation are not due to atypical fallback schools: compliers denied vouchers score below the RSD average and attend mostly traditional public schools with achievement comparable to schools in a disadvantaged urban district. The negative impacts of LSP vouchers are instead due to extremely low scores for compliers in private schools.

5.4 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 in to 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 measures of school quality 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 0.08σ decrease in a school's math effect.[6]

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

[6] 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 $0.34\sigma \times 0.2 = 0.07\sigma$.

show that a \$1,000 increase in tuition is associated with a 0.26σ increase in a school's math effect. The interaction model predicts a math effect of -0.06σ for a private school with average tuition, compared to -0.36σ for an average oversubscribed LSP school.[7] 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 participating 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., forthcoming); 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 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, existing accountability rules do not appear to identify the low-quality schools that drive the negative effects of the LSP.

6 Conclusion

This paper evaluates the Louisiana Scholarship Program, a large targeted voucher plan providing private school tuition payments for poor students attending low-performing public schools. The LSP allocates seats at oversubscribed private schools by random lottery, a feature that facilitates quasi-experimental estimation of program effects. Lottery-based estimates show that LSP participation 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.

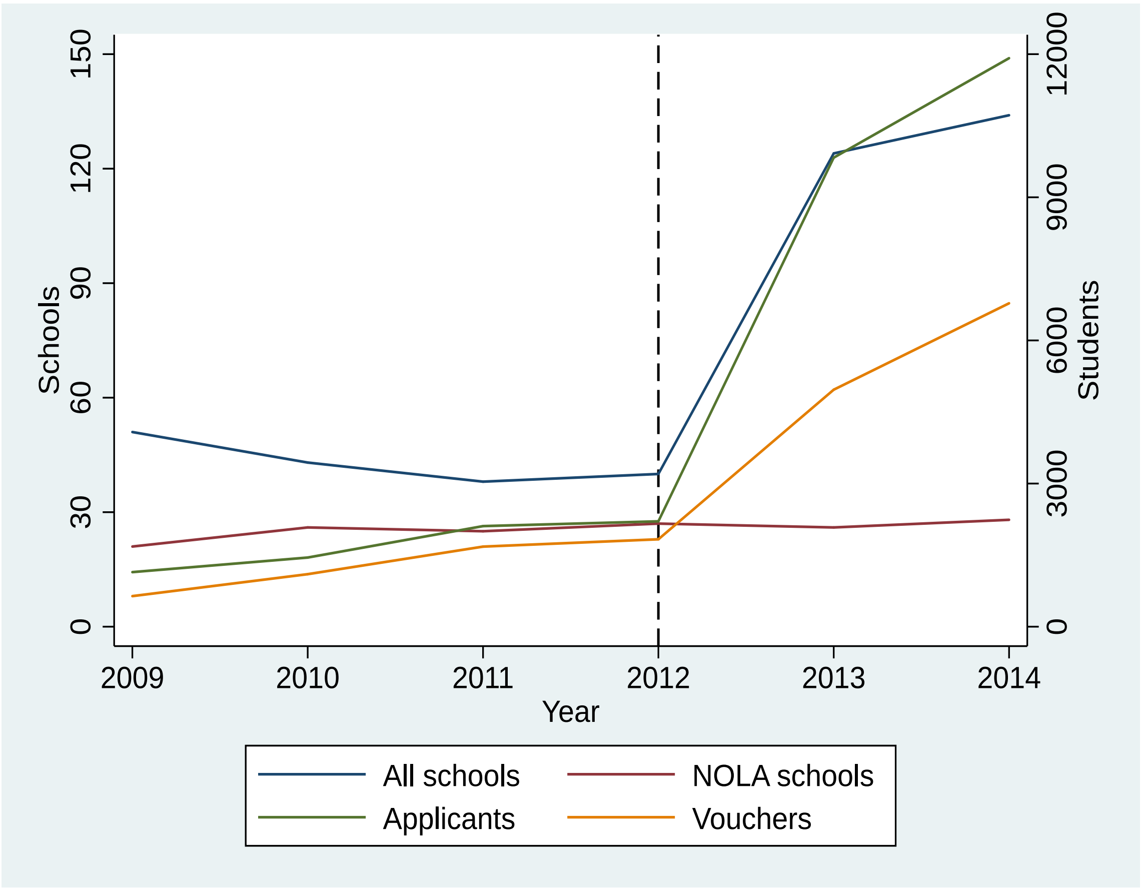
Private schools must apply for eligibility to enroll LSP voucher students. Survey data indicate that LSP-eligible schools charge lower tuition and experience rapid enrollment declines relative to other nearby private schools before entering the program. In addition, tuition is inversely related to math achievement effects among participating schools. These facts suggests that the LSP attracts a negatively-selected set 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 previous literature. The link between voucher effectiveness, program design and market characteristics is an important direction for future research.

