Working Paper 2020-3

COMPETITIVE EFFECTS OF THE INDIANA CHOICE SCHOLARSHIP PROGRAM ON TRADITIONAL PUBLIC SCHOOL ACHIEVEMENT AND GRADUATION RATES

Anna J. Egalite
Anna_Egalite@ncsu.edu

Andrew D. Catt
dcatt@edchoice.org

August 3, 2020

ABOUT EDCHOICE

EdChoice is a nonprofit, nonpartisan organization dedicated to advancing full and unencumbered educational choice as the best pathway to successful lives and a stronger society. EdChoice believes that families, not bureaucrats, are best equipped to make K–12 schooling decisions for their children. The organization works at the state level to educate diverse audiences, train advocates and engage policymakers on the benefits of high-quality school choice programs. EdChoice is the intellectual legacy of Milton and Rose D. Friedman, who founded the organization in 1996 as the Friedman Foundation for Educational Choice.

The contents of this publication are intended to provide empirical information and should not be construed as lobbying for any position related to any legislation.

EdChoice Working Papers are meant for wide circulation, inviting reader feedback, and for informing national and state conversations about reform ideas, policymaking, and implementation, driven by educational choice. EdChoice internally reviews each paper but does not necessarily send out for external reviews.

This paper, however, received extensive comments from external reviewers that were acted upon by the authors.

We welcome and encourage hearing from you. Please feel free to contact the authors directly—or Paul DiPerna, our Vice President of Research and Innovation—about your impressions and comments.

Anna Egalite: Anna_Egalite@ncsu.edu

Drew Catt: dcatt@edchoice.org

Paul DiPerna: paul@edchoice.org

Table of Contents

List of Figures

List of Tables

Abstract

As school choice options grow, it is helpful to test for indirect effects on non-choosers who are left behind in district-run schools. Can a competitive system built on the principle of choice serve as a rising tide that lifts all boats or will such systems further existing inequities? The largest competitive effects analysis of a voucher program in the United States, this study helps address existing gaps in knowledge by examining how the Indiana Choice Scholarship Program affects students in traditional public schools, looking at the change in academic outcomes and graduation rates of students in Indiana traditional public schools based on their proximity to private schools participating in this statewide K–12 private school voucher program. We use network proximity data to generate a “drive-time” measure that accounts for road lengths, intersection turn times, speed limits, and traffic data and allows us to calculate precise travel time between two points—down to the minute. We use this precise measure in a difference-in-differences framework to examine impacts on student math and English Language Arts test scores and graduation rates. We find little evidence that the average student in traditional public schools has been affected—either positively or negatively— by the enactment and growth of the Indiana Choice Scholarship Program. We do find, however, consistent evidence of small positive effects for low-income children.

Keywords: School choice; education policy; market reforms

Introduction

Passed as part of House Enrolled Act 1003-2011, the Indiana Choice Scholarship Program (ICSP) is a means-tested private school choice program that currently provides $159 million in state funds annually for more than 36,000 students to offset the cost of private school tuition. The largest voucher program in the nation, enrollment in the program has grown every year since the program was first implemented in the 2011–12 school year (Table 1). During the program's first year of operation, ICSP scholarships were limited to 7,500 students. Student participation fell short of the cap that year when 3,911 students participated, receiving an average award of $4,105 (Grades 1–8) or $5,518 (Grades 9–12) each. The program participation cap doubled in the program's second year and for the 2013–14 school year, the enrollment cap was removed altogether so that ICSP scholarships could be awarded to all eligible applicants. By 2019–20, enrollment had reached 36,707, with eligible students receiving an average award of $5,900 (grades 1–8) or $5,909 (Grades 9–12).[1]

One of the main reasons for this rate of program growth is the expansion of program eligibility criteria over time to include a broader cross-section of students. The program is currently open to applicants with special educational needs, siblings of current ICSP scholarship users, and students coming from failing public schools, in addition to children from low-income families. Although there was one in the first few years of the program, there is currently no enrollment cap on participants. Along with existing evidence on participants' test scores (Waddington & Berends, 2018), this raises important questions about the impact of a rapidly-growing private school choice program in the Hoosier State. Given the many pathways to entry and lack of enrollment cap, some may worry that students who don't choose a private school will be left behind in more than one way. Can a competitive system built on the principle of choice serve as a rising tide that lifts all boats, or will such systems further existing inequities?

[1] These are the award amounts for students receiving a full scholarship (what is deemed a "90% award" by the Indiana Department of Education). Partial scholarships were also available in these years (what is deemed a "50% award"). The average award for a student on a 50% award was $2,928 (Grades 1–8) or $2,954 (Grades 9–12) in 2011–12. The average award for a student on a 50% award was $3,173.26 (Grades 1–8) or $3,202 (Grades 9–12) in 2019–20.

Table 1. Growth in the Indiana Choice Scholarship Program, 2011–12 through 2019–20

Year	Student Participation	Private School Participation
2011–12	3,911	241
2012–13	9,139	289
2013–14	19,809	313
2014–15	29,148	314
2015–16	32,686	316
2016–17	34,299	313
2017–18	35,458	318
2018–19	36,290	329
2019–20	36,707	326

Source: Indiana Department of Education, Office of School Finance

Various arguments have been proposed in support of the idea that the competitive pressures induced by an education marketplace will serve as a tide that lifts all boats. In response to the fiscal pressure to attract and retain students, schools may seek out innovative approaches to improve student learning. On the other hand, opponents argue a potential drawback is the possibility that non-choosers will be left behind, furthering inequitable schooling experiences. For example, the executive director of the Indiana Association of Public School Superintendents, J. T. Coopman, has said about the program: "There's a price that's going to be paid down the road because we're going to end up with the haves and the have-nots."(Wang, 2014). To test these theories, we conduct a systemic effects analysis of Indiana's private school voucher program, one of the largest voucher programs in the nation. We analyze the ICSP's competitive effect on the achievement and attainment outcomes of 837,000 students.

To accomplish this, we rely on geocoded competition measures in a difference-in-differences framework, focusing on district-operated public schools as the population of interest. The intuition behind the research design is that while the establishment of the ICSP may have represented a shock for all public schools in Indiana, those facing a more competitive landscape to begin with would have felt this shock more acutely.

First, we look at the competitive impact of the ICSP on student test scores in the program's first year of operation (2011–12), when enrollment numbers were still small. This helps us cleanly isolate the competitive impact of the private school choice option from school compositional changes resulting from student transfers and resource changes associated with the loss of per-pupil funds, which occurs when there is an enrollment decline in the public-school sector. We then repeat the analysis incorporating five years of outcome data to maximize statistical power and to take advantage of higher "dosage" in later years when greater numbers of students exercised this choice option. These data include school years ending in 2011 (the pre-policy year), 2012 (the first post-policy year), 2013, 2014, 2015, and 2016. We also use this model to test for changes in public school students' likelihood of graduation that could be attributed to the program.

We use a variety of drive-time measures—the travel time in minutes to the nearest private school competitor and the average count of private school competitors within X minutes—to assess distance between schools. We calculate drive-time before the policy was enacted to avoid potential endogeneity bias that could occur if new private schools were established in response to the voucher program and systematically chose to establish near low-performing public schools, for example, where they might find it easier to recruit applicants. Because we take this caution, any observed changes in student achievement associated with the distance measure are likely to reflect a public school response to the competitive pressure associated with the passage of the voucher law.

It may be the case that public schools only respond to the program once the competitive pressure they face passes beyond a certain threshold or they lose a meaningful number of students. We therefore conduct a dosage analysis of test score changes to search for a potential "tipping point" in the school choice marketplace. That is, we are interested in estimating if there is a point at which public school test scores change as a significant mass of students transfer from their assigned traditional public school to a private school of choice by way of the state's private school choice program, the ICSP. To estimate this number, we calculate the competitive response associated with the drive-time to the nearest private school with a minimum of 10, 20 30, 40, or 50 voucher students in the program's first year.

In summary, the methods used for our competitive analysis of the nation's largest voucher program employ multiple measures of competition, including drive-time to the nearest private school and a count of private school competitors within a given drive-time radius, each implemented in a separate model to avoid collinearity. The findings are as follows: null to small, positive impacts on public school achievement and null effects on a student's likelihood of graduation. Specifically, drive-time models find no impact on student test scores, whereas competitor count models find small, positive, and statistically significant test score increases in both math and English Language Arts.

We also test for subgroup impacts, finding very small but statistically significant math benefits for low-income and special education students in models that rely on the drive-time measure.

The methods used for our tipping point analysis leverage the drive-time to private schools with a minimum count of students attending private schools by way of the ICSP to estimate impacts. The findings are as follows: a very small, positive, statistically significant achievement effect associated with the distance to a private school accepting at least 10 voucher students, but no additional response associated with distance to schools serving a greater mass of voucher students.

The remainder of this study proceeds as follows. First, we review the theoretical framework underlying the competitive effects literature. We then place the ICSP in the context of the broader private school choice landscape. Next, we review prior studies and identify our contribution. The methods section describes our data and methods and is followed by a presentation of results. The article concludes with a discussion of the main findings and an acknowledgement of the limitations of the current study.

Theoretical Framework

Theoretically, the competitive impact of the ICSP on public school students' test scores and likelihood of graduation could be positive, negative, or neutral. On the one hand, we may observe efficiency gains if public school leaders respond in creative and productive ways to the increased market pressure to achieve better academic outcomes at the same cost (Friedman, 1962). The theory of action by which schools would accomplish this might consist of some combination of the following reforms: a renewed focus on effective instructional leadership practices (Grissom, Loeb, & Master, 2013); better use of technology and strategic staffing (Jacob, 2011); overall efforts to boost organizational capacity and efficiency (Chubb & Moe, 1990), the development of human capital by way of teacher coaching (Kraft, Blazar, & Hogan, 2018), and revisions to school start schedules to better match teenagers' sleep habits (Heissel & Norris, 2018). Even non-action on the part of public schools could result in the perception of achievement gains if the hardest-to-serve and lowest-performing students depart with a voucher, a scenario our research design will have to account for.

On the other hand, we may observe lower achievement outcomes in traditional public schools in response to competition from private schools if, for example, public schools pursue incoherent curricular or pedagogical reforms or misuse resources. It is worthwhile spending some time to understand how school funding formulas contribute to potential outcomes in a competitive environment. In the absence of "hold-harmless" provisions in public school funding formulas, such as those enacted in response to the expansion of the charter school sector in Massachusetts (Ridley & Terrier, 2018), student transfers through Indiana's voucher program automatically lower the total amount of funding received at the school level, even if the per-pupil funding amount remains unchanged. Because it has already been established there is a causal relationship between school spending and student achievement (Jackson, 2018), we might expect to observe a decline in traditional public school student achievement if public school budgets are dramatically impacted by the voucher program. If, however, school budgets are only marginally impacted by a small number of student transfers, or public schools are able to absorb reductions by cutting fixed costs to adapt to their new funding reality, students may be unaffected. Another avenue by which we could observe negative impacts on public schools is if private schools "cream skim" the brightest and most motivated students and families (Altonji, Huang, & Taber, 2015).

This scenario would result in the perception of lower overall achievement scores even in the face of non-response to the competitive threat by public schools.

Alternatively, we may observe neutral impacts on student outcomes. This might result from investment in cosmetic reforms that are not strongly associated with improvements to the instructional climate, such as creating brochures or applying fresh paint to the walls of the school facility.

As a final theoretical note, it is worth observing that any initial changes to achievement or attainment outcomes may level off. This latter point about non-linearity is important and the motivation for us to examine the "tipping point" theory in addition to overall impacts. That is, it is unclear a priori if any achievement or attainment changes—either positive or negative—will be linearly related to competitive pressures because schools will likely respond to the program dynamically, adjusting their response to the voucher program in real time with changes to school practices, staffing, curriculum, in-service opportunities, and so on.

Ultimately, the direction of effects is unclear. Therefore, our null hypothesis is that traditional public schools will demonstrate no change in academic outcomes in response to student transfers through Indiana's private school voucher program. The alternative hypothesis is that traditional public-school achievement will either increase or decrease in response to competition from this targeted private school voucher program.

The Indiana Choice Scholarship Program (ICSP)

With 36,707 students participating in 2019–20, the ICSP is the largest statewide private school voucher program in the United States. Launched in 2011, there has been an eight-fold increase in student participation since its inception. The regulatory environment is unusual, but school participation remains high with 326 participating private schools in 2019–20.[2] In addition to common regulations such as the school accreditation requirement, non-discrimination rules, employee background checks, and compliance with health and safety codes, voucher-accepting private schools in Indiana are required to administer the battery of state tests of the state standards that is administered annually in public schools and report those data to the state, which then reports an A–F grade for all public schools and voucher-participating private

[2] School participation counts for prior years are as follows: 2011–12: 241 schools; 2012–13: 289 schools; 2013–14: 313 schools; 2014–15: 314 schools; 2015–16: 316 schools; 2016–17: 313 schools; 2017–18: 318 schools and 2018–19: 329 schools.

schools based on combined test scores and graduation rate data. Participating private schools are subject to continuing eligibility regulations, including rules that they must allow the state to review their curriculum, observe classroom instruction, and review instructional materials and the stipulation that they cannot receive a D or F grade in two consecutive years.

There are a number of features of program design that may generate meaningful competitive effects in Indiana. First, private schools receive accountability grades from the state, and those grades are made available to the general public. Unlike choice programs in other states, this practice makes it easy for parents, legislators, advocates, and members of the general public to draw comparisons across the public and private sectors and to make assessments about which particular schools are performing well. However, compelling private schools to teach and assess the same standards may limit differentiation and attenuate any potential effects of competition from the program. Second, the program serves a broad and diverse population, which means a greater segment of the general population is incentivized to pay attention to school performance and to consider switching sectors in search of better outcomes for their children. For children aged between five and 22, there are eight eligibility pathways, any one of which can qualify a student for a voucher. These various pathways cover students from low- and middle-income households (e.g., to receive a full scholarship in 2019–20, the annual household income cannot exceed $32,703 for a household of four people), students with special educational needs, students zoned to failing traditional public schools (defined as those receiving an F grade from the state), siblings of voucher recipients, and students who previously received an Early Education Grant to attend prekindergarten at a participating private school.

Prior Literature

In this section, we review the prior literature on this program, specifically, and the prior literature on the indirect impacts of private school choice nationally.

