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Means-Tested Vouchers: Impacts on Public School Performance & Racial Stratification

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Means-Tested Vouchers: Impacts on Public School Performance & Racial Stratification

Means-Tested Vouchers: Impacts on Public School Performance & Racial Stratification

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy in Education Policy

by

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Abstract

This dissertation examines the systemic effects of private school choice in the context of two statewide, means-tested school voucher programs— the Indiana Choice Scholarship Program (ICSP) and the Louisiana Scholarship Program (LSP). Specifically, I examine public school responses to private school competition from the ICSP and the LSP and the direct impacts of the LSP on racial stratification in public and private schools. In Louisiana, I show that the lowest-graded public schools had a modest, statistically significant, positive response to the injection of competition, with impacts ranging from .001 to .06 SD. In Indiana, the evidence is slightly weaker. In math, none of the four competition measures are significantly related to school-average performance whereas in English Language Arts, three out of eight results provide evidence of a statistically significant, positive competitive effect. Depending on the radius selected, a one-unit increase in the concentration measure (a modified Herfindahl Index) is associated with a .04 to .05 SD increase in school-average ELA achievement.

Regarding racial stratification in Louisiana's schools, I show that LSP transfers reduce racial stratification in the voucher students' former public schools, but marginally increase racial stratification in the private schools. Specifically, 82% of all student transfers reduce racial stratification in the traditional public schools, compared to 45% in private schools. Overall, the articles presented in this dissertation demonstrate that private school choice programs have null to modest positive impacts on the students who remain in public schools. Given that traditional public schools are and will continue to be the primary provider of educational services in K-12, this is good news for public school students in states with expanding private school choice programs.

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Finally, I would like to thank my incredible family members, who have been a source of constant love, laughter, and support. To my husband, CJ; my parents, Brian and Margaret; my brother, Mark; and my sister and best friend in the world, Karen, thank you from the bottom of my heart.

Go raibh míle maith agaibh go léir.

Dedication

I dedicate this dissertation to my loving husband CJ and to my wonderful parents, Brian and Margaret. Your sacrifices have not gone unnoticed. Thank you for your constant love and encouragement.

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Introduction

Publicly funded voucher programs currently operate in ten states plus the District of Columbia and Douglas County, CO. Furthermore, thirteen states use personal or corporate state income tax policies to indirectly subsidize private school tuition payments (Frendewey, Kump, Martinez, Malin, & Marcavage, 2014). In 2013-14 alone, the number of school choice programs in operation nationwide grew from 32 to 39 programs, bringing the total annual expenditures on private school choice programs to approximately \$1.2 billion. The new programs that were passed this same year relied on political support from both sides of the aisle, demonstrating a blurring that has occurred within both major political parties of the traditional definitions of “public education.” Given the significant growth rate of private school choice programs over the past two decades and considerable bipartisan support for this issue, should we be hopeful or worried about what that means for the educational prospects of the 50.1 million students attending public schools in this country?

The theory of reform behind market-based school choice programs is that all students—both participants and non-participants— would directly benefit from expanded choice and competition. In an open school choice marketplace, a wide variety of diverse schools with different pedagogies, curricula, and approaches to learning would spring up (Friedman, 1955), increasing the probability that an individual student would find an optimal school to match his needs and learning style. Families would benefit from being able to express their views through both exit and voice (Hirschman, 1970). Furthermore, it is thought that increasing the number of choice-based school reform programs would exert pressure on traditional public schools to improve, resulting in “a rising tide” of school improvement (Hoxby, 2001). By shifting

enrollment and the associated financial resources away from public school districts, competition among schools would be stimulated, leading to the more efficient use of available resources.

On the other hand, opponents of market-based school reforms have raised concerns about the unanticipated consequences of these reforms, such as diminished resources and suboptimal academic experiences for the students who are left behind as well as increased stratification by race and income among schools. National and state media outlets regularly run opinion columns in which prominent politicians, teachers' union leaders, and activists accuse private school vouchers of siphoning funds from public schools, arguing that private school choice programs remove financial resources from those public schools that are most in need of revenue in order to improve (McCall, 2014; Rich, 2014; Schrier, 2014; Weingarten, 2013). Moreover, others have criticized vouchers for removing academic and social capital from public schools, arguing that private school vouchers rob the public school students who are left behind of the positive peer effects of higher achieving classmates and the influence of motivated families who would push for overall school improvements (Epple & Romano, 1998; Ladd, 2002).

Much of the school choice research up until now has focused on the direct impacts of school choice reforms on program participants—the students who depart their assigned public schools with a voucher or scholarship to attend a private school with state assistance. Yet, if choice is to have a profound impact on general educational outcomes, we need research that focuses on the public schools affected by school choice, since this is where the majority of students will continue to be educated. Despite impressive growth rates over the past twenty years, private school choice programs still only educate 308,000, or less than 1%, of all public school students in the U.S. As such, even if private school voucher and tuition tax credit scholarship programs continue to expand at their current rates, public schools will remain the

majority provider of schooling in this country for the foreseeable future. The impact of choice reforms will be most widely felt, therefore, through second-level effects on those students who remain in district-run public schools. It is vital that research documents what happens to student achievement and racial integration in traditional public schools as we change the circumstances in which education is provided. This dissertation addresses this need by studying the systemic impacts of two of the three largest statewide, means-tested school voucher programs in the country—the Louisiana Scholarship Program (formally known as the Student Scholarships for Educational Excellence Program) and the Indiana Choice Scholarship Program.

There are two primary research questions addressed in the articles to follow. First, is public school achievement influenced by competition from a private school choice program? Articles one and two address this question in the context of the Louisiana and Indiana school voucher programs, using multiple measures of competition. Second, how do student transfers through a private school voucher program impact racial stratification in Louisiana’s public and private schools? Article three addresses this question in the context of the Louisiana Scholarship Program.

Measuring the competitive impacts and effects on racial stratification associated with a school voucher program can be methodologically tricky so in the next two sections, I discuss the specific challenges of each research question separately to ensure clarity.

The Challenges Associated with Measuring Competitive Impacts

In an ideal scenario, researchers would use experimental data to measure the impact of competition on public school achievement. The design of such an experiment would proceed as follows. At the outset, families in a pair of neighboring cities would apply for a voucher and take

baseline tests. At the time of voucher application, families would not yet be aware of the results of the two-stage randomization process to determine the voucher winners, which would only take place after applications are complete. First, cities would be randomized into treatment and control groups. Second, within the treatment cities only, voucher applicants would be randomized into treatment and control groups, with treatment group students only receiving the offer of a voucher. By comparing the outcomes of non-applicants in treatment cities to non-applicants in control cities, we could rigorously measure the spillover effects of the voucher program on those students left behind in public schools. Such a study has actually been conducted across a set of villages in the Indian state of Andhra Pradesh (Muralidharan & Sundararaman, 2013). Figure 1 presents a graphical presentation of their research design.

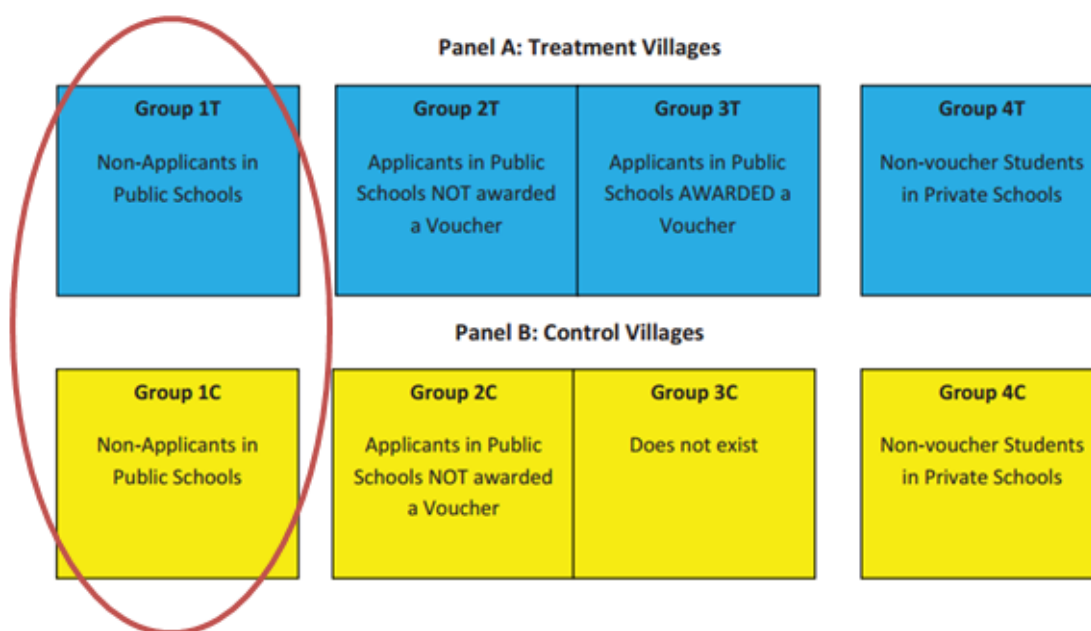


Figure 1. Design of Andhra Pradesh school choice program. From “The aggregate effect of school choice: Evidence from a two-stage experiment in India,” by K. Muralidharan and V. Sundararaman, 2013, *NBER Working Paper No. 19441*, p. 31. Reprinted with permission.

Of course, actually being able to implement an experimental research design in an industrialized economy like the U.S. would be highly unlikely. Thus, the primary challenge in a study of competitive impacts on U.S. public schools is generating the explanatory variable that documents the degree of competition experienced by different schools. The researchers must make a case for why the competition variable selected is not confounded with other factors of the educational landscape that influence schools' achievement such as the characteristics of the particular population under study or the economic success of that particular community, for instance.

It is also important for the researcher to select a window in time that offers an exogenous shock that disrupts the equilibrium so that the researcher can cleanly isolate the change in public school performance that is attributable to competition induced by the new private school choice policy. The unexpected passage of a piece of legislation authorizing a new private school voucher or tuition tax credit scholarship program can serve as a useful inflection point for researchers attempting to compare public school performance present and absent the competitive pressure of the choice program. As such, the studies of the competitive impacts of the two means-tested voucher programs presented in this dissertation rely on a panel data set that features a competitive shock at some point in the panel.

In order to choose the specific set of competition measures to be utilized in these two panel studies of competition in Indiana and Louisiana, I review existing measures from other fields, such as international banking, healthcare, and management.

In banking, competition is most commonly measured by market concentration measures. There are at least ten of these concentration ratios in use but the most widely used is the

Herfindahl-Hirschman Index (HHI), which measures the distribution of production across firms within an industry (Cetorelli, 1999). In the United States, the HHI is also commonly used in the enforcement of antitrust laws in banking.

In healthcare, the typical measure of market structure is also the HHI, although count-based measures and distance measures have also been employed. Mukamel, Zwanziger, and Tomaszewski (2001) employ the HHI to measure the relationship between hospital mortality rates and competition. Held and Pauly (1983) also employ the HHI to study the quality of care provided to patients with end stage renal disease. Shen (2003), meanwhile, uses an indicator for whether or not there are five or more hospitals within a 15 mile radius of a hospital. She interacts this with the change in Medicare price and change in health maintenance organization (HMO) penetration to predict the quality of hospital care. Finally, both Gowrisankaran and Town (2003) and Kessler and Geppert (2005) instrument for HHI with hospital market shares predicted by multinomial logit models of hospital demand that rely on distance as the main determinant of hospital choice.

Finally, the management field has relied upon density counts as a common competition measure (Carroll & Wade, 1991; Luca & Zervas, 2013; Swaminathan & Delacrois, 1991). For instance, Luca and Zervas (2013) show that an increase in competition— measured by the count of nearby restaurants serving similar types of food— is a significant predictor of unfavorable, fraudulent restaurant reviews of competitors on the popular online consumer review platform, Yelp.com.

Because this particular study takes place in a different field than the competition studies described thus far, it could be problematic to simply transfer these measures to my analysis

without any evidence that density measures such as the count of competitors, distance to the nearest competitor, or concentration measures such as the HHI function appropriately as proxies for competition between schools. Fortunately, there has been at least one study that attempts to validate these measures for my context.

Jabbar (2014) surveys all public schools in New Orleans and asks principals to name all of the schools they perceive as their competitors. She follows up with logistic regressions to test a large set of variables to see which variables best predict the existence of a competitive tie between two schools, as revealed by who the principals named as their competitors on the surveys. Jabbar demonstrates that the geocoded measures of competition commonly used in other fields perform adequately well, although they cannot fully explain who the principals perceive to be their competitors. For instance, results for one of the geocoded variables I use in my analyses—the distance measure— show that for every mile between two schools, the odds that two schools share a competitive tie decrease by 5%. Jabbar also shows that schools tend to compete with similar schools. For every one-unit increase in the absolute difference between two schools' school performance score, the odds that the sending school will name that school as a competitor decrease by 3 percent. Interestingly, Jabbar finds that charter school brand and authorizer are also predictive of competitive ties. Although my study excludes charter schools, I generate a “diversity” measure of competition that captures private school type. Given that private school religious types have been shown to have distinctive “brands” (Trivitt & Wolf, 2011), this variable should capture some of the same variation that charter school brand and authorizer captures in New Orleans.

In sum, the competition measures selected for this study are distance, density, diversity, and concentration measures, which have all been previously used in competition studies in other fields and have been validated for use in an educational context.

The Challenges Associated with Measuring Racial Stratification

Because the literature on choice and its impacts on racial stratification is relatively nascent, the tools available to conduct these analyses are still evolving. Relatively few studies rely on simple descriptive comparisons of choosers and eligible non-choosers, which rely on the theory that the departure of relatively advantaged students through school choice programs worsens segregation (Henig, 1996; Willms & Echols, 1993). The majority of analyses in this area draw upon translations of residential segregation indices commonly used by the Census Bureau and scholars studying segregation effects at the housing level (Archbald, 2000; Clotfelter, 1999; Garcia, 2008; Massey & Denton, 1993; Reynolds, Thernstrom, Braceras, Heriot, Kirsanow, Melendez, Taylor & Yaki, 2007). There are over 20 formal indices available with which to judge the degree of residential segregation. Popular translations of these measures include the dissimilarity index and exposure index, which are used to describe racial dispersion patterns within a school system. Yet these measures are largely inappropriate to judge school segregation because they are blind to segregation at the system (ie. district) level. More appropriate analyses use a community standard such as the surrounding metropolitan area to act as a benchmark of the desired racial composition (Bifulco & Ladd, 2006; Forster, 2006a, 2006b; Greene, 1998; Greene, Mills, & Buck, 2010; Greene & Winters, 2007; Ritter, Rush and Rush, 2002). A number of newer methods have also been developed that take advantage of panel datasets that track individual students over time (Greene, Mills, & Buck, 2010; Ritter, Jensen,

Kisida, & Bowen, 2012; Zimmer, Gill, Booker, Lavertu, Sass, & Witte, 2009). A number of these studies track individual students' migration patterns as they transfer between schools, judging whether these transfers increase or reduce racial stratification by whether they take the racial composition of a school's student body towards or away from the racial composition of the surrounding metropolitan or micropolitan area.

To assist the reader in reviewing the most popular measures used to study school choice and its effect on school segregation, Figure 2 presents a typology of segregation measures used in an educational context. The particular measure used in the third article of this dissertation appears in the lower right quadrant of Figure 2: Transfer Measures with a CBSA Benchmark.

Typology of Segregation Measures		Uses a Racial Composition Benchmark	
		NO	YES
Data Structure	Cross-Sectional	Descriptive Comparisons of Users v. Eligible Non-Users (Henig, 1996; Willms & Echols, 1993)	Within-District Sector Comparisons of School Racial Composition (Burgess, Wilson, & Lupton, 2005; Clotfelter, 1999; Frankenberg & Lee, 2002; Frankenberg, Siegel-Hawley, & Wang, 2010; Fuller & Greiveldinger, 2002; Fuller & Mitchell, 1999, 2000; Garcia, 2008) Within-CBSA Sector Comparisons of School Racial Composition (Bifulco & Ladd, 2006; Forster, 2006a, 2006b; Greene, 1998; Greene, Mills, & Buck, 2010; Greene & Winters, 2007; Ritter, Rush and Rush, 2002)
	Panel	Transfer Measures with No Benchmark (Zimmer et al., 2009)	Transfer Measures with a District Benchmark (Bifulco & Ladd, 2006) Transfer Measures with a CBSA Benchmark (Greene, Mills, & Buck, 2010; Ritter, Jensen, Kisida, & Bowen, 2012)

Figure 2. Typology of Segregation Measures.

Primary Contributions

In light of the rapid expansion of private school choice programs nationwide, this dissertation examines the systemic impacts of two means-tested voucher programs on public school achievement and racial stratification in Indiana and Louisiana. In doing so, this dissertation makes three primary contributions. First, it validates a set of easy-to-implement geocoded measures for understanding how private school competition affects public school productivity. Second, it provides empirical evidence that public school performance is

significantly, positively influenced by an injection of competition. This is valuable insight for the policymakers who have forged ahead with 39 private school voucher programs across the country. Third, the estimates presented here cast doubt on allegations by the United States Justice Department that a private school voucher program in Louisiana impedes court-ordered desegregation efforts in public schools. To the contrary, the estimates presented here demonstrate that the statewide voucher program in question actually helps *decrease* racial stratification in Louisiana's public schools.

