

An Impact Analysis of North Carolina's Opportunity Scholarship Program on Student Achievement

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Abstract

The North Carolina Opportunity Scholarship Program is a statewide private school voucher program, enacted in 2013 and launched in 2014 to provide state-funded vouchers worth up to \$4,200 to eligible students entering Kindergarten through twelfth grade. Because public schools administer the criterion-referenced state test, whereas private schools administer any nationally normed test of their choosing, the research team recruited 698 students from the public and private school sectors to take a common third-party nationally-normed standardized test in spring 2017. Relying on baseline achievement and demographic data from the North Carolina Department of Public Instruction, a matching approach is utilized to maximize comparability between the public and private school student samples. Our preferred specification utilizes observations from 245 public and private students, controlling for a host of covariates, revealing first year impact estimates of .36 SD in math and .44 SD in language. Policy implications are discussed.

Keywords: school choice; education policy; vouchers; academic achievement; private schooling

An Impact Analysis of North Carolina’s Opportunity Scholarship Program on Student Achievement

Established by North Carolina General Statute 115C-562 in 2013 and launched in the 2014-15 school year, the North Carolina Opportunity Scholarship Program provides state-funded vouchers of up to \$4,200 per year for eligible students in kindergarten through twelfth grade to attend participating private schools. The program is funded by an annual appropriation established during the biennial state budget process and administered by the North Carolina State Education Assistance Authority (SEAA). Since the inception of the program in 2014-15, nearly \$53 million dollars have been disbursed in scholarship funds.

This analysis focuses on identifying the impact of the North Carolina Opportunity Scholarship Program on participating students’ academic achievement in math and reading. Although the legislation that established the program calls for an evaluation of the “learning gains or losses of students receiving scholarship grants,” as compared to similar students in public schools, no such evaluation has been conducted until now. Two primary challenges have inhibited an evaluation to date. First, the accountability requirements associated with the program require participating non-public schools to administer and submit scores from any nationally-normed standardized test of their choosing, whereas comparable public school students take the criterion-referenced state test, the North Carolina End of Grade exam. Thus, participating students do not have scores on a common metric that could be pooled for comparison to students in the public school sector. Second, although the establishing legislation calls for an evaluation of the program, it doesn’t incentivize applicants to participate in one, for example as a condition for applying to the program. This introduces recruitment challenges with regard to the collection of original test score data.

In the spring of 2017, this research team traveled to the four regions of North Carolina with the highest number of Opportunity Scholarship users. Relying on a volunteer sample of students in the public and private school sectors in those regions, we administered Form E of the Iowa Test of Basic Skills—a nationally-normed standardized assessment of math and reading achievement. Using inverse propensity weights in a regression model to compare similar public school students to those students attending private schools with an Opportunity Scholarship, we generate impact estimates in mathematics, reading, and language. Although we do not claim to present causal estimates of the program impact, we can account for observed differences between the two groups in terms of demographic characteristics, disciplinary records, and prior test scores. The effect sizes observed are positive, large, and statistically significant, ranging from .25 to .49 standard deviations (henceforth SD).

Background

Program description. North Carolina’s Opportunity Scholarship Program joins a wave of recently-enacted private school choice programs that feature relatively broad eligibility criteria, expanding access to an entire state instead of a single city such as earlier programs established in Cleveland, OH; Milwaukee, WI; or Washington D.C. Compared to similar programs in other states, students participating in North Carolina’s voucher program are eligible to receive a relatively modest scholarship of up to \$4,200 per year for tuition and fees for books, transportation, or school equipment (Table 1). Eligibility is determined by a two-part test. First, the student must meet at least one of the following initial criteria: students must have been enrolled in a North Carolina public school during the previous semester; students must be entering kindergarten or first grade; students must have received a scholarship for the previous semester; students must be in foster care or recently adopted; or students must have a parent or

legal guardian who is on full-time active duty in the military. Second, students must reside in a household with an income level that doesn't exceed 133 percent of the amount required for the federal free or reduced-price lunch program.

« Table One About Here »

The youngest of the four programs highlighted here, North Carolina's Opportunity Scholarship program is relatively small. In 2016-17, 5,624 students used a state-funded voucher to attend a private school but changes to the statute in April 2016 added eligibility pathways for children in military families and the 2016 Appropriations Act called for increasing funds by at least ten million dollars every year for ten years, which will likely drive up enrollment numbers in future years. Of the four statewide programs highlighted in Table 1, Florida's program is the largest with 98,936 participants in 2016-17, followed by Indiana's program with 34,299 participants. A notable feature of Florida's program that has likely contributed to recent enrollment growth in the program is that students are no longer required to have been enrolled in a public school before applying for a scholarship. Eligibility is determined solely by household income or a child's foster care status. The second-largest program, Indiana's Choice Scholarship Program stands out for having the highest number of eligibility pathways by which students can qualify for the program. With eight pathways in total, Hoosier students can qualify through a sibling, by applying from a failing public school, or numerous other routes. None of these pathways allow a student's household income to exceed 200% of federal free/ reduced price lunch program limit, however, which ensures the program retains its means-tested focus.

Although currently in third place in terms of student participants (6,995 in 2016-17), Louisiana's program has the strictest eligibility criteria. Students older than kindergarten age must meet the income requirement *and* be currently attending an underperforming public school.

Thus, it is reasonable to expect enrollment in North Carolina's program to overtake Louisiana's program in the coming years based on eligibility criteria alone.

Evaluation mandate. The legislation that created the Opportunity Scholarship Program calls for an evaluation that compares the learning gains or losses of participating students on a statewide basis (North Carolina General Statute 115C- 562, 2013). One way to accomplish this task would be to rely on annual test results that are submitted annually to the SEAA and corresponding records of comparable public school students who took the same assessment. This is how researchers have evaluated Indiana's Choice Scholarship Program (Waddington & Berends, 2017), Ohio's EdChoice Program (Figlio & Karbownik, 2016), and Louisiana's Opportunity Scholarship Program (Abdulkadiroglu, Pathak, & Walters, 2018; Mills & Wolf, 2017), where students in both the public and private school sector took the state assessment. However, participating schools in North Carolina's Opportunity Scholarship program are not required to administer the state test or any other common assessment; rather, they are free to administer any nationally-normed standardized test of their choosing. This has resulted in schools' use of a diverse set of tests from different publishers, which means student performance cannot be easily compared within the non-public school sector or between the public and non-public school sectors (Table 2).

<< Table Two About Here >>

Because there is no common metric on which to judge student performance, a second way to accomplish the task set forth by the legislation would be to invite students to participate in new data collection efforts, for example by incentivizing or requiring their participation in an evaluation at the time of application to the program or by reaching out to scholarship recipients and inviting them to attend an optional test administration session. In such a scenario, researchers

could administer a common standardized test to a sample of OS students and comparable public school students with similar socioeconomic backgrounds. This approach has been used to evaluate the Milwaukee Parental Choice Program (Witte, Carlson, Cowen, Fleming, & Wolf, 2012), the Charlotte Scholarship Fund (Greene, 2001), the Indianapolis Voucher Program (Weinschrott & Kilgore, 1998) and the Washington D.C. Opportunity Scholarship Program (Wolf et al., 2013) and it is the approach adopted here to evaluate the academic impact of North Carolina's Opportunity Scholarship Program.