Prior Literature on the ICSP

Prior research on the direct impacts of the ICSP sheds light on program participants by examining changes in their test scores and parental satisfaction rates. Research on the achievement impacts for students who use vouchers in Indiana shows a modest negative impact in math and null effects in English Language Arts. Waddington and Berends (2018) exact-match on the following variables: race, sex, and baseline year, grade, and school. This matching strategy still results in meaningful differences between the two groups at baseline, such as differences in prior test scores, however. To account for existing differences between the two groups under comparison, which may bias their results, they include prior achievement as a covariate in the ordinary least squares regression model used to estimate impacts. They report an average achievement loss of 0.15 standard deviations in mathematics during students' first year of attending a private school compared to similar students in traditional public schools. This negative finding in math is consistent across all four years of outcome data examined and across most student subgroups. One important point of clarification is that this study focuses on students in the upper elementary and middle school grades, whereas nearly 50 percent of students receiving vouchers are actually in the lower elementary grades of kindergarten through fourth grade. While the exclusion of students in non-tested grades was unavoidable for their participant effects analysis, in our study we pay attention to all voucher users when judging the competitive effects of the program, incorporating information on all voucher-accepting private schools.

Beyond student test scores, parent survey data from Catt and Rhinesmith (2017) offer further insight into Indiana's targeted voucher program. The majority of ICSP participants who responded to the survey are somewhat or completely satisfied with the program, generally (86 percent), and their child's school, specifically (81 percent). Compared to a child's previous school, ICSP families in the sample communicate more frequently with teachers at their child's new school (61 percent), participate in school activities at a higher rate (61 percent), spend more time working on math or arithmetic with their students (55 percent), and volunteer or perform more community service than before (55 percent).

Survey findings are also useful in aiding our understanding of the program as they shed light on how much time Indiana students currently travel to attend school and how much time families would be willing to have their child travel to attend a different school that might be a better fit. This information can offer insight on any issues of school supply, private school quality, and the competitive threat to public schools associated with private school proximity. The children of ICSP families in this survey sample are more likely than traditional public school students to attend a school that is located more than 10 minutes away from home, indicating a willingness to take on a longer commute in order to reach a desirable school (EdChoice, 2017). ICSP families are also more likely to say they are willing to have their child travel more than 30 minutes to attend a school that might be a better fit. Thus, it seems reasonable to infer from these findings that as more voucher-accepting private schools establish in closer proximity to traditional public schools, more families will consider sector-switching by way of a choice scholarship.[3]

[3] From a methodological perspective, if Indiana experienced a sudden increase in the entry of private schools after the establishment of the ICSP, a dynamic ex-ante distance measure could introduce selection bias into the model. To account for this, we rely on a priori distance measures, as explained in our methodology section.

The present study broadens our understanding of the general effects of Indiana's voucher program by focusing on traditional public school students who might be indirectly affected by the program.

Prior Literature on the Indirect Impacts of Private School Choice

There have been 21 evaluations of the effect of increased competition resulting from private school vouchers in the United States to date (Egalite, 2013). Additionally, there have been two studies of the competitive effects of Florida's tax-credit scholarship program (Figlio & Hart, 2014; Figlio, Hart, & Karbownik, 2020), which is also a private school choice program but with a funding stream that's structured a little differently. A 2019 meta-analysis on the competitive effects of charter school, voucher school, and public school choice policies concludes the overall effect of competition on student achievement is positive (Jabbar, Fong, Germain, Li, Sanchez, Sun, & Devall, 2019). For the 17 studies with schools as the unit of analysis, the coefficient on private school competition from voucher or other forms of private school choice programs is .22 standard deviations, significant at $p < .05$. For studies with students (k= 17) or districts (k =1) as the unit of analysis, the researchers report null effects. This meta-analysis finds no overall negative impact of school choice on the test scores of students who are "left behind."

Prior research on the competitive effects of private school choice has focused on test score outcomes. Table 2 organizes this literature by methodological approach: difference-in-differences (7 studies), fixed effects (7 studies), regression discontinuity design (5 studies), ordinary least squares regression (5 studies), and hierarchical linear modeling (1 study).[4] The difference-in-differences designs mirror the approach pursued here. The first difference is between schools facing differing levels of competitive pressure, the second difference is time.

In addition to the variation in identification strategy just described, these studies also differ in how they operationalize the measure of a "competitive threat." For example, Greene and Winters (2004), Greene (2001), and Chakrabarti (2013) use a binary measure indicating the receipt of an F grade to identify which Florida public schools experience a competitive threat because students in those traditional public schools suddenly became eligible to transfer to a private school at state expense. Other studies measure competitive pressures by proximity. These include the distance to the nearest private school, for

[4] The study counts documented here may appear greater than what is shown in Table 2 due to some studies employing multiple research methods.

example, or the density and diversity of private school competitors within a given radius (Carnoy et al., 2007; Egalite & Mills, 2019; Figlio & Hart, 2014; Greene & Marsh, 2009; Greene & Winters, 2007; Greene & Winters, 2011; Jacob, 2014).

Table 2 summarizes the overarching takeaways from this literature. Fourteen studies found the overall impact of competition on outcomes for students who remain in public schools was positive; seven studies found the impact was neutral-to-positive, meaning competitive pressure resulted in zero losses overall and gains for at least some students in some subject areas; and one study found the impact of increased competitive pressure from voucher programs on traditional public school performance was neutral. Interestingly, three studies report that traditional public school performance was strongest when there was a dramatic increase in the intensity of the competition from voucher programs (Forster, 2008a; Carnoy et al, 2007; Gray, Merrifield, & Adzima, 2014). This suggests that increased competitive pressure may result in higher test scores in traditional public schools up to some unknown point, but the specific nature of this relationship (e.g., if it takes a linear, quadratic, or cubic form) and the tipping point at which positive effects start to level off or change direction is unknown.

Table 2. Prior Studies of the Competitive Effects of Vouchers

#	Study	Location	Competition Measure	Identification Strategy	Study Period	Test Score Effects
1	Figlio, Hart, & Karbownik (2020)	Florida (Tax-Credit Scholarship)	Distance, Density, Diversity, Slots, Churches; (all five are also summarized in a Competitive Pressure Index)	Grade, year, and student-school fixed effects	2002-03 to 2016-17	Positive
2	Figlio & Hart (2014)	Florida (Tax-Credit Scholarship)	Distance, Density, Diversity, Slots, Churches	Difference-in-differences	1998–99 to 2006–07	Positive
3	Greene & Winters (2004)	Florida (Voucher)	Receipt of an 'F' grade	Difference-in-differences	2001–02 to 2002–03	Positive
4	Greene (2001)	Florida (Voucher)	Receipt of an 'F' grade	Difference-in-differences	1998–99 to 1999–2000	Positive
5	Chakrabarti (2008)	Milwaukee, WI (Voucher)	Share of poor children who would qualify for vouchers	Difference-in-differences	1986–87 to 2001–02	Neutral to Positive
6	Carnoy et al. (2007)	Milwaukee, WI (Voucher)	Share of poor children who would qualify for vouchers; Density	Difference-in-differences	1996–97 to 2004–05	Positive
7	Hoxby (2003)	Milwaukee, WI (Voucher)	Share of poor children who would qualify for vouchers	Difference-in-differences	1996–97 to 1999–2000	Positive
8	Figlio & Rouse (2006)	Florida (Voucher)	Receipt of an 'F' grade	Difference-in-differences with school and year fixed effects	1998–99 to 1999–00	Positive (accountability stigma and vouchers, combined)
9	Greene & Winters (2011)	Florida (Disability Voucher)	Density (voucher-accepting private schools within 5 & 10 miles)	Grade, year, and student-school fixed effects	2000–01 to 2004–05	Positive
10	Greene & Marsh (2009)	Milwaukee, WI (Voucher)	Density (relevant private schools within five different radii)	Student fixed effects	1999–2000 to 2006–07	Positive
11	Jacob (2014)	Indiana (Voucher)	Distance, Density, Diversity, Concentration	School fixed effects	2008–09 to 2011–12	Neutral to Positive
12	Carr (2011)	Ohio (Voucher)	Public school is designated as underperforming	School fixed effects	2002–03 to 2007–08	Positive

13	Egalite and Mills (2019)	Louisiana (Voucher)	Distance, Density, Diversity, Concentration; Receipt of a High "C" grade	School fixed effects; Regression Discontinuity Design	2010–11 to 2012–13	Neutral to Positive
14	Chakrabarti (2013)	Florida (Voucher)	Receipt of an 'F' grade	Regression Discontinuity Design	1997–98 to 2001–02	Positive
15	Rouse et al. (2013)	Florida (Voucher)	Receipt of an 'F' grade	Regression Discontinuity Design	2002–03 to 2004–05	Positive (accountability stigma and vouchers, combined)
16	West & Peterson (2006)	Florida (Voucher)	Receipt of an 'F' grade	Regression Discontinuity Design	2001–02 to 2003–04	Positive
17	Figlio & Karbownik (2016)	Ohio (Voucher)	Compared public schools on either side of the margin of becoming voucher eligible	Regression Discontinuity Design	2005–06 to 2008–09	Neutral to Positive
18	Forster (2008)	Florida (Voucher)	Receipt of an 'F' grade	Regression	2001–02 to 2006–07	Positive
19	Greene & Forster (2002)	Milwaukee, WI (Voucher)	Share of poor children who would qualify for vouchers	Regression	1996–97 to 2000–01	Neutral to Positive
20	Greene & Winters (2007)	District of Columbia (Voucher)	Distance and Density (participating private schools within 1 mile)	Regression	2003–04 to 2004–05	Neutral
21	Forster (2008)	Ohio (Voucher)	Public school is designated as chronically underperforming	Regression	2005–06 to 2006–07	Neutral to Positive
22	Greene & Forster (2002)	San Antonio, TX (Voucher)	Compared Edgewood district to other districts with no voucher program	Regression	1997–98 to 2000–01	Positive
23	Gray, Merrifield, & Adzima (2014)	San Antonio, TX (Voucher)	Compared Edgewood district to other districts with no voucher program	Two-level hierarchical linear model	1993–94 to 2007–08	Neutral to Positive

Note: Full references for each study cited in this table are provided in the References section.

We end this section with a discussion of the outstanding questions that have yet to be addressed by the competitive effects literature. First, in what ways are outcomes other than math and reading test scores impacted by such programs? Second, what explains the neutral to positive test score impacts across these various studies? Further research of additional school choice programs in other states is needed in order to understand if these differences are related to specific features of program design, methodological differences in how "competition" is operationalized, or local context.

For example, programmatic differences include opportunities and incentives for collaboration and cooperation across school sectors. Differences in local context include variations in local education policies and labor market conditions. Perhaps competitive effects are strongest in states where the workforce is young and highly-educated so that public school principals are easily able to attract effective new teachers. Perhaps the strength of teacher unions plays a role by influencing how nimble school administrators can be in devising a response to attract and keep students enrolled in their school (Cowen & Strunk, 2015; Strunk & Grissom, 2010). Perhaps effects are strongest where public schools' starting performance is lowest and there is much room for growth. There may also be locales where private school choice is welcomed by the public schools, instead of being viewed as a threat, as it is seen as a release valve for students who cannot be well served in their assigned school because of overcrowding or insufficient resources.

Third, what other methodologies are available to study this phenomenon? We note that the bulk of prior studies on this subject have operationalized competition by using a geocoded measure such as the crow's flight distance between a public and voucher-accepting private school. Rather than relying upon this Euclidean-distance measure, we use the actual drive-time between two points, accounting for road lengths, intersection turn times, speed limits, and so on. These data were collected by millions of anonymous cell phones and vehicle sensors and allow us to calculate precise travel time in minutes. We argue drive-time represents an improvement over the crows-flight distance between two points because it accounts for all manner of topographical, traffic, and social differences that can make the perceived distance between two points feel larger or smaller than the straight-line distance expressed in miles might otherwise communicate.

Fourth, might variation in local education policies play a role in influencing competitive impacts? It could be the case that competitive effects are strongest when choice is less constrained, such as when charter school caps are lifted or limits on the size of a voucher program are removed. Only by drawing on data from multiple states can researchers begin to identify patterns in the variation in findings. Data from an additional state beyond those listed in Table 2 would be an improvement. Given that Indiana's program is the nation's largest voucher program, a competitive effects analysis of the ICSP provides a great opportunity to shed light on the systemic effects of a school choice program in a previously unstudied state.

A final outstanding question that has not yet been addressed by the competitive effects literature relates to the concept of a tipping point, which we define as the level at which the penetration of private schools reaches sufficient saturation to prompt a public school response. We argue that before this point is reached, public schools can more easily dismiss the private school transfer option as a legitimate threat to their anticipated student enrollment, squashing any efforts to enact meaningful changes in response. To explore this possibility, we test a variety of thresholds to explore whether or not a tipping point exists in Indiana's context.

Research Methodology

Research Questions

We address the following three research questions:

1. Does increased private school competition predict changes in student achievement and graduation at a traditional public school?
2. Are there heterogeneous impacts by student and school subgroups?
3. Is there a tipping point at which increases in student transfers by way of the ICSP predict changes in student achievement at nearby traditional public schools?

Data

The data for this project come from two sources. First, student-level data on traditional public school students' graduation status and math and English Language Arts (ELA) performance on the state test—the ISTEP+—were provided by the Indiana Department of Education and cover school years 2010–11 to 2015–16. The ISTEP+ is administered annually in grades 3 through 8 and used for school and student accountability purposes. Test scores are standardized within grade and year to have a mean of zero and standard deviation of one. Physical addresses for all Indiana public and ICSP schools were also provided by the Indiana Department of Education. Using the Maptitude software package, we calculate the expected drive-time from each public school to every voucher program-participating private school. These data then allow us to subsequently calculate drive-time to the closest private school or the count of private schools within a given drive-time cap. This measure incorporates road lengths, intersection turn times, and speed limits to calculate precise travel time in minutes. For purposes of our analyses, we assume average driving speeds for all road types.[5] We also limit travel times to no more than 90 minutes, since an estimated 98 percent of the state's population has a work commute of less than 90 minutes (U.S. Census Bureau, 2015). Furthermore, prior school choice survey research notes that fewer than three percent of parents utilizing any form of public or private school choice are willing to commute for more than one hour (Catt & Rhinesmith, 2017).