The first article exploits variation in the geographic location of Louisiana private schools to estimate the competitive impact of a statewide private school voucher program on public school math and English language arts achievement. Using a school fixed effects model over a three year panel, I compare "A" and "B" graded public schools whose students are not voucher eligible to "C," "D," and "F" graded public schools whose students are eligible. This article then employs a sensitivity analysis that has stronger internal validity— a regression discontinuity design— but a smaller sample to empirically validate the measure of competition used in the primary analysis. The goal of this article is to provide the first comprehensive view of the competitive effects of school vouchers across the state of Louisiana and to validate an easily implemented approach to measuring competition when a more reliable method such as an experimental or quasi-experimental design is not feasible. If the results from the rigorous regression discontinuity design are consistent with the results from the models relying upon geocoded measures, this study will build confidence in the geocoded approach.

The second article presents an analysis of the competitive impacts of the ICSP, a statewide school voucher program that provides public funds to low- and middle-income families to cover tuition costs at participating private schools, both religious and non-religious. The

primary goal of this article is to examine the competitive impact of this school voucher program by exploiting variation in the geographic location of private schools in the state. Given that the ICSP has the potential to grow into the nation's largest school voucher program, evidence of a positive competitive response or even of a null effect would reassure policymakers that public school achievement will not be harmed as the program continues to expand.

The last article analyzes the racial-stratification impacts of the LSP, which has primarily targeted low-income, minority students. The primary objective of this article is to track individuals as they switch schools in order to measure the impact of the LSP on racial stratification in both public and private schools across the state of Louisiana. If LSP transfers have increased racial stratification in the voucher students' former public schools, then this study would provide evidence that the program is impeding court-ordered desegregation efforts, which the United States Justice Department claimed when it sought an injunction against the program in August 2013. If LSP transfers have reduced racial stratification, however, then this study would provide evidence that the LSP is a voluntary mechanism to reduce racial homogeneity in Louisiana's public schools.

The remainder of this dissertation consists of three separate articles and a conclusion. Articles one and two present analyzes of the competitive impacts of the statewide, means-tested voucher programs in Indiana and Louisiana, whereas article three examines the racial stratification impacts of the Louisiana Scholarship Program. The concluding chapter summarizes the findings of these three papers, offers policy implications, acknowledges any limitations of the research, and proposes a path for future research in this area.

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**The Effect of the Louisiana Scholarship Program on Student Achievement in Louisiana
Public Schools**

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Abstract

The Louisiana Scholarship Program (LSP) is a school choice program that provides public funds for low-income, mostly minority students in low-performing public schools to enroll in participating private schools, both religious and non-religious. In order to be eligible for a voucher in 2012-13, students had to have a family income that did not exceed 250% of the federal poverty guidelines and must have been entering Kindergarten or coming from a public school that received a “C,” “D,” or “F” grade in October 2011. In its first year of statewide operation, almost 5,000 students from low-performing public schools used these vouchers to enroll in private schools at public expense. The primary analysis presented in this article exploits variation in the geographic location of private schools to estimate the competitive impact of the LSP on public school math and English language arts achievement, finding modest, statistically significant, positive impacts. Secondary analyses using a stronger identification strategy validate the primary findings, showing no impacts on the subgroup of public schools that experienced the weakest competitive threat.

The Effect of the Louisiana Scholarship Program on Student Achievement in Louisiana Public Schools

The Louisiana Scholarship Program (LSP) is a school choice program that provides public funds for low-income students in low-performing public schools to enroll in participating private schools, both religious and non-religious. Initially piloted in New Orleans in 2008, Act 2 of the 2012 Regular Session expanded the LSP statewide, allowing thousands of public school students to transfer out of their residentially-assigned schools and into private schools across the state of Louisiana. In order to be eligible for a voucher, students had to have a family income that did not exceed 250% of the federal poverty guidelines and must have been entering Kindergarten or coming from a public school that received a “C,” “D,” or “F” grade in October 2011. In school year 2012-13, 9,831 eligible Louisiana students applied for an LSP voucher. Ultimately 4,954 students from low-performing public schools used these vouchers to enroll in private schools. All of these students were low-income and approximately 80 percent were African American.

This article examines public school test performance in those schools that were exposed to competition from the LSP in the first year of the program’s statewide expansion. The primary analysis exploits variation in the geographic location of private schools to estimate the competitive impact of the LSP on public school math and English language arts achievement, finding modest and statistically significant positive impacts in “D” and “F”-graded public schools. Effects are largest and consistently significant across multiple measures of competition in math achievement, ranging from .0011 to .0639 standard deviations. Sensitivity tests using a regression discontinuity design confirm that student achievement in “C”-graded public schools — which were the least affected by student transfers through the LSP — was neither helped nor

harmful as a result of the competitive pressures induced by the LSP. Thus, this study concludes that public school performance in math and English language arts experienced a modest, statistically significant, positive impact in those public schools that were most affected by the voucher program.

Study Background

Figure 1 displays a timeline of events related to the implementation of the LSP. On April 18, 2012, Louisiana Gov. Bobby Jindal signed Act 2 into law and one month later, the Louisiana Department of Education launched a public awareness campaign to advertise the private school voucher program, which would begin in August of 2012. Students were eligible for the program if their family income was below 250% of the federal poverty guidelines and they were either entering Kindergarten or currently attending a public school that was graded “C” or lower by the state. Those students applying from “D” and “F” graded schools would be given preference in the voucher allocation lottery. School performance scores and letter grades had already been released to the public in October 2011, before the voucher program had even been announced. This satisfies an important condition for a sensitivity analysis we conduct, which uses a regression discontinuity design, by guaranteeing that school performance couldn’t have been manipulated so that schools would or would not qualify for the LSP.

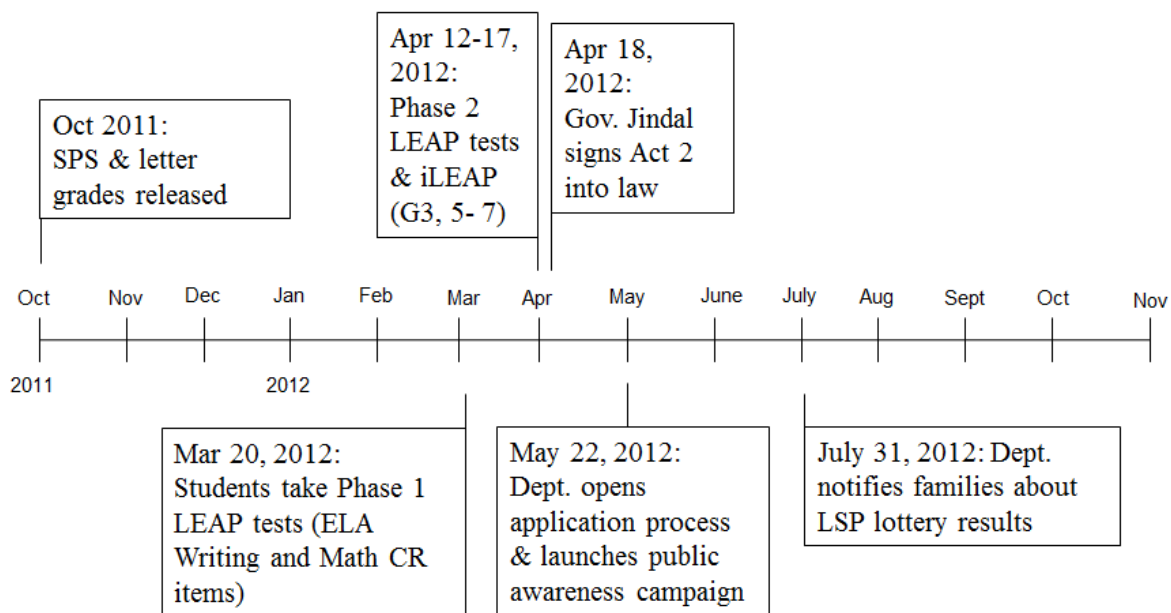


Figure 1. Timeline of events related to competition from the statewide expansion of the LSP. SPS stands for School Performance Score; LEAP stands for Louisiana Educational Assessment Program; iLEAP stands for integrated Louisiana Educational Assessment Program; ELA stands for English Language Arts; CR stands for Constructed Response; LSP stands for Louisiana Scholarship Program.

Theoretical Framework

There are three channels through which competition from the LSP may impact public school performance. The first is a pure competitive response by public schools seeking to retain students and the associated revenues. By granting students the financial resources to exit a public school they are dissatisfied with, vouchers may provide public schools with a financial incentive to improve their performance. Those public schools that do not improve in measurable ways may find it difficult to retain students and the associated revenue. Further, this general competitive pressure may be heightened by a motivation to maintain a school's reputation and avoid political embarrassment by preventing a mass exodus of students. This could be magnified by pressure

from parents, prominent community members, and the state and national media, which has reported on the LSP extensively (Dreilinger, 2013; Santos & Rich, 2013; Simon, 2012).

The second channel through which we might expect the LSP to impact public school performance is through a change in school composition as a result of losing the low-income students who qualified for a voucher. If these students are uniformly low-achieving and they exit in significant numbers, then their departure could result in higher average scores for the school as a whole. Conversely, if the voucher program has a “cream-skimming” effect and results in the departure of the most able students, we might expect to see lower average scores in the public schools they depart.

The final channel through which competition may impact public school performance is the resource change associated with losing voucher students. By taking resources from low-performing schools, it could be argued that the LSP prevents public schools from performing optimally, resulting in a general lowering of average school performance. In addition to losing the financial resources associated with a student transfer through the LSP, if the voucher program attracts the most motivated families, public schools will also lose the positive influence of these active, involved families and the example they set for other parents and students.

The remainder of this article proceeds as follows. First, we present a summary of the literature examining the competitive impacts of private school choice programs. In the next section, we describe the empirical methodology and the data used in this analysis. The following section presents the results. Finally, the article concludes with a summary of the main findings and a discussion of the implications.

Previous Literature

Studies of the competitive effects of voucher and tuition tax credit scholarship programs on traditional public school student achievement have been conducted in seven locations across the United States. Of the 21 total studies conducted thus far, all find neutral to positive results (Egalite, 2013). The majority of studies have taken place in the state of Florida and in Milwaukee, Wisconsin.

Various competition measures have been used in the literature but regression discontinuity design (RDD) appears to be the most rigorous estimation strategy capable of identifying the causal effect of competition threats on traditional public schools. By comparing observations from two similar groups that fall on either side of and close to a pre-specified cutoff point, this approach approximates a random-assignment research design. Six Florida studies use the RDD approach, all finding positive, statistically significant impacts of choice-based competition on at least one of the subjects examined (Chakrabarti, 2008a; Figlio & Rouse, 2006; Greene, 2001; Greene & Winters, 2004; Rouse, Hannaway, Goldhaber, & Figlio, 2013; West & Peterson, 2006). No RDD studies of the competitive effects of school choice have been conducted, thus far, outside of Florida.

Another very popular competition measure, used in the Milwaukee studies, is the percentage of public school students eligible for a voucher to transfer to a private school (Carnoy, Adamson, Chudgar, Luschei & Witte, 2007; Chakrabarti, 2008b; Forster, 2008a; Greene & Forster, 2002; Hoxby, 2003; Mader, 2010). All six Milwaukee studies find significantly positive or neutral-to-positive impacts.

Additionally, at least four geocoded competition measures have been used in the literature, which involve mapping public and private school addresses in the location under study and using information such as the distance to the nearest private competitor or a count of competitors surrounding a public school as the competition measure. One criticism of geocoded measures is that they suffer from endogeneity bias because the locations where public schools demonstrate poor performance might be attractive to choice schools with a mission to enroll underserved students. This is more likely to be a problem in studies of competitive effects of charter schools, however. In Louisiana and Florida, the private schools in question — mostly Catholic schools — existed for many years prior to the creation of any voucher programs. Many of these schools were established in response to Catholic doctrine, which dictates that Catholic children should be educated in a Catholic school (Herbermann, 1912) and not in response to unsatisfactory public school performance.

The first type of geocoded measure commonly used is a density measure, which uses the count of private competitors within a given radius (typically 2, 5, or 10 miles) to gauge the degree of competition experienced by a public school (Carnoy et al., 2007; Greene & Marsh, 2009; Figlio & Hart, 2014; Greene & Winters, 2008). Proximity measures, on the other hand, use the distance between a public school and its nearest private school competitor as the competition measure (Figlio & Hart, 2014; Greene & Winters, 2007). A diversity measure considers the number of different types (eg. Catholic, Lutheran, etc.) of local private schools located near a given public school (Figlio & Hart, 2014). Finally, proxies for private school size include counting the number of enrollment slots available at neighboring private school competitors or using private school physical seating capacity as a proxy for private school size (Mader, 2010).

In Vermont and Maine, Hammons (2002) counts the percentage of a district's budget attributable to students who were "tuitioned-in" as the competition measure, which is analogous to counting student transfers, finding significant, positive impacts on a composite of high school math, English, and science scores. Finally, both Carr (2011) and Forster (2008b) use a dichotomous variable to indicate whether or not a school faced the competitive threat of vouchers in a particular year, finding positive effects on proficiency passage rates and on 5th and 7th grade math and 7th grade reading scores, respectively.

Of the 21 total studies of competition effects, all but one study finds neutral-to-positive or positive results—a 2007 study by Greene and Winters of the federal voucher program, the D.C. Opportunity Scholarship Program, which found null impacts. This program is distinguishable from the other voucher programs that have been examined, however, because it was restricted to a relatively small number of students at the time the competition study was conducted and a "hold harmless" provision protected public schools from experiencing any loss in revenues as a result of students' exiting public schools to attend a private school using an LSP voucher.

In sum, a diverse set of identification strategies, ranging in rigor, have been employed to deduce estimates of the competitive effect of school vouchers or tuition-tax credits. This study is the first, however, to employ two strategies simultaneously. First, four geocoded competition measures—with strong external validity and incorporating all relevant public schools in Louisiana—are used to estimate the impacts of competition on "C," "D," and "F"-graded public schools. A rigorous regression discontinuity design with stronger internal validity is then used to validate the results of the geocoded measures where their samples overlap, resulting in the first comprehensive view of the competitive effects from school vouchers across the state of Louisiana.

Data

The data for this analysis come from four total sources. First, student-level data on 2010-11 through 2012-13 public school test scores for students in grades three through eight in math and English Language Arts (ELA) come from a restricted-use data file provided by the Louisiana Department of Education. Second, data on 2010-11 school performance scores and letter grades are publicly available on the Louisiana Department of Education's website. Third, street addresses, latitude, and longitude for all public schools in Louisiana in 2010-11 were retrieved from the National Center for Education Statistics' Common Core of Data, "Public Elementary/Secondary School Universe Survey." Finally, private school street addresses and information on religious orientation were retrieved from the National Center for Education Statistics' Private School Universe Survey, 2011-12.

Sample selection. Figure 2 describes the screening process for generating the analysis sample. Starting with the universe of public schools that appear in the NCES 2010-11 file, the first screen kept only those public schools that could be successfully mapped using ArcGIS software (approximately 90% of schools). The second screen required each school to have a minimum of three students taking the state test — the Louisiana Educational Assessment Program (LEAP) or integrated Louisiana Educational Assessment Program (iLEAP) — in grades 3 through 8, reducing the sample from 1,326 to 981 schools. The third and fourth screens excluded charter schools, which already experience competition for enrollment and thus are not relevant for this study, and schools in New Orleans, where a pilot version of the LSP was already operating. This reduced the final sample to 939 schools, a total of 676 of which received a "C," "D," or "F" grade at baseline, making their students voucher-eligible.

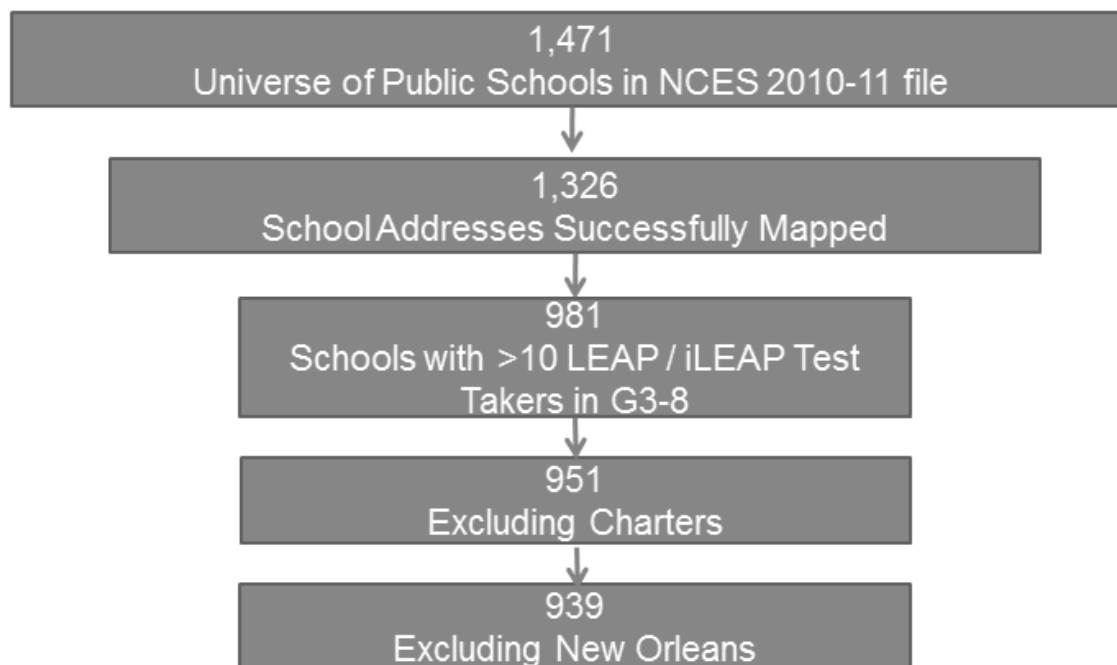


Figure 2. Sample Selection Process

Description of the analysis sample. Table 1 displays descriptive statistics for the primary analysis sample; the voucher-eligible schools are highlighted in bold. These lower-graded schools are distinguished from “A” and “B” schools by having higher proportions of students qualifying for free and reduced price lunch, higher proportions of special education students, higher proportions of African American students, the lowest average test scores in both math and ELA and the lowest proportion of schools scoring above the median for those two subjects. Voucher applicants and voucher winners are mostly like to come from a “D” school and the average number of voucher winners per school is highest for “D” and “F” schools.