The estimates reported here capture causal impacts of oversubscribed private schools. Evidently, many

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

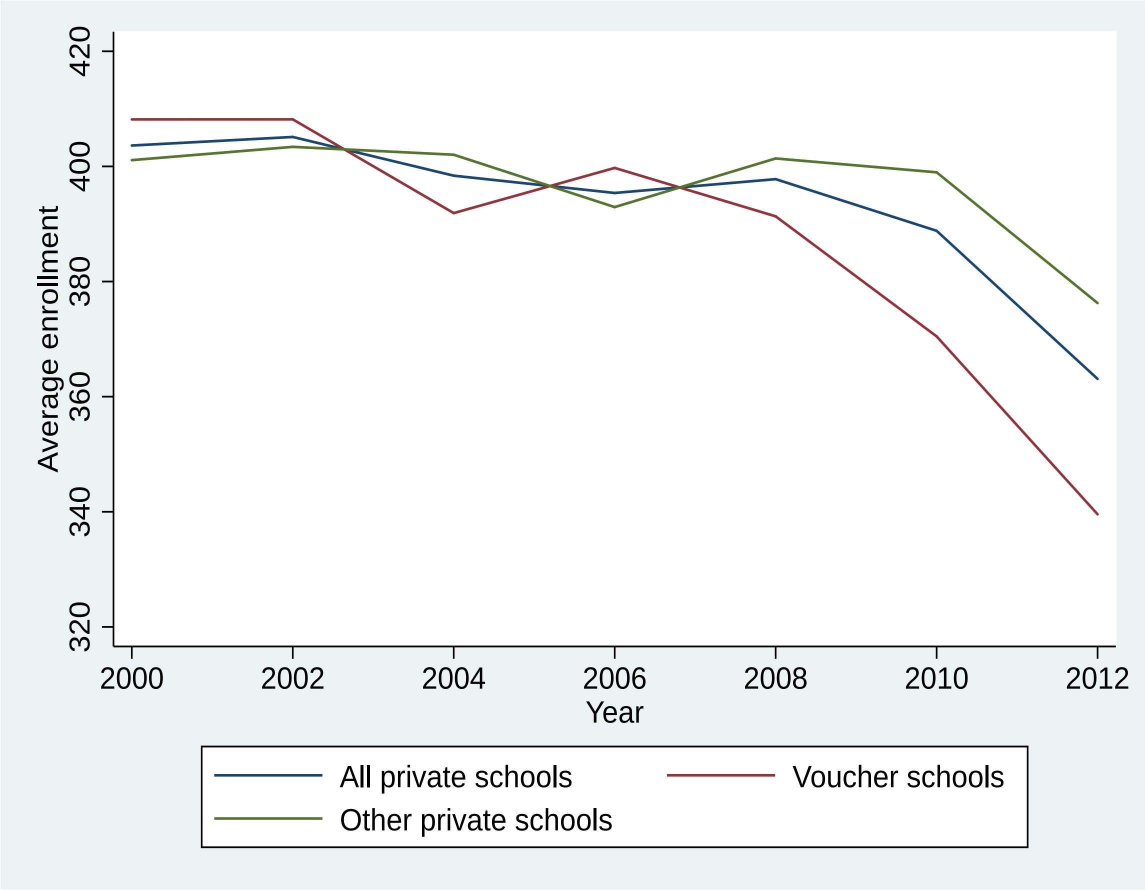
parents wish to enroll their children in these schools despite their negative test score impacts. This may reflect a lack of knowledge about achievement effects, or demand for school characteristics other than academic quality. Parents may be willing to accept achievement losses in exchange for religious instruction or a change in peer environment, for example. 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 ongoing work.