Drive-time Measure

Distance to school is consistently shown to play an important role in families' school decisions (Harris & Larsen, 2017). The majority of previous school choice competitive effect studies use some measure of physical distance to school to account for this fact, but distance is typically operationalized in a simplistic way as the Euclidean distance between two points (e.g., Egalite & Mills, 2019; Figlio & Hart, 2014; Misra, Grimes, & Rogers, 2012), which ignores the psychological aspect of the distance between two points. That is, Euclidean distance may not accurately reflect a parent's perception of how near or far a school actually is from home, especially where there is notable variation in traffic patterns across regions that affects the total time spent in a car when transitioning between school and home. If this is true of parents, it likely is also true of schools. Therefore, we use a measure of competitive pressure based on actual drive-time between schools, which we argue is superior to the alternative available measures because it presents a more accurate reflection of the relative distance between schools.

Empirical Approach

We adopt a two-part empirical approach to address our three research questions, first testing for a competitive impact in the program's initial implementation year, then testing in all post-policy years. The difference-in-differences approach is powerful in this setting as it does not require random assignment for causal inference, just the implementation of a school choice policy that differentially impacts different groups of schools. Finally, the reader should note that charter schools are excluded from the analysis to

[5] We were unable to specify a departure time in Maptitude that would approximate times of day students travel to or from school; the drive-time tool uses historical traffic conditions (calculated using billions of anonymous measurements from cell phones and vehicle sensors) to determine the speeds used.

allow us to more cleanly estimate the competitive pressure experienced by district-operated public schools.

Testing for a Competitive Impact in the First Year of the ICSP

In the first part of our analysis, a school fixed effects model is employed in a difference-in-differences framework to estimate the effect of private school competition on public school performance in the program's first year of operation, similar to the model estimated by Figlio and Hart (2014). The first difference is between schools facing differing levels of competitive pressure, the second difference is time. The intuition behind the model is that schools with low levels of competitive pressure from private schools form a useful counterfactual for schools with high levels of competition, after accounting for fixed differences between the schools and common time effects.

$$\mathbf{Y}_{ist} = \boldsymbol{\beta}_1 \boldsymbol{C}_s * \boldsymbol{P}_t + \boldsymbol{\beta}_2 \mathbf{X}_{it} + \boldsymbol{\beta}_3 \boldsymbol{S}_{st} + \boldsymbol{\alpha}_s + \boldsymbol{\beta}_1 \boldsymbol{\gamma}_t + \boldsymbol{\epsilon}_{ist} \qquad \textbf{[1]}$$

Y_{ist} is the average standardized math or reading score for student *i* in school *s,* in year *t*; α_s is a school fixed effect; C_s is the measure of pre-policy competitive pressure facing school *s* (i.e., the drive-time to the nearest private school or the count of ICSP-participating private schools within a drive-time based radius). Because C is measured a priori, its value is constant within schools and therefore it does not need to be entered into the model separately as a standalone variable. P_t is an indicator variable identifying the post-policy year, 2011-12; X_i is a vector of student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education); S_{st} is a vector of time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers)[6]; γ is a year fixed effect, and ϵ is an idiosyncratic disturbance term. The β_1 coefficient on the two-way-interaction of the competition measure and the post-policy indicator is the parameter of interest. Robust standard errors are clustered at the school level. The key identifying assumption underlying this research design is that public

---

[6] A case could be made for excluding principal gender and principal race as control variables from these models as these factors are under the control of the traditional public school and could be endogenous if affected by the competitive threat, such as a school that previously prioritized hiring principals of a certain race and/or gender now selecting the most promising candidate, irrespective of race and/or gender. The same logic could be true of the share of teachers who are first year teachers. We test this theory by running the models with and without these control variables and find little change to the results.

schools with greater or lesser competitive pressure would have trended similarly in the absence of state-funded tuition vouchers.

As a follow-up to this analysis, we also generate a version of the drive-time variable that only counts drive-time to a private school that meets an enrollment threshold (e.g., drive-time to nearest private school that has accepted 10+ choice students). Those results are presented separately.

Testing for a Competitive Impact in all Post-Policy Years

It may be the case that first year impacts were muted because of the program's small size at that time or because traditional public schools assumed the state supreme court would rule against the ambitious voucher program, which was the first program of its type to target middle-income families for vouchers. Fortunately, because we have data from 2010–11 through 2015–16, we are not restricted to looking for impacts in the first year alone. Multi-year results will be presented separately. Using this larger sample also allows us to estimate a dynamic model that accommodates the possibility of time-varying treatment effects. To accomplish this, we change the model specification in the following way:

$$\mathbf{Y}_{ist} = \sum_{\tau=0}^{2+} \boldsymbol{\beta}_{\tau}\, \boldsymbol{C}_{s} * \boldsymbol{P}_{t+\tau} + \boldsymbol{\beta}_{2}\mathbf{X}_{it} + \boldsymbol{\beta}_{3}\boldsymbol{S}_{st} + \boldsymbol{\alpha}_{s} + \boldsymbol{\gamma}_{t} + \boldsymbol{\epsilon}_{ist} \qquad [2]$$

τ allows the policy to have separate effects in the immediate post-policy year (2011-12), one year after initial adoption (2012-13), and two or more years after adoption (2013-14, 2014-15, or 2015-16).

We argue our empirical approach avoids endogeneity bias in two primary ways. First, we ensure we are focusing on private schools that made location decisions many years ago by mapping private school location from the pre-policy period. Second, our analysis focuses on a period of dramatic and sudden expansion in student participation in the voucher program. This means it is unlikely that private schools had time to respond to the program by establishing or moving to a new location in such a short time frame.

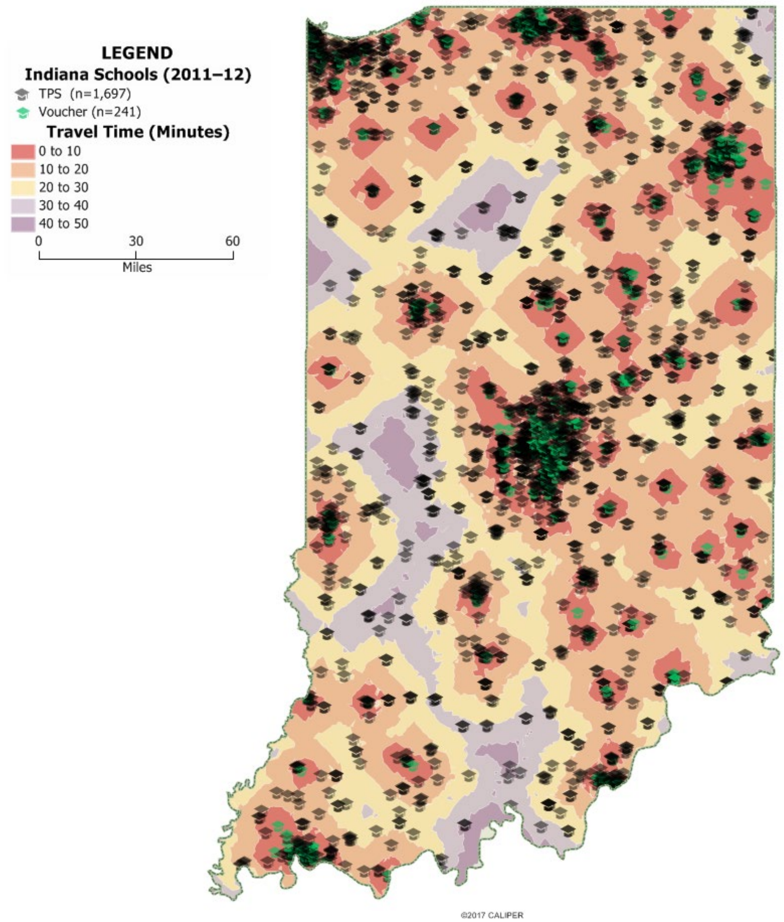
Mapping Procedure

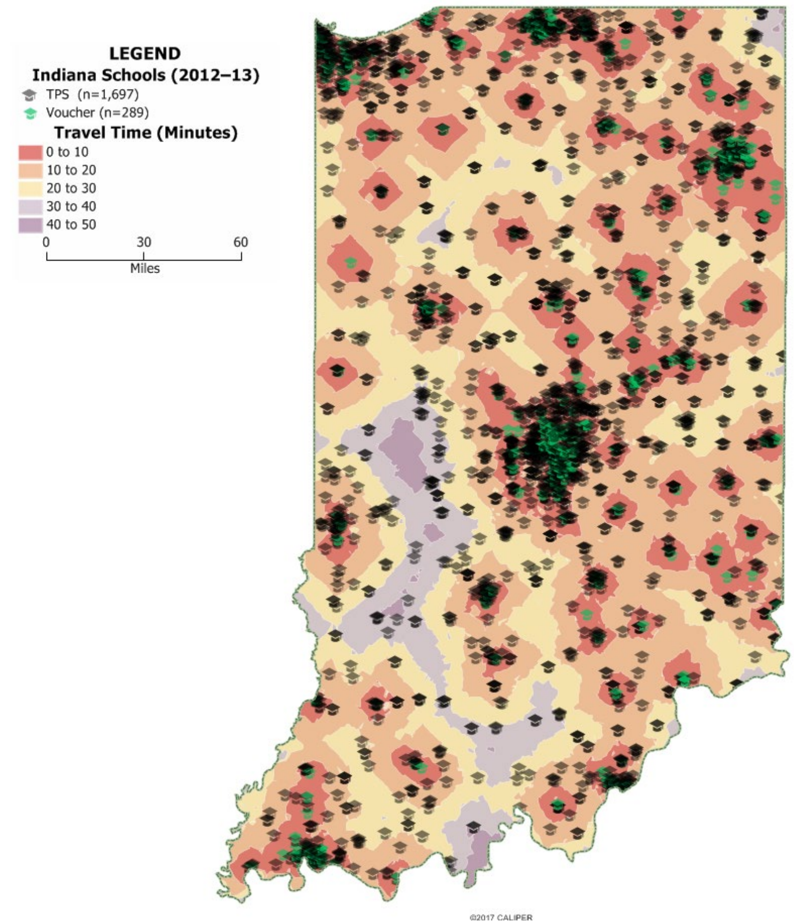
We use precise drive-time measures to plot the distance between a traditional public school and the nearest private school competitor enrolling ICSP students, accounting for variables such as traffic patterns, road types, and intersection turn times.

After an initial attempt to geolocate all 2,178 Indiana public schools and voucher-accepting private schools (henceforth, ICSP schools) using the software, Maptitude, 346 addresses were modified in order to be recognized by the software. This involved replacing P.O. box addresses with physical addresses, changing “State Rd.” to “SR,” changing “Co. Road” to “CR,” and so on. There were ten addresses the software still failed to recognized that were replaced by latitude and longitude coordinates identified from Google Maps. Thus, we successfully geolocated 100 percent of the 1,697 traditional public schools and the 342 ICSP schools in our sample. We then created distance and travel time matrices between all traditional public schools and ICSP schools for each school year under study.

For visualization purposes, not analytic, we start by creating drive-time rings in five increments of 10 minutes from each ICSP school for 2011–12 and 2012–13 (Figure 1). Although the rings in Figure 1 are not used in the analysis—which instead relies on the continuous drive-time measure for more precision—they permit identification of geographic areas in Indiana where there are traditional public schools that do not have any participating private schools located within 30 minutes of their location. When comparing the two figures, it is also easy to spot those geographic areas experiencing change over time. For example, there are areas where the traditional public schools did not have any close ICSP schools in 2011–12 but then did have at least one of the 58 additional ICSP schools in 2012–13.

Figure 1. Drive-Time Rings from Indiana Voucher Schools to Traditional Public Schools, 2011–12 and 2012-13





Sample Description

The analysis sample contains 2,420,574 observations of 837,797 unique students from 2010-11 to 2015-16 (Table 3). Approximately three-quarters (74 percent) of students are white, 10 percent are Black, and 10 percent are Hispanic. Almost half (47 percent) are low-income, 16 percent of students are designated as having special educational needs, and 6 percent have limited English proficiency.

There are 1,403 unique traditional public schools in our sample, with an average enrollment of 503 students. Over half of schools (55 percent) have a male principal and 91 percent of all principals are white, compared to just seven percent Black and one percent Hispanic. Teacher demographic characteristics largely mirror those of principals, with the average value of the teacher percent white variable at 94 percent, teacher percent black at just 4 percent, and teacher percent Hispanic at 1 percent. On average, 5 percent of teachers are in their first year of teaching. Charter schools are excluded from the analysis as they are not the population of interest for this particular study.

Table 3 also presents descriptive statistics of the competition measures on which we rely. The average drive-time to the nearest private school competitor is 8.54 mins, the average count of private school competitors within 90 minutes is 104 schools, the average count of private school competitors within 30 minutes is 23 schools. Finally, the competitor count within 20 and 10 minutes is 11 and three schools, respectively. Histograms of the drive-time and competitor count measures are presented in Figures 2 and 3.

Table 3. Descriptive Characteristics of Analysis Sample, Math and English Language Arts Outcomes

	Mean	SD	Min	Max
Student Characteristics				
Female	0.49	0.50	0.00	1.00
White	0.74	0.44	0.00	1.00
Black	0.10	0.30	0.00	1.00
Hispanic	0.10	0.30	0.00	1.00
Other Race	0.07	0.25	0.00	1.00
Low-Income	0.46	0.50	0.00	1.00
Special Education	0.16	0.36	0.00	1.00
Limited English Proficiency	0.06	0.24	0.00	1.00
Traditional Public School Characteristics				
Enrollment	503.10	320.33	37.00	1,786.00
Percent White	0.74	0.25	0.00	1.00
Percent Black	0.10	0.17	0.00	0.98
Percent Hispanic	0.10	0.12	0.00	0.81
Percent Other Race	0.07	0.05	0.00	0.47
Percent Low-Income	0.48	0.22	0.01	1.00
Principal Male	0.55	0.50	0.00	1.00
Principal White	0.92	0.28	0.00	1.00
Principal Black	0.07	0.25	0.00	1.00
Principal Hispanic	0.01	0.10	0.00	1.00
Principal Other Race	0.01	0.08	0.00	1.00
Teacher Percent Male	0.23	0.13	0.00	0.67
Teacher Percent White	0.94	0.13	0.00	1.00
Teacher Percent Black	0.04	0.12	0.00	1.00
Teacher Percent Hispanic	0.01	0.03	0.00	0.57
Teacher Percent Other Race	0.01	0.01	0.00	0.17
Count of Voucher Students in All Private Schools Within 10 Mins' Drive (2012)	33.56	60.51	0.00	416.00
Count of Voucher Students in All Private Schools Within 20 Mins' Drive (2012)	156.32	221.30	0.00	1186.00
Count of Voucher Students in All Private Schools Within 30 Mins' Drive (2012)	572.59	401.95	0.00	1353.00
Count of Voucher Students in All Private Schools Within 90 Mins' Drive (2012)	1309.35	572.59	94.00	2565.00
Competition Measures				
Drive-time (mins)	8.54	7.61	0.02	42.35
Drive-time (10+ Voucher Students)	16.43	13.23	0.02	77.89
Competitor School Count (90 mins)	103.61	39.38	15.00	176.00
Competitor School Count (30 mins)	22.88	20.69	0.00	77.00
Competitor School Count (20 mins)	10.47	10.83	0.00	53.00

Competitor School Count (10 mins)	2.46	2.90	0.00	16.00

Notes: n = 2,420,574 observations, 837,797 unique students, 1,403 unique schools; 'Competitor school count' refers to the number of participating private schools within 90 mins' drive of a given public school.