Literature Review

Since the establishment of the nation's first private school choice program in Milwaukee in 1990, researchers have conducted numerous lottery-based studies to quantify the achievement impact of these programs, which have been both publicly-funded (such as the voucher programs in Milwaukee, WI; Cleveland, OH; and Washington D.C.) and privately-funded (such as the New York City School Choice Scholarships Foundation Program and the Charlotte Scholarship Fund). In general, these lottery-based studies have revealed small positive impacts overall, with larger positive impacts observed for subgroups of interest, such as African American students, students of low socioeconomic status, or students who were initially low-achieving (Table 3).

« Table Three About Here »

Among the 15 random-assignment studies detailed in Table 1, eight have revealed positive impacts in math or reading overall or for at least one subgroup of interest (Barnard, Frangakis, Jin, & Rubin, 2003; Cowen, 2008; Greene, 2001; Greene, Peterson & Du, 1999; Howell, Wolf, Campbell, & Peterson, 2002; Jin, Barnard, & Rubin, 2010; Rouse, 1998), two studies revealed null impacts (Bitler, Domina, Penner, & Hoynes, 2014; Krueger & Zhu, 2004), and three studies revealed negative impacts for the overall sample or for at least one subgroup

(Abdulkadiroglu, Pathak, and Walters, 2018; Dynarski, Rui, Webber Gutmann & Bachman, 2017; Mills & Wolf, 2017). Although the weight of the evidence, therefore, points towards a positive impact associated with voucher usage, the three most recent studies all reveal negative impacts. This raises interesting questions about the characteristics of contemporary programs and the contexts in which they've been studied.

In Washington D.C., for example, a 2017 lottery analysis of the D.C. Opportunity Scholarship Program reveals null impacts on reading scores and a statistically significant negative impact of 7.3 percentile points on the math achievement of students who used a voucher (Dynarski, Rui, Webber Gutmann & Bachman, 2017). Given the dramatic spread of charter schooling in the nation's capital, it is important to acknowledge that 42 percent of students in the control group were attending a charter school. Thus, the negative impact of private school voucher usage in this study should be interpreted as being relative to the impact of attending a traditional public *or* a charter school whereas in earlier studies detailed in Table 2, the counterfactual was almost always a traditional public school.

The other two negative studies in Table 2 are evaluations of the Louisiana Scholarship Program. Given that statewide private school choice programs are a newer policy development than the highly targeted citywide programs that predominated the private school choice landscape throughout the 1990s and early 2000s, it is not surprising that there have been fewer lottery-based studies of such programs. Nonetheless, the evidence marshalled to date has raised concerns among many in the school choice community because of the unprecedented and large negative impacts associated with voucher usage in the Pelican State.

Abdulkadiroglu, Pathak, and Walters (2018) leverage Louisiana's oversubscribed application lottery to experimentally examine the impact of the Louisiana Scholarship Program

in the first year after its statewide expansion in 2012, finding large and statistically significant negative impacts across all subjects examined. Attending a private school with a voucher reduced student achievement in math by 41 percent of a SD, with reductions of 8, 26, and 33 percent in reading, science, and social studies achievement, respectively. Second year impact estimates by Mills and Wolf (2017) reveal similarly large and statistically significant negative impacts in both English Language Arts and mathematics for students who enrolled in their first-choice private school through the program. Specifically, voucher users score 18 percent of a SD lower in English Language Arts and 34 percent of a SD lower in math after two years in the program. Given these large negative effects, many have questioned what differs about the Louisiana context that might explain the negative impacts. An ancillary analysis by Abdulkadiroglu and colleagues raises questions about the quality of private schools accepting voucher students in Louisiana, as participating schools appear to have been experiencing declining enrollment prior to the passage of the voucher law. Further evaluation work will be necessary to determine how the quality of participating private schools is evolving as the program ages and regulations to address private school quality are implemented (White, 2016).

Two other evaluations of statewide voucher programs have been conducted in recent years that do not rely on a lottery-based design but employ alternative approaches to causal inference that attempt to approximate a gold-standard design. First, Figlio and Karbownik (2016) employ a propensity score matching approach to evaluate Ohio's EdChoice program. Utilizing student records from 2003-04 to 2012-13, Figlio and Karbownik report substantial negative program impacts in both math and English Language Arts, with the largest negative effects observed in math. The authors note that these negative effects cannot be explained by the transition effects of switching to a new private school because they persist over time.

Second, Waddington and Berends (2017) employ a difference-in-differences model with propensity score matched students to evaluate achievement changes for students in grades three through eight in the first four years of the Indiana Choice Scholarship Program. After one year in program, there are no differences in ELA outcomes but math scores decline by 10 percent of a SD, on average, for students who participate in the voucher program. After four years, however, there are no longer any statistically significant differences in either math or ELA outcomes, leading the authors to conclude that the program had a null average impact.

In summary, recent evaluations of the relatively young, statewide voucher programs in Louisiana, Ohio, and Indiana point to potentially substantial negative impacts associated with initial voucher use, contrary to a consistent body of evidence prior to 2017 that showed null to positive impacts associated with voucher usage in the United States. A high-quality analysis of North Carolina's Opportunity Scholarship Program, a close cousin of these three programs, would provide a valuable data point on the impact of contemporary private school choice programs.

Literature on the OS Program. Enacted in 2013 and first implemented in the 2014-15 school year, the North Carolina Opportunity Scholarship program is currently serving its fourth cohort of voucher students. To date, the only evaluation of the program's impact on student achievement is a report by the Children's Law Clinic at Duke Law School (Wettach, 2017). No statistical tests are employed to compare student performance in this report, nor are students assessed using a common test. Instead, the author presents the percentage of voucher students scoring above average on *any* of the standardized tests given in a subset of private schools (i.e., those schools with greater than 25 voucher enrollees) and compares this percentage to the average National Assessment of Educational Progress (NAEP) performance by all low-income

students in the public schools. Vastly different sample sizes in the two groups under comparison raise questions about uneven measurement error in the two sectors being compared. Further, students in these two groups differ in terms of gender, race, age, family background, prior academic achievement, and numerous other background characteristics that are not accounted for with a statistical model of any kind. This is an important omission because descriptive data on program participants demonstrates that voucher students represent a distinct subgroup of students in the state (Egalite, Porter, & Stallings, 2017). For example, voucher students reside in some of the lowest-income households in North Carolina, with an adjusted median household income in 2016-17 of \$16,213 for new voucher recipients and \$15,000 for renewal voucher recipients.¹

Methodology

Recruitment. In Spring 2017, the research team collaborated with the leadership of public schools, non-public schools, and partner organizations in four geographic regions of North Carolina to recruit a volunteer sample of low-income students in grades 4 through 8 who likely qualified for the Opportunity Scholarship Program, which has an eligibility threshold of 133% of the federal free and reduced lunch guideline. Student eligibility for the free and reduced-price lunch program was later confirmed by accessing public school student records made available by the Department of Public Instruction and those students who did not meet the income criteria were excluded from our analyses.

Recruitment on the public school side adhered to the following protocol: We first applied for separate IRB approval in each participating school district, as well as at the university level, then commenced a four-stage approach to recruiting unpaid student volunteers to participate in

¹ Adjusted per-household income is calculated as household income / square root of household size, a commonly used approach to account for household savings resulting from economies of scale and resource-sharing.

data collection. Starting at the district level, we approached the public school superintendents of those geographic regions that featured high numbers of OS students to request permission to recruit volunteers in their schools to serve as our comparison group. Once a cooperation agreement was established with four public school superintendents, we coordinated with the head of testing in each district to identify the highest-poverty public schools in those districts. We next reached out to individual school principals to request their cooperation with the study. Once principals consented, school personnel distributed explanatory letters to families on our behalf, describing the goals of the research project and requesting parental consent for student participation in the research project. Finally, we requested student assent to participate in data collection. Specifically, on the day of testing, students were given a children's version of the parental information letter, written in simple language that explained the purpose of the research, any expected risks or benefits, and notifying them that their participation was voluntary and they were free to opt out at any time.