Figure 1: Louisiana Scholarship Program Students and Schools



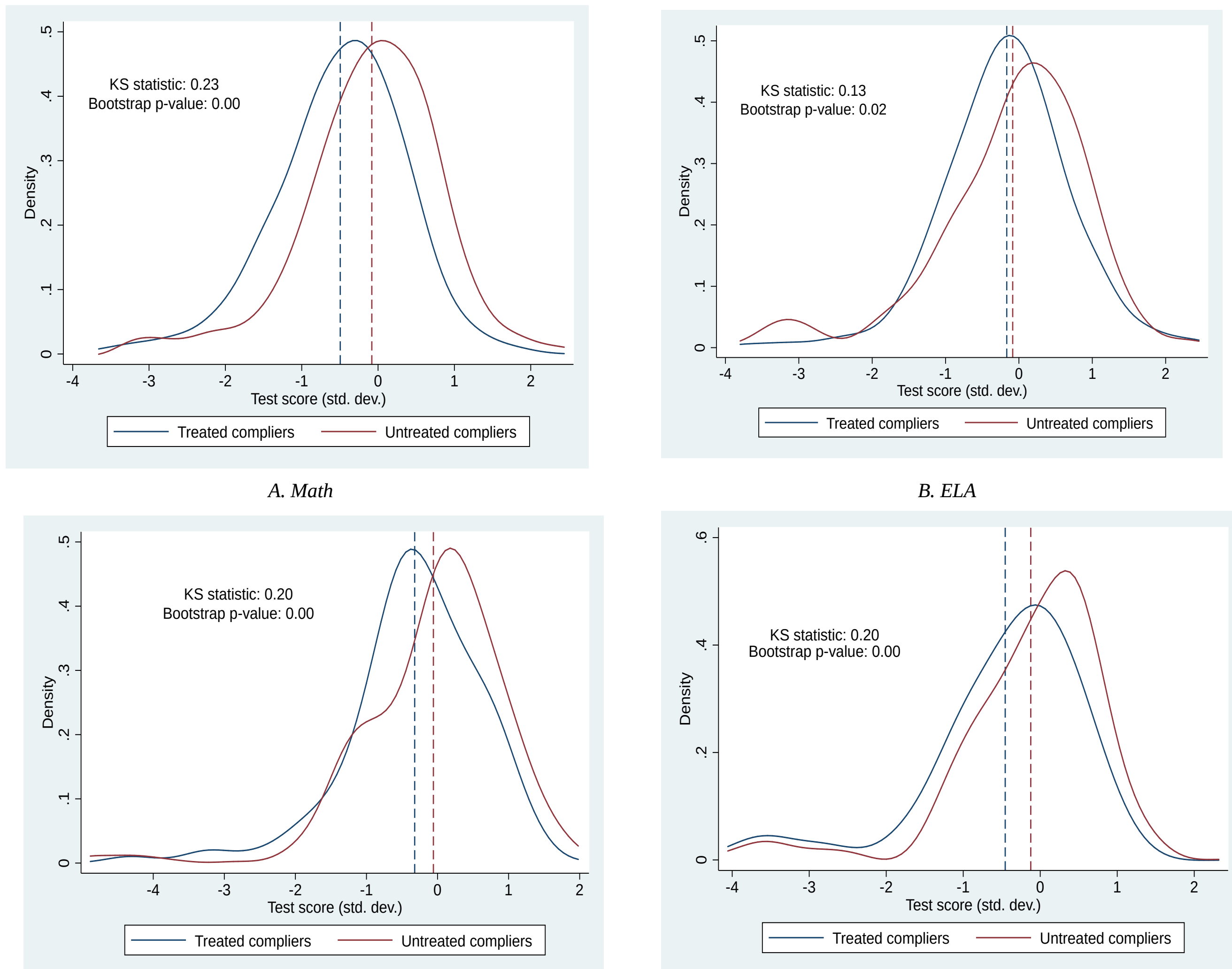
Notes: This figure plots the number of schools participating in the Louisiana Scholarship Program (LSP; left axis) and the number of students applying for and receiving LSP vouchers (right axis). The blue line shows the total number of schools by year, and the red line shows the number of schools in New Orleans. The green line shows the number of applicants, and the orange line shows the number of vouchers awarded. The vertical dashed line indicates the 2011-2012 school year.