Figure 2. Histogram of Drive-Time Measure

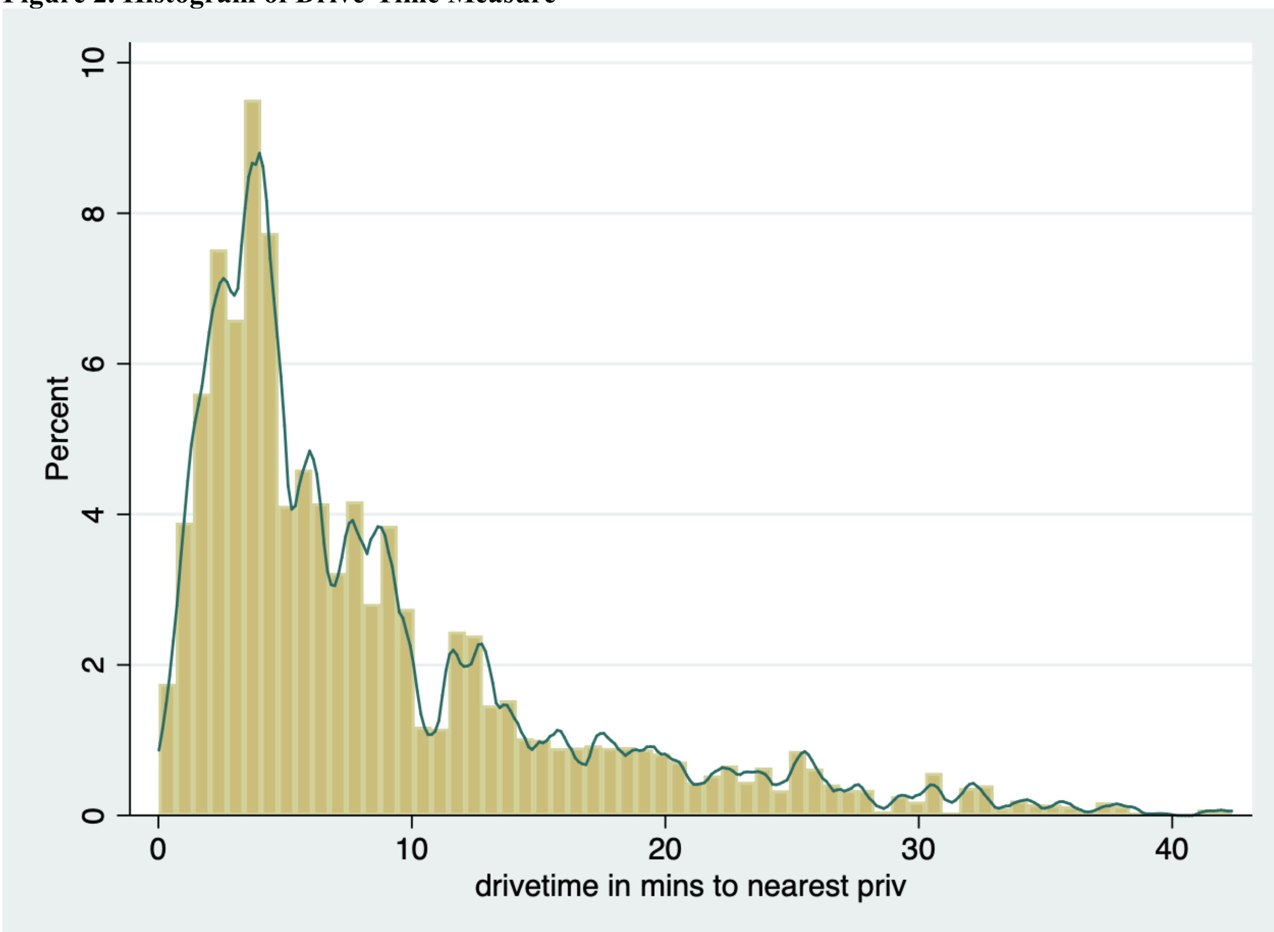
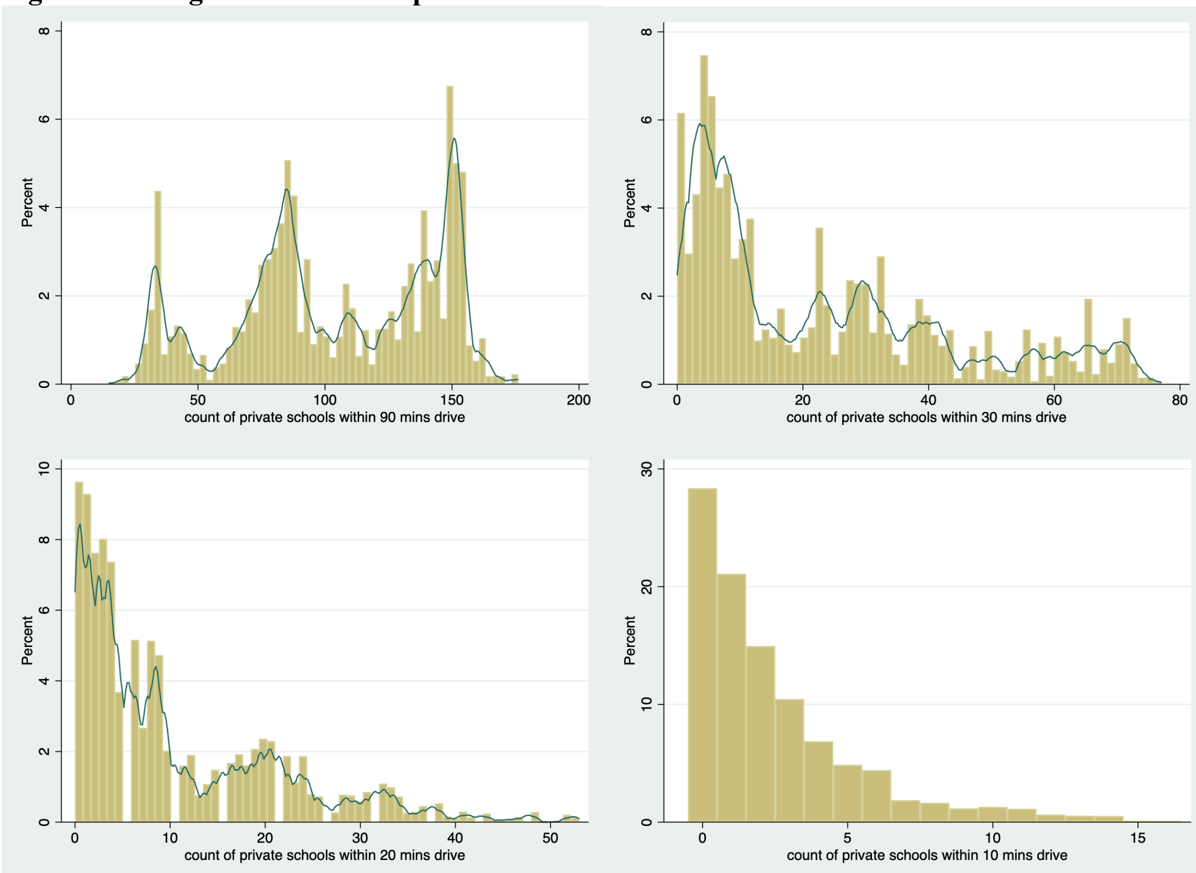


Figure 3. Histograms of the Competitor Count Measures



Results

Findings from Analysis of Test Score Impacts

We first present the findings of the impact of competition on test scores of students in traditional public schools, then discuss the estimates of the effects of competition on graduation rates. Table 4 presents estimates of the effects of private school competition on public school students' math and English Language Arts achievement in the first year (Columns 1, 3) and in all post-policy years (Columns 2, 4) for the full sample of students. For the drive-time model results presented in the first row, we report null effects in math and a very small positive effect in English Language Arts, significant at $p < .10$. Across multiple model specifications, we never uncover a negative effect on math or English Language Arts achievement.

Table 4. Effects of Private School Competition on Public School Achievement, Math and ELA

	(1)	(2)	(3)	(4)
Subject	Math		ELA	
Outcome Year	First Post-Policy Year	All Post Policy Years	First Post-Policy Year	All Post Policy Years
Competition Measure				
A: Drive-time				
Drive-time	-0.0001	0.0007	0.0006	-0.0003
	(0.0004)	(0.0005)	(0.0004)	(0.0004)
B: Private School Density				
Competitor School Count (90 mins)	0.0002**	0.0003**	0.0003***	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Competitor School Count (30 mins)	0.0002	0.0002	0.0002	-0.0000
	(0.0002)	(0.0002)	(0.0001)	(0.0002)
Competitor School Count (20 mins)	0.0004	0.0003	0.0002	-0.0001
	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Competitor School Count (10 mins)	0.0007	0.0010	0.0017*	0.0006
	(0.0012)	(0.0016)	(0.0010)	(0.0011)
R-squared	0.2177	0.2177	0.2427	0.2427

Notes: These coefficients represent the interaction between the measure of competition and the post-policy years: school years ending in 2012 (the first post-policy year), 2013, 2014, 2015, and 2016. To ease interpretation, drive-time has been reverse-coded. 'Competitor school count' refers to the number of participating private schools within 90 mins' drive of a given public school. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch and an indicator if that share exceeds 40 percent after 2014–15, triggering the Community Eligibility Provision; principal gender and race/ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Robust standard errors clustered by school in parentheses. N = 2,420,574. *** p<0.01, ** p<0.05, * p<0.10

In what follows, we discuss the findings associated with each of the competition measures employed. We also discuss the findings associated with the dosage analysis.

Drive-time to Nearest Private School as a Measure of Competition

A decrease in the drive-time to the nearest private school competitor is associated with no change in public school math achievement in the years after the policy went into effect. Looking at English Language Arts achievement, a ten-minute decrease in the drive-time to the nearest private school competitor is associated with a 0.007 standard deviation (SD) increase in public school student achievement in the year after the policy went into effect.[7] Although the finding is statistically significant, this is a very small effect. A ten-minute decrease in drive-time is associated with an increase in English Language Arts achievement of less than one-percent of a standard deviation, which is smaller than a one-percentile change. To make sure this finding isn't being driven by our choice of competition measure, we also use the drive-time data to generate alternative measures of competition, including a private school density measure we call the "Competitor School Count." This represents the count of private schools within a given drive-time radius of a public school. Findings from these models are consistent with the drive-time results already discussed.

Competitor School Count as a Measure of Competition

Examining public school responses to the competitor school count in 90-, 30-, 20-, and 10-minute radii, we find essentially null effects. That is, although some specifications reveal a statistically significant, positive effect, it is too small to rule out a spurious relationship. For example, an increase of one private school competitor within a 90-minute radius of a given public school is associated with a 0.0002 SD increase in math achievement and a 0.0003 SD increase in English Language Arts achievement in the first year after the policy went into effect. Examining impacts across all post-policy years, the math effect remains statistically significant at 0.0003 SD whereas the English Language Arts effect is essentially zero. In sum, increased private school competition is associated with an increase in student test scores of zero to approximately one percent of a standard deviation, depending on the particular measure used.

Results of Tipping Point Analysis

It might be the case, however, that only private schools receiving a critical mass of voucher students actually represent a tangible competitive "threat" to the traditional public schools in that area. To test this dosage theory, we check if there is a change in public school students' achievement in the first post-policy year that is associated with a reduction in the drive-time distance to the nearest private school that has accepted a critical mass of voucher students—a minimum of 10, 20, 30, 40, or 50 voucher students (Table 5). At the lowest threshold of 10 voucher students, we observe a statistically significant positive effect on English Language Arts achievement. Specifically, a ten-minute decrease in the drive-time to a private school with at least ten voucher students is associated with an increase in public school

[7] To ease interpretation, we multiply coefficients by ten to get the change in achievement associated with a ten-minute decrease in drive-time.

achievement of 0.006 SD in English Language Arts (i.e., approximately half of one percent of a standard deviation), which is a very small but statistically significant positive effect. The effect at higher concentrations of voucher students remains similar in both magnitude and direction, suggesting there is not a strong dosage effect at play. Thus, it appears that the private school threat elicits a small positive change in public school achievement after the voucher program is enacted but the public school response is not mediated by the perceived size of the competitive threat, at least in terms of voucher student count.