Table 1

Descriptive Statistics by Accountability Grade

	Progress report grade				
	F	D	C	B	A
Number of schools	35	343	297	200	63
Type of school					
% elementary/ middle	62.86	87.17	80.47	79.00	87.30
% combination	37.14	12.83	19.53	21.00	12.70
School Characteristics					
% free lunch	89.42	87.45	69.06	53.80	38.15
% special education	23.80	14.21	12.42	11.28	8.10
% limited English proficient	0.47	2.28	1.89	1.48	1.09
% Black	84.28	69.69	33.65	19.53	24.69
% Hispanic	1.05	3.53	3.65	3.28	2.88
% white	13.32	24.11	58.83	73.13	67.65
% other race	1.35	2.67	3.87	4.06	4.79
Origin of Voucher Students					
Proportion of voucher applicants	8.43	66.34	25.00	0.24	0
Proportion of voucher winners	7.87	74.00	17.93	0.20	0
Average number of apps per school	5	5	2	0	0
Average number of winners per school	2	2	1	0	0
Test Score Outcomes 2010-11					
Average ELA standardized score	-0.38	-0.25	0.03	0.22	0.50
Average math standardized score	-0.39	-0.27	0.02	0.23	0.58
% of schs above median ELA score	0.06	0.07	0.64	0.97	0.97
% of schs above median math score	0.06	0.06	0.65	0.97	0.97
Test Score Outcomes 2011-12					
Average ELA standardized score	-0.35	-0.26	0.02	0.19	0.48
Average math standardized score	-0.35	-0.27	0.00	0.21	0.58
% of schs above median ELA score	0.06	0.08	0.64	0.95	0.97
% of schs above median math score	0.06	0.10	0.62	0.94	0.97
Test Score Outcomes 2012-13					
Average ELA standardized score	-0.34	-0.25	0.00	0.16	0.47
Average math standardized score	-0.32	-0.26	-0.02	0.17	0.51
% of schs above median ELA score	0.09	0.11	0.62	0.94	0.97
% of schs above median math score	0.09	0.15	0.59	0.90	0.97

Note: Student characteristics are from the 2010-11 school year

Source: Data on public school characteristics from The National Center for Education Statistics' "Public Elementary/Secondary School Universe Survey," 2010-11; Data on voucher applicants and winners and on test-score outcomes provided by the Louisiana Department of Education

Competition measures. The primary research question addressed in this study is whether public school achievement is influenced by competition from private schools brought about by a policy shock occurring in the 2012-13 school year. In order to capture the different degrees of competition experienced by public schools, we exploit variation in the geographic location of private schools to create four geocoded competition measures: distance, density, diversity, and concentration. In order to avoid reverse causation bias, these four variables are generated using data from before the LSP was announced.

The first competition variable is a distance measure, referring to the distance between a public school and its nearest private competitor. The underlying assumption informing this measure is that shorter distances equate to higher levels of competition for students (Figlio & Hart, 2014; Greene & Winters, 2006). We create this variable by plotting the geographic location of all public and private schools in the state of Louisiana.¹ The proximity value is recorded as the crow's flight distance—recorded in meters and converted to miles for analysis—between each public school and its nearest private competitor.

The second competition variable is a density measure; a widely used approach which approximates the degree of competition a public school is facing by the count of private competitors within a given radius (Carnoy et al., 2007; Figlio & Hart, 2014; Greene & Marsh, 2009; Greene & Winters, 2008). We draw either a 10-mile or 5-mile radius around each public school and count the number of private schools that fall within that boundary (Figure 3).

¹ All private school addresses (n= 359) and 90% of all public school addresses (n = 1,326) are successfully geocoded.

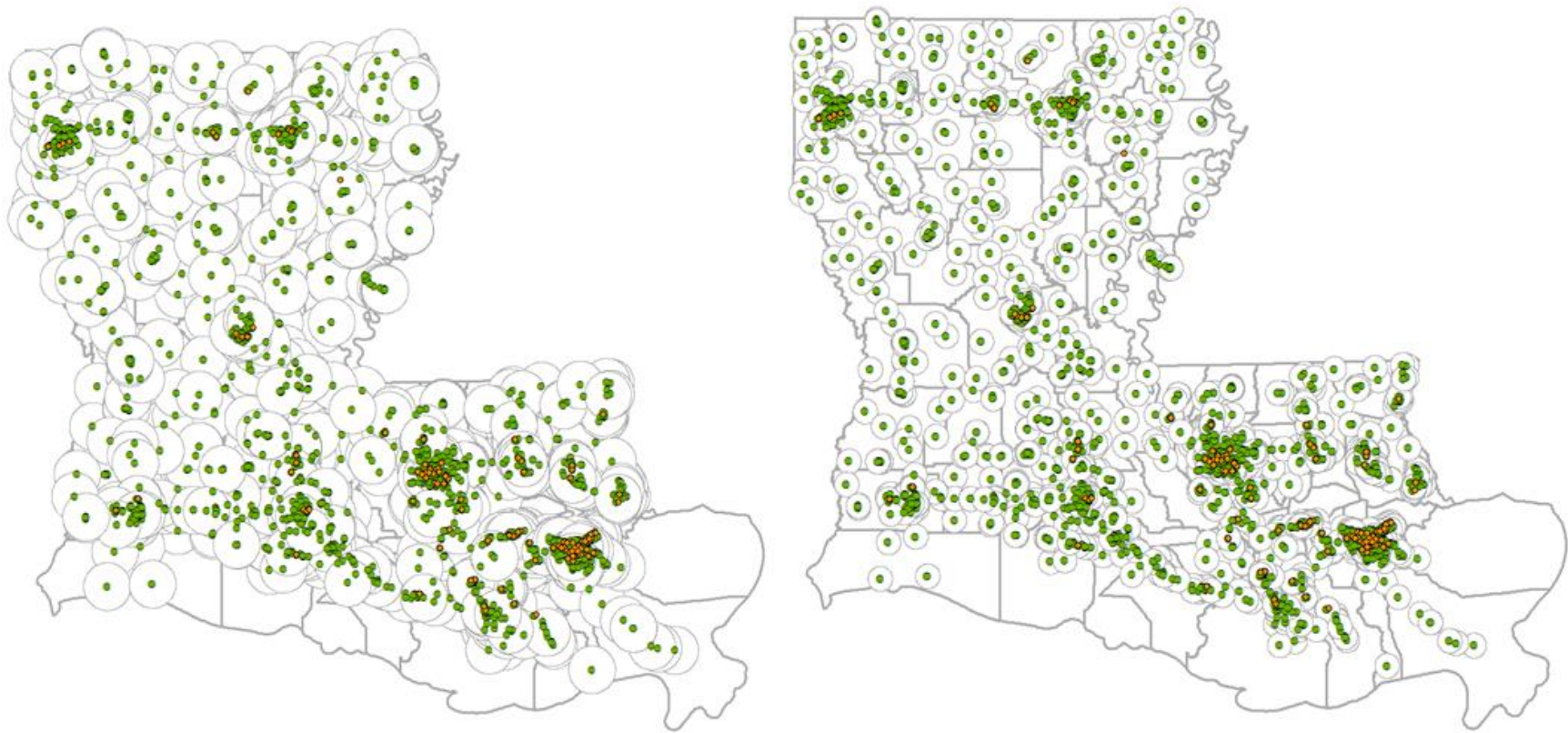


Figure 3. Public and private school locations in Louisiana. Public schools shaded green; private schools shaded orange. Left panel shows 10 mile radius around all public schools; right panel shows 5 mile radius.

The third competition variable is a diversity measure, which counts the different types of local private schools that are located within that 5 or 10-mile radius of a given public school. The underlying theory is that a more diverse selection of private schools should appeal to a broader range of families, thus increasing the level of competition (Figlio & Hart, 2014). School type is defined here by religious orientation. In order of frequency, the 18 religious orientations represented in the Private School Universe Survey for Louisiana in 2011-12 are: Roman Catholic, Nonsectarian, Christian (no specific denomination), Baptist, Episcopal, Assembly of God, Seventh- Day Adventist, Lutheran Church- Missouri Synod, Islamic, Presbyterian, Church of God in Christ, Jewish, Pentecostal, Church of Christ, Church of the Nazarene, Methodist, Other, and Other Lutheran.

The final competition variable generated is a concentration measure or modified Herfindahl index, computed as the sum of the squares of the market shares held by each private school's religious type. The values for the Herfindahl Index range from 0 to 1, with lower values corresponding to a smaller concentration of the share of private schools in the hands of just one religious denomination. Conversely, a Herfindahl score of 1 would indicate a monopoly on the private school market by just one religious type.

Table 2 contains descriptive statistics for the four geocoded competition variables. The average public school is 6.39 miles from a private competitor. Within a 10-mile radius, the average school has 11 private competitors and approximately two religious denominational types are represented. Just one school has the maximum density value of 100 and a further 13 schools have density values greater than or equal to 90; all of these schools are located in Jefferson Parish. Finally, the modified Herfindahl Index has an average value of .11, which is indicative of strong competitive pressure. The mean values for this set of variables are predictably smaller

within a 5-mile radius — the average school has five private competitors and only one religious denominational type is represented. The modified Herfindahl Index has an average value of .16, which is suggestive of strong competitive pressure.

Table 2

Descriptive Statistics of Geocoded Competition Measures

	Mean	SD	Min	Max
Distance: Miles to nearest private school competitor	6.39	8.23	.04	49.85
<i>Competition Measures in 10 mile Radius</i>				
Density: Number of local private schools	11.46	19.73	0	100
Diversity: Number of religious types represented	2.06	2.25	0	9
Concentration: Herfindahl Index	.11	.19	0	1
<i>Competition Measures in 5 mile Radius</i>				
Density: Number of local private schools	5.07	9.10	0	59
Diversity: Number of religious types represented	1.25	1.77	0	8
Concentration: Herfindahl Index	.16	.26	0	1

Note: Authors' calculations

Source: Public school data from The National Center for Education Statistics' "Public Elementary/Secondary School Universe Survey," 2010-11; Private school addresses from the National Center for Education Statistics' "Private School Universe Survey," 2011-12.

Methodology

A school fixed effects model is employed to estimate the effect of private school competition on public school performance, building upon the model estimated by Figlio and Hart (2014).

$$(1) \quad Y_{ist} = Y_{ist-1} + \alpha_s + \beta C_s * P_t * CDF_t + \gamma X_{it} + \mu S_{st} + \delta T_t + \epsilon_{ist}$$

Y_{ist} is the standardized math or reading score for student i in school s in year t ; Y_{ist-1} is the student's lagged test score; α_s is a school fixed effect; C_s is the measure of pre-policy competitive pressure facing school s ; P_t is an indicator variable identifying the post-policy year; CDF_t is an

indicator variable identifying those schools that became voucher eligible because they received a “C,” “D,” or “F” grade from the state in 2011; X_{it} is a vector of student demographic control variables including gender, race, special education status, an indicator for limited English proficiency (LEP), and eligibility for free/reduced lunch for student i in year t ; S_{st} is a vector of time-varying school characteristics (shares of students of each race and gender, the share eligible for free/reduced lunch, and the shares classified as LEP or special education); and T_t is a set of dummy variables indicating year. The β coefficient on the three-way-interaction of competition measures, post-policy year indicator, and a school’s “C,” “D,” or “F” grade is the parameter of interest. Standard errors are clustered at the school level.

Results

Table 3 presents the primary results. Each cell represents the coefficient on the three-way interaction between competition measure, post-policy year, and “C,” “D,” or “F” school grade. Regressions are run separately for each competition measure. For the discussion of results, we focus on the smaller of the two radii— the 5-mile radius — in order to be consistent with previous work in this area (eg. Figlio & Hart, 2014) although the results for both a 10 mile and 5 mile radius are presented.

Table 3

School Fixed Effects Regression Estimates of the Impact of LSP Competition on Public School Achievement, First Year Impacts

	10 Mile Radius		5 Mile Radius	
	ELA (1)	Math (2)	ELA (3)	Math (4)
<i>Main Results</i>				
Distance (r)	-.06 (.04)	.02 (.06)	-.06 (.04)	.02 (.06)
Density	.01 (.01)	.06 (.02)	.04 (.03)	.09** (.05)
Diversity	.26 (.17)	0.90*** (.25)	.21 (.16)	.61** (.26)
Concentration (r)	2.25*** (.65)	3.74*** (.91)	1.96*** (.74)	3.57*** (1.08)
<i>"C" Schools Only (n=297)</i>				
Distance (r)	-.10** (.04)	.00 (.06)	-.10** (.04)	.00 (.06)
Density	.01 (.03)	.06 (.04)	.02 (.06)	.10 (.09)
Diversity	.10 (.25)	.75** (.35)	.02 (.26)	.38 (.38)
Concentration (r)	1.12 (.86)	3.08*** (1.20)	.80 (.97)	3.28** (1.47)
<i>"D" Schools Only (n=344)</i>				
Distance (r)	-.06 (.08)	.10 (.11)	-.06 (.08)	-.10 (.11)
Density	.02 (.02)	.06** (.03)	.06 (.04)	.11** (.05)
Diversity	.49** (.21)	1.16*** (.31)	.46** (.21)	.86*** (.33)
Concentration (r)	2.88*** (.74)	4.18*** (1.09)	2.57*** (.83)	3.94*** (1.24)
<i>"F" Schools Only (n=35)</i>				
Distance (r)	.02 (1.21)	1.90* (.98)	.02 (1.21)	1.90* (.98)
Density	.01 (.08)	.23*** (.09)	.03 (.17)	.43** (.18)
Diversity	.93 (.74)	1.62* (.91)	.09 (.69)	1.88** (.84)
Concentration (r)	5.40* (2.78)	6.17* (3.37)	4.56 (2.94)	6.39* (3.69)

Table 3 (Cont.)

Note: The dependent variable is the 2012-13 standardized math or English Language Arts; Robust standard errors in parentheses; Variables followed by (r) are reverse coded to ease interpretation; Each cell represents the coefficient estimate on the interaction between the competition measure, being a "C," "D," or "F" school in Oct 2011, and a post-policy indicator from a separate regression model. Coefficients are multiplied by 100 for interpretability. Controls include indicators for gender, race, subsidized lunch eligibility, limited English proficiency (LEP), special education, year, percent of student body eligible for subsidized lunch, percent of each race, percent special education, percent LEP, as well as school fixed effects and prior year achievement; *** p<0.01, ** p<0.05, * p<0.1

The top panel displays the main results across all schools, “C” through “F.” Competition is shown to have statistically significant, positive impacts on student achievement in math or ELA for three of the four competition measures used- density, diversity, and concentration. Each additional private school located within a 5-mile radius of a given public school is associated with a .0009 standard deviation (SD) increase in math performance.² In terms of diversity, the addition of one private school religious type is associated with a .0061 SD increase in math. Finally, a one-unit increase in the Herfindahl Index is associated with a .0196 SD and .0357 SD increase in ELA and math, respectively. Overall, a one-unit increase in competition is associated with a 0 to .0357 SD increase in student academic performance.

In order to parse out the treatment effect to better understand which, if any, public schools may be affected by the LSP, the next three panels compare only a segment of all voucher- eligible public schools at a time. The second panel of Table 2 compares “A” and “B” graded public schools to just “C” schools, which produced around 18% of the total voucher winners (not including students coming from charter schools or schools within New Orleans, which are excluded from this analysis). Given the small number of voucher users coming from “C”- graded public schools, it is perhaps unsurprising that there is very limited evidence of a

² Coefficients in the table are multiplied by 100 to assist with the interpretation of very small effects.

competitive response by this group of schools. In each subject, just one of the four measures has a statistically significant impact. Specifically, we find that a reduction in the distance between a public school and its nearest private competitor is actually associated with a *decrease* in student ELA achievement of .0010 SD. On the other hand, student math achievement *increases* by .0328 SD with a one-unit increase in the Herfindahl Index. Given that we observe null effects using all other measures of competition with this group of schools, we conclude that it was unlikely that “C” schools responded strongly to competition from the LSP.