Figure 2: Enrollment Trends in Louisiana Private Schools



Notes: This figure plots average annual enrollment for private schools in Louisiana. Enrollment is measured from 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 3. Test Score Distributions for Voucher Compliers



A. Math

B. ELA

C. Science

D. Social Studies

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 dashed lines indicate mean potential outcomes. KS statistics are maximum differences in complier CDFs. The bootstrap procedure used to test distributional equality is described in Appendix B.

Table 1. Descriptive Statistics for Students

			Louisiana Scholarship Program	
	Louisiana	RSD	Applicants	Enrollees
	(1)	(2)	(3)	(4)
Female	0.487	0.473	0.489	0.539
Black	0.451	0.939	0.861	0.805
Hispanic	0.044	0.031	0.031	0.039
White	0.468	0.010	0.086	0.131
NSECD	-	-	0.004	0.006
Household income: Mean	-	-	15,471	17,400
25th percentile			1,300	1,452
Median			12,000	15,000
75th percentile			24,781	28,032
N	715,012	14,689	3,723	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 enrollees.

Table 2: Descriptive Statistics for Private Schools

	All Louisiana private schools			Matched city sample		
	LSP voucher schools	Oversubscribed LSP schools	Other private schools	LSP voucher schools	Oversubscribed LSP schools	Other private schools
	(1)	(2)	(3)	(4)	(5)	(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
N	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.

Table 3. Covariate Balance

		Offer differential	
	Non-offered mean	All applicants	With followup
	(1)	(2)	(3)
Female	0.474	0.012	0.008
		(0.033)	(0.035)
Black	0.900	-0.034*	-0.028
		(0.021)	(0.022)
Hispanic	0.030	0.003	0.001
		(0.012)	(0.013)
White	0.050	0.019	0.018
		(0.015)	(0.016)
NSECD	0.004	-0.001	-0.002
		(0.006)	(0.006)
Household income	15,410	1,636	1,025
		(1097)	(1118)
Income below p_{25}	0.254	-0.007	0.000
		(0.029)	(0.030)
Income below p_{50}	0.503	-0.030	-0.017
		(0.035)	(0.036)
Income below p_{75}	0.753	-0.048	-0.028
		(0.034)	(0.035)
Joint p-value	-	0.659	0.932
N		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 25th, 50th and 75th 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.
* significant at 10%; ** significant at 5%; *** significant at 1%.

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***	-0.281***	-0.413***	-0.386***
	(0.029)	(0.061)	(0.091)	(0.066)
N		1247		
ELA	0.679***	-0.055	-0.081	-0.120**
	(0.029)	(0.053)	(0.079)	(0.056)
N		1248		
Science	0.689***	-0.181***	-0.263***	-0.282***
	(0.030)	(0.066)	(0.095)	(0.065)
N		1221		
Social studies	0.690***	-0.229***	-0.331***	-0.270***
	(0.030)	(0.060)	(0.089)	(0.059)
N		1220		

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 a first-choice offer 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.

* significant at 10%; ** significant at 5%; *** significant at 1%.