Table 5. Dosage Effects of Private School Competition on TPS Achievement, Math and ELA

	(1)	(2)
Subject	Math	ELA
Drive-time to Private School with 10+ Voucher Students	0.0001	0.0006***
	(0.0003)	(0.0002)
Drive-time to Private School with 20+ Voucher Students	-0.0002	0.0001
	(0.0002)	(0.0001)
Drive-time to Private School with 30+ Voucher Students	-0.0000	0.0001
	(0.0002)	(0.0001)
Drive-time to Private School with 40+ Voucher Students	0.0002	0.0001
	(0.0002)	(0.0001)
Drive-time to Private School with 50+ Voucher Students	0.0002	0.0001
	(0.0002)	(0.0001)
R-squared	0.2175	0.2424

Notes: These coefficients represent the interaction between the measure of competition and the first post-policy year: school year ending in 2012. Drive-time is reverse-coded for ease of interpretation. To ease interpretation, coefficients can be multiplied by 10 to represent the achievement change associated with a 10-minute decrease in drive-time. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch and an indicator if that share exceeds 40 percent after 2014–15, triggering the Community Eligibility Provision; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Standard errors clustered by school in parentheses. n = 2,385,571

When interpreting these findings, it is helpful to note the jump in student participation in Year 3, which may have represented a sea change for traditional public schools who had been casually observing up to that point how eligible families were responding to the voucher option. Specifically, student enrollment more than doubled from Year 1 to Year 2, increasing from 3,911 to 9,139 students and then jumped to 19,809 students by Year 3. Given the dramatic increase in student participation over time due to the expansion and then removal of participation caps, we might expect to observe different effects for each of the outcome years examined in this analysis. In particular, Year 3 may be an inflection point at which it became clear to traditional public schools that the voucher program was a legitimate threat that was now beginning to grow at a substantially faster rate. Also relevant context is that the Republican candidate for governor and a supporter of the state's voucher program, Congressman Mike Pence, won the gubernatorial election in November 2012, signaling to voucher opponents that political support for the school choice program would continue. Finally, it may also be the case that traditional public schools needed multiple years to respond to the voucher "threat" and the initial improvements that had taken root in earlier years took until Year 3 to bear fruit.

Figure 4 documents impact estimates for each outcome year separately. It is interesting to note the positive and statistically significant increase in math scores that occurred in Year 3, which is consistent with the hypothesis that the competitive effect became most salient in this year. In contrast, the effects for English Language Arts are statistically equivalent to zero in all outcome years.

Figure 4. Competitive Effects by Individual Outcome Years, Math and ELA, Drive-time Measure

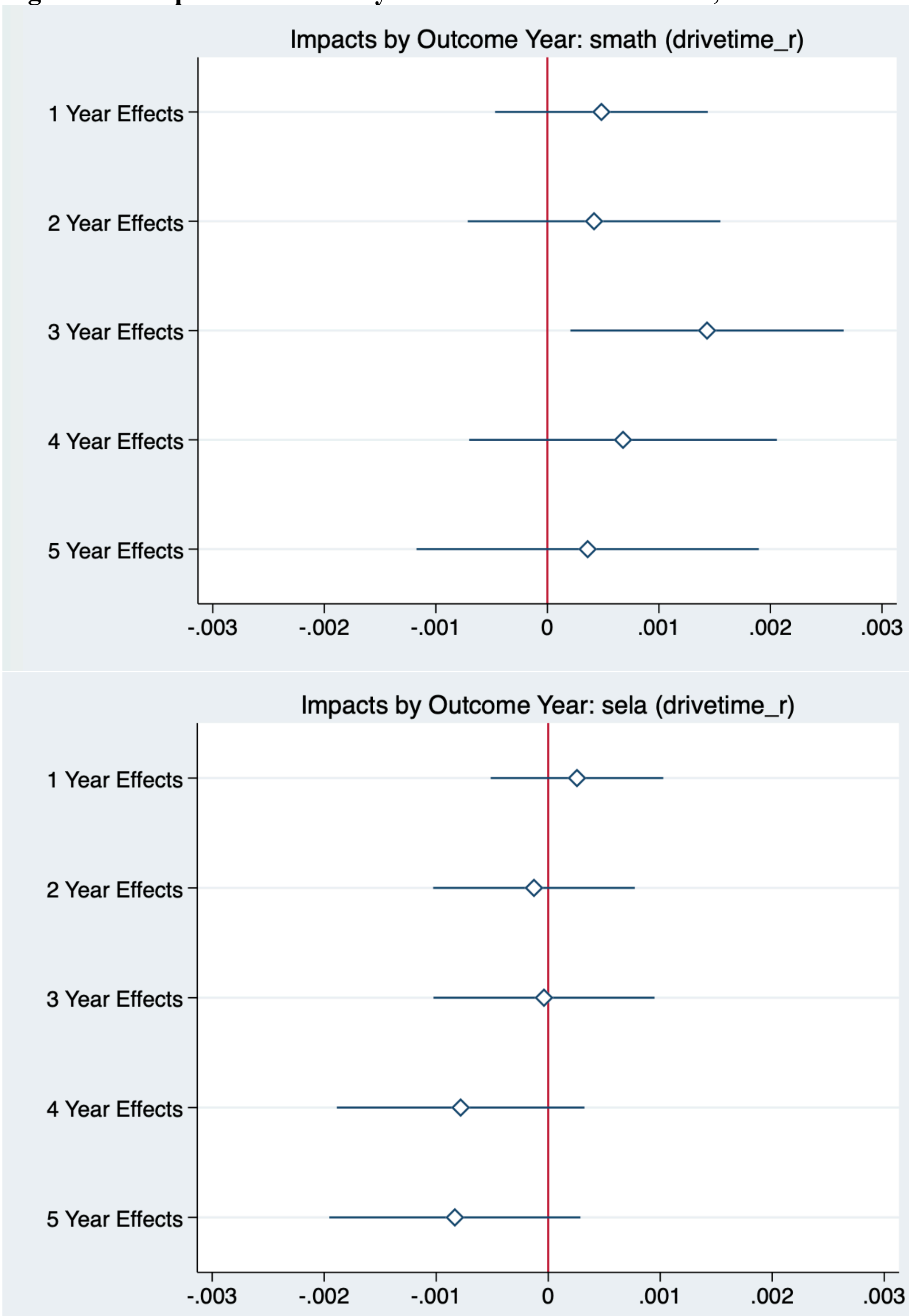
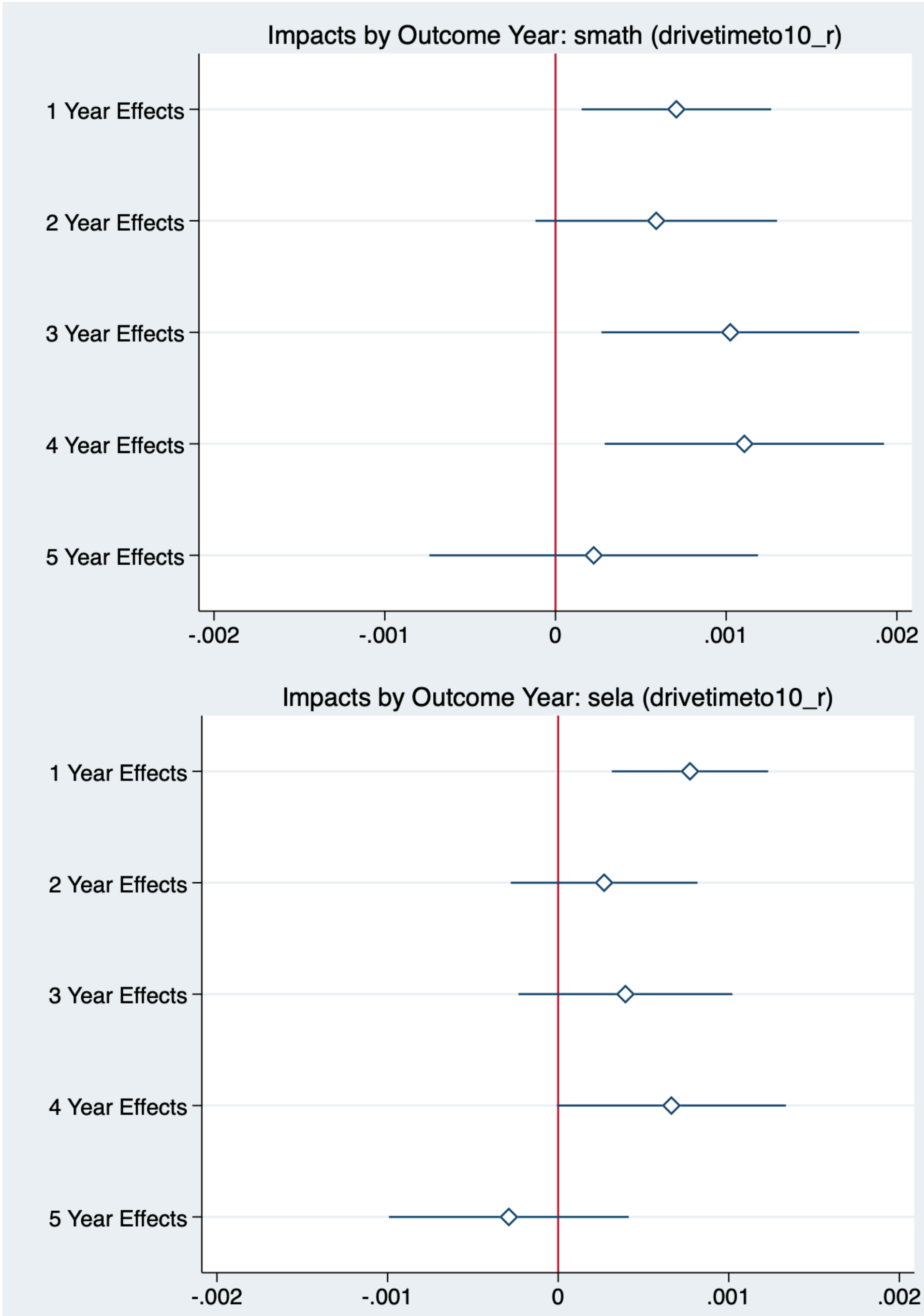


Figure 5 documents a similar pattern using the dosage analysis, which examines the effect of drive-time to the nearest private school with 10, 20, 30, 40, or 50 voucher students.[8]

Figure 5. Competitive Effects by Individual Outcome Years, Math and ELA, Drive-time to Nearest Private School with at Least 10 Voucher Students



[8] We also repeat this analysis using the competitor count variable measured in 90, 30, 20, and 10- minute radii. The effects for individual outcome years are zero in the majority of cases. Any small positive or negative individual year effects never exceed one percent of a standard deviation. Results are available by request.

Subgroup Findings, Achievement Analysis

Thus far, we have identified neutral to small positive overall responses to private school competition after Indiana's voucher bill became law. We now check for subgroup effects. The public school student subgroups examined are female, white, black, Hispanic, low-income, special education, limited English proficient, and elementary-aged students (Table 6). Interpreting the model that incorporates all available years of data, which is our preferred specification, we observe positive math effects associated with the drive-time measure for low-income and special education students. Specifically, a ten-minute decrease in drive-time to the nearest private school competitor is associated with an achievement increase of 0.012 (approximately one percent of a standard deviation) for low-income students and 0.021 (approximately two percent of a standard deviation) for students with special needs, which is an interesting finding given that these are two subgroups of students specifically targeted by the voucher program's eligibility rules.[9] Still looking at math outcomes, the count of private school competitors within a 90-minute drive is associated with increases in public school achievement for the following student subgroups: female, white, Hispanic, low-income, special-education, limited English proficient, and elementary-aged students.

[9] The ten-minute increment is used here as it is more intuitive than a one-minute change in drive-time. Adjustments of this nature are commonly used to show the marginal effects of continuous variables. For reference, 10 minutes is less than one standard deviation of drive-time (SD = 7.63).

Table 6. Effects of Private School Competition on Public School Achievement, Student Subgroups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female	White	Black	Hispanic	Low-Income	Special Education	LEP	Elementary Grades
Math, First Post-Policy Year								
Drive-time	-0.0002	0.0004	-0.0046	-0.0011	-0.0007	-0.0002	-0.0027*	0.0005
	(0.0004)	(0.0004)	(0.0032)	(0.0011)	(0.0005)	(0.0008)	(0.0015)	(0.0006)
Competitor Sch Count (90 mins)	0.0001	0.0002**	-0.0001	0.0002	0.0001	0.0002	0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Competitor Sch Count (30 mins)	0.0002	0.0004**	0.0002	0.0006	0.0000	0.0004	0.0003	0.0003
	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0002)	(0.0003)	(0.0005)	(0.0002)
Competitor Sch Count (20 mins)	0.0002	0.0007**	0.0006	0.0014**	0.0002	0.0010*	0.0009	0.0009*
	(0.0003)	(0.0004)	(0.0008)	(0.0006)	(0.0004)	(0.0006)	(0.0008)	(0.0004)
Competitor Sch Count (10 mins)	0.0002	0.0017	-0.0005	0.0040*	0.0004	0.0030	0.0042	0.0026*
	(0.0013)	(0.0014)	(0.0025)	(0.0023)	(0.0013)	(0.0022)	(0.0031)	(0.0016)
Math, All Post-Policy Years								
Drive-time	0.0008	0.0001	0.0085	0.0004	0.0012**	0.0021**	-0.0007	0.0003
	(0.0005)	(0.0005)	(0.0054)	(0.0013)	(0.0006)	(0.0010)	(0.0020)	(0.0007)
Competitor Sch Count (90 mins)	0.0003**	0.0002**	0.0004	0.0007***	0.0003**	0.0004**	0.0007**	0.0006***
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Competitor Sch Count (30 mins)	0.0002	-0.0000	0.0003	0.0002	0.0004	0.0005	-0.0003	0.0003
	(0.0002)	(0.0002)	(0.0005)	(0.0004)	(0.0003)	(0.0004)	(0.0006)	(0.0003)
Competitor Sch Count (20 mins)	0.0004	-0.0002	0.0007	-0.0002	0.0007	0.0011	-0.0017*	-0.0001
	(0.0004)	(0.0005)	(0.0010)	(0.0007)	(0.0005)	(0.0008)	(0.0009)	(0.0005)
Competitor Sch Count (10 mins)	0.0013	-0.0004	0.0030	-0.0040	0.0016	0.0055*	-0.0094**	-0.0014
	(0.0017)	(0.0017)	(0.0041)	(0.0026)	(0.0018)	(0.0030)	(0.0038)	(0.0019)
ELA, First Post-Policy Year								
Drive-time	0.0004	0.0011***	-0.0010	-0.0015	-0.0001	-0.0001	-0.0029*	0.0009*
	(0.0004)	(0.0004)	(0.0019)	(0.0011)	(0.0004)	(0.0008)	(0.0015)	(0.0005)
Competitor Sch Count (90 mins)	0.0002***	0.0003***	-0.0000	0.0002	0.0002**	0.0002	0.0002	0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Competitor Sch Count (30 mins)	0.0001	0.0006***	0.0001	0.0002	0.0000	0.0004	-0.0003	0.0004**
	(0.0001)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)	(0.0004)	(0.0002)
Competitor Sch Count (20 mins)	0.0000	0.0010***	0.0003	0.0004	0.0000	0.0009*	-0.0005	0.0007**
	(0.0003)	(0.0003)	(0.0005)	(0.0005)	(0.0003)	(0.0005)	(0.0008)	(0.0003)
Competitor Sch Count (10 mins)	0.0011	0.0038***	0.0005	0.0031	0.0011	0.0037*	0.0018	0.0033***