Recruitment procedures on the private school side adhered to the following protocol: We started by making courtesy calls to the private school associations in the state, notifying them about the project so they would have information at their fingertips to answer questions from private school leaders in their network. We then commenced one-on-one recruitment phone calls with individual school principals and meetings with advocacy groups and legislators on both sides of the issue. The bulk of recruitment support was ultimately provided by the organization, Parents for Educational Freedom in North Carolina, which encouraged private schools to respond to our request to participate in the study and helped schools schedule data collection sessions with us. Parent and student consent procedures followed the protocols already described and were identical for private and public school students.

All test administration was conducted by our research team and by third-party research contractors on school grounds during the school day. The test administrators were all trained in common protocols and ethical considerations to ensure that the testing conditions were consistent across sites. The team arrived at each school site at the start of the school day and coordinated with school personnel to set up the room with testing booklets, scratch paper, and pencils. Parental consent letters were collected prior to our arrival by school personnel. In addition, students were provided with an assent form on the day that further allowed them to opt out of testing, if necessary. Testing sessions took approximately two hours in total. Unfortunately, students with special educational needs were excluded from all data collection efforts as the researchers did not have the manpower to provide special testing accommodations as needed.

School description. In total, we collected test scores from volunteer students in 24 private schools and 14 public schools (Table 4). Sample sizes were larger in the public schools, with a mean of 35 tested students per site and a median of 23 students. On the private school side, there was a mean of 12 tested students per site and a median of seven students. We visited every private school that agreed to be in the study, including a school with just one student volunteer and another with 43 students. Data for 19 private schools were located in federal data files and data for 16 schools were located in state records from the Division of Non Public Education. The reader should note that no descriptive data was located for three private schools in the testing sample so those schools are excluded from the descriptive data presented in Table 4 and the discussion presented in the next section.

<< Table Four About Here >>

Almost all (95 percent) of the private schools in our sample had a library or media center, one indicator of how well resourced a given school is. Approximately half (53 percent) of the

private schools visited were Catholic schools, 26 percent were Christian (no specific denomination), 16 percent were Baptist, and 5 percent were some other religion such as Methodist or Episcopal. The private schools visited had a median enrollment of 225, with a minimum enrollment of 107 and a maximum of 1,402. The majority of the private schools (74 percent) were located in a city or suburb (16 percent), the median pupil teacher ratio was 12, and the median number of full time equivalent teachers was 28. All private schools in the state are required to report which standardized tests they use for accountability purposes to the North Carolina Division of Non Public Education so we also report this information for the schools in our sample. The most commonly used test was the Iowa Test of Basic Skills (used by 44 percent of the schools), followed by the Terra Nova (31 percent) and the Stanford Achievement Test (25 percent).

Data for all 14 of the public schools in our sample were located in federal files. All of the public schools visited were traditional public (93 percent) or magnet schools (7 percent) and all qualified for school wide Title 1, a common proxy for high-poverty schools. This latter selection criteria was by design, to maximize the chance of recruiting comparable low-income matches for the OS students in the treatment condition. The public schools were slightly larger than the private schools visited, with a median enrollment of 650, a minimum of 340 and a maximum of 930. Although half of the public schools in our sample are coded as being located in a rural area, they were all located in the same geographic regions of the state as the private schools visited. This difference in urban locale may also reflect differences in how the Private School Universe Survey and the Public Elementary/Secondary School Universe Survey code urbanity (i.e., the private school survey uses the categories city, suburb, town, rural; whereas the public school survey uses the categories city, large; city, mid-size; city, small; suburb, large; suburb, midsize;

suburb, small; town, fringe; town, distant; town, remote; rural, fringe; rural, distant; rural, remote). Finally, the median public school pupil teacher ratio was 14, and the median number of full time equivalent teachers was 45. All of the public schools administer the North Carolina End of Grade tests.

Estimation strategy. We conduct a propensity score analysis to estimate the impact of the program, a commonly used approach to causal inference in the voucher literature when a lottery study is not possible. Students' observable characteristics are used to predict their likelihood of using a voucher and these propensities are then used to construct estimation weights that result in comparable treatment and control groups. Within-study comparisons are one approach researchers have employed to assess the validity of matching approaches such as the one employed here, with generally encouraging results.

Ackerman and Egalite (2017) leverage data on Florida charter schools between 2001 and 2009 to assess the validity and sensitivity of a commonly-used matching approach in the school choice literature, popularized by the Stanford Center for Research on Education Outcomes (CREDO). One of the primary contributions of the Ackerman and Egalite paper is its test of the sensitivity of results that rely on "virtual control records" that are generated from the records of up to seven comparison group students for every one student in the treatment group. They find no differences in estimates produced by models that feature up to seven-to-one versus one-to-one matching. Second, this analysis assesses the internal validity of the matching approach generally, by comparing the point estimates produced by a matching approach to those produced by an instrumental variables analysis. The authors note that the two sets of results are comparable in both magnitude and direction.

Bifulco (2012) employs an even more rigorous test of the validity of nonexperimental estimators by judging their performance against experimental data from two interdistrict magnet schools. He concludes that the use of pretreatment measures of academic performance is critical, reducing the bias in a nonexperimental approach by 64 to 96 percent. Similarly, within-study comparisons of non-experimental methodological approaches by Angrist, Cohodes, Dynarski, Fullerton, Kane, Pathak, & Walters (2011), Abdulkadiroğlu, Angrist, Cohodes, Dynarski, Fullerton, Kane, & Pathak (2009) and Fortson, Verbitsky-Savitz, Kopa, & Gleason (2012) report promising replication results when the non-experimental method accounts for prior test scores and draws the comparison group from the same traditional public schools previously attended by the students in the treatment group.

Data and Methodology

Sample. In total, there were 698 low-income students tested in spring 2017: 297 in private schools and 401 in public schools (Table 5). Student answer sheets were machine-scored by Houghton Mifflin Harcourt and the scores were returned to us in a digital format. To construct the final analysis sample, we merge these records with student data from the North Carolina Department of Public Instruction (DPI). This allows us to add rich demographic data and prior test scores to each student record. Because we have to merge by student name, 124 observations are dropped at this stage, either because a reliable match could not be made with DPI data (e.g., the student reported a nickname instead of their legal name on the ITBS answer sheet), or because a name matched with multiple records for that grade level in the DPI data (e.g., in the case of students with very common first and last names), creating uncertainty as to the true match. In both of these scenarios, we opt for the conservative approach of dropping records when we cannot be sure of an exact match. A further eight students are dropped because

they were mistakenly given the wrong test for their grade level in 2017 (i.e., some 4th graders took the 5th grade test and vice versa). An additional seven students opted not to complete the tests on testing day and are dropped from the sample as a result. To ensure comparability between OS students and students in public schools, 58 public school students who were not listed as free and reduced lunch eligible in the 2016 DPI data are removed from the analysis, along with three students who could not be verified in the OS records. These final two screens ensure that every student in both public and private schools in our sample has been identified as low-income. The final student count in the analytic dataset is 497 students, with 245 in private schools and 252 in public schools.