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***	-0.216***	-0.067***	-0.012
		(0.045)	(0.047)	(0.024)	(0.011)
	CCM	[0.802]	[0.567]	[0.090]	[0.017]
	N		1214		
ELA		-0.022	-0.107**	-0.032	0.002
		(0.034)	(0.047)	(0.031)	(0.011)
	CCM	[0.844]	[0.563]	[0.100]	[0.009]
	N		1222		
Science		-0.035	-0.153***	-0.040**	-0.001
		(0.047)	(0.049)	(0.018)	(0.004)
	CCM	[0.810]	[0.468]	[0.062]	[0.003]
	N		1211		
Social studies		-0.096**	-0.160***	-0.026	-0.004
		(0.041)	(0.045)	(0.020)	(0.003)
	CCM	[0.759]	[0.513]	[0.044]	[0.004]
	N		1209		
Qualify for promotion (4th and 8th grade)			-0.284***		
			(0.086)		
	CCM		[0.786]		
	N		347		

Notes: This table reports 2SLS estimates of the effects of attendance at Louisiana Scholarship Program (LSP) schools on 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 4th and 8th 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.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Voucher Effects by Subgroup

	By family income ($1,000s)		By location		By Catholic affiliation		By grade	
	Main effect	Interaction	New Orleans/ Baton Rouge	Other	Catholic	Not Catholic	3rd-5th	6th-8th
Subject	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Math	-0.413***	-0.002	-0.276	-0.436***	-0.462***	-0.286***	-0.631***	-0.207*
	(0.093)	(0.005)	(0.284)	(0.095)	(0.144)	(0.104)	(0.140)	(0.110)
N	1247		133	1114	643	471	664	583
P- value	0.636		0.593		0.319		0.016	
ELA	-0.078	-0.001	-0.034	-0.086	-0.090	-0.034	-0.301**	0.135*
	(0.082)	(0.004)	(0.259)	(0.083)	(0.119)	(0.121)	(0.119)	(0.080)
N	1248		133	1115	643	472	664	584
P- value	0.787		0.847		0.747		0.002	
Science	-0.266***	0.002	-0.412	-0.242**	-0.222	-0.238	-0.396***	-0.132
	(0.096)	(0.005)	(0.298)	(0.099)	(0.135)	(0.148)	(0.119)	(0.137)
N	1221		132	1089	630	463	656	565
P- value	0.708		0.588		0.936		0.145	
Social studies	-0.338***	0.003	-0.542**	-0.301***	-0.470***	-0.105	-0.387***	-0.276**
	(0.091)	(0.005)	(0.268)	(0.092)	(0.135)	(0.106)	(0.131)	(0.122)
N	1220		132	1088	629	463	656	564
P- value	0.000		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) show interact LSP participation with family income. Income is de-meaned in the estimation sample, so that main effects are at the mean. Column (3) shows effects for students in New Orleans and Baton Rougue, 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 details on the 2SLS sample and 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.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7. Robustness to Adjustments for Differential Attrition

	Full sample			Without imbalanced risksets			Bounds	
Subject	Non-offered followup rate (1)	Offer differential (2)	2SLS estimate (3)	Non-offered followup rate (4)	Offer differential (5)	2SLS estimate (6)	Lower bound (7)	Upper bound (8)
Math	0.856	0.079***	-0.413***	0.908	0.017	-0.397***	-0.494***	-0.178**
			(0.091)		(0.013)	(0.099)	(0.091)	(0.091)
N		1412	1247		1059	962	1412	
ELA	0.857	0.078***	-0.081	0.905	0.019	-0.098	-0.208***	0.101
		(0.015)	(0.079)		(0.013)	(0.095)	(0.080)	(0.087)
N		1412	1248		1059	958	1412	
Science	0.836	0.078***	-0.263***	0.890	0.006	-0.272***	-0.362***	-0.016
		(0.016)	(0.095)		(0.015)	(0.104)	(0.096)	(0.097)
N		1412	1220		1059	942	1412	
Social studies	0.835	0.079***	-0.331***	0.888	0.008	-0.362***	-0.404***	-0.032
		(0.016)	(0.089)		(0.015)	(0.112)	(0.104)	(0.102)
N		1412	1221		1059	941	1412	

Notes: This table explores the robustness of estimated voucher effects on test scores to adjustments for differential attrition between offered and non-offered students. Column (1) shows the fraction of non-offered applicants with followup test scores. Column (2) shows coefficients from regressions of a followup 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 followup 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 Appendix C. Standard errors, clustered by risk set, are in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