	(0.0010)	(0.0012)	(0.0018)	(0.0019)	(0.0010)	(0.0020)	(0.0030)	(0.0012)
ELA, All Post-Policy Years								
Drive-time	-0.0003	-0.0009**	0.0053	0.0009	0.0004	0.0006	0.0012	-0.0003
	(0.0004)	(0.0004)	(0.0033)	(0.0010)	(0.0005)	(0.0008)	(0.0016)	(0.0005)
Competitor Sch Count (90 mins)	0.0001	-0.0000	0.0004*	0.0005***	0.0002*	0.0000	0.0003	0.0003**
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Competitor Sch Count (30 mins)	-0.0001	-0.0005***	0.0001	0.0004	0.0003	-0.0000	0.0002	0.0000
	(0.0002)	(0.0002)	(0.0004)	(0.0003)	(0.0002)	(0.0003)	(0.0004)	(0.0002)
Competitor Sch Count (20 mins)	-0.0001	-0.0006*	-0.0001	0.0000	0.0004	0.0002	-0.0011	-0.0002
	(0.0003)	(0.0004)	(0.0007)	(0.0005)	(0.0003)	(0.0006)	(0.0007)	(0.0004)
Competitor Sch Count (10 mins)	0.0006	-0.0006	0.0025	-0.0011	0.0015	0.0021	-0.0046*	-0.0010
	(0.0011)	(0.0014)	(0.0029)	(0.0019)	(0.0012)	(0.0025)	(0.0028)	(0.0013)
Observations	1,191,407	1,779,735	236,939	241,088	1,142,533	383,648	148,886	1,221,057
R-squared	0.2258	0.2060	0.2104	0.2396	0.1896	0.1456	0.1229	0.2454

Notes: These coefficients represent the interaction between the measure of competition and an indicator for the post-policy year(s). Drive-time is reverse-coded for ease of interpretation. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch and an indicator if that share exceeds 40 percent after 2014–15, triggering the Community Eligibility Provision; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.10

Turning next to the student subgroup findings for English Language Arts achievement under our preferred specification, we observe small negative impacts for white students of -0.001 SD under the drive-time measure (scaled for a ten-minute reduction instead of one-minute) and null effects for other student subgroups. Using the competitor school count with a 90 min drive-time radius, we observe positive impacts for Hispanic students (0.0005 SD) and elementary-aged students (0.0003 SD).

Table 7. Dosage Effects of Private School Competition on Public School Achievement, Student Subgroups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female	White	Black	Hispanic	Low-Income	Special Education	LEP	Elementary Grades
Math								
Drive-time to 10+	0.0007**	0.0004	0.0042**	0.0002	0.0012***	0.0023***	-0.0007	0.0004
	(0.0003)	(0.0003)	(0.0020)	(0.0008)	(0.0004)	(0.0007)	(0.0010)	(0.0004)
Drive-time to 20+	0.0007***	0.0005**	0.0013***	0.0007	0.0009***	0.0017***	0.0001	0.0009***
	(0.0002)	(0.0002)	(0.0005)	(0.0005)	(0.0002)	(0.0004)	(0.0007)	(0.0003)
Drive-time to 30+	0.0005**	0.0005**	0.0006	-0.0006	0.0006**	0.0014***	-0.0012**	0.0006**
	(0.0002)	(0.0002)	(0.0005)	(0.0004)	(0.0002)	(0.0004)	(0.0006)	(0.0003)
Drive-time to 40+	0.0003	0.0001	0.0002	-0.0010**	0.0004	0.0009**	-0.0016**	0.0003
	(0.0002)	(0.0002)	(0.0007)	(0.0005)	(0.0003)	(0.0004)	(0.0007)	(0.0003)
Drive-time to 50+	0.0004*	0.0003	-0.0001	-0.0009*	0.0005*	0.0011**	-0.0015**	0.0004
	(0.0002)	(0.0002)	(0.0006)	(0.0004)	(0.0003)	(0.0004)	(0.0006)	(0.0003)
Drive-time to 100+	-0.0005	-0.0005	-0.0017**	-0.0008	-0.0008*	-0.0006	-0.0017***	-0.0001
ELA								
Drive-time to 10+	0.0004	0.0001	0.0036**	0.0003	0.0009***	0.0014***	-0.0007	0.0003
	(0.0002)	(0.0003)	(0.0014)	(0.0006)	(0.0003)	(0.0006)	(0.0009)	(0.0003)
Drive-time to 20+	0.0004**	0.0001	0.0005	0.0005	0.0005***	0.0008**	0.0002	0.0004**
	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0002)	(0.0004)	(0.0006)	(0.0002)
Drive-time to 30+	0.0002	0.0000	-0.0002	-0.0006*	0.0003	0.0007*	-0.0006	0.0002
	(0.0002)	(0.0002)	(0.0004)	(0.0003)	(0.0002)	(0.0003)	(0.0005)	(0.0002)
Drive-time to 40+	0.0001	-0.0002	-0.0005	-0.0008**	0.0001	0.0003	-0.0007	0.0002
	(0.0002)	(0.0002)	(0.0006)	(0.0004)	(0.0002)	(0.0004)	(0.0006)	(0.0002)
Drive-time to 50+	0.0002	-0.0001	-0.0007	-0.0007**	0.0002	0.0004	-0.0007	0.0002
	(0.0002)	(0.0002)	(0.0005)	(0.0004)	(0.0002)	(0.0004)	(0.0006)	(0.0002)
Observations	1,174,054	1,754,107	233,207	237,600	1,124,118	377,529	146,872	1,209,532
R-squared	0.2163	0.1770	0.1599	0.1762	0.1466	0.1452	0.1476	0.2144

Notes: These coefficients represent the interaction between the measure of competition and the post-policy years. Drive-time is reverse-coded for ease of interpretation. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch and an indicator if that share exceeds 40 percent after 2014–15, triggering the Community Eligibility Provision; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.10

We also check for dosage effects among these student subgroups (Table 7). As before, no clear patterns emerge, indicating that the overall perceived threat of private school proximity is not differentiated by the number of voucher students attending nearby private schools in the post-policy years.

Thus far, we have examined the achievement outcomes for specific student subgroups within the public school sample. It may be the case, however, that the more relevant subgroup is at the school level, not the student level. That is, the voucher program may be more likely to elicit a competitive response from certain types of public schools defined by the populations they serve, such as schools serving large proportions of low-income students or students of color, many of whom qualify for the voucher program. Thus, Table 8 presents the competitive effects impacts, broken out this time by the following school-level subgroups: high-poverty schools (defined as public schools with student poverty levels above the 75th percentile for the state of Indiana), low-poverty schools (defined as public schools with student poverty levels below the 25th percentile), high-minority schools (defined as schools in which the percent of students who are white is below the 25th percentile), low-minority schools (defined as schools in which the percent of students who are white is above the 75th percentile), and urban/suburban schools (defined as schools located in a locale coded as a large, mid-size, or small city or suburb, as compared to schools located in locales coded as a town or rural area). No clear patterns emerge from these data, leading us to conclude there is a null effect.

Table 8. Effects of Private School Competition on Public School Achievement, School Subgroups

	(1)	(2)	(3)	(4)	(5)
	High-Poverty Schools	Low-Poverty Schools	High-Minority Schools	Low-Minority Schools	Urban or Suburban Schools
Math, First Post Policy Year					
Drive-time	-0.0019	-0.0003	-0.0017	0.0010	-0.0006
	(0.0012)	(0.0017)	(0.0019)	(0.0008)	(0.0021)
Competitor School Count (90 mins)	0.0000	0.0003	-0.0000	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Competitor School Count (30 mins)	0.0003	0.0004	0.0004	0.0004	0.0000
	(0.0003)	(0.0004)	(0.0004)	(0.0009)	(0.0003)
Competitor School Count (20 mins)	0.0009*	0.0014	0.0013*	0.0018	0.0004
	(0.0006)	(0.0012)	(0.0007)	(0.0027)	(0.0005)
Competitor School Count (10 mins)	0.0034*	-0.0017	0.0050**	-0.0012	0.0011
	(0.0019)	(0.0044)	(0.0024)	(0.0099)	(0.0015)
Math, All Post Policy Years					
Drive-time	0.0010	-0.0022	-0.0000	-0.0001	0.0024
	(0.0018)	(0.0022)	(0.0030)	(0.0009)	(0.0026)
Competitor School Count (90 mins)	0.0002	0.0004	0.0002	0.0002	0.0004**
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Competitor School Count (30 mins)	-0.0000	-0.0003	-0.0003	0.0021*	-0.0001
	(0.0004)	(0.0006)	(0.0004)	(0.0010)	(0.0003)
Competitor School Count (20 mins)	-0.0000	-0.0028*	-0.0006	0.0041	-0.0003
	(0.0007)	(0.0014)	(0.0008)	(0.0031)	(0.0006)
Competitor School Count (10 mins)	-0.0003	-0.0047	-0.0022	-0.0200*	-0.0012
	(0.0029)	(0.0052)	(0.0031)	(0.0108)	(0.0021)
ELA, First Post Policy Year					
Drive-time	-0.0019	0.0033**	-0.0014	0.0011	0.0019
	(0.0012)	(0.0015)	(0.0021)	(0.0007)	(0.0017)
Competitor School Count (90 mins)	0.0002	0.0005***	0.0000	0.0003*	0.0001
	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Competitor School Count (30 mins)	0.0000	0.0003	0.0001	0.0011	-0.0002
	(0.0002)	(0.0004)	(0.0003)	(0.0009)	(0.0002)
Competitor School Count (20 mins)	0.0001	0.0001	0.0004	0.0028	-0.0005
	(0.0004)	(0.0011)	(0.0006)	(0.0024)	(0.0003)
Competitor School Count (10 mins)	0.0017	0.0022	0.0041**	0.0014	0.0002
	(0.0015)	(0.0046)	(0.0020)	(0.0084)	(0.0013)
ELA, All Post Policy Years					
Drive-time	0.0016	-0.0018	0.0020	-0.0009	0.0011

	(0.0014)	(0.0017)	(0.0019)	(0.0006)	(0.0018)
Competitor School Count (90 mins)	0.0003	-0.0002	0.0003	-0.0000	0.0002*
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Competitor School Count (30 mins)	0.0004	-0.0011***	0.0001	-0.0008	-0.0000
	(0.0003)	(0.0004)	(0.0003)	(0.0007)	(0.0002)
Competitor School Count (20 mins)	0.0002	-0.0031***	-0.0004	-0.0024	-0.0001
	(0.0005)	(0.0011)	(0.0006)	(0.0022)	(0.0005)
Competitor School Count (10 mins)	0.0017	-0.0066	-0.0011	-0.0158*	0.0005
	(0.0021)	(0.0041)	(0.0024)	(0.0092)	(0.0015)
R-squared	0.2063	0.1833	0.2404	0.1896	0.2665

Notes: These coefficients represent the interaction between the measure of competition and the post-policy year(s). Drive-time is reverse-coded for ease of interpretation. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Robust standard errors clustered by school in parentheses. High poverty schools are defined as schools with poverty levels above the 75th percentile; low poverty schools are defined as schools with poverty levels below the 25th percentile. High-minority schools are defined as schools in which the percent of students who are white is below the 25th percentile; low-minority schools are defined as schools in which the percent of students who are white is above the 75th percentile. The 'Urban or Suburban' subgroup refers to schools located in a locale coded as a large, mid-size, or small city or suburb, as compared to schools located in locales coded as a town or rural area. n = 1,264,384 *** p<0.01, ** p<0.05, * p<0.10

We also check for dosage effects among these school-level subgroups (Table 9) but there is little evidence of a pattern of impacts that corresponds with the dosage hypothesis.

Table 9. Dosage Effects of Private School Competition on Public School Achievement, School Subgroups

	(1)	(2)	(3)	(4)	(5)
	High-Poverty Schools	Low-Poverty Schools	High-Minority Schools	Low-Minority Schools	Urban or Suburban Schools
Math					
Drive-time to Private School With 10+	0.0010	-0.0005	-0.0005	0.0012*	0.0023**
	(0.0011)	(0.0011)	(0.0019)	(0.0006)	(0.0010)
Drive-time to Private School With 20+	0.0006	0.0005	0.0009	0.0007*	0.0012***
	(0.0005)	(0.0008)	(0.0008)	(0.0004)	(0.0003)
Drive-time to Private School With 30+	0.0000	0.0004	-0.0008	0.0006	0.0007**
	(0.0005)	(0.0007)	(0.0007)	(0.0004)	(0.0003)
Drive-time to Private School With 40+	-0.0001	-0.0002	-0.0018***	0.0009*	-0.0000
	(0.0005)	(0.0008)	(0.0006)	(0.0005)	(0.0004)

Drive-time to Private School With 50+	0.0001	0.0005	-0.0018***	0.0010**	0.0001
	(0.0005)	(0.0007)	(0.0005)	(0.0005)	(0.0004)
ELA					
Drive-time to Private School With 10+	0.0018**	-0.0009	0.0013	0.0006	0.0020**
	(0.0008)	(0.0007)	(0.0014)	(0.0005)	(0.0008)
Drive-time to Private School With 20+	0.0009**	-0.0000	0.0004	0.0001	0.0007***
	(0.0004)	(0.0004)	(0.0005)	(0.0003)	(0.0003)
Drive-time to Private School With 30+	0.0003	-0.0002	-0.0009**	-0.0000	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Drive-time to Private School With 40+	0.0003	-0.0002	-0.0013***	0.0000	0.0001
	(0.0004)	(0.0006)	(0.0005)	(0.0003)	(0.0003)
Drive-time to Private School With 50+	0.0004	-0.0002	-0.0012***	0.0000	0.0001
	(0.0004)	(0.0006)	(0.0005)	(0.0003)	(0.0003)
R-squared	0.2053	0.1833	0.2397	0.1884	0.2664

Notes: These coefficients represent the interaction between the measure of competition and the post-policy years. Drive-time is reverse-coded for ease of interpretation. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch and an indicator if that share exceeds 40 percent after 2014–15, triggering the Community Eligibility Provision; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Robust standard errors clustered by school in parentheses. High poverty schools are defined as schools with poverty levels above the 75th percentile; low poverty schools are defined as schools with poverty levels below the 25th percentile. High-minority schools are defined as schools in which the percent of students who are white is below the 25th percentile; low-minority schools are defined as schools in which the percent of students who are white is above the 75th percentile. The 'Urban or Suburban' subgroup refers to schools located in a locale coded as a large, mid-size, or small city or suburb, as compared to schools located in locales coded as a town or rural area. n = 1,261,763 *** p<0.01, ** p<0.05, * p<0.10

Findings from Analysis of Graduation Impacts

We turn next to the findings of the impact analysis examining traditional public school graduation rates as the outcome variable. Graduation rates are calculated using the standard four-year adjusted cohort method. The student sample for this analysis consists of public school students who were expected to graduate in 2011 (i.e., one year before the voucher policy became law), 2012 (the first outcome year), or later. This includes 434,596 students in 369 traditional public schools. Descriptive statistics (Table A1 of the appendix) reveal a very similar distribution of student characteristics as in the test score impact sample. The majority (77 percent) of students are white, 10 percent are Black, and seven percent are Hispanic. Thirty-nine percent are low-income, 13 percent are labeled as having special educational needs, and three percent are limited English proficient. School-level characteristics are also similar as before, although school enrollment sizes are much larger now, with a mean enrollment value of 1,521 students, which is unsurprising as the sample for this particular analysis consists exclusively of high-school students.