<< Table Five About Here >>

Once the analytic dataset has been established, we next define the matching algorithm. Every public and private school student in our sample has already been verified as being low-income so our primary matching covariates are End of Grade (EOG) standardized test scores from 2016. As detailed in the description of the analytic approach below, we also account for demographic differences between the two groups.

EOG scores. All public school students have EOG records from 2016, but only 89 out of 245 private school students have EOG records from 2016. Of these, 29 (i.e., 12%) attended a public school in 2016 but the number of membership days suggest they did not attend for the entire school year, which may explain why they are missing EOG exam scores from spring 2016. Another 31 (i.e., 13%) have EOG records from 2015, suggesting that their missing 2016 EOG records are due to participation in the OS program for two years rather than just one. In the latter scenario, we run the analyses both with and without these students in order to cleanly estimate

one-year impacts, as compared to running models that would estimate a mixture of one and two-year impacts.

Outcome measures. The Iowa Test of Basic Skills (ITBS) is a standardized, nationally norm-referenced test. This evaluation relies upon Form E, which is the ITBS Survey Battery, a shorter version of the ITBS Complete Battery, which would take several days to administer. The survey battery consists of three 30-minute tests in the areas of reading, language, and math, which we administered in a single testing session. We relied upon the tests at Levels 10 through 14, which correspond to grade levels four through eight. Students were administered the test that matched their current grade level. Students' answer booklets were machine-scored by the test publishers and the results were made available to the research team for analysis.²

Analytical approach. We use inverse propensity weights (IPW) in a regression model to estimate the effect of private school attendance through the Opportunity Scholarship Program on students' academic performance. IPW adjusts for differences between the treatment and control groups by using some form of the inverse of the predicted probability of treatment as a weight in a statistical model. IPW is one of a general class of techniques commonly referred to as matching or propensity score analysis. Propensity score analysis relies on two assumptions (Rosenbaum & Rubin, 1983):

1. The covariates used to generate the propensities are such that the potential outcomes under treatment and control are independent of treatment status, conditional on the covariates.

² Specific information on the validity and reliability of the Iowa tests is available here: <https://itp.education.uiowa.edu/ia/documents/ITBS-Research-Guide.pdf>

2. There are a sufficient number of observations with similar propensities for both treatment and control.

The first assumption cannot be tested empirically and is instead a matter of argument. In other words, reasonable people can disagree as to whether a particular propensity score analysis has used the relevant set of covariates in the propensity model. In the next section, therefore, we present an argument in favor of the covariates selected in this analysis.

We rely upon those variables that are expected to differ between voucher and non-voucher students and that are expected to influence student outcomes on a standardized assessment. These include gender, race/ethnicity, school grade level in 2017, and whether the student had any disciplinary incidents in public school in 2016. In addition, we match on state EOG math and reading scores from 2016, which are vertically scaled and thus appropriate for matching in this manner. The charter school literature shows that matching on gender, race, cohort, and pretreatment test scores typically yields treatment effect estimates similar to analyses using lotteries (Dobbie & Fryer, 2017). Of these variables, pretreatment test scores are the most important (Bifulco, 2012). We note that by limiting the public school sample to students who qualify for free or reduced lunch, we have exact matched both groups by low income status.

We tested three versions of the logistic regression model predicting voucher receipt. The base model included the covariates listed above and squared terms for math and reading EOG scores. The second model added interactions between the grade indicators and EOG scores, and the third included interactions between the grade indicators and the squared EOG terms. For each analysis we chose the model specification that yielded the lowest average standardized bias across the covariates. Given our small sample sizes after establishing common support, we reset individual student's propensity weights to 4 if they were greater than 4, to prevent undue

influence over our results (Cole and Hernan, 2008; Lee et al., 2011). The second assumption can be verified visually by inspecting the distribution of propensities for treated and control students. Figure 2 shows the distribution of the propensities for private and public school attendees in our five sets of analyses, and indicates sufficient overlap for analysis. We established common support by removing from the analysis students whose propensities were less than the maximum of the minimum propensities for the two groups, or greater than the minimum of the maximum propensities of the two groups. The majority of students removed due to lack of overlap were public school students with very low propensities of treatment.

<<Figure Two About Here >>

Because families who choose to use a voucher are typically unrepresentative of the general population (Egalite, Porter, & Stallings, 2017), we use inverse propensity weights to estimate the average treatment effect of the treated (ATT), rather than the average treatment effect. Table 6 shows the raw and weighted covariate means for the public and private school students in the five analytic samples. The weighted means use the ATT weights as survey weights, adjusting for the propensity of being a voucher participant.

<< Table Six About Here >>

Table 6 reveals any differences between the two groups before the matching process occurs. Focusing on the sample of new OS recipients (top panel), students attending private schools are less likely to be female (-8 percentage points), less likely to be Black (-3 percentage points), and less likely to be Latino/a (-9 percentage points). They are also more likely to be in lower grades and less likely to be in higher grades in the 2017 school year. Private school students were less likely to have experienced a disciplinary incident during the 2016 school year

(-.10 percentage points) and scored higher on the previous year reading EOG exam (.16 SD). The average covariate bias before weighting is .17 SD.

After weighting, these differences largely disappear. All of the bias measures for the covariates are less than .10, with over half less than .05. The average covariate bias after weighting drops to .04 SD. Results are similar for the other analytics samples.

Results

We estimate the effect of the Opportunity Scholarship Program in two ways (Table 7). First, we use a bivariate regression model with a dummy variable indicating treatment; this is equivalent to a simple *t*-test between the two groups. Second, we use a multiple regression model with the matching covariates included as control variables. The second approach controls for remaining differences between the two groups after weighting and increases our statistical power; this is our preferred specification. For each analysis, the ATT inverse propensity weights are used to ensure comparability between the two groups.

<< Table Seven About Here >>

We first estimate the effect of voucher receipt on the sample of new voucher recipients. These are students in their first year of private school using the voucher during the 2016-2017 school year. The top panel of Table 7 shows the differences in performance on the ITBS for this treatment group compared to the public school sample. In general, the results show that in our testing sample, voucher recipients scored higher than their public school counterparts in all three subject areas examined—mathematics, reading, and language. The standard deviation in our sample for the three tests is around 18 points, so the effect sizes for voucher recipients range from .22 to .44 SD).

This analysis accounts for static differences between OS students and public school students, but not dynamic changes. For example, both sets of students may appear similar in terms of previous year EOG test scores, but parents may pull students from public schools based on dynamic changes in their EOG test performance over time. As a robustness check, therefore, we repeat our analyses, but include as well the change in math and reading EOG test scores from 2015 to 2016. Results are generally still positive, but only the math effect is statistically significant now, which may be related to the reduction in statistical power associated with going from 245 observations to 170, a 31 percent drop in sample size.

One downside associated with focusing on new OS students is that the estimated impact of the voucher cannot be separated from the impact of switching schools. If the treatment group students are slow to adjust to the standards, expectations, and culture of their new schooling environment, this may attenuate the estimated impact of the voucher. The bias could work in the opposite direction too, if the initial excitement of having “won” a voucher seat motivates students to put forth greater effort in the short run, temporarily boosting achievement but not resulting in sustained gains into the second year of voucher use.