The third panel reduces the treatment group to just “D” schools. Given that approximately three-quarters of voucher- winners in our sample came from a “D” -graded public school, this is the group for which we most strongly expect to find a competitive response to the LSP, if there was one. Indeed, the results observed for this group of schools are statistically significant and positive for three out of four measures- density, diversity, and concentration. Examining the impact on just “D” schools, the competitive effect ranges from .0011 to .0394 of a standard deviation in math and from .0046 to .0257 in ELA.

Finally, looking at the treatment impact on just “F” schools, there are null effects in ELA and consistently significant, positive results in math. Even though only 8% of voucher winners came from “F” –graded public schools, there were approximately five applicants and two winners per school. We find the largest statistically significant impacts for this group of schools. A one-mile reduction in the distance to the nearest private school is associated with a .0190 SD increase in student math achievement. Similarly, each additional private school competitor within a 5-mile radius is associated with a .0043 SD increase in math achievement. The result is even larger using the diversity measure, where each additional type of nearby private school is associated with an increase of .0188 SD in math outcomes. Finally, a one-unit increase in the

concentration measure is associated with a .0639 SD increase in student math achievement. Overall, the results for “D” and “F” schools suggest that those schools in the sample that experienced the greatest loss of students to the LSP responded with a modest yet positive increase in student outcomes, particularly in math.

Sensitivity Analysis

While the primary analysis has strong external validity given that it incorporates all “C” through “F” public schools in the state, it is possible that these four competition measures are not accurately measuring the competitive pressure experienced by public schools in Louisiana. As a sensitivity check of the primary results, we employ an alternative identification strategy with stronger internal validity — a regression discontinuity design (RDD) — to see if the main results can be replicated.

The LSP is an ideal situation to apply an RDD analysis because school exposure to competition from the LSP depends upon ratings from the Louisiana letter grade system for public schools, part of the school and district accountability system. Letter grades are determined by a continuous measure known as the school performance score, which is an index of proficiency status in ELA, math, science, and social studies, and expected normative student longitudinal growth. Intervals along the school performance score continuum equate to a given letter grade. Low-income students wishing to participate in the LSP must have attended a public school that received a letter grade of “C”, “D”, or “F” for the most recent school year in order for students to qualify for voucher eligibility. It is reasonable to expect that schools that scored at the lowest threshold for receiving a “B” do not differ in substantial ways from those schools that scored at the highest threshold for receiving a “C,” allowing us to directly compare schools in these two

groups. Those schools that received a “C” grade or lower in October 2011 were directly exposed to vouchers for their low-income students. A subset of “high-C” schools, therefore, constitutes the treatment group for the RDD competitive effects analysis. It’s important to note that, in contrast to the primary analysis, “high-C” schools in the RDD are deemed to experience the threat of competition even if there are no private schools nearby. Meanwhile, the schools that received a low “B” grade had a school performance score that was close to the “C” schools, but they were not directly treated by the program because they were just above the cut-point. A subset of “low-B” schools therefore, constitutes the control group.

If the estimates obtained from the RDD analysis are largely consistent with the measured estimates for “C” schools in the primary analysis, we can be more confident in the validity of the primary findings.

RDD sample selection. Starting with the universe of public schools that appear in the state’s school performance score file for 2010-11, we merge this information with the state’s testing file in order to measure school-average achievement in Math and ELA over the three-year time period of this study, from 2010-11 to 2012-13. The first screen excludes schools that do not appear in the testing files such as lower elementary schools serving non-tested grades, reducing the sample to 1,352. The second screen excludes schools that do not have more than 10 LEAP or iLEAP test takers in grades 3 through 8 in each year of the study, which reduces the sample to 1,058. The third and fourth screens exclude charter schools, which already experience competition for enrollment and thus are not relevant for this study, and schools in New Orleans, where a pilot version of the LSP was already operating.³ This leaves us with 987 public schools,

³ The sample size reduction associated with excluding New Orleans schools is small because the majority of New Orleans schools were already excluded by the charter school screen.

521 of which are schools that received a “B” or “C” grade in October 2011 (Figure 4). The final analysis sample will be chosen from these 521 schools, depending on the bandwidth selected for the RDD analysis, which is explained in greater detail in the next section.

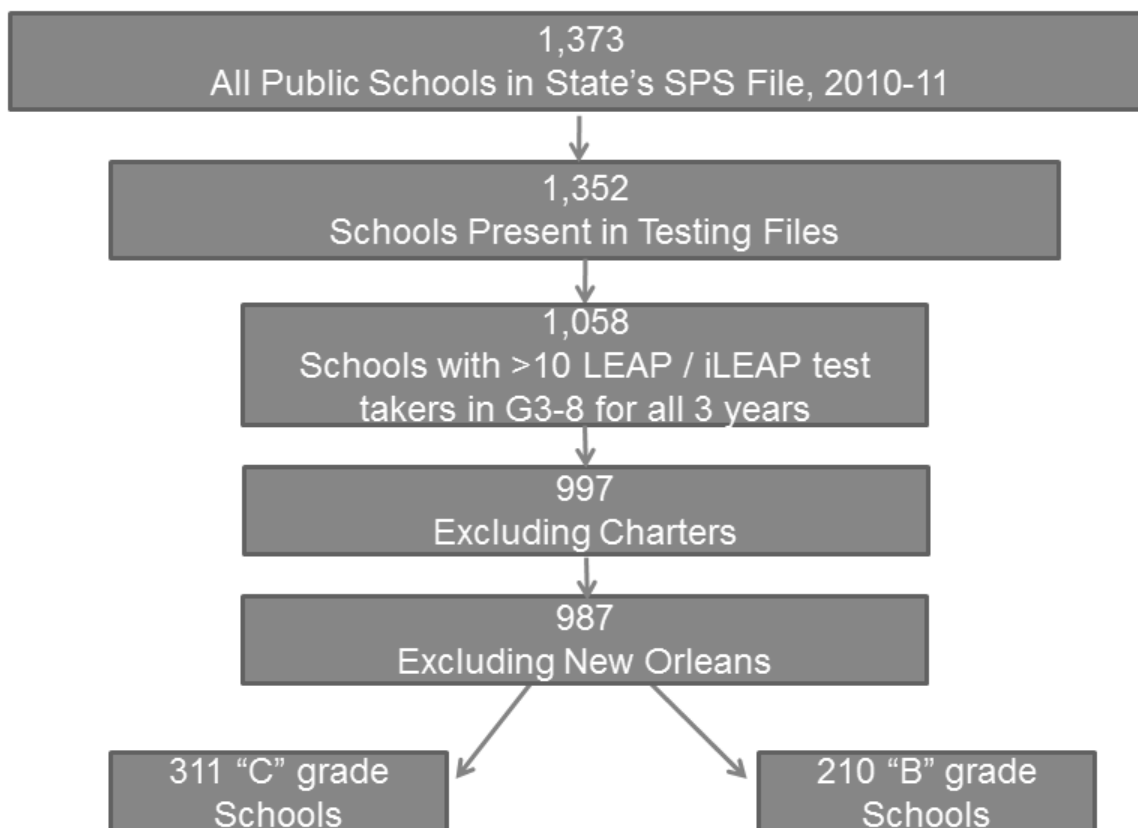


Figure 4. Sample Selection for RDD Analysis of LSP on Public School Performance

RDD research design. To estimate the competitive impact of the LSP, a reduced-form regression is used,

$$(2) \quad A_{jt} = \alpha + \gamma D_{jt} + \beta f(P_{jt}) + \lambda X_{jt} + \varepsilon_{jt}$$

A_{jt} is the average achievement of students in school j , in year t ; D_{jt} is an indicator for experiencing the threat of competition — i.e., it is a binary variable that takes on a value of one if the school’s SPS score is 105 or lower and zero otherwise⁴; P_{jt} is a vector containing the school performance score (SPS) used to assign school grades; X_{jt} is a vector of school level covariates including an indicator for school type (elementary/middle or combination school), school percent female/Black/Hispanic/special education/limited English proficient, and percent qualifying for free or reduced price lunch. Finally, ϵ_{jt} is an idiosyncratic error term. A quartic polynomial is included in P_{jt} to control for the functional form of the SPS. The estimated impact of competition from private schools through the LSP, γ , can be interpreted as causal under the assumption that, conditional on the school performance score, the assignment of grades is uncorrelated with the error term ϵ_{jt} .

The strength of the RDD is that it doesn’t incorporate all eligible public schools — only a narrow set of schools above and below the 105- point SPS cut-off that distinguishes “C” schools from “B” schools. The more similar the SPS score of the “B” and “C” schools on either side of this cut-off, the more similar we expect these schools to be in both observable and unobservable ways, strengthening the internal validity of the analysis. An analysis of the distribution of school performance scores in 2011 shows that over half (53%) of all schools received “B” or “C” grades, ensuring that a sufficiently large number of school observations can be drawn from in the RDD analysis to generate reliable estimates of the causal effects of school competition on “C” schools (Figure 5).

⁴ The reader should note that none of the spatial measures of competition used thus far are reflected in this indicator.

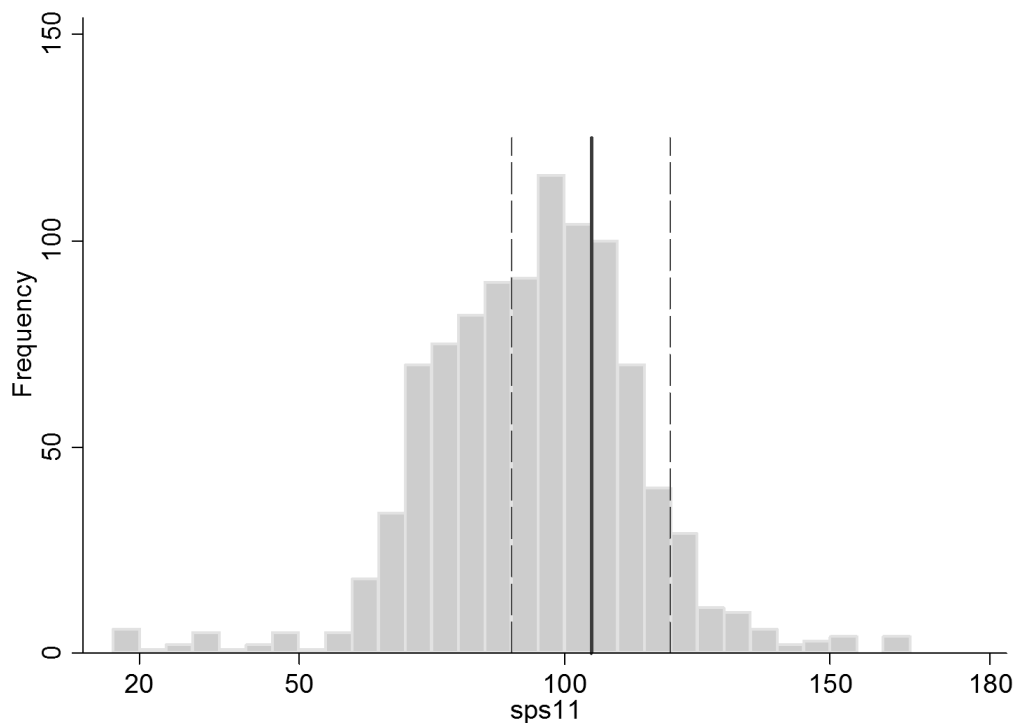


Figure 5. Distribution of school performance scores in 2010-11 (excluding charters and schools in New Orleans). The dark vertical line represents the cut-off between “B” and “C” graded schools (105 SPS points). The thinner, dashed lines represent lower bound of a “C” score (90 points) and upper bound of a “B” score (119.9 points), $n = 987$ schools total, 521 of which fall within the specified window.

In selecting the width of the “window” of observations to be used for the RDD, we start by using the smallest bandwidth feasible, which is one point above and one point below the B/C cutoff. We also experiment with using wider bandwidths of five and ten points above and below the cutoff, which allow us to incorporate a larger sample of schools. Table 4 presents descriptive statistics of the samples of “high-C” treatment schools and the “low-B” control schools, depending on the bandwidth selected. Although only 47 schools are included in the sample when we select the one point trim, t -tests confirm baseline equivalence between the two groups. There are no statistically significant differences in any of the school characteristics in our data

such as the percent of students belonging to minority races, the percent of students with special educational needs, or the percent of the student body that qualifies for the federal free and reduced price lunch program — a common proxy for poverty status. Furthermore, when we compare school-average, standardized ELA and math scores across the treatment and control groups with a one-point trim, the “high-C” schools have lower mean scores, but these differences are not statistically significant. Once we expand the bandwidth to a five-point trim, the tests of differences in school characteristics remain statistically insignificant but the lower mean scores of the treatment group of schools in both math and ELA attain statistical significance. Finally, once we expand the bandwidth to a ten-point trim, four out of seven of the school characteristics examined are statistically significantly different between the treatment and control groups. Additionally, in this widest bandwidth sample, the treatment group has significantly lower baseline math and ELA scores.

Although the RDD analysis controls for all of the school characteristics tested in Table 4 and the school performance score that was determined by 2010-11 test score outcomes, it is possible that other differences between the treatment and control groups created by a five-point or ten-point trim could confound the RDD results, which is why we rely on the results from the regression analysis with a one-point trim as our primary RDD findings.

Table 4

Descriptive Statistics for RDD Analysis

	1pt Trim				5pt Trim				10pt Trim			
	Treat.	Control	Diff.	<i>p</i>	Treat.	Control	Diff.	<i>p</i>	Treat.	Control	Diff.	<i>p</i>
Count	23	24			104	104			220	173		
<i>School Type</i>												
% Elem/ middle	0.70	0.88	-0.18	0.139	0.77	0.82	-0.05	0.394	0.80	0.79	0.01	0.757
% Combination	0.30	0.13	0.18	0.139	0.23	0.18	0.05	0.394	0.20	0.21	-0.01	0.757
<i>School Characteristics</i>												
% Special Education	10.85	12.58	-1.73	0.157	11.40	11.55	-0.15	0.796	12.38	11.31	1.07**	0.015
% Black	23.89	23.25	0.64	0.926	25.90	21.48	4.42	0.15	29.13	19.34	9.79***	0.000
% Hispanic	3.21	5.04	-1.83	0.368	3.34	3.81	-0.46	0.496	3.55	3.41	0.14	0.792
% White	68.74	67.78	0.96	0.900	66.66	70.13	-3.47	0.29	63.66	73.16	9.50***	0.000
% Other Race	4.16	3.94	0.22	0.890	4.09	4.58	-0.49	0.647	3.66	4.10	-0.44	0.481
% LEP	1.43	2.40	-0.97	0.441	1.49	1.41	0.08	0.847	1.80	1.40	0.40	0.336
% Free Lunch	60.59	60.53	0.06	0.990	63.13	56.80	6.33	0.002	66.17	55.22	10.95***	0.000
<i>Test Scores, 2010-11</i>												
Average ELA Z Score	0.09	0.13	-0.04	0.293	0.07	0.19	-0.11***	0.000	0.06	0.20	-0.14***	0.000
Average Math Z Score	0.07	0.12	-0.06	0.228	0.07	0.19	-0.12***	0.000	0.05	0.22	-0.16***	0.000

Note: LEP = Limited English Proficient

Source: Data on public school characteristics from The National Center for Education Statistics' "Public Elementary/Secondary School Universe Survey," 2010-

11

To better help the reader visualize changes in student achievement in the baseline and outcome year, we first plot kernel densities of school average scale scores for all “B” and “C” schools in the two time periods and academic subjects under examination (Figure 6). In the presence of a strong competitive effect, one might expect to see the performance of “C” schools to have shifted less to the left than the performance of “B” schools. A visual analysis of these graphs does not provide much suggestive evidence for the presence of a competitive effect in math. In ELA, the mean performance for “B” schools appears to have decreased but there does not appear to have been any shift in the mean performance for “C” schools.

These graphs, of course, cannot signify a causal relationship between competition from the LSP and student achievement, however, because they are confounded by other factors such as student demographics and include all “C” and “B” schools. One must switch instead to the analysis that relies on a regression discontinuity design and a tighter comparison of “high-C” to “low-B” schools to reliably measure the impact of the LSP.