Student graduation status is a binary variable, coded one to indicate a student graduated on time (85 percent of students in our overall sample) and zero otherwise. Reasons offered for non-graduation include removal by parents (31 percent of non-graduates), disinterest in curriculum (7 percent of non-graduates), and expulsion (1 percent). Foreign exchange students and those who moved out of state are removed from the sample altogether.

Table 10 displays the overall and subgroup effects of private school competition on traditional public school students' probability of graduating on time. To ease interpretation, we use a linear probability model. Effects can be interpreted in the following manner: An increase in private school competition operationalized as a one-minute reduction in drive-time to the nearest competitor is associated with a β percent change in a student's probability of graduating on time from high school, holding everything else constant. Overall, we find null effects on students' probability of graduating from high school. Subgroup findings are largely null, although there is some evidence that students in high-poverty traditional public schools have experienced a one percent increase in their probability of graduating in the first year after the voucher policy went into effect. However, this effect fades away in subsequent years.

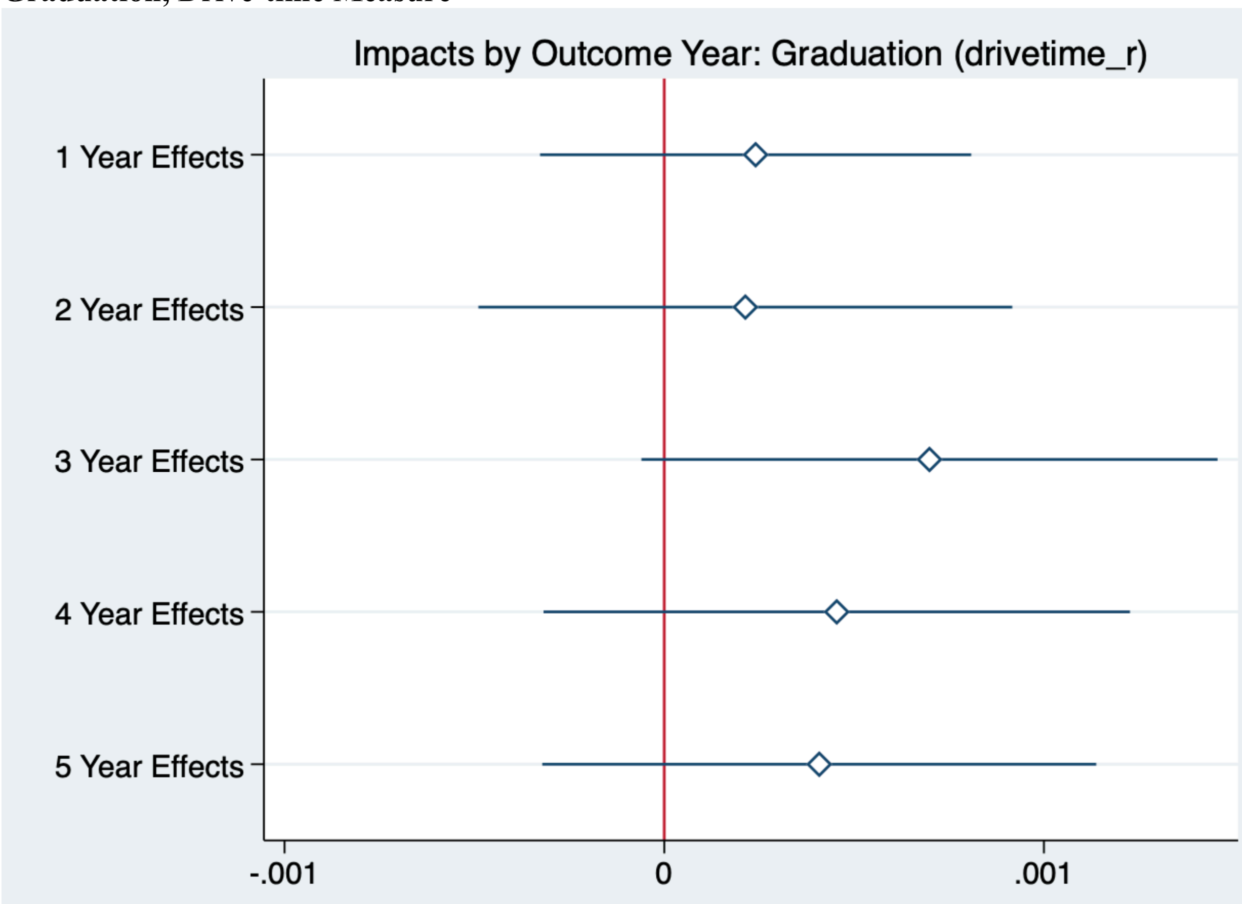
Table 10. Effects of Private School Competition on Traditional Public School Students' Probability of Graduating On Time

	(1)	(2)
	First Post-Policy Year	All Post-Policy Years
Main Effects		
Drive-time	-0.0001	0.0004
	(0.0002)	(0.0003)
Student Subgroups		
Female	-0.0002	0.0004
	(0.0003)	(0.0003)
White	0.0001	0.0006**
	(0.0003)	(0.0003)
Black	0.0007	0.0009
	(0.0025)	(0.0021)
Hispanic	-0.0010	-0.0009
	(0.0010)	(0.0010)
Low-Income	-0.0001	0.0004
	(0.0004)	(0.0004)
Special Education	-0.0011	0.0003
	(0.0007)	(0.0007)
LEP	0.0003	0.0013
	(0.0020)	(0.0017)
School Subgroups		
High Poverty Schools	0.0111**	-0.0014
	(0.0041)	(0.0056)
Low Poverty Schools	-0.0006	0.0010*
	(0.0006)	(0.0006)
High Minority Schools	0.0022	-0.0009
	(0.0036)	(0.0021)
Low Minority Schools	0.0005	0.0005
	(0.0004)	(0.0004)
Urban or Suburban	0.0010	0.0001
	(0.0012)	(0.0021)

Notes: These coefficients represent the interaction between the measure of competition and the post-policy years: school years ending in 2012 (the first post-policy year), 2013, 2014, 2015, and 2016. Drive-time is reverse-coded for ease of interpretation. Models include controls for pre-policy competitive pressure, student characteristics (gender, race, and indicators for free/reduced price lunch eligibility, limited English proficiency, and special education), a school fixed effect, time-varying school characteristics (annual enrollment, shares of students of each race/ethnicity, the share of students eligible for free/reduced price lunch and an indicator if that share exceeds 40 percent after 2014–15, triggering the Community Eligibility Provision; principal gender and race/ ethnicity; teacher gender, race/ethnicity, and an indicator for the share of teachers who are first year teachers), and a year fixed effect. Standard errors clustered by school in parentheses. n = 434,596 *** p<0.01, ** p<0.05, * p<0.10

It is also helpful to consider the graduation impact by individual outcome year, in case the overall effect masks heterogeneity over time, but that does not appear to be the case in Indiana in this time period. Figure 10 plots the coefficient estimates for each individual outcome year. Graduation effects are insignificant in all five years examined, although it is interesting to note the graduation effect is approaching statistical significance at standard levels (specifically, p = .063) in Year 3, which is when the program experienced dramatic growth for the first time.

Figure 6. Competitive Effects on Traditional Public School Students' Likelihood of Graduation, Drive-time Measure



Discussion

We use five specifications of a new and innovative competition measure to estimate the traditional public school response to competitive pressure from the Indiana Choice Scholarship Program, uncovering null to small, positive changes in math and English Language Arts achievement and null effects on students' probability of graduating from high school.

Another way of interpreting these findings is that we find no evidence that increased private school competition is associated with decreases in student achievement in Indiana's traditional public schools in the programs' first year of operation, when 3,911 students transferred to the private sector by way of this school choice program, or in later years when as many as 32,000 students participated. These findings may help assuage the concerns of some choice opponents, who expressed fears of negative effects for non-choosers at the program's inception.

We do find, however, consistent evidence of small positive effects for low-income children. This finding holds across multiple models using five different measures of competitive pressure. We also find benefits for special education students in models that use the drive-time measure and benefits for students who are Hispanic, low-income, have limited English proficiency, and are in the elementary grades in models that use the competitor count measure. Subgroup analyses of the graduation rate impact reveal a small benefit for students in high-poverty traditional public schools but this effect disappears after the first year of policy implementation. It is also worth noting our coefficients are likely conservative estimates of impact, representing lower-bound effects because we are not comparing the effect of operating in a competitive environment to that of operating in a non-competitive environment. We are examining whether test scores and graduation outcomes improved more after the establishment of the ICSP for students attending public schools with more and more varied private school options than for students attending public schools with fewer and less varied private school options.

The largely null effects observed here may be a result of market dominance by a traditional public school sector that enrolls over 1 million students, compared to fewer than 40,000 students participating in the state-funded private school choice program. Thus, the enrollment decreases associated with individual student transfers may be too small to be perceived as a serious threat by traditional public school teachers and administrators. To test the "tipping point" theory, we search for evidence of a point at which increases in student transfers by way of the Indiana Choice Scholarship Program predict changes in student achievement at nearby traditional public schools. We find little evidence to support this theory. That is, public schools appear to respond positively to the voucher program if they are close to a private school accepting at least 10 voucher students. This initial positive test score increase holds constant, however, even as the number of voucher recipients in neighboring private schools increases. That is, we don't observe a larger response by traditional public schools that are close to private schools accepting a greater number of voucher students, which casts doubt on the tipping point theory. This finding is consistent with findings from Milwaukee where Carnoy, Adamson, Chudgar, Luschei, and Witte reported, "this effect seems to have been a one-time response of all public schools to a change in contextual conditions rather than a continuous and differentiated improvement based on the degree of competition in the Milwaukee school market" (Carnoy et al., 2007).

However, we believe it is important to note our dosage analysis requires more assumptions than our primary difference-in-differences design because it leverages variation in the precise number of voucher-using students attending private schools in proximity to a given traditional public school. The unmeasured factors leading to the differences in the voucher student count at neighboring private schools might also be causing changes in traditional public school student achievement, a potential bias we don't have to worry about in the main models. An example of such a factor is a worsening academic climate at the traditional public school owing to frequent and disruptive leadership changes, for example, which would drive eligible students toward enrolling in a private school of choice and also drive down achievement at the traditional public school they depart. We reduce the possibility of endogeneity bias in the dosage findings by restricting the dosage analyses to the first year of post-policy outcomes when only 3,911 students received vouchers, statewide, but we cannot totally rule out the possibility of bias affecting the dosage results. Thus, the reader is advised to interpret this particular set of findings with caution.

Conclusion

Our goal with this study is to measure the competitive impact of the nation's largest private school choice program, an education policy that currently provides $159 million in state funds annually for more than 36,000 eligible students. We test if increased private school competition predicts changes in student achievement and graduation rates at traditional public schools. We also test for a tipping point at which increases in student transfers by way of the ICSP might predict changes in student achievement at nearby traditional public schools. We accomplish these goals by using an intuitively better measure than prior research in this area and a statewide dataset featuring more than 837,000 unique students, making this the largest competitive effects study of a voucher program conducted to date and the first to examine graduation rates as an outcome.

One of this study's primary contributions to the broader competitive effects literature is that competition is measured five different ways, where each measure leverages drive-time to the nearest voucher-participating private school, a unique measure that incorporates road lengths, intersection turn times, speed limits, and traffic data to calculate precise travel time between schools in minutes. Prior studies in the competitive effects literature have never before used this metric to quantify the competitive threat from private school choice.

Across models that examine student math and English Language Arts test scores and graduation rates, we report null to small positive impacts. We find no evidence that students in traditional public schools have been negatively affected by the enactment and growth of the Indiana Choice Scholarship Program, as measured by their test score outcomes or probability of graduating from high school. We do find, however, consistent evidence of small positive effects for low-income children. These findings may offer some degree of reassurance to those who expressed fears of negative effects for non-choosers at the program's inception.

Our findings are largely consistent with prior literature in this area, which finds that competitive effects are approximately zero or positive, but very modest in magnitude. In states where stronger positive competitive effects have been observed, it may be the case that those programs have design features that are more likely to yield competitive effects than the ICSP or it may be an errors-in-variables issue that biases their estimates of competition on student outcomes toward zero because the researchers are mis-measuring competition. Future studies could rule out the latter explanation by applying the drive-time measure used here in other states and contexts.