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	-0.41σ***	Public	Income < 2.5 x FPL, low-performing school	Min. of tuition and public PPE	No	Yes	Yes
DC Opportunity Scholarship Program (Washington, DC)	Wolf et al. (2007)	0.13σ*[a]	Public	Income < 1.85 x FPL	Min. of tuition and $7,500 (2004)	Yes[b]	Yes	Yes
Parents Advancing Choice in Education (Dayton, OH)	Howell et al. (2002)	0.08σ[c]	Private	Income < 2 x FPL	Min. of 0.6 x tuition and $1,200 (1998)	Yes	No	Yes
School Choice Scholarships Foundation (New York, NY)	Howell et al. (2002)	0.08σ[c]	Private	Income < 1.3 x FPL	$1,400 (1997)	Yes	No	Yes
Washington Scholarship Fund (Washington, DC)	Howell et al. (2002)	-0.02σ[c]	Private	Income < 2.7 x FPL	Min. of 0.6 x tuition and $1,700 (1998)	Yes	No	Yes
Milwaukee Parental Choice Program (Milwaukee, WI)	Rouse (1998)	0.12σ***[d]	Public	Income < 1.75 x FPL	Public PPE[e]	No[e]	Yes	No[e]

Notes: This table compares program characteristics and achievement effects for school voucher programs. 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.

[a] ITT estimate from Table 4-1 is scaled by first stage effect from Table 2-5.

[b] Footnote 4 suggests that families rarely paid out of pocket when tuition exceeded the voucher amount.

[c] ITT estimates from Table 4 are scaled by baseline math standard deviations from Table 3 and first stage effects from Table 6.

[d] This is an annual gain estimate from a student fixed effects specification pooling data for four years (Table VI, column (2)).

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

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9. Voucher Effects by Experience with the Program

	By year school entered program		By student application year (OLS)		By voucher enrollment share	
	In 2012	Before 2012	2008-2011	2012	Main effect	interaction
Subject	(1)	(2)	(3)	(4)	(5)	(6)
Math	-0.410***	-0.425**	-0.350***	-0.442***	-0.347**	-0.434***
	(0.103)	(0.174)	(0.095)	(0.050)	(0.158)	(0.100)
N	757	490	615	3261	540	572
P- value	0.942		0.389		0.641	
ELA	-0.078	-0.083	-0.185*	-0.165***	-0.100	-0.030
	(0.100)	(0.131)	(0.110)	(0.040)	(0.127)	(0.114)
N	758	490	616	3259	540	573
P- value	0.978		0.865		0.682	
Science	-0.291**	-0.217	-0.515***	-0.286***	-0.249*	-0.219
	(0.114)	(0.174)	(0.115)	(0.041)	(0.131)	(0.153)
N	739	482	613	3189	533	558
P- value	0.723		0.060		0.882	
Social studies	-0.354***	-0.291*	-0.423***	-0.295***	-0.290**	-0.338**
	(0.110)	(0.157)	(0.128)	(0.041)	(0.124)	(0.150)
N	738	482	613	3189	532	558
P- 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 samples 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 interacting LSP participation with the share of enrolled students receiving LSP vouchers, instrumenting with the interaction of the voucher enrollment share and the lottery offer. The voucher share is jackknifed to leave out a student's own contribution, and it is de-meaned in the estimation sample, so that main effects are at the mean. See notes to Table 4 for details on the 2SLS sample and 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.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10. Characteristics of Treatment and Fallback Schools for Voucher Applicants

	All applicants		Voucher compliers	
	Offered	Not offered	Offered	Not offered
	(1)	(2)	(3)	(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.