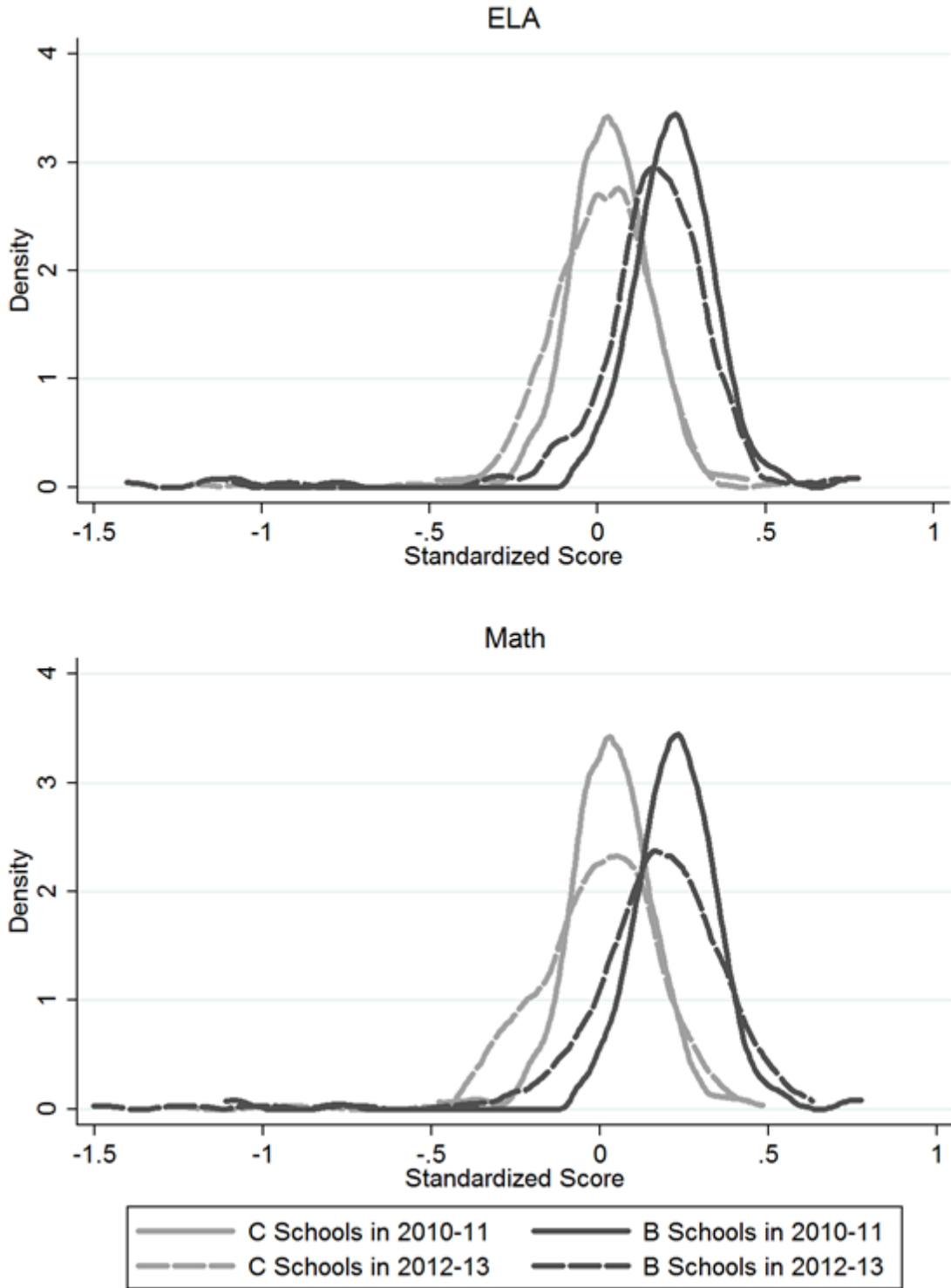


Figure 6. Distribution of School Average Standardized Scores in ELA and Math in 2010-11 and 2012-13 for “C” and “B” graded schools. Densities shown for the 521 schools used in the RDD analysis.

RDD results. We next present a graphical analysis of the regression discontinuity estimation approach. The null results for “C” schools from the geocoded analysis are confirmed, as there does not appear to be strong evidence of a competitive response in “C” schools. Figure 7 plots school average math and English language arts standardized scores against the “B” or “C” letter grade received. To aid with interpretation, these scatterplots include a locally weighted “Fan” regression line, which uses an Epanechnikov kernel function (Fan and Gijbels, 1996). Regressions are calculated separately for “B” and “C” schools and provide a weighted average of math and ELA performance for a given school performance score. Because the calculation of school letter grades was informed by proficiency rates on ELA and math assessments, one expects to see a relationship between the school performance score and the raw scale scores. As expected, school-level scores rise gradually within letter grade bands. There is a minor break between “B” and “C” schools in math outcomes in both years but nothing of significant magnitude to suggest any impact of competition on school performance. Figure 8 provides additional support for this finding. Plots of residuals from regressions that control for the factors used to determine school performance scores show there is no noticeable break in the regression lines for “C” and “B” schools.

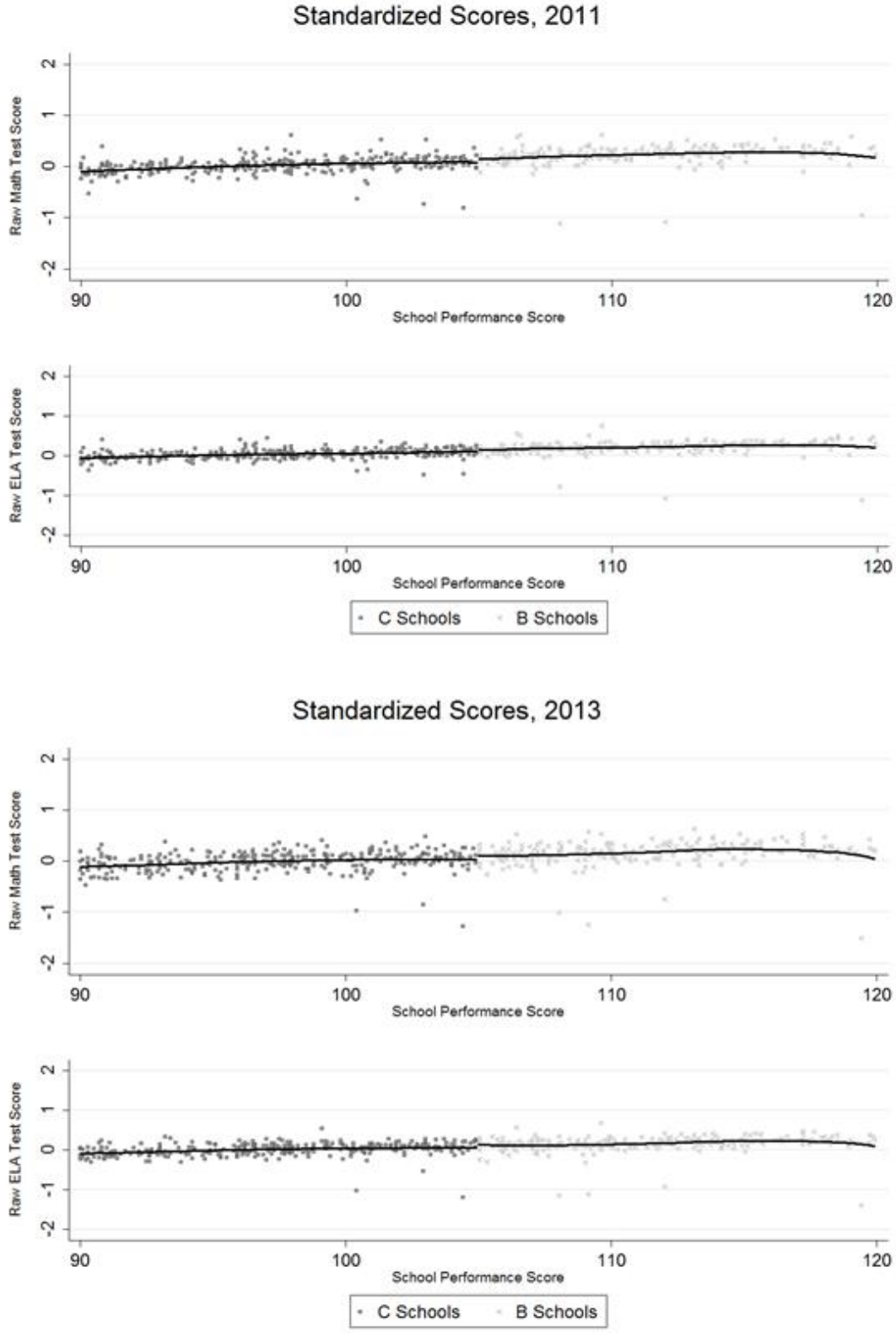


Figure 7. School Average Math and ELA Standardized Scores in 2010-11 and 2012-13, by “B” and “C” letter grades (Raw). The solid lines plot estimates from a locally weighted “Fan” regression line with a bandwidth of 5 points.

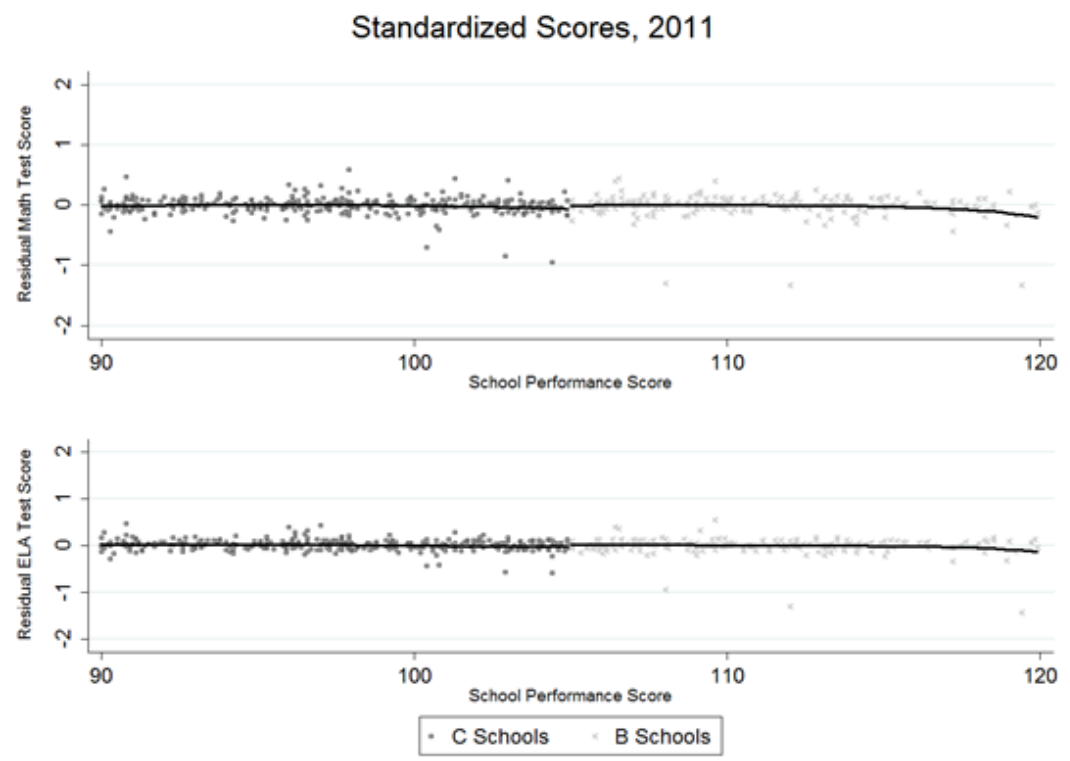


Figure 8. School Average Math and ELA Standardized Scores in 2010-11 and 2012-13, by “B” and “C” letter grades (Residuals). The solid lines plot estimates from a locally weighted “Fan” regression line with a bandwidth of 5 points.

Finally, Table 5 presents the results of regressions in the form of equation (2), which confirm the graphical finding of null effects. School average standardized scores in ELA and math are regressed on the school performance score, an indicator for experiencing the competitive threat (ie. being a “high-C” school), school-level demographic control variables, and indicators for school type (elementary/ middle or a combination school that consists of grades K-7 or K-8, for instance). All regressions are weighted by the number of tested students in that subject and have robust standard errors. Models 1 and 2 examine test scores from 2010-11, before the introduction of the LSP. We expect to find no significant differences in student outcomes, which is confirmed by the data. When we examine test scores from 2012-13, when the competitive threat was now present, there is still no difference in test scores between “high-C” and “low-B” schools, conditional on the school performance score. As a robustness check and to maximize the power of the RDD, we also increase the size of this bandwidth to five and ten points above and below the cut off to see if the inclusion of more schools alters the results, which it doesn’t. Finally, we also experiment with including lagged test scores but the indicator for competitive threat does not approach statistical significance in any of these specifications.

Table 5

The Impact of Competition on Average School-Level Achievement, RDD Results Comparing "High-C" (Treatment) to "Low-B" (Control) Schools

	2011 (Baseline)		2013					
	Math	ELA	Math			ELA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
"High-C" Schools	-.10 (.09)	-.06 (.07)	-.09 (.08)	-.02 (.05)	-.04 (.04)	-.01 (.06)	.02 (.04)	-.04 (.03)
1-Point Bandwidth	✓	✓	✓			✓		
5-Point Bandwidth				✓			✓	
10-Point Bandwidth					✓			✓
Observations	47	47	47	208	393	47	208	393

Note: Estimates presented in standard deviation units. Robust standard errors in parentheses. Regressions weighted by number of test takers in each subject. All models include controls for school level demographics and school type (elementary/middle v. combination school). *** significant at $p < .01$, ** significant at $p < .05$, * significant at $p < .1$

As a final sensitivity test, we rerun the RDD, this time comparing "High-D" to "Low-C" schools. One could argue that because the "D" and "F" schools received a higher preference in the lottery, the "C" schools didn't actually experience a real competitive threat. Table 6 presents the results when we run the RDD on the margin between "C" and "D" schools. As before, all regressions are weighted by the number of tested students in that subject and have robust standard errors. Within any of the three bandwidths selected, there is still no difference in test scores between the treatment ("high-D" schools this time) and control ("low-C" schools), conditional on the school performance score.

Table 6

The Impact of Competition on Average School-Level Achievement, RDD Results Comparing "High-D" (Treatment) to "Low-C" (Control) Schools

	2011 (Baseline)		2013					
	Math	ELA	Math			ELA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
"High-D" Schools	.06 (.09)	.06 (.08)	.05 (.10)	-.05 (.07)	-.04 (.05)	.08 (.08)	-.05 (.05)	-.04 (.04)
1-Point Bandwidth	✓	✓	✓			✓		
5-Point Bandwidth				✓			✓	
10-Point Bandwidth					✓			✓
Observations	47	47	47	181	380	47	181	380

Note: Estimates presented in standard deviation units. Robust standard errors in parentheses. Regressions weighted by number of test takers in each subject. All models include controls for school level demographics and school type (elementary/middle v. combination school). *** significant at $p < .01$, ** significant at $p < .05$, * significant at $p < .1$

Discussion

The results presented in this article are stable across two separate analyses relying on equally plausible specifications. For the primary analysis, geocoded competition measures of distance, density, diversity, and concentration are employed. These measures have high external validity because they incorporate all Louisiana public schools, "A" through "F" and have been successfully utilized in similar studies of private school choice programs (Carnoy et al., 2007; Figlio & Hart, 2014; Greene & Marsh, 2009; Greene & Winters, 2007 & 2008). Results reveal statistically significant, modest, positive impacts on student achievement in those schools that experienced the greatest average loss of students through the LSP, the lowest-graded public schools. These impacts are largest in math and range from .0011 to .0639 SD in the "D" and "F" schools.

As a sensitivity analysis, we also use a regression discontinuity design to test whether students in "high-C" schools that are exposed to competition from the LSP realize greater

performance gains than their peers in “low-B” public schools that are similar in many respects but are unaffected by competition from the program. Similar to the mostly null findings for “C” schools reported in the primary analysis, RDD estimates also find null effects across both Math and ELA.

One interpretation of these results could be that the lowest-graded public schools simply experienced regression to the mean in later tests. That explanation is unlikely responsible for the findings of this particular study, however, given that the geocoded competition measures show that, within the categories of “D” and “F” schools, those facing a greater competitive threat improved to a greater degree.

Overall, the results of this analysis indicate that Louisiana public school performance improved slightly in those schools that lost the largest average number of students to the LSP, which is suggestive of a modest competitive response to the voucher program. It will be important to keep studying the systemic effects of the program as the number of voucher users grows over time and public school administrators and teachers have time to potentially coordinate a response.

Caveats

There are a number of contextual factors that the reader should bear in mind when interpreting these results. First, recall that the degree of competition felt by individual public schools was quite small. On average, just 2 students left each “D” or “F” school and a single student left each “C” school in this sample. As enrollment in the program grows, we might expect the competitive response to grow too. Second, given the design of this analysis, it is

necessary to exclude charter schools (which already experienced competition for students) and all schools in New Orleans (where a pilot version of the program was already well- established). Thus, the results presented here only apply to traditional public schools outside of the largest city in Louisiana. Third, it is unclear how much power a traditional public school principal really has in order to respond to competition. In many schools, budget setting, policy development, and hiring decisions are made at the district level, leaving the principal with few assets to deploy in ways that might measurably impact student performance. Fourth, this analysis examines student achievement on standardized tests to judge the competitive impact of the LSP but public schools may respond to the competitive pressure of the LSP in other ways that are not captured by test score gains and thus would not show up in this type of analysis. This might include effects on students' non-cognitive skills such as persistence and conscientiousness that could lead to improved attainment outcomes later in life.

Conclusion

This article presents an analysis of the competitive impacts of the Louisiana Scholarship Program by comparing public school achievement in schools exposed to the threat of the LSP to achievement in those public schools that were unaffected by the program. Using two separate analyses relying on different specifications— one with strong external validity (the primary, geocoded measures approach); the other with strong internal validity (the regression discontinuity design)— we find modest, statistically significant, positive impacts in the first year of the statewide expansion of the program in those public schools that experienced the greatest competitive threat.

The primary analysis presented in this article exploits variation in the geographic location of private schools to estimate the competitive impact of the LSP on public school math and English language arts achievement using four geocoded measures of competition— distance, density, diversity, and concentration. We find modest, statistically significant, positive impacts ranging from 0 to .05 SD in ELA and .06 SD in math. In particular, we tend to find significant, positive effects in the “D” and “F”-graded public schools, which were the schools from which over 80% of LSP voucher winners originated.