References

Altonji, J. G., Huang, C.-I., & Taber, C. R. (2015). Estimating the cream skimming effect of school choice. *Journal of Political Economy*, *123*(2), 266–324. https://doi.org/10.1086/679497

Carnoy, M., Adamson, F., Chudgar, A., Luschei, T. F., & Witte, J. F. (2007). *Vouchers and public school performance: A case study of the Milwaukee Parental Choice Program*. Washington, D.C.: Economic Policy Institute. Retrieved from http://www.epi.org/publication/book_vouchers

Carr, M. (2011). The impact of Ohio's EdChoice on traditional public school performance. *Cato Journal, 31*(2), 257–284. Retrieved from http://object.cato.org/sites/cato.org/files/serials/files/cato-journal/2011/5/cj31n2-5.pdf

Catt, A. D. (2014). *Exploring Indiana's Private Education Sector*. School Survey Series 1. Indianapolis, IN: Friedman Foundation for Educational Choice. Retrieved from http://www.edchoice.org/wp-content/uploads/2015/07/Exploring-Indianas-Private-Education-Sector.pdf

Catt, A. D., & Rhinesmith, E. (2017). *Why Indiana parents choose: A cross-sector survey of parents' views in a robust school choice environment*. Indianapolis, IN: EdChoice. Retrieved from https://www.edchoice.org/wp-content/uploads/2017/09/Why-Indiana-Parents-Choose-2.pdf

Chakrabarti, R. (2008). Can increasing private school participation and monetary loss in a voucher program affect public school performance? Evidence from Milwaukee. *Journal of Public Economics, 92*(5-6), 1371–1393. https://doi.org/10.1016/j.jpubeco.2007.06.009

Chakrabarti, R. (2013). Impact of voucher design on public school performance: Evidence from Florida and Milwaukee voucher programs. *The B.E. Journal of Economic Analysis & Policy, 13*(1), 349–394. https://doi.org/10.1515/bejeap-2012-0037

Chubb, J. E., & Moe, T. M. (1990). *Politics, markets, & America's schools*. Washington, D.C.: Brookings Institution Press.

Cowen, J. M. & Strunk, K. A. (2015). The impact of teachers' unions on educational outcomes: What we know and what we need to learn. *Economics of Education Review*, *48*, 208-223. https://doi.org/10.1016/j.econedurev.2015.02.006

EdChoice (2017). *Questionnaire and topline results: Why Indiana parents choose*. Indianapolis, IN: EdChoice. Retrieved from https://www.edchoice.org/wp-content/uploads/2017/09/Why-Indiana-Parents-Choose-Questionnaire-and-Topline-Results.pdf

Egalite, A. J. (2013). Measuring competitive effects from school voucher programs: A systematic review. *Journal of School Choice, 7*(4), 443-464. https://doi.org/10.1080/15582159.2013.837759

Egalite, A. J. & Mills, J. N. (2019) Competitive impacts of means-tested vouchers on public school performance: Evidence from Louisiana. Forthcoming in *Education Finance and Policy.* https://doi.org/10.1162/edfp_a_00286

Figlio, D. N., & Hart, C. M.D. (2014). Competitive effects of means-tested school vouchers. *American Economic Journal: Applied Economics, 6*(1), 133–156. https://doi.org/10.1257/app.6.1.133

Figlio, D., & Karbownik, K. (2016). *Evaluation of Ohio's EdChoice Scholarship Program: Selection, Competition, and Performance Effects*. Columbus, OH: Thomas B. Fordham Institute. Retrieved from http://edex.s3-us-west-2.amazonaws.com/publication/pdfs/FORDHAM%20Ed%20Choice%20Evaluation%20Report_online%20edition.pdf

Figlio, D., & Rouse, C. E. (2006). Do accountability and voucher threats improve low-performing schools? *Journal of Public Economics, 92*(1-2), 239–255. https://doi.org/10.1016/j.jpubeco.2005.08.005.

Forster, G. (2008a). *Lost opportunity: An empirical analysis of how vouchers affected Florida public schools*. School Choice Issues in the State. Indianapolis, IN: Friedman Foundation for Educational Choice. Retrieved from http:// www.edchoice.org/wp-content/uploads/2015/09/Lost-Opportunity-How-Vouchers-Affected-FloridaPublic-Schools.pdf

Forster, G. (2008b). *Promising start: An empirical analysis of how EdChoice vouchers affect Ohio public schools*. School Choice Issues in the State. Indianapolis, IN: Friedman Foundation for Educational Choice. Retrieved from http://www.edchoice.org/wp-content/uploads/2015/09/Promising-Start-How-EdChoice-Vouchers-AffectOhio-Public-Schools.pdf

Friedman, M. (1962). *Capitalism and freedom*. Chicago, IL: University of Chicago Press.

Gray, N. L., Merrifield, J. D., & Adzima, K. A. (2014). A private universal voucher program's effects on traditional public schools. *Journal of Economics and Finance, 39*(1), 1–26. https://doi.org/10.1007/s12197-014-9309-z

Greene, J. P. (2001). *An evaluation of the Florida A-Plus Accountability and School Choice Program*. Civic Report. New York City: The Manhattan Institute for Policy Research. Retrieved from https://media4.manhattan-institute.org/pdf/cr_aplus.pdf.

Greene, J. P., & Forster, G. (2002). *Rising to the challenge: The effect of school choice on public schools in Milwaukee and San Antonio*. Civic Bulletin No. 27. New York City: Manhattan Institute for Policy Research. Retrieved from http://files.eric.ed.gov/fulltext/ED474258.pdf

Greene, J. P., & Marsh, R., H. (2009). *The effect of Milwaukee's Parental Choice Program on student achievement in Milwaukee public schools*. SCDP Comprehensive Longitudinal Evaluation of the Milwaukee Parental Choice Program Report 11. Fayetteville, AR: School Choice Demonstration Project, University of Arkansas. Retrieved from http://www.uaedreform.org/the-effect-of-milwaukees-parental-choice-program-on-student-achievement-in-milwaukee-public-schools

Greene, J. P., & Winters, M., A. (2004). Competition passes the test. *Education Next, 4*(3), 66–71. Retrieved from http://educationnext.org/files/ ednext20043_66.pdf

Greene, J. P., & Winters, M., A. (2007). An evaluation of the effect of DC's voucher program on public school achievement and racial integration after one year. *Catholic Education: A Journal of Inquiry and Practice, 11*(1), 83–101. Retrieved from http://digitalcommons.lmu.edu/ce/vol11/iss1/7

Harris, D. N. & Larsen, M. F. (2017). *The identification of schooling preferences: Methods and evidence from post-Katrina New Orleans.* [Unpublished manuscript]. Retrieved from https://educationresearchalliancenola.org/files/publications/Harris-Larsen-How-Parents-Choose-2019-07-05.pdf.

Heissel, J. A., & Norris, S. (2018). Rise and shine: The effect of school start times on academic performance from childhood through puberty. *Journal of Human Resources*, *53*(4), 957–992. https://doi.org/10.3368/jhr.53.4.0815-7346R1

Hoxby, C., M. (2003). School choice and school productivity. In Hoxby, Caroline, M. (Ed.), *The economics of school choice*. Chicago, IL: University of Chicago Press. Retrieved from http://www.nber.org/chapters/c10091.pdf

Jabbar, H., Fong, C., Germain, E., Li, D., Sanchez, J., Sun, W.-L., & Devall, M. (2019). The competitive effects of school choice on student achievement: A systematic review. *Educational Policy,* 1–35. https://doi.org/10.1177/0895904819874756

Jackson, C. K. (2018). Does school spending matter? The new literature on an old question. *NBER Working Paper No. 25368*. Retrieved from https://www.nber.org/papers/w25368.

Jacob, A. (2011). Benefits and barriers to the hybridization of schools. *Journal of Education Policy, Planning and Administration,* 1(1), 61-82.

Jacob, A. (2014). Means-tested vouchers: Impacts on public school performance and racial stratification (Doctoral dissertation, University of Arkansas). Retrieved from http://scholarworks.uark.edu/etd/2231.

Kraft, M. A., Blazar, D., & Hogan, D. (2018). The effect of teacher coaching on instruction and achievement: A meta-analysis of the causal evidence. *Review of Educational Research*, *88*(4), 547-588. https://doi.org/10.3102/0034654318759268

Misra, K., Grimes, P. W., & Rogers, K. E. (2012). Does competition improve public school efficiency? A spatial analysis. *Economics of Education Review, 31*(6), 1177–1190. https://doi.org/10.1016/j.econedurev.2012.08.001

Ridley, M., & Terrier, C. (2018). *Fiscal and education spillovers from charter school expansion* (SEII Discussion Paper No. 2018.02). Cambridge, MA: School Effectiveness & Inequality Initiative at MIT. Retrieved from https://seii.mit.edu/wp-content/uploads/2018/07/SEII-Discussion-Paper-2018.02-Ridley-Terrier.pdf.

Rouse, C., Hannaway, J., Goldhaber, D., & Figlio, D. (2013). Feeling the Florida Heat: How low performing schools respond to voucher and accountability pressure. *American Economic Journal: Economic Policy, 5*(2), 251–281. https://doi.org/10.1257/pol.5.2.251

Strunk, K. O. & Grissom, J. A. (2010). Do strong unions shape district policies? Collective bargaining, teacher contract restrictiveness, and the political power of teachers' unions. *Educational Evaluation and Policy Analysis,* 32(3): 389-406. https://doi.org/10.3102/0162373710376665

U.S. Census Bureau (2015). Travel time to work, American Community Survey. Retrieved from https://data.census.gov/cedsci/table?q=B08303&g=8600000US77597&tid=ACSDT5Y2015.B08303&vintage=2015.

Waddington, R. J., & Berends, M. (2018). Impacts of the Indiana Choice Scholarship Program: Achievement effects for students in upper elementary and middle school. *Journal of Policy Analysis and Management*, 37(4): 783-808. https://doi.org/10.1002/pam.22086

Wang, S. (2014, January 27). *Indiana voucher students double to nearly 20,000.* IndyStar. Retrieved from https://www.indystar.com/story/news/politics/2014/01/27/indiana-voucher-students-double-to-nearly-20000/4939801

West, M., R., & Peterson, P., E. (2006). The efficacy of choice threats within school accountability systems. *The Economic Journal, 116*(510), C46–C62. https://doi.org/10.1111/j.1468-0297.2006.01075.x

Winters, M., A., & Greene, J. P. (2011). Public school response to special education vouchers: The impact of Florida’s McKay Scholarship Program on disability diagnosis and student achievement in public schools. *Educational Evaluation and Policy Analysis, 33*(2), 138–158. https://doi.org/10.3102/0162373711404220

Wolf, P. J., & Egalite, A. J. (2016). *Pursuing innovation: How can educational choice transform K–12 education in the U.S.?* Indianapolis, IN: Friedman Foundation for Educational Choice. Retrieved from http://www.edchoice.org/wp-content/uploads/2016/05/2016-4-Pursuing-Innovation-WEB-1.pdf

Appendix: Descriptive Characteristics of Analysis Sample, Graduation Outcome

	Mean	SD	Min	Max
Outcome Measure				
Student Graduates On Time	0.85	0.35	0.00	1.00
Student Characteristics				
Female	0.49	0.50	0.00	1.00
White	0.77	0.42	0.00	1.00
Black	0.10	0.30	0.00	1.00
Hispanic	0.07	0.26	0.00	1.00
Other Race	0.05	0.23	0.00	1.00
Low-Income	0.39	0.49	0.00	1.00
Special Education	0.13	0.34	0.00	1.00
Limited English Proficiency	0.03	0.17	0.00	1.00
Traditional Public School Characteristics				
Enrollment	1520.87	942.38	53.00	4930.00
Percent White	0.76	0.23	0.00	1.00
Percent Black	0.10	0.16	0.00	0.96
Percent Hispanic	0.08	0.10	0.00	0.72
Percent Other Race	0.06	0.04	0.00	0.25
Percent Low-Income	0.41	0.18	0.04	1.00
Principal Male	0.81	0.39	0.00	1.00
Principal White	0.91	0.29	0.00	1.00
Principal Black	0.08	0.28	0.00	1.00
Principal Hispanic	0.01	0.09	0.00	1.00
Principal Other Race	0.00	0.05	0.00	1.00
Teacher Percent Male	0.44	0.06	0.03	0.91
Teacher Percent White	0.93	0.13	0.05	1.00
Teacher Percent Black	0.05	0.12	0.00	0.95
Teacher Percent Hispanic	0.01	0.02	0.00	0.21
Teacher Percent Other Race	0.01	0.01	0.00	0.17
Competition Measures	0.05	0.03	0.00	0.36
Drive-time (mins)				
Drive-time (10+ Voucher Students)	8.25	7.89	0.07	40.96
Competitor School Count (90 mins)	16.48	13.71	0.38	82.18
Competitor School Count (30 mins)	101.74	39.44	21.00	176.00
Competitor School Count (20 mins)	22.14	20.75	0.00	77.00
Competitor School Count (10 mins)	10.30	10.79	0.00	52.00

Notes: n = 434,596 unique students, 369 unique schools; ‘Competitor school count’ refers to the number of participating private schools within 90 mins' drive of a given public school.

About the Authors

Anna J. Egalite is an associate professor in the Department of Educational Leadership, Policy, and Human Development in the College of Education at North Carolina State University. She holds a Ph.D. in Education Policy from the University of Arkansas and an M.Ed. from the University of Notre Dame's Alliance for Catholic Education. She completed her postdoctoral fellowship in the Program on Education Policy and Governance at Harvard University. Her research focuses on the evaluation of education policies and programs intended to close racial and economic achievement gaps, such as the introduction of market forces into education through school choice and competition.

Andrew D. Catt is the director of state research and special projects for EdChoice. In that role, Drew conducts analyses on private educational choice programs, conducts surveys of private school leaders and parents of school-age children, and conducts geospatial analyses. Drew graduated from Vanderbilt University in 2008 with a bachelor's degree in Human and Organizational Development, specializing in Leadership and Organizational Effectiveness. During that time, he researched the effects of homeschooling on socialization. Drew received his Master of Public Affairs in Nonprofit Management at Indiana University's School of Public and Environmental Affairs in Indianapolis. He also received his Master of Arts in Philanthropic Studies through the Lilly Family School of Philanthropy. While in graduate school, Drew's research focused on teacher performance incentives and cross-sector collaboration. He recently received a Graduate Certificate in Geographic Information Science (GIS) from IUPUI. Drew is a native of central Indiana and currently resides in downtown Indianapolis with his wife Elizabeth and their son Theodore

Acknowledgements

We are grateful to the Indiana Department of Education for providing access to the state administrative records necessary to conduct these analyses. Drs. Jonathan Mills, Ben Scafidi, and R. Joe Waddington provided extremely helpful comments on an earlier draft of the manuscript. We are also grateful to those who provided feedback on a presentation of the preliminary methodology at the 39th Annual Fall Research Conference of the Association for Public Policy Analysis and Management and to those who provided feedback on the preliminary results at the 44th Annual Conference of the Association for Education Finance and Policy. All opinions expressed in this paper represent those of the authors and not necessarily the institutions with which they are affiliated. All errors in this paper are solely the responsibility of the authors.