To generate estimates that are clean of these potential biases therefore, we next estimate the impact of voucher use on renewal students instead. Here, the treatment group consists of voucher students in their *second* year of voucher use, and the propensity weights for both the public and private school matched groups are constructed using the 2015 EOG instead of the 2016 EOG. In these models, impacts in all three subjects—math, reading, and language—remain positive, although only the language impacts are statistically significant at $p < .05$, which may be related to the drop in sample size required to run these models. Of note, the estimated impacts on reading and language scores actually increase by 3.742 points (i.e., $7.644 - 3.902$) and 1.521

(i.e., 9.412- 7.891), respectively, whereas math scores decrease slightly, by approximately one-half of one point (i.e., 6.487-6.012). Thus, we conclude that students in our sample who are still participating in the OS program after two years perform nine points higher on the language assessment than comparable students who remain in public schools. This is equivalent to .52 SD.

A central issue in the evaluation of voucher programs is the assessment used to compare private and public school students. If private schools are required to administer the state assessment to voucher recipients, they may find themselves at a disadvantage if their curriculum is not aligned with the test in the same way as the public school curriculum is aligned. Although we do not rely on the criterion-referenced North Carolina state test for this evaluation, which would potentially advantage the public school students, a similar issue could be at play here if some of the private schools have aligned their curriculum to the assessment we use. While it is important to note that none of the schools in our sample actually use the assessment administered for this evaluation—the short battery (Form E) of the ITBS—some of the private schools in our sample do administer the complete battery of the ITBS to students in 3rd, 6th, and 12th grade for internal assessment purposes. While they may not explicitly focus their instruction to ensure their students perform well on the ITBS, they may have selected the ITBS because the scope and sequence of tested content most closely aligns to their curriculum. Thus, our positive results could be explained, in part, by an alignment with the assessment used to measure student achievement.

One way to test this theory is to repeat our analysis of new OS recipients and split the treatment group into two, depending on whether or not the private school attended by a student administers the ITBS. The bottom panel of Table 7 shows the results of these analyses. The large positive effects from our previous analyses largely disappear when analyzing students from

private schools that do not use ITBS, implying that OS students attending these schools perform about the same as their public school counterparts on this assessment. The treatment variable is no longer statistically significant, and the effect sizes range from .04 to .10. The results for the ITBS sample, meanwhile, remain statistically significant, with implausibly large effect sizes that range from .39 to .86.

These large, positive effect sizes may, in part, reflect curricular alignment with the ITBS, but we also cannot rule out other explanations. For instance, it could be the case that use of the ITBS is correlated with other influential school characteristics, such as school age, school size, teacher quality, school leader quality, peer group quality, etc. It could also be driven by student selection into certain types of schools. It is also worth noting that all of the Catholic schools in our sample use the ITBS, such that half of the students in the ITBS-only private school group attend Catholic schools, versus none in the non-ITBS sample, making it challenging to tease apart the impact of attending a Catholic school from the impact of attending an ITBS-using school. In conclusion, we cannot rule out the possibility that the large effect sizes for this analysis may reflect factors other than curricular alignment with the assessment.

Discussion

The results presented here reveal large positive impacts associated with voucher usage in North Carolina. Our preferred specification utilizes observations from 245 students, controlling for a host of covariates, revealing first year impact estimates of .36 SD in math and .44 SD in language. Two-year impact estimates suggest that student achievement in language improved by .52 SD. These findings are surprising in that they run contrary to recent evidence from Louisiana (Abdulkadiroglu, Pathak, & Walters, 2018; Mills & Wolf, 2017), Ohio (Figlio & Karbownik, 2016), and Indiana (Waddington & Berends, 2017), but the positive impacts observed here are in

line with the bulk of the literature on voucher usage in the United States since the 1990s (Table 2). It may be the case that the North Carolina Opportunity Scholarship Program truly has a positive impact on student achievement, perhaps because it reaches highly economically-disadvantaged students (Egalite, Stallings, & Porter, 2017) who have few school choice options in the absence of the program and perhaps the highest potential for academic growth, as a result. Two other potential explanations also merit consideration.

The first alternative explanation for the positive impacts reported here relates to the research design. As with any quasi-experimental design, it is possible that unobserved characteristics of students not captured in the statistical model explain the variation in test scores that is currently attributed to the voucher indicator. For instance, even though all students in the analysis sample are low-income by sample construction criteria, voucher students may still be relatively advantaged compared to their public school matched peers and these unobserved characteristics may lead to higher test scores after the student switches into private school. OS parents have to provide school transportation and pay for breakfast and lunches now that the child is in a private school (Egalite, Gray, and Stallings, 2017), which suggests they might have more disposable income to draw upon but a descriptive analysis of household income suggests that voucher-receiving families are actually among the poorest households in the state (Egalite, Stallings, & Porter, 2017). It may still be the case that parents of voucher-receiving students are more motivated, but this would have to be a dynamic characteristic or it would already be accounted for by the variables documenting students' prior test scores and disciplinary records.

Second, it is possible that the choice of test unfairly advantaged the private school students. This would be a serious concern if the test chosen for this analysis was a criterion-referenced test, aligned to a specific set of standards and content sequence, such as the North

Carolina End-of-Grade tests, but the Iowa Test of Basic Skills is a norm-referenced test that is not aligned with any one curriculum. A related concern is if the private schools in our sample use the Iowa Test of Basic Skills for their annual assessment and students in the study sample were seeing the same set of questions for the second time. We tried to avoid this scenario in two ways—by testing early enough in the spring so that the students took the test for our study before taking the test for their own school’s accountability protocol. A complementary benefit of this timing decision was that we avoided unfair test fatigue among public and private school students, because neither side had started their own spring testing yet. We also avoided this potential pitfall by selecting a short battery test that had not previously been reported to the SEAA as being used among private schools for accountability purposes (recall, Table 2).

Although not a threat to internal validity, it is also important to consider sample construction when interpreting these results. Because our sample is not comprehensive and students were not randomly selected to be in the study, our sample is not representative of all voucher users in 2016-17. On both the public and private school sides, we were forced to rely on volunteer samples, which may have resulted in unusually high performers in our sample. Recruitment efforts were supported by public and private school partners including individual school leaders, school district central office staff, and a school choice interest group. Thus, the results reported here are not reflective of the average test score impact on a typical voucher student attending a North Carolina private school by way of the Opportunity Scholarship program. The sample construction protocol also raises interesting questions about how we should conceptualize the “treatment.” Students in the comparison group attend large, high-poverty public schools, whereas students in the treatment group attend smaller, less racially diverse private schools (Table 4). As with any private school voucher study, the treatment impact may

reflect differences in peer effects, school size, and resources as much as it reflects a simple switch from the public to private sector.

Limitations. There are three important limitations to this analysis that merit further discussion. The first limitation relates to our research design. Propensity score matching allows us to mitigate potential selection bias by accounting for observable differences between students in the treatment and comparison groups. This is greatly strengthened by our inclusion of pretreatment measures of students' academic performance (Bifulco, 2012). Nonetheless, it is possible that the matched students are not perfectly aligned along unobservable dimensions, which could introduce potential selection bias. This is always a possibility in research designs that cannot leverage a lottery for causal inference.

Second, the outcome examined, student performance on a nationally-normed standardized test, captures just one dimension of student learning. Many in the private school community would likely wish to see more diverse outcomes examined, such as less tangible but equally important character skills, such as perseverance, grit, and persistence in the face of failure. Nonetheless, there is a strong argument to be made for a close examination of student test scores as they are strongly related to high school completion and college attendance (Rivkin, 1995), college quality (Behrman, Kletzer, McPherson, & Schapiro, 1998), college completion (Hanushek & Pace, 1995), and individual incomes (Neal & Johnson, 1996).