Secondary analyses use a stronger identification strategy to validate the primary findings, revealing null effects for the schools that experienced the lowest competitive pressure. We compare the achievement of students in public schools that received a high “C” grade from the state— thus making their low-income students voucher-eligible— to the achievement of students in public schools that received a low “B” grade, in which no students could’ve applied for a voucher. By comparing student achievement around the narrow “C”/ “B” grade cutoff, we are comparing two groups of schools that are similar in all other respects, apart from the competition experience, which creates the basis for a rigorous quasi-experimental analysis of the competitive impacts of the LSP. The regression discontinuity analysis finds no impact on math or English language arts performance in the “high-C” schools exposed to competition from the program.

Based on the evidence from two separate identification strategies, we can conclude that competition from the LSP has had a null to modest positive impact on student achievement in Louisiana public schools, with positive impacts most likely to be found in the lowest-performing public schools.

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Appendices



February 22, 2013

*Office of Research Compliance
Institutional Review Board*

MEMORANDUM

TO: Patrick Wolf Jay Greene
 Anna Jacob Egalite Jonathan Mills
 Daniel Bowen Albert Cheng
 Collin Hitt Martin Lueken
 Thomas Waggoner

FROM: Ro Windwalker
 IRB Coordinator

RE: New Protocol Approval

IRB Protocol #: 13-02-501

Protocol Title: *State Mandated Evaluation of the Louisiana Students Scholarships for Excellence Program and Course Choice Program*

Review Type: EXEMPT EXPEDITED FULL IRB

Approved Project Period: Start Date: 02/22/2013 Expiration Date: 02/21/2014

Your protocol has been approved by the IRB. Protocols are approved for a maximum period of one year. If you wish to continue the project past the approved project period (see above), you must submit a request, using the form *Continuing Review for IRB Approved Projects*, prior to the expiration date. This form is available from the IRB Coordinator or on the Research Compliance website (<http://vpred.uark.edu/210.php>). As a courtesy, you will be sent a reminder two months in advance of that date. However, failure to receive a reminder does not negate your obligation to make the request in sufficient time for review and approval. Federal regulations prohibit retroactive approval of continuation. Failure to receive approval to continue the project prior to the expiration date will result in Termination of the protocol approval. The IRB Coordinator can give you guidance on submission times.

This protocol has been approved for 570,000 participants. If you wish to make *any* modifications in the approved protocol, including enrolling more than this number, you must seek approval *prior to* implementing those changes. All modifications should be requested in writing (email is acceptable) and must provide sufficient detail to assess the impact of the change.

If you have questions or need any assistance from the IRB, please contact me at 210 Administration Building, 5-2208, or irb@uark.edu.



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June 16, 2014

To: The Graduate School

Re: Confirmation of Authorship

This letter is to confirm that Anna Jacob is the lead author of the article, “The Effect of the Louisiana Scholarship Program on Student Achievement in Louisiana Public Schools.” She completed more than 51% of the work that went into this paper.

Sincerely,

Patrick J. Wolf, Ph.D.

**The Competitive Effect of the Indiana Choice Scholarship Program on Student
Achievement in Indiana Public Schools**

By: Anna Jacob and Kate Dougherty

Abstract

This article presents an analysis of the competitive impacts of the Indiana Choice Scholarship Program (ICSP), a statewide school voucher program that provides public funds to low- and middle-income families to cover tuition costs at participating private schools, both religious and non-religious. Geocoded measures are included in a school-fixed effects model to calculate the impacts of private school competition on public school performance in Math and ELA in the first year of the program, school-year 2011-12. These measures include distance, density, diversity, and concentration of private schools within a five- and ten-mile radius. Overall, the results presented here do not provide strong evidence of a competitive response to the first year of the ICSP. In math, none of the four competition measures is significantly related to school-average performance, whereas in English Language Arts, three out of eight results provide evidence of a statistically significant, positive competitive effect. Depending on the radius selected, a one-unit increase in the concentration measure (a modified Herfindahl Index) is associated with a .04 to .05 SD increase in school-average ELA achievement.

The Competitive Effect of the Indiana Choice Scholarship Program on Student Achievement in Indiana Public Schools

This study analyzes the competitive impact of the Indiana Choice Scholarship Program (ICSP), passed as part of House Enrolled Act 1003-2011 (Public Law 92-2011). This school voucher program provides state-funded vouchers to assist low- and middle-income students with the payment of tuition and fees at participating private schools, including both religious and non-religious options. Enrollment in the ICSP has grown every year the program has been in operation (Table 1). During the 2011-12 school year, ICSP scholarships were limited to 7,500 students; that number doubled in the program's second year of operation and, for the 2013-14 school year, the enrollment cap was removed altogether so that ICSP scholarships could be awarded to all eligible student applicants. During the 2013 legislative session, eligibility criteria were expanded to include a broader cross-section of students, incorporating students with special educational needs, siblings of current ICSP scholarship users, and students coming from failing public schools.

Table 1. Growth in Indiana Choice Scholarship Program Participation, Students and Schools

Year of Operation	Student Enrollment Cap	Student Program Participation	Private School Program Participation
First Year (2011-12)	7,500	3,911	241
Second Year (2012-13)	15,000	9,139	289
Third Year (2013-14)	N/A	19,809	313

Source: Choice Scholarship Program Annual Report: Participation and Payment Data 2011-12, 2012-13 & 2013-14. Prepared by the Indiana Department of Education Office of School Finance, January 27, 2014.

Due to limitations on the availability of data, this study measures the competitive impacts of the ICSP in its first year of operation only, the 2011-12 school year. It is appropriate to test for

a competitive response in the program's first year for at least two reasons. First, even though the size of the program was relatively small in 2011-12, stakeholders and the general public were aware that the enrollment cap would increase substantially every year, with estimates at the time suggesting that as many as 62 percent of Indiana families would eventually be eligible to participate.⁵ Despite its initial size, the program was clearly perceived by many observers as a serious threat to Indiana's public schools. Shortly after the program's announcement, for instance, a group of taxpayers represented by the National Education Association filed suit against the program in an attempt to block it. This lawsuit progressed through the courts until it reached the Indiana Supreme Court, where it was settled in March 2013. As such, it is reasonable to assume that the program was sufficiently threatening to generate a competitive response from Indiana's public schools in its first year of operation.

Second, because student enrollment numbers were so small the first year of the ICSP, analyzing just this year of data actually allows us to measure the pure competitive impact more cleanly than is possible in later years, when compositional and resource changes altered the populations of the sending public schools. As larger numbers of students continue to migrate from their residentially-assigned public schools to participating private schools, it becomes harder to isolate the incentive effect of the ICSP using the available aggregate school-level data. In the third year of the program, for instance, almost 20,000 students used an ICSP voucher to transfer schools, resulting in perceptible revenue impacts and changes in the student bodies of traditional public schools that could equally affect student achievement as much as a pure competitive shock.

⁵ See for instance the July 21, 2011 press release titled, 'Institute for Justice Moves to Intervene in Defense of School choice Program', which cites the 62 percent figure, which was later repeated in numerous media reports: <http://www.ij.org/indiana-school-choice-release-7-21-2011>

Theoretical Framework

Reform policies that foster the development of market forces in education are increasingly popular in the United States as a channel through which to address widespread discontent with student performance (Chubb & Moe, 1990; Friedman, 1962). Economic theory suggests that such policies will result in participant and systemic impacts that will raise the quality of education in the private schools accepting publicly-funded school vouchers or tuition tax credit scholarships and in the traditional public schools as well.

Across much of the United States, traditional public schools operate a monopoly over neighborhood schooling options, resulting in weak incentives for these residentially-assigned public schools to address instructional, curricular, or organizational deficits whose improvement would appeal to students and their families. Although families are free to exercise residential choice and thus simultaneously choose their desired public school (Tiebout, 1956), Tiebout choice is limited and often impacted by other factors such as cost, access to popular transportation routes, or proximity to friends and family. This can be especially limiting for economically disadvantaged families, resulting in unequal schooling opportunities for students from lower-income backgrounds. The introduction of market mechanisms via voucher or tuition tax credit scholarships, however, alters this dynamic and may strengthen the competitive incentive for public schools so that only those schools with higher student achievement per dollar spent are rewarded (Friedman, 1962).

There are at least three responses we might hypothesize that would result from injecting competitive pressure into K-12 public schooling. On the one hand, school leaders may implement reforms that use existing resources more efficiently, encourage innovation, and

organize staffing and curricula in a manner that is maximally responsive to student needs. If this is the case, we might observe a general rise in test scores for those schools experiencing the strongest competitive pressures. On the other hand, competition from the ICSP may force traditional public schools to cut expensive programs, limit instructional and administrative staff, increase class sizes, and result in a general lowering of morale and decline in school-wide performance. This could be exacerbated by compositional and resource changes if the highest-achieving and most motivated students exit the system en masse. Of course, the third hypothesized response is none at all. If the threat from competition is trivial or schools simply respond with empty symbolic gestures (Hess, 2002), the impact of the choice program will not be detectable in school-average academic outcomes.

The remainder of this article proceeds as follows. First, we present a summary of literature on the competitive impacts of private school choice programs. We then describe the data and research design used in this analysis. The following section presents the results. The paper concludes with a discussion of the policy implications and acknowledges the limitations of the current study.

Previous Literature

There is a wealth of literature examining competition effects in K-12 education. A significant number of studies measure competition between public schools, for instance (Blair & Staley, 1995; Borland & Howsen, 1992; Hanushek & Rivkin, 2003; Hoxby, 2000; Marlow, 1997; Millimet & Rangaprasad, 2007, Zanzig, 1997). Additionally, numerous studies have examined competition between public and private schools (Andersen & Serritzlew, 2007; Arum, 1996; Couch, Shughart & Williams, 1993; Dee, 1998; Hoxby, 1994; Jepsen, 2002; Kasman &

Loeb, 2013; McMillan, 2000; Sander, 1999; Scheld, 2014; West & Woessman, 2010). The work that is most relevant for this study, however, is competition responses in traditional public schools occurring as the result of a private school choice program such as a tuition tax credit or voucher program. There have been 21 such studies examining impacts on student academic outcomes in 7 locations across the United States (Egalite, 2013). The majority of these studies have taken place in Florida (nine studies) and Milwaukee, Wisconsin (six studies).

In the state of Florida, three programs have provided publicly-funded vouchers for the tuition of public school students wishing to transfer to private schools. The first is the Florida Opportunity Scholarship Program, established as part of the reform program known as the A+ Plan, which offered school vouchers to students attending public schools that were designed as failing twice in a four-year period. This program ran from June 1999 until the Florida Supreme Court ruled it unconstitutional in January 2006. In total, there have been 7 studies of the competitive effects of this program, all of which found positive competitive impacts on affected traditional public schools (Chakrabarti, 2008a; Figlio & Rouse, 2006; Forster, 2008a; Greene, 2001; Greene & Winters, 2004; Rouse, Hannaway, Goldhaber, & Figlio, 2013; West & Peterson, 2006). The second program is the Florida Tax Credit Scholarship Program, established in 2001 and still in operation today, providing vouchers to students from low-income families. Figlio and Hart (2014) found that increases in competition as a result of this tax credit program were associated with improvements in student test scores across a variety of competition measures. The third Florida program is the McKay Scholarships for Students with Disabilities Program, established in 1999 and currently serving approximately 24,000 students. A 2008 study by Greene and Winters found that increased exposure to this voucher program is associated with

substantial improvements in the test scores of students with disabilities that remain in the public school system.

Established in 1999, meanwhile, the Milwaukee Parental Choice Program (MPCP) provides vouchers to low and middle-income families to attend private schools at state expense. All six studies of the competitive effects of the MPCP have shown a mixture of neutral-to-positive to positive results (Carnoy et al., 2007; Chakrabarti, 2008b; Greene & Forster, 2002; Greene & Marsh, 2009; Hoxby, 2003; Mader, 2010). Meanwhile, studies of competition effects from school voucher or tuition tax credit programs have also been conducted in Ohio (Carr, 2011; Forster, 2008b), Texas (Greene & Forster, 2002; Merrifield & Gray, 2009), Washington D.C. (Greene & Winters, 2007), Maine (Hammons, 2002), and Vermont (Hammons, 2002). Of these 21 studies, only one — an analysis of a voucher program in Washington D.C. — showed no impacts across all subjects (Greene & Winters, 2007).

A competitive effects analysis of the Indiana Choice Scholarship Program offers a number of distinct advantages over existing studies in this area. First, this study can take advantage of the geographic diversity of a major school voucher program affecting students across the entire state of Indiana whereas much of the previous work in this area has examined impacts within a single city. The broad geographic scope of the program maximizes the variation in competition faced by public schools throughout Indiana, increasing the external validity of the study and, thus, the opportunity for results to be relevant in other contexts. Second, this analysis of the competitive effects of the ICSP has a strong identification strategy that takes advantage of a panel dataset instead of running a descriptive analysis of cross-sectional data. By applying a school fixed effects model, this analysis takes full advantage of the policy change that resulted in the introduction of the program, comparing pre-program trends to achievement outcomes after

the introduction of the policy. Third, although a number of studies already exist that examine the impact of a statewide school voucher program in Florida, such studies are unable to disentangle the accountability effects of the A-F school letter grading policy from the competitive effects of the voucher threat for consistently low-performing schools because both policies were implemented at the same time. In Indiana, however, the A-F school letter grading policy predates the introduction of the voucher program making it possible to compare achievement trends before and after the introduction of the voucher program, net of the accountability impact of the letter grading policy.

Data

The data for this project come from three total sources. First, data on public school math and English Language Arts (ELA) performance on the state test, the ISTEP+, were downloaded from the Indiana Department of Education's Assessment website for school years 2008-09 through 2011-12. The ISTEP+ is administered annually in grades 3 through 8 and used for school and student accountability purposes. Test results are provided to the public in a format that is broken out by grade level within individual schools. Second, the physical addresses of all public schools and general demographic information for these schools were retrieved from the Public Elementary/Secondary School Universe Survey, which is publicly available from the National Center for Education Statistics' Common Core of Data. Third, the physical addresses of all private schools in the state were retrieved from the Private School Universe Survey (PSS), which can also be downloaded from the National Center for Education Statistics' Common Core of Data.

The Indiana Department of Education publishes the grade-level means and standard deviations for the entire state on the annual ISTEP+. This information was used to standardize test scores within grade before averaging across grades to create a school-average standardized score.

Sample selection. Figure 1 describes the sample selection process for the ICSP competition analysis. Starting with the universe of public schools that appeared in the state's testing files from 2008-09 through 2011-12, only schools that could be successfully merged with demographic data from NCES' Common Core of Data were kept. This reduced the sample to 1,442. One hundred percent of these schools were successfully mapped using the geocoding software, Arc-GIS (Figures 2 and 3). Finally, 38 charter schools were removed from the sample as charter schools are not relevant for this study, given that they already experience competition for enrollment.

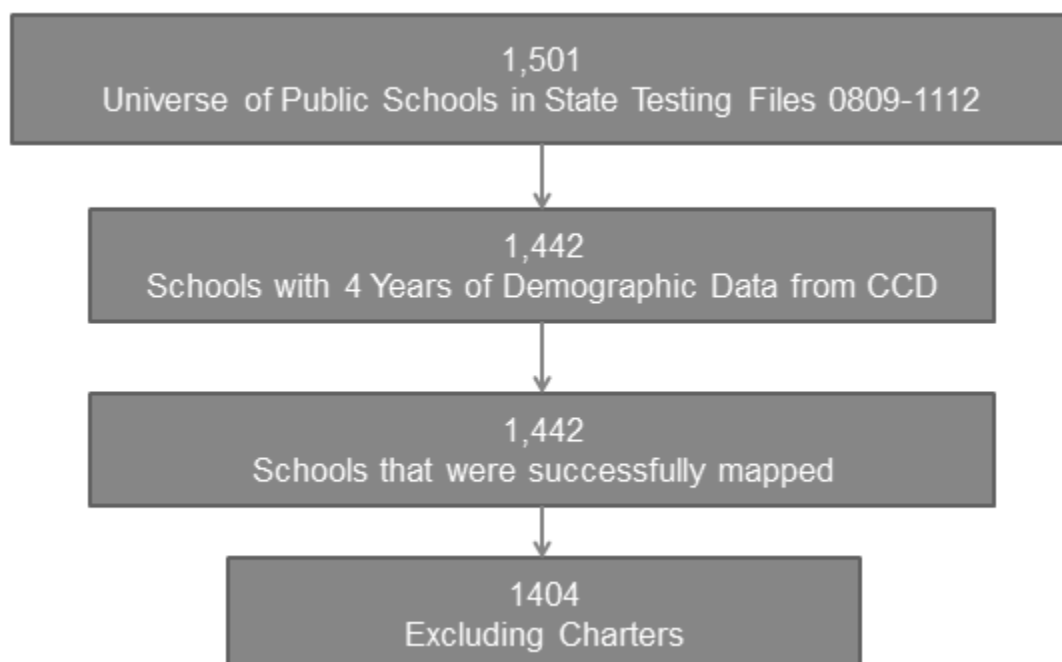


Figure 1. Sample Selection for Competition Analysis of ICSP on Public School Performance.