Table 11. Voucher Effects by Measures of School Quality

	By change in log enrollment		By tuition ($1,000s)		By performance sanction	
	Main effect	Interaction	Main effect	Interaction	Sanctioned	Not sanctioned
Subject	(1)	(2)	(3)	(4)	(5)	(6)
Math	-0.352***	-0.092	-0.355***	0.263**	-0.384***	-0.452***
	(0.098)	(0.223)	(0.091)	(0.121)	(0.118)	(0.139)
N	938		1050		672	575
P- value	0.679		0.030		0.709	
ELA	-0.039	-0.015	-0.037	0.167	-0.129	-0.023
	(0.091)	(0.332)	(0.087)	(0.106)	(0.113)	(0.111)
N	939		1051		673	575
P- value	0.963		0.114		0.501	
Science	-0.214*	-0.397	-0.196**	0.118	-0.277*	-0.248**
	(0.111)	(0.276)	(0.100)	(0.113)	(0.149)	(0.113)
N	918		1031		653	568
P- value	0.150		0.299		0.876	
Social studies	-0.273***	0.186	-0.265***	0.170	-0.322***	-0.341***
	(0.104)	(0.313)	(0.090)	(0.121)	(0.125)	(0.129)
N	917		1030		653	567
P- 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 de-meaned in the estimation sample, so that main effects are at the mean. See notes to Table 4 for details on the 2SLS sample and 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.

* significant at 10%; ** significant at 5%; *** significant at 1%.

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Appendix A: Data

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.

A.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 includes 16,739 application records with information on a ranked list 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, and 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 eligibility for the LSP.

We extract first-time 2012-2013 applicants to grades 3-8 from the raw data. 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 an indicator for a voucher offer at the first choice school, and risk sets are coded as interactions of application grade and first choice school.

A.3 Test score data

The second data source in our analysis 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 recording 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 *sitecd* coding the testing location; this identifier is also used in most public data files provided by the Louisiana Department of Education. We use the *sitecd* 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 a matching record in the test score file.

A.4 Private School Universe Survey

Characteristics for Louisiana private schools are measured from the Private School Universe Survey (PSS). The PSS is a census of all US private schools conducted every two years by the National Center for Education Statistics. PSS data files are available from 1990 through 2012. Key variables include total enrollment and enrollment shares by race, number of teachers, indicators for various religious affiliations, 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 of participating LSP private schools provided by the Louisiana Department of Education 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" when 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.

A.5 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 for 116 of 124 voucher schools (94 percent) and 216 of 235 non-voucher schools (92 percent).

Appendix B: 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 $Y_i = Y_i(P_i)$. 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

$$\frac{E\left[g(Y_i, X_i)P_i|Z_i = 1\right] - E\left[g(Y_i, X_i)P_i|Z_i = 0\right]}{E\left[P_i|Z_i = 1\right] - E\left[P_i|Z_i = 0\right]} = E\left[g\left(Y_i(1), X_i\right)|P_i(1) > P_i(0)\right], \tag{5}$$

$$\frac{E\left[g(Y_i, X_i)(1 - P_i)|Z_i = 1\right] - E\left[g(Y_i, X_i)(1 - P_i)|Z_i = 0\right]}{E\left[1 - P_i|Z_i = 1\right] - E\left[1 - P_i|Z_i = 0\right]} = E\left[g\left(Y_i(0), X_i\right)|P_i(1) > P_i(0)\right]. \tag{6}$$

The left-hand side of (5) 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 (6) 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 (5) and (6) 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 distributions. In practice our IV models control for lottery risk set indicators; the arguments in Angrist and Imbens (1995) imply that 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 (6). Counterfactual school characteristics in Table 9 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 (5) and (6). 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 σ_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 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 (5) and (6). This yields estimates of the first two noncentral moments of $Y_i(1)$ and $Y_i(0)$ for compliers, which are then used to construct an estimate of σ_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 $p_z \equiv Pr\left[Z_i = 1\right]$.

The number of treated compliers is the fraction of lottery winners times the population share of compliers (equal to the first stage coefficient π) 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 in the empirical lottery offer probability and first stage coefficient to 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 drawing samples with replacement stratified by risk set, 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 (5) and (6) at the same 100 points used in the bootstrap tests for distributional equality.