The third limitation is one of external validity. The students in this sample were not randomly selected among all Opportunity Scholarship users in the state so they may not be representative of the average Scholarship user. Furthermore, the private schools that participated in our data collection efforts were also not randomly selected among all participating private schools so they too may not be representative of the average private school attended by a student

utilizing an Opportunity Scholarship. We are not concerned that either of these limitations introduces bias by compromising internal validity; they just limit the generalizability of the results.

Conclusion

We use inverse propensity weights in a regression model to estimate the impact of North Carolina's Opportunity Scholarship Program on student performance in mathematics, reading, and language. The effect sizes observed are positive, large, and statistically significant, ranging from .25 to .49 SD. As a robustness check, we also run models that account for pre-existing trends in student achievement by controlling for both 2015 and 2016 test scores. Impacts remain positive and statistically significant, but shrink slightly to .21 to .39 SD. Although not a comprehensive analysis of all 5,624 voucher recipients in the 2016-17 school year, this analysis presents the first estimate of the achievement impacts associated with the use of a means-tested voucher in the Tar Heel state.

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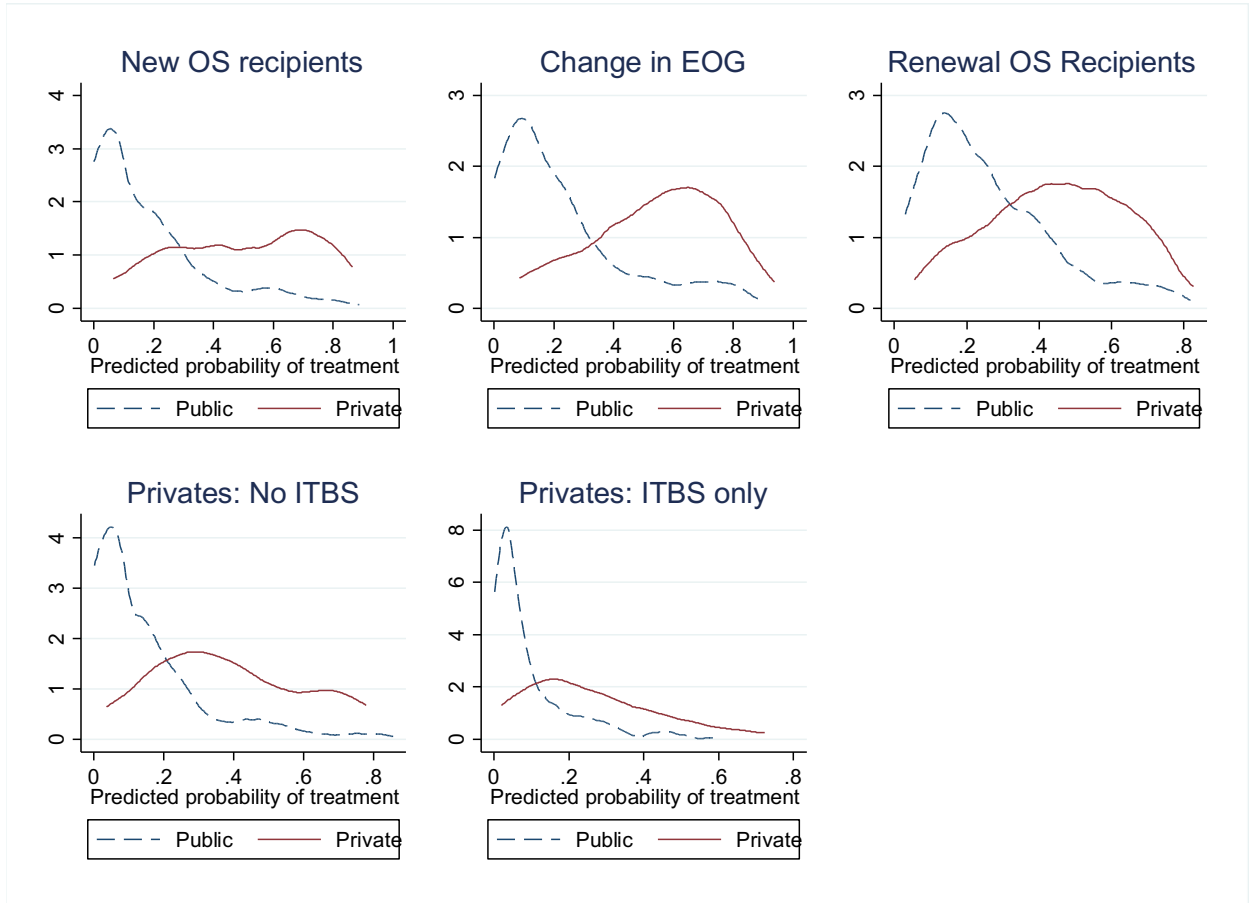


Figure 1. Distribution of Student Propensity Scores by 2017 School Type

Table 1.
North Carolina's Opportunity Scholarship Program, Compared to Selected Programs in Other States

State	North Carolina	Florida	Indiana	Louisiana
Program Name	NC Opportunity Scholarship Program	FL Tax Credit Scholarship Program	IN Choice Scholarship Program	LA Scholarship Program
Program Type	Voucher	Tax Credit	Voucher	Voucher
Year Enacted	2013	2001	2011	2008
Eligibility Pathways	a. Household income doesn't exceed 133% of the federal free/ reduced-price lunch program limit, and b. Student either has prior public school attendance, is entering K or G1, parent/guardian is active-duty military, or child is in foster care/ recently adopted	Either the student is in foster care or meets the household income requirement (260% of federal poverty line). Note: prior year enrollment in a public school no longer required	Eight potential pathways, none of which allow household income to exceed 200% of federal free/ reduced price lunch program limit: Continuing Choice Scholarship Student, Previous Choice Scholarship Student, Previous Scholarship Granting Organization (SGO) Award, Special Education, "F" Public School, Two Semesters in a Public School, Sibling, and Pre-K.	Student must be a Louisiana resident; Household income does not exceed 250% of the federal poverty guidelines; Either entering kindergarten or enrolled in a public school with a C, D, F, or T grade; OR a continuing Scholarship student
Max Value of Voucher Award	\$4,200	\$5,886	The lesser of private school tuition and fees OR the 50% or 90% award amounts based on the funding formula for the student's public school corporation of residence	The lesser of private school tuition and fees OR the maximum state allocation per student
Parental Co-Pay? Private School Admissions Screening Permitted?	Yes Yes	Yes Yes	Yes Yes	No No
Accountability Tests	Any nationally-normed test	Any nationally-normed test	State Test	State Test

Participants (2016-17)	5,624	98,936	34,299	6,995
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Sources: Florida program info: <http://www.fldoe.org/schools/school-choice/k-12-scholarship-programs/ftc/ftc-faqs.stml>; Indiana program info: <https://www.doe.in.gov/sites/default/files/choice/1-choice-parent-faq-february-2018.pdf> ; Louisiana program Info: <https://www.louisianabelieves.com/schools/louisiana-scholarship-program>. North Carolina program info: Statute 115C-562. Number of participating students across all four programs comes from www.edchoice.org.

Note: Parental co-pay is when parents are responsible for the cost of tuition and fees that exceed the maximum voucher value. This is sometimes referred to as tuition “top-up.” All programs generally specify that students must be of school-going age (i.e., high school graduates would be ineligible) and residents of the state in which the program is administered.