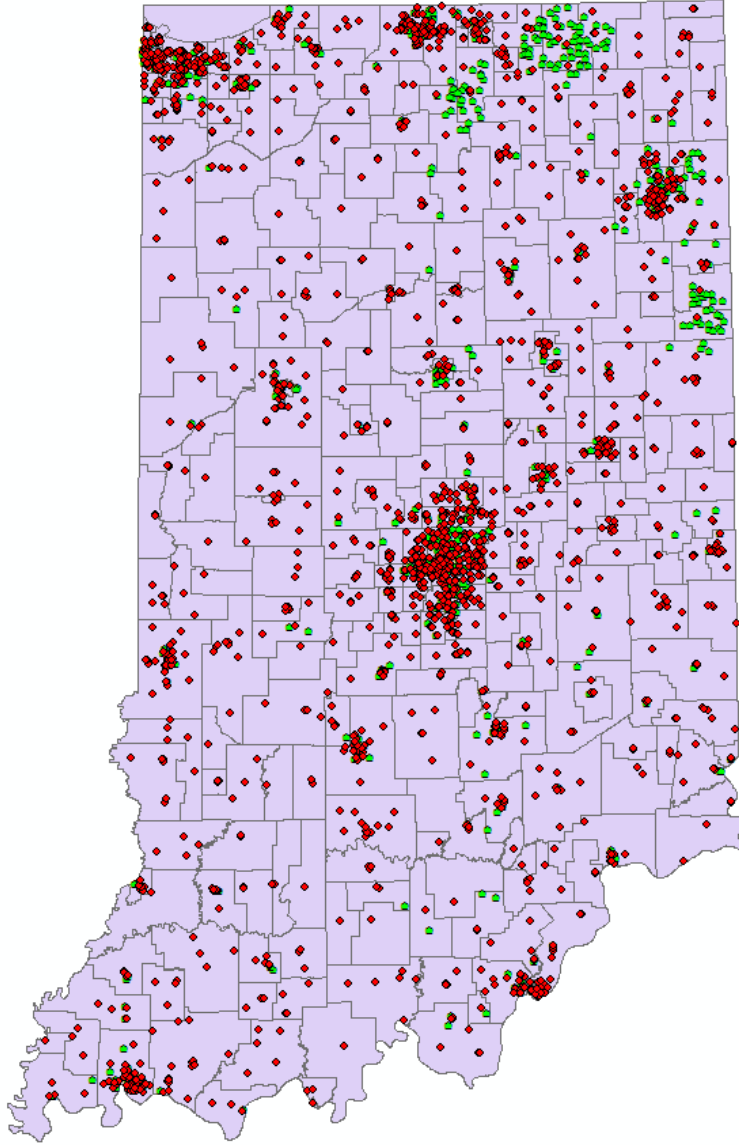


Figure 2. Public and Private School Locations in Indiana. Public schools shaded red; Private schools shaded green.

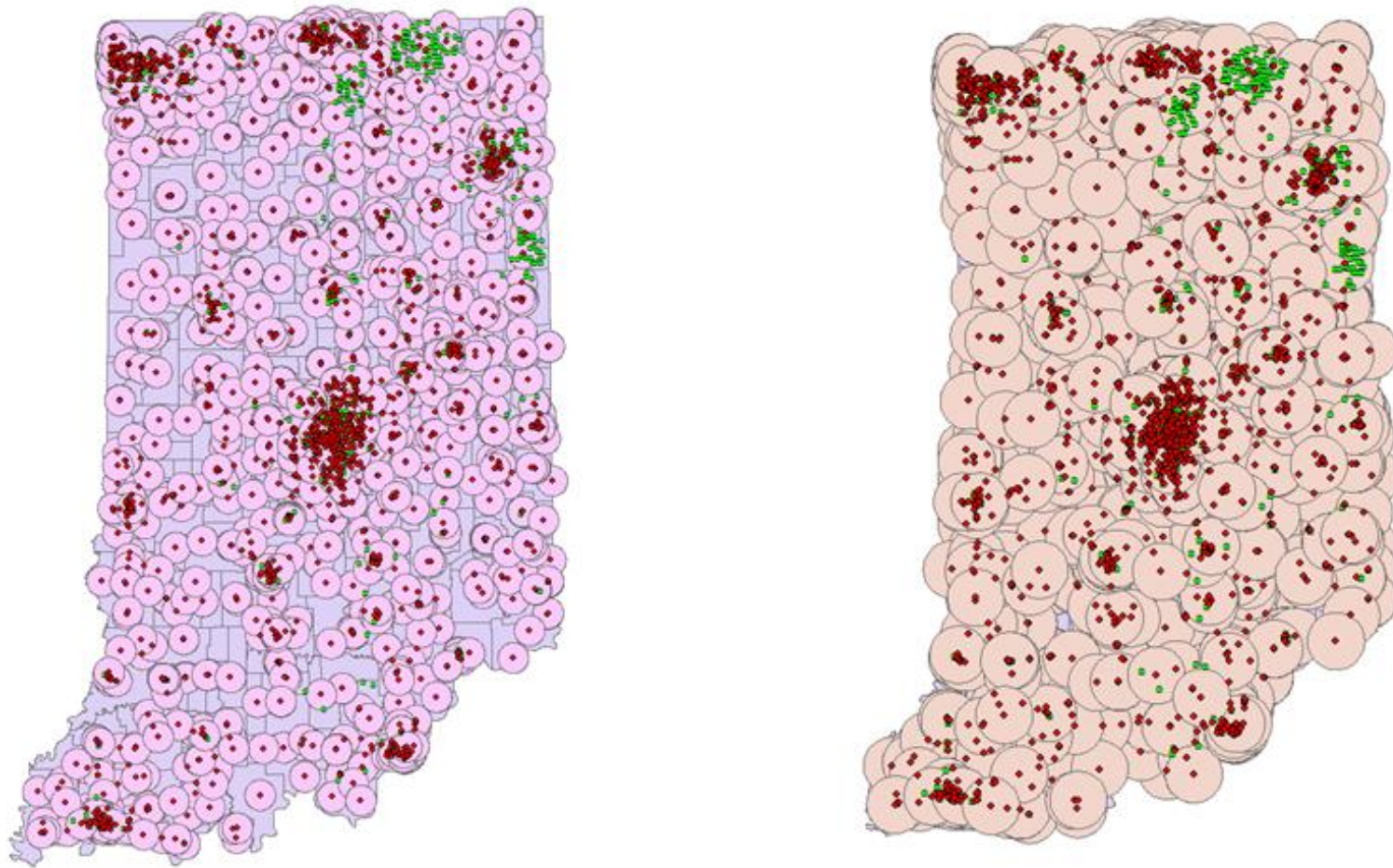


Figure 3. Five and Ten Mile Radii Around Every Indiana Public School. Public schools shaded red; Private schools shaded green.

Research Design

A. Competition Measures. We use the introduction of the Indiana Choice Scholarship Program as a policy shock that dramatically increased access to private school options starting in school year 2011-12. In order to capture variation in the competition experienced by public schools, we use a variety of geocoded competition measures that have been commonly used in the literature. These variables allow us to examine achievement impacts of the voucher shock on students in public schools exposed to the threat of competition. These methods can be organized into four distinct categories: Distance, density, diversity, and concentration measures.

A distance measure quantifies competition by measuring the distance between a public school and its nearest private school competitor. In a metropolitan area, it is not uncommon for this value to be under a mile. The underlying assumption for using distance as a measure of competitive pressure is that shorter distances equate to a higher level of school choices for students and thus increased competition for enrollees by public schools. This measure has been previously used in studies of the competition effect of the Florida Tax Credit Scholarship Program and the D.C. Opportunity Scholarship Program (Figlio & Hart, 2010; Greene & Winters, 2007). For each eligible public school, we calculate the crow's-flight distance in miles to the nearest private school that was in existence before the announcement of the program. To ease interpretation, we multiply the distance variable by -1 so that a positive coefficient on the distance variable would represent the impact of *closer* competitors positively impacting student outcomes.

A density measure, on the other hand, quantifies the degree of competition faced by a school by counting the number of private competitors within a given radius. Such measures have

been previously used in studies of competition effects of the Milwaukee Parental Choice Program, the Florida Tax Credit Scholarship Program, and Florida's McKay Scholarship Program for Students with Disabilities (Carnoy et al., 2007; Figlio & Hart, 2010; Greene & Marsh, 2009; Greene & Winters, 2008). We generate density counts within 5 and 10 mile radii.

A diversity measure counts the number of different *types* of local private schools that are close to a given public school. Using this method, competition is quantified by measuring the variety of schooling options available to students. Such a method has been previously used in a study of the Florida Tax Credit Scholarship Program (Figlio & Hart, 2010). In Indiana, we define private school type by religious affiliation. Potential values range from 0 to 23, which is the number of religious categories identified in the Private School Universe Survey.⁶ Thus, a given public school might have a value of 6 on the density measure, but if all 6 schools are Roman Catholic, it would only score a 1 on the diversity measure.

The final competition measure uses a modified Herfindahl Index to capture market concentration. As described by Figlio and Hart (2010), this index is generated by summing the squared market shares held by each private school religious type within a given public school radius. Suppose, for instance, there are five private schools that fall within a ten mile radius of a given public school- four of these are Catholic schools and one is a Lutheran school. The market share for each school type is calculated as $\frac{Count_r}{\Sigma R Count_r}$. Catholic school market share, therefore, is .80 (4/5) and Lutheran market share is .20 (1/5). The Herfindahl Index is the sum of the squares

⁶ These categories are: Amish, Assembly of God, Baptist, Brethren, Calvinist, Christian (no specific denomination), Church of Christ, Church of God, Church of the Nazarene, Evangelical Lutheran Church in America, Friends, Islamic, Jewish, Lutheran Church- Missouri Synod, Mennonite, Methodist, Nonsectarian, Other, Pentecostal, Presbyterian, Roman Catholic, Seventh-Day Adventist, Wisconsin Evangelical Lutheran Synod.

of the market shares held by each school type — in this case $(.80)^2 + (.20)^2 = .68$. Lower values of the Herfindahl Index are indicative of increased competitive pressure, as a lower concentration of the share of private schools is in the hands of just one particular religious type. Thus, a Herfindahl Index score of 1 suggests a monopoly market environment, where just one religious type has control of all private competitors within that radius. Conversely, a Herfindahl Index score of 0 represents a school market that is well-served by a diverse set of private schools. To ease interpretation of results, we use $1 - \text{the Herfindahl Index}$ so that a positive coefficient on this variable would mean increased competition is associated with higher student outcomes and a negative coefficient would mean increased competition is associated with lower student outcomes.

In order to avoid reverse causation bias, all four competition measures are measured before the ICSP was announced, using public school addresses from school year 2010-11 and matching private school addresses from the closest administration of the Private School Universe Survey, the 2009-10 school year. Additionally, we note that these measures are based on private school counts that weight all schools equally, regardless of school size. We deliberately chose such measures because we expect that public school administrators are more likely to be aware of the existence of neighboring private schools than to be knowledgeable about school size or the number of enrollment slots that would be made available to students using a Choice Scholarship. Finally, for those public schools that are not matched to a single private school within each radius examined, it is appropriate to assign a zero as the competition measure for all of the competition measures described above except for the concentration measure, where a zero implies a perfectly competitive market. As such, those public schools not matched to a single private school must be dropped for those analyses relying upon the modified Herfindahl Index.

In the results tables presented in the next section, the sample size is always smaller for those regressions measuring competition with the concentration index.

Table 2 summarizes the four competition measures across both radii considered in this study. The average distance from a public school to its nearest private competitor is 4.49 miles, with a standard deviation of just over 5 miles. Within a 5-mile radius, there are typically 5 private competitors, although the median value is just 2. One school in downtown Indianapolis has as many as 40 private competitors within a 5 mile radius. On average, 2 religious denominational types are represented and the mean value for the Herfindahl Index within a 5-mile radius is .46, which is about halfway on the scale from perfect competition (0) to a perfect monopoly (1). Within a 10-mile radius, each public school has an average of 15 private competitors, approximately 4 religious denominational types are represented, and the mean value for the Herfindahl Index is 0.40.

Table 2. Descriptive Statistics of Competition Measures

	Mean	Median	SD	Min	Max
Distance: Miles to nearest private school competitor	4.49	1.99	5.17	0	27.88
Competition Measures in 5 mile Radius					
Density: Number of local private schools	5.36	2	7.21	0	40
Diversity: Number of religious denominational types represented	2.10	1	2.15	0	8
Concentration: Herfindahl Index	.46	.33	.32	0	1
Competition Measures in 10 mile Radius					
Density: Number of local private schools	15.33	8	18.64	0	79
Diversity: Number of religious denominational types represented	3.76	3	3.02	0	11
Concentration: Herfindahl Index	.40	.25	.30	0	1

Note: Authors' calculations

Source: Data from the National Center for Education Statistics, Common Core of Data (CCD), "Public Elementary/Secondary School Universe Survey", 2010-11 and Private School Universe Survey, 2009-10

We also plot kernel densities of each of the competition measures to show variation in the measures by radius, summarized in Figure 4. The top left panel shows the distance to nearest private school competitor, with the majority of values concentrated around the 2 mile marker. The top right panel of Figure 4, meanwhile, shows how the number of neighboring private schools varies by radius, with a higher proportion of zeroes occurring within the 5-mile radius. The bottom left panel of Figure 4 shows the distribution of private school diversity, measured by the number of different religious denominational types represented within a given radius. As we might expect, this number increases in the larger radius, attaining a maximum value of 11. Finally, the bottom right panel of Figure 4 shows the distribution of private school density, measured using a modified Herfindahl Index. Both radii show a concentration of values around .2, which indicates a strongly competitive environment. Taken together, these kernel densities

suggest that Indiana public schools would have experienced a significant competitive shock with the announcement of the ICSP.

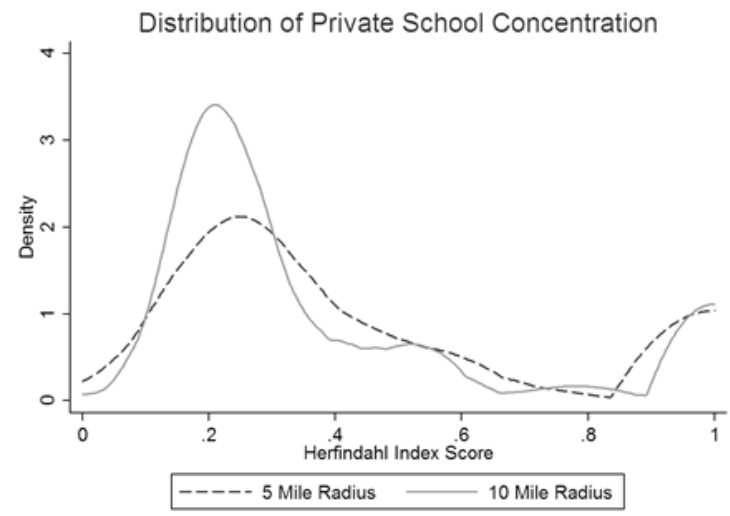
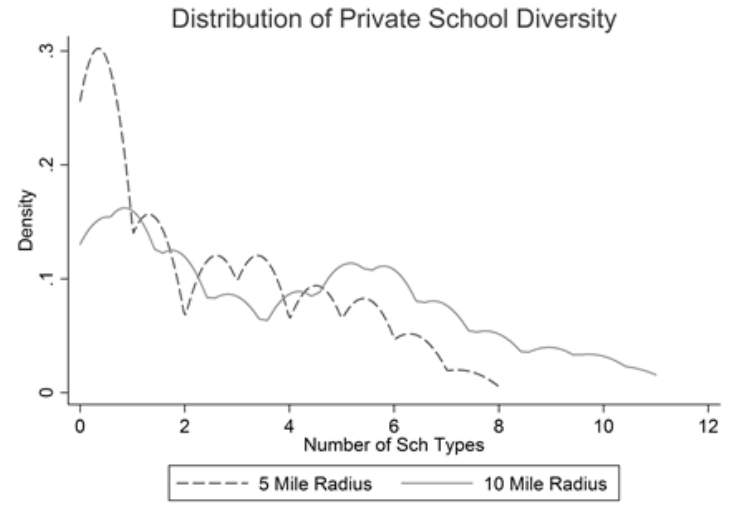
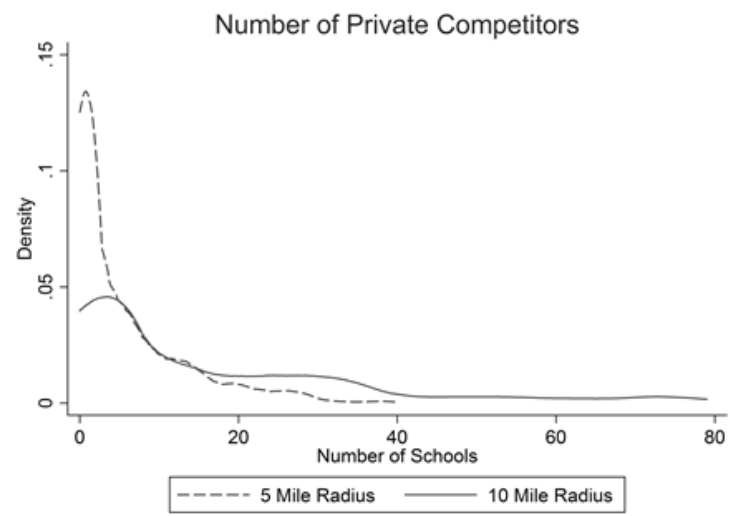
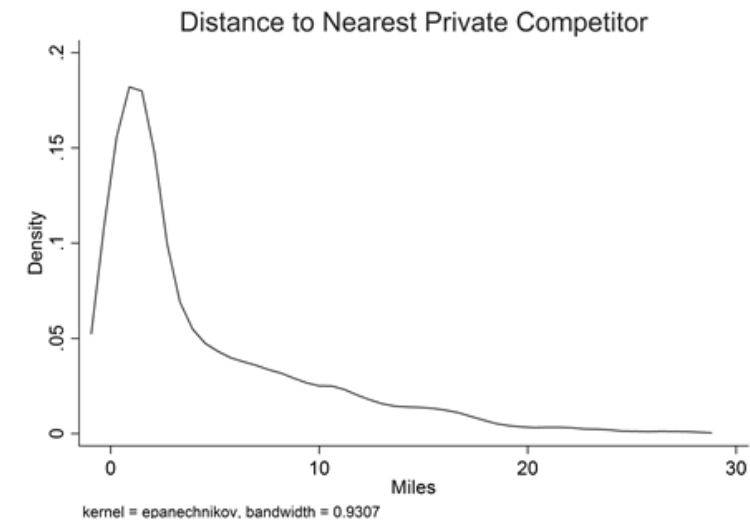


Figure 4. Distribution of Competition Variables, by Radius.