Appendix C: Bounds on Voucher Effects

This appendix describes 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 B 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:

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

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:

1. Always takers: $P_i(1) = P_i(0) = 1$
2. Never takers: $P_i(1) = P_i(0) = 0$
3. Always attriters: $P_i(1) = P_i(0) = a$
4. Compliers: $P_i(1) = 1$, $P_i(0) = 0$
5. 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 π^g denote population shares of the five groups for $g \in \{at, nt, aa, c, ar\}$. Likewise, let μ_p^g denote the mean of potential outcome $Y_i(p)$ for group g and $p \in \{0, 1\}$. The average causal effect of voucher receipt for compliers is $LATE = \mu_1^c - \mu_0^c$. To bound this quantity, first note that the population shares of each group are identified since

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

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

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

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

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

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

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

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

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

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

It is straightforward to show that the moments in this equation are equivalent to those used in equation (6) when $g(Y_i, X_i) = Y_i$, substituting $1\left\{P_i = 0\right\}$ for $(1 - P_i)$ since P_i is now an unordered treatment.

The presence of at-risk students prevents us from backing out μ_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

$$\frac{E\left[1\left\{Y_i \le y\right\}1\left\{P_i = 1\right\}|Z_i = 1\right] - E\left[1\left\{Y_i \le y\right\}1\left\{P_i = 1\right\}|Z_i = 0\right]}{E\left[1\left\{P_i = 1\right\}|Z_i = 1\right] - E\left[1\left\{P_i = 1\right\}|Z_i = 0\right]} = Pr\left[Y_i(1) \le y|P_i(1) \neq P_i(0)\right] \qquad (7)$$

$$\equiv F_1(y).$$

This result follows by applying equation (5).

The minimum possible value of μ_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

$$\mu_1^c \ge E\left[Y_i(1)|Y_i(1) \le F_1^{-1}\left(\tfrac{\pi^c}{\pi^c+\pi^{ar}}\right), P_i(1) \neq P_i(0)\right]$$

$$= \frac{E\left[Y_i1\left\{Y_i \le F_1^{-1}\left(\tfrac{\pi^c}{\pi^c+\pi^{ar}}\right)\right\}1\left\{P_i = 1\right\}|Z_i = 1\right] - E\left[Y_i1\left\{Y_i \le F_1^{-1}\left(\tfrac{\pi^c}{\pi^c+\pi^{ar}}\right)\right\}1\left\{P_i = 1\right\}|Z_i = 0\right]}{E\left[1\left\{P_i = 0\right\}|Z_i = 0\right] - E\left[1\left\{P_i = 0\right\}|Z_i = 1\right]}$$

$$\equiv \mu_{min},$$

where the second line follows from another application of equation (5), rescaling appropriately by the probability that the event $\left\{Y_i \le F_1^{-1}\left(\tfrac{\pi^c}{\pi^c+\pi^{ar}}\right)\right\}$ occurs in the mixture of treated compliers and at-risk students. Similarly, an upper bound for the treated complier mean is

$$\mu_1^c \le E\left[Y_i(1)|Y_i(1) \ge F_1^{-1}\left(\tfrac{\pi^{ar}}{\pi^c+\pi^{ar}}\right), P_i(1) \neq P_i(0)\right]$$

$$= \frac{E\left[Y_i1\left\{Y_i \ge F_1^{-1}\left(\tfrac{\pi^{ar}}{\pi^c+\pi^{ar}}\right)\right\}1\left\{P_i = 1\right\}|Z_i = 1\right] - E\left[Y_i1\left\{Y_i \ge F_1^{-1}\left(\tfrac{\pi^{ar}}{\pi^c+\pi^{ar}}\right)\right\}1\left\{P_i = 1\right\}|Z_i = 0\right]}{E\left[1\left\{P_i = 0\right\}|Z_i = 0\right] - E\left[1\left\{P_i = 0\right\}|Z_i = 1\right]}$$

$$\equiv \mu_{max}.$$

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:

1. Estimate the probabilities π^{ar} and π^c as minus the shifts in the probability of attrition and non-participation induced by the lottery offer.

2. Estimate the CDF of $Y_i(1)$ for the mixture of compliers and at-risk students using equation (7).

3. 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 $F_1(y)$.

4. Use the expressions above to estimate μ_{max} and μ_{min}.

5. Estimate μ_0^c using equation (6), setting $g(Y_i, X_i) = Y_i$ and substituting $1\{P_i = 0\}$ for $(1 - P_i)$.

6. Construct bounds for LATE using the estimates of μ_{max}, μ_{min} and μ_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.