Table 2.
Standardized Assessments Taken by Opportunity Scholarship Students, 2014-15

Test Name	Publisher	Number of Students
Basic Achievement Skills Inventory (BASI)	Brewer Testing Services	3
BJU Press Assessments	BJU Press	2
California Achievement Test (CAT)	Seton Testing Services	33
Comprehensive Testing Program (CTP)	Educational Records Bureau	19
Iowa Assessments	Houghton Mifflin Harcourt	204
Otis-Lennon School Ability Test (OLSAT)	Pearson	3
Stanford Achievement Test	Pearson	272
TerraNova	Data Recognition Corporation	303
Woodcock Johnson	Houghton Mifflin Harcourt	9
TOTAL		848

Source: The North Carolina State Education Assistance Authority

Table 3.
Lottery-Based Studies of the Achievement Impacts of School Vouchers

Citation	Program Name	Program Funding	Program Scope	Impact
Cowen (2008)	Charlotte Scholarship Fund	Private	Citywide	Positive Overall
Greene (2001)	Charlotte Scholarship Fund	Private	Citywide	Positive Overall
Howell, Wolf, Campbell, & Peterson (2002)	The D.C. Opportunity Scholarship Program.	Public	Citywide	Positive Overall
Wolf, Kisida, Gutmann, Puma, Eissa, & Rizzo (2013)	The D.C. Opportunity Scholarship Program	Public	Citywide	Null
Dynarski, Rui, Webber Gutmann & Bachman (2017)	The D.C. Opportunity Scholarship Program	Public	Citywide	Negative Overall
Howell, Wolf, Campbell, & Peterson (2002)	The Dayton, Ohio Parents Advancing Choice in Education Program	Private	Citywide	Positive for Subgroups
Bettinger & Slonim (2006)	The Children’s Scholarship Fund of Toledo, Ohio	Private	Citywide	Null
Howell, Wolf, Campbell, & Peterson (2002)	New York City School Choice Scholarships Foundation Program	Private	Citywide	Positive for Subgroups
Barnard, Frangakis, Jin, & Rubin (2003)	New York City School Choice Scholarships Foundation Program	Private	Citywide	Positive for Subgroups
Krueger & Zhu (2004)	New York City School Choice Scholarships Foundation Program	Private	Citywide	Null
Jin, Barnard, & Rubin (2010)	New York City School Choice Scholarships Foundation Program	Private	Citywide	Positive for Subgroups
Bitler, Domina, Penner, & Hoynes (2014)	New York City School Choice Scholarships Foundation Program	Private	Citywide	Null
Rouse (1998)	Milwaukee Parental Choice Program	Public	Citywide	Positive Overall
Greene, Peterson, & Du (1999)	Milwaukee Parental Choice Program	Public	Citywide	Positive Overall
Mills & Wolf (2017)	Louisiana Scholarship Program	Public	Statewide	Negative Overall and for Subgroups
Abdulkadiroglu, Pathak, & Walters (2018)	Louisiana Scholarship Program	Public	Statewide	Negative Overall and for Subgroups

Note: The designations “Positive Overall” and “Negative Overall” indicate that a statistically significant positive or negative

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achievement impact were observed for the overall sample in at least one subject

Table 4.
Description of Public and Private Schools in Sample

	Private	Public
Tested Students Per School		
Median	7	23
Mean	12	35
Min	1	4
Max	43	166
School Has a Library or Media Center	0.95	n/a
Religious Affiliation		
Catholic	0.53	n/a
Baptist	0.16	n/a
Christian (no specific denomination)	0.26	n/a
Other Religion (e.g., Methodist, Episcopal)	0.05	n/a
School Type		
Private	1.00	0.00
Traditional Public School	0.00	0.93
Charter	0.00	0.00
Magnet	0.00	0.07
School Wide Title 1	n/a	1.00
Enrollment		
Median	225	649
Mean	399	640
Min	107	342
Max	1402	933
School Community Type		
City	0.74	0.36
Suburb or Town	0.16	0.14
Rural	0.11	0.50
Racial Composition		
Average of School Percent White	0.64	0.14
Average of School Percent Black	0.15	0.49
Average of School Percent Hispanic	0.13	0.31
Pupil Teacher Ratio		
Median	12	14
Mean	12	14
Min	8	12
Max	19	17
Full Time Equivalent Teachers		
Median	28	45

Mean	35	45
Min	11	26
Max	94	73
Standardized Tests Used		
Iowa Test of Basic Skills	0.44	n/a
Terra Nova	0.31	n/a
Stanford Achievement Test	0.25	n/a
California Achievement Test	0.06	n/a
Preliminary Scholastic Assessment Test (PSAT)	0.06	n/a
North Carolina End of Grade Tests	n/a	1.00

Notes: n/a implies this data point was not available for schools in this sector; n = 14 public schools and 24 private schools in the testing sample. All testing was conducted in spring 2017. Tested students per school refers to the number of students tested by the NC State research team for this project. An elementary school configuration is defined as a school that has one or more of grades K-6 and does not have any grade higher than the 8th grade. A secondary school configuration is defined as a school that has one or more of grades 7-12 and does not have any grade lower than 7th grade. A combined school configuration is defined as a school that has one or more of grades K-6 and one or more of grades 9-12. Some schools use more than one standardized test so these individual percentages are not expected to sum to 100 percent. "Standardized Tests Used" refers to the accountability tests used to assess all students in a school, not just Opportunity Scholarship students.

Sources: Private school data are from the 2015-16 Private School Universe Survey and the North Carolina Division of Non Public Education records. Public school data are from the 2015-16 U.S. Department of Education Public Elementary/Secondary School Universe Survey.

Table 5.
Constructing the Analysis Sample

	Private	Public	Total
Took ITBS in 2017	297	401	698
Not included in analysis because:			
No unique match with DPI data	48	76	124
Took wrong version of ITBS in 2017	0	8	8
Did not take/finish ITBS in 2017	0	7	7
Missing 2016 EOG	1	0	1
Not free/reduced lunch in 2016	-	58	58
Could not verify OS receipt	3	-	3
Final student count	245	252	497
Status of OS recipients			
New	90	-	-
Renewal	155	-	-
Analytic dataset <i>n</i>	245	252	497

Note: ITBS stands for Iowa Test of Basic Skills; DPI stands for Department of Public Instruction; EOG stands for the North Carolina End of Grade assessments, our measure of baseline achievement.

Table 6.
Covariate Means Before and After Inverse Propensity Weighting

<i>Analysis /covariates</i>	Unweighted				Weighted			
	Private	Public	Difference		Private	Public	Difference	
			Raw	Std.			Raw	Std.
<i>New OS recipients</i>								
Female	0.56	0.64	-0.08	-0.16	0.56	0.58	-0.02	-0.03
Black	0.48	0.51	-0.03	-0.06	0.48	0.52	-0.03	-0.07
Latino	0.06	0.15	-0.09	-0.30	0.06	0.04	0.02	0.05
Other race/ethnicity	0.24	0.19	0.04	0.11	0.24	0.21	0.03	0.06
Grade 5	0.25	0.20	0.05	0.12	0.25	0.27	-0.02	-0.05
Grade 6	0.30	0.11	0.19	0.49	0.30	0.29	0.01	0.03
Grade 7	0.15	0.15	0.00	0.00	0.15	0.14	0.00	0.01
Grade 8	0.15	0.27	-0.12	-0.31	0.15	0.16	-0.01	-0.03
Disciplinary incident	0.12	0.16	-0.04	-0.10	0.12	0.13	-0.01	-0.02
Math state EOG score	449.47	449.54	-0.07	-0.01	449.47	449.64	-0.17	-0.02
Reading state EOG score	449.75	448.15	1.60	0.16	449.75	450.14	-0.38	-0.04
Mean difference				0.17				0.04
<i>Change in EOG</i>								
Female	0.56	0.63	-0.08	-0.16	0.56	0.55	0.01	0.01
Black	0.49	0.54	-0.05	-0.11	0.49	0.54	-0.05	-0.10
Latino	0.07	0.12	-0.05	-0.18	0.07	0.04	0.03	0.11
Other race/ethnicity	0.26	0.19	0.07	0.17	0.26	0.25	0.01	0.03
Grade 5	0.31	0.23	0.07	0.16	0.31	0.31	-0.01	-0.01
Grade 6	0.36	0.18	0.18	0.40	0.36	0.38	-0.01	-0.03
Grade 7	0.15	0.19	-0.04	-0.11	0.15	0.15	0.00	0.00
Grade 8	0.18	0.39	-0.21	-0.47	0.18	0.16	0.02	0.04
Disciplinary incident	0.14	0.19	-0.05	-0.15	0.14	0.13	0.01	0.03
Math state EOG score	448.75	448.67	0.08	0.01	448.75	448.95	-0.20	-0.02
Reading state EOG score	450.85	450.59	0.26	0.03	450.85	451.05	-0.21	-0.02
Math EOG change	0.15	0.22	-0.07	-0.01	0.15	0.50	-0.35	-0.06
Reading EOG change	5.82	4.46	1.36	0.22	5.82	6.03	-0.21	-0.03
Mean difference				0.17				0.04
<i>Renewal OS recipients</i>								
Female	0.43	0.62	-0.19	-0.40	0.43	0.38	0.05	0.10
Black	0.43	0.36	0.06	0.13	0.43	0.38	0.04	0.09
Latino	0.18	0.36	-0.19	-0.43	0.18	0.16	0.02	0.05
Other race/ethnicity	0.16	0.14	0.02	0.05	0.16	0.23	-0.07	-0.20
Grade 5	0.03	0.11	-0.08	-0.33	0.03	0.04	-0.01	-0.03
Grade 6	0.29	0.33	-0.03	-0.07	0.29	0.26	0.04	0.08
Grade 7	0.24	0.13	0.11	0.28	0.24	0.29	-0.06	-0.15
Grade 8	0.29	0.21	0.08	0.18	0.29	0.25	0.04	0.09
Disciplinary incident	0.18	0.31	-0.13	-0.31	0.18	0.18	-0.01	-0.02
Math state EOG score	450.03	448.79	1.24	0.15	450.03	450.18	-0.16	-0.02
Reading state EOG score	445.57	444.80	0.77	0.07	445.57	445.88	-0.31	-0.03
Mean difference				0.22				0.08

Table 6 continued

<i>Analysis /covariates</i>	Unweighted				Weighted			
	Private	Public	Difference		Private	Public	Difference	
			Raw	Std.			Raw	Std.
<i>No privates using ITBS</i>								
Female	0.59	0.64	-0.05	-0.10	0.59	0.58	0.01	0.03
Black	0.47	0.58	-0.11	-0.21	0.47	0.55	-0.07	-0.15
Latino	0.05	0.08	-0.03	-0.13	0.05	0.03	0.02	0.09
Other race/ethnicity	0.24	0.20	0.03	0.08	0.24	0.25	-0.01	-0.04
Grade 5	0.22	0.17	0.05	0.14	0.22	0.23	-0.01	-0.01
Grade 6	0.34	0.10	0.24	0.59	0.34	0.33	0.01	0.02
Grade 7	0.14	0.12	0.01	0.04	0.14	0.15	-0.02	-0.05
Grade 8	0.12	0.26	-0.14	-0.37	0.12	0.12	0.00	-0.01
Disciplinary incident	0.08	0.15	-0.07	-0.21	0.08	0.09	0.00	-0.01
Math state EOG score	450.76	449.17	1.59	0.18	450.76	451.04	-0.28	-0.03
Reading state EOG score	450.08	446.77	3.31	0.33	450.08	450.49	-0.40	-0.04
Mean difference				0.22				0.04
<i>Only privates using ITBS</i>								
Female	0.55	0.65	-0.10	-0.21	0.55	0.56	-0.01	-0.02
Black	0.48	0.46	0.02	0.04	0.48	0.50	-0.02	-0.04
Latino	0.10	0.26	-0.15	-0.40	0.10	0.08	0.02	0.05
Other race/ethnicity	0.24	0.16	0.08	0.20	0.24	0.22	0.02	0.04
Grade 5	0.34	0.24	0.11	0.24	0.34	0.33	0.01	0.02
Grade 6	0.17	0.09	0.08	0.23	0.17	0.23	-0.06	-0.16
Grade 7	0.17	0.16	0.01	0.03	0.17	0.15	0.02	0.05
Grade 8	0.21	0.25	-0.04	-0.10	0.21	0.20	0.00	0.01
Disciplinary incident	0.21	0.15	0.06	0.16	0.21	0.21	-0.01	-0.02
Math state EOG score	447.34	448.87	-1.53	-0.16	447.34	445.96	1.38	0.15
Reading state EOG score	448.69	446.97	1.72	0.16	448.69	448.22	0.47	0.05
Mean difference				0.18				0.06

Table 7.
Achievement Impact Associated with Participation in the North Carolina Opportunity Scholarship Program

Treatment group	Covariates	OLS results			Effect sizes			<i>n</i>		
		Math	Reading	Language	Math	Reading	Language	T	C	Total
New OS recipients	No	6.890*	3.863	7.852*	0.38	0.21	0.44	89	156	245
		(2.789)	(3.041)	(2.914)						
	Yes	6.487**	3.902+	7.891**	0.36	0.22	0.44	89	156	245
		(2.221)	(2.136)	(2.483)						
	Yes & change in EOG	6.709*	4.482	8.807	0.37	0.25	0.49	72	98	170
		(2.553)	(2.403)	(2.498)						
Renewal OS recipients	No	5.987	7.497*	9.733*	0.33	0.42	0.54	68	140	208
		(4.012)	(3.560)	(4.050)						
	Yes	6.012+	7.644+	9.412**	0.33	0.42	0.52	68	140	208
		(3.235)	(3.848)	(3.347)						
New OS recipients; school does not use ITBS	No	0.114	1.690	0.842	0.01	0.09	0.05	59	157	216
		(2.812)	(3.263)	(3.020)						
	Yes	0.744	1.835	1.347	0.04	0.10	0.07	59	157	216
		(1.983)	(2.151)	(1.874)						
New OS recipients; school uses ITBS	No	13.122**	8.586	17.144**	0.73	0.48	0.95	29	191	220
		(2.944)	(5.441)	(3.452)						
	Yes	11.291**	7.105*	15.495**	0.63	0.39	0.86	29	191	220
		(2.875)	(2.982)	(3.204)						

Notes: Coefficients are for private school status from regressions with ITBS composite scores in math, reading, and language as the dependent variable, using inverse propensity weighting. Standard errors in parentheses; clustered at the 2017 school level. + $p < .10$, * $p < .05$, ** $p < .01$.