B. Model. A school fixed effects model is employed to estimate the effect of private school competition on public school performance, building upon the model estimated by Figlio and Hart (2014).

$$Y_{st} = \alpha_s + \beta C_s * P_t + \mu S_{st} + \delta T_t + \epsilon_{st}$$

Where Y_{st} is the average standardized math or reading score for 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 ; P_t is an indicator variable identifying the post-policy year, 2011-12; S_{st} is a vector of time-varying school characteristics (shares of students of each race and gender and the share eligible for free/reduced lunch); and T_t is a set of dummy variables indicating year. The β coefficient on the two-way-interaction of competition measures and post-policy year indicator is the parameter of interest. Standard errors are clustered at the school level.

Results

The results of the ICSP competition analysis are reported in Table 3. Each cell represents the coefficient on the two-way interaction between the competition measure and post-policy year. Regressions are run separately for each of the four competition measures. The coefficients on the competition variables are multiplied by 100 to assist the reader in differentiating between small differences in effect sizes. The left panel of Table 3 displays the results within a 10 mile radius whereas the right panel displays the results within a 5 mile radius. As discussed previously, models using the concentration measure must exclude those public schools not matched to a single private school. This explains the decreased sample size in these models.

Table 3

School Fixed Effects Regression Estimates of the First Year Impacts of ICSP Competition on Public School Achievement

	10 Mile Radius				5 Mile Radius			
	Distance (r)	Density	Diversity	Concentration (r)	Distance (r)	Density	Diversity	Concentration (r)
ELA	.07 (.08)	-.02 (.02)	.17 (.14)	4.01*** (1.47)	.07 (.08)	.04 (.06)	.38** (.19)	4.95*** (1.67)
Observations	5,615	5,615	5,615	4,731	5,615	5,615	5,615	3,799
Unique Schools	1,404	1,404	1,404	1,183	1,404	1,404	1,404	950
Adj. R-Squared	0.90	0.90	0.90	0.91	0.90	0.90	0.90	0.92
Math	.13 (.10)	-.03 (.03)	-.01 (.18)	.56 (1.83)	.13 (.10)	-.03 (.07)	-.04 (.24)	.75 (1.95)
Observations	5,615	5,615	5,615	4,731	5,615	5,615	5,615	3,799
Unique Schools	1,404	1,404	1,404	1,183	1,404	1,404	1,404	950
Adj. R-Squared	0.86	0.86	0.86	0.87	0.86	0.86	0.86	0.89

Note: The dependent variable is the school average 2011-12 standardized math or English Language Arts score; Standard errors clustered by school in parentheses; Variables followed by (r) are reverse coded to ease interpretation; Each cell represents the coefficient estimate on the interaction between the competition measure and a post-policy indicator from a separate regression model. Coefficients are multiplied by 100 for interpretability. Controls include indicators percent of student body eligible for subsidized lunch, percent of each race, percent male, as well as school and year fixed effects; *** p<0.01, ** p<0.05, * p<0.1

As can be seen in Table 3, none of the four competition measures is significantly related to student math performance in either radius examined. In English Language Arts, meanwhile, half of the measures in a 5-mile radius are positively and significantly related to student achievement. For instance, each additional type of private school within a 5-mile radius is associated with an increase in ELA achievement of .0038 of a standard deviation (SD). This positive and significant effect does not hold when we extend the radius to 10-miles, however. In fact, only one measure provides consistent evidence of a significant competitive effect- the concentration measure, which uses a modified Herfindahl Index. A one-unit increase in the concentration measure within a 5-mile radius is associated with a .0495 SD increase in ELA achievement. Extending the radius to a 10-mile zone, this concentration measure decreases in magnitude but remains statistically significant. Within a 10-mile radius, a one-unit increase in the concentration measure is associated with a .0401 SD increase in ELA achievement.

Table 4

School Fixed Effects Regression Estimates of the First Year Impacts of ICSP Competition on Public School Achievement, Controlling for Lagged Achievement

	10 Mile Radius				5 Mile Radius			
	Distance (r)	Density	Diversity	Concentration (r)	Distance (r)	Density	Diversity	Concentration (r)
ELA	.04 (.08)	-.01 (.02)	.21 (.14)	3.53** (1.48)	.04 (.08)	.05 (.06)	.37** (.19)	3.95** (1.63)
Observations	4,211	4,211	4,211	3,548	4,211	4,211	4,211	2,849
Unique Schools	1,404	1,404	1,404	1,183	1,404	1,404	1,404	950
Adj. R-Squared	0.91	0.91	0.91	0.92	0.91	0.91	0.91	0.92
Math	.14 (.10)	.01 (.03)	.22 (.18)	1.96 (1.84)	.14 (.10)	.05 (.07)	.20 (.25)	1.32 (1.90)
Observations	4,211	4,211	4,211	3,548	4,211	4,211	4,211	2,849
Unique Schools	1,404	1,404	1,404	1,183	1,404	1,404	1,404	950
Adj. R-Squared	0.88	0.88	0.88	0.89	0.88	0.88	0.88	0.90

Note: The dependent variable is the school average 2011-12 standardized math or English Language Arts score; Robust standard errors in parentheses; Variables followed by (r) are reverse coded to ease interpretation; ; Each cell represents the coefficient estimate on the interaction between the competition measure and a post-policy indicator from a separate regression model. Coefficients are multiplied by 100 for interpretability. Controls include indicators percent of student body eligible for subsidized lunch, percent of each race, percent male, school fixed effects and lagged achievement; *** p<0.01, ** p<0.05, * p<0.1

Table 4 displays results from a model that also includes a lagged achievement variable, which reduces the sample size but improves the model fit (the adjusted R-squared values uniformly increase when we include lagged achievement). As we saw in Table 3, none of the four competition measures is significantly related to student math performance in either radius examined. In English Language Arts, on the other hand, half of the measures in a 5-mile radius are positively and significantly related to student achievement. Each additional type of private school within a 5-mile radius is associated with an increase in ELA achievement of .0037 SD. As we saw in Table 3, however, this positive and significant effect does not hold when we extend the radius to 10 miles. Once again, only one measure provides consistent evidence of a significant competitive effect- the concentration measure. A one-unit increase in the concentration measure within a 5-mile radius is associated with a .0395 SD increase in ELA achievement. Within a 10-mile radius, this concentration measure is associated with a .0353 SD increase in ELA achievement.

Discussion

Although the competition measures employed in this analysis are highly similar to each other and draw upon similar geographic information, we have reported results using all four measures in order to test the stability of our results. If all four measures had found consistent results, this would demonstrate that any significant findings are not spurious and help lend confidence to our interpretation of the results. Unfortunately, this is not the case. The results presented in this article provide only weak and inconsistent evidence of a positive competitive response to the first year of the ICSP. None of the measures show any relationship with school-average, standardized math achievement. At the same time, three out of eight results provide

evidence of a statistically significant, positive impact of competition on school-average standardized ELA achievement, which is more than we would expect to find by mere chance but not an especially robust result.

The primary contribution of this study is that it addresses a common criticism of private school choice programs generally, which is that they destabilize traditional public schools in a way that causes significant harm to public school students' academic outcomes, especially in areas where competition from choice programs is high. The results presented here are entirely inconsistent with that hypothesis, providing no evidence that competition from private school choice programs decreases student achievement in public schools.

There is an important caveat to note, however, when interpreting these results. Our estimated effects could be underestimated if the ICSP hadn't had enough time to become sufficiently well established after just a single year to generate the type of competitive pressure that might prompt a clearer public school response. This explanation is plausible, considering that the program only enrolled around 4,000 students during that first year of operation. Further, the lawsuit filed against the program just months before the ICSP officially began sought a preliminary injunction to block the ICSP and may have affected school administrators' perceptions of the permanency of the program, diluting any potential competitive pressure by giving the impression that the program could be quickly halted. For these reasons, it will be important to continue studying the systemic impacts of the ICSP as later years of data become available.

Conclusion

This article presents an analysis of the competitive impacts of the Indiana Choice Scholarship Program, a statewide school voucher program with the potential to grow into the national's largest school voucher program. By using variation in private school penetration across the state of Indiana and taking advantage of a policy shock occurring in school year 2011-12, we are able to test for changes in average public school performance in Math and English Language Arts with a school fixed effects model.

Overall, the results presented here provide only weak evidence of a positive competitive response to the first year of the ICSP. In math, none of the four competition measures are significantly related to school-average performance whereas in English Language Arts, three out of eight results provide evidence of a statistically significant, positive competitive effect. Depending on the radius selected, a one-unit increase in the concentration measure (a modified Herfindahl Index) is associated with a .04 to .05 SD increase in school-average ELA achievement.

Given that the impact of the choice program is not consistently detectable in school-average outcomes, we must conclude that the competitive threat of the ICSP in its first year of operation was modest to negligible and that traditional public schools did not respond in a manner that can be clearly captured by changes in school-average test scores. We can conclude with confidence, however, that competition from private school choice programs has not *decreased* student achievement in Indiana's public schools.

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Appendices



*Office of Research Compliance
Institutional Review Board*

May 15, 2014

MEMORANDUM

TO: Anna Jacob Egalite
Kate Dougherty
Patrick Wolf

FROM: Ro Windwalker
IRB Coordinator

RE: New Protocol Approval

IRB Protocol #: 14-05-725

Protocol Title: *The Competitive Effect of the Indiana Choice Scholarship Program on Student Achievement in Indiana Public Schools*

Review Type: EXEMPT EXPEDITED FULL IRB

Approved Project Period: Start Date: 05/15/2014 Expiration Date: 05/14/2015

Your protocol has been approved by the IRB. Protocols are approved for a maximum period of one year. If you wish to continue the project past the approved project period (see above), you must submit a request, using the form *Continuing Review for IRB Approved Projects*, prior to the expiration date. This form is available from the IRB Coordinator or on the Research Compliance website (<http://vpred.uark.edu/210.php>). As a courtesy, you will be sent a reminder two months in advance of that date. However, failure to receive a reminder does not negate your obligation to make the request in sufficient time for review and approval. Federal regulations prohibit retroactive approval of continuation. Failure to receive approval to continue the project prior to the expiration date will result in Termination of the protocol approval. The IRB Coordinator can give you guidance on submission times.

This protocol has been approved for 1,050,000 participants. If you wish to make *any* modifications in the approved protocol, including enrolling more than this number, you must seek approval *prior to* implementing those changes. All modifications should be requested in writing (email is acceptable) and must provide sufficient detail to assess the impact of the change.

If you have questions or need any assistance from the IRB, please contact me at 210 Administration Building, 5-2208, or irb@uark.edu.

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June 16, 2014

To: The Graduate School

Re: Confirmation of Authorship

This letter is to confirm that Anna Jacob is the lead author of the article, “The Competitive Effect of the Indiana Choice Scholarship Program on Student Achievement in Indiana Public Schools.” She completed more than 51% of the work that went into this paper.

Sincerely,

Patrick J. Wolf, Ph.D.

The Impact of Targeted School Vouchers on Racial Stratification in Louisiana Schools

By: Anna Jacob, Jonathan N. Mills, and Patrick J. Wolf

Abstract

This article analyzes the racial stratification impacts of the Louisiana Scholarship Program (LSP), a statewide school voucher program that has provided public funds for low-income, mostly minority students in low-performing public schools to enroll in participating private schools since the fall of 2012. We use a student-level panel data set to track individuals as they switch schools in order to measure the impact of the LSP on racial stratification in both public and private schools across the state of Louisiana. In general, our analysis indicates that LSP transfers have reduced racial stratification in the voucher students' former public schools, but have marginally increased stratification in the private schools. Eighty-two percent of all student transfers reduce racial stratification in traditional public schools that students depart when they participate in the voucher program. On the other hand, transfers by voucher students reduce school-level racial stratification just 45% of the time in participating LSP private schools. When we restrict the sample to include only the districts under active federal desegregation orders, the large reduction in traditional public schools' stratification levels holds whereas there is no impact on private schools.

The Impact of Targeted School Vouchers on Racial Stratification in Louisiana Schools

The Louisiana Scholarship Program (LSP) is a statewide school choice program that provides public funds for low-income students in under-performing public schools to enroll in participating private schools. Initially piloted in New Orleans in 2008, Act 2 of the 2012 Regular Session expanded the LSP statewide, allowing thousands of public school students to transfer out of their residentially-assigned public schools and into private schools of their choosing. In school year 2012-13, almost 10,000 eligible Louisiana students applied for an LSP voucher. Ultimately approximately 5,000 students from low-performing public schools used these vouchers to enroll in private schools. All of these students were low-income and 80 percent were African American. LSP vouchers covered students' school tuition costs at 117 private, mostly Catholic, schools in the program's first year.

This article examines how transfers by LSP voucher users have impacted racial stratification in public and private schools in Louisiana in the first year of the expanded program. Critics of school choice programs have typically raised the concern that such programs can harm desegregation efforts by allowing students to transfer out of their assigned public schools and into private schools that are stratified by race/ ethnicity (Berliner, Farrell, Huerta, & Mickelson, 2000; Cobb & Glass, 1999; Frankenberg, Siegel-Hawley, & Wang, 2010). Such concerns are particularly relevant in Louisiana, a state with a history of state-sponsored segregation. The decades since the 1954 *Brown v. Board of Education of Topeka, Kansas* decision have seen a significant level of judicial oversight of school desegregation efforts nationally. In Louisiana, the 1975 *Brumfield v. Dodd* case signaled the end of public financial support for private schools that segregate or discriminate in admissions by declaring such schools ineligible for state assistance of any kind. This includes funding for textbooks, school supplies, student transportation, or

classroom materials. Today, the federal government continues to monitor schools to ensure their compliance with desegregation plans. The United States remains a party to desegregation suits in 24 Louisiana school districts. Additionally, any districts not under court orders can submit voluntary desegregation plans known as Form 441-b plans for their schools to the U.S. Department of Education's Office for Civil Rights.

In August 2013, the U.S. Justice Department filed a motion in the *Brumfield v. Dodd* lawsuit, seeking an injunction against the LSP and alleging that the program increases racial segregation. After several months of negotiations between the State of Louisiana and the DOJ, the U.S. District Court issued a decree that the state must provide the federal government with information on LSP applicants, including student race, at least 10 days before scholarships are awarded.

Given the ongoing efforts to reduce stratification in Louisiana's public schools as well as the legal attention surrounding this issue, it is important to understand how the LSP affects racial stratification. In this study, we empirically examine the issue using data on LSP voucher users. By tracking individual students across time as they move from the public to private sector, we can determine if these transfers increased or reduced racial stratification at students' former public schools (*sending schools*) and current private schools (*receiving schools*) by nudging the school's racial composition nearer or further from the racial composition of the broader community.

In general our analysis indicates that access to additional educational choices for low-income students has not increased racial stratification in public schools in Louisiana, a welcome outcome for a state with a history of state-sponsored segregation. Specifically, we find that LSP

voucher users have overwhelmingly reduced racial stratification in traditional public schools. Findings for private schools, however, suggest that just 45% of transfers reduce racial stratification. The results of this analysis provide reliable empirical evidence that can be used to inform ongoing debates both inside and outside of the courtroom over whether or not parental choice is harming current desegregation efforts in Louisiana's public schools.

The remainder of this article proceeds as follows. First, we provide a summary of the literature examining the integration impacts of school choice programs. In the next section, we describe the data used in our analysis and describe our empirical methodology. The following section presents the results. Finally, we conclude with a summary of the findings and a discussion of the implications for public policy.

Previous Literature

There is a relatively large literature examining the racial integration impacts of school choice programs, which can be broadly divided into four categories based on the methods employed: (1) descriptive comparisons of choice users and eligible non-users; (2) cross-sectional analyses of school racial composition compared across the public and private school sectors; (3) student transfer measures with no racial composition benchmark; and (4) student transfer measures with a racial composition benchmark. Studies using the fourth method provide the most accurate representation of the impacts of school choice programs on racial stratification and have generally found encouraging results.

1. Descriptive comparisons of choice users and eligible non-users. Early studies of school choice programs rely on cross sectional data to generate simple descriptive comparisons of choosers and eligible non-choosers but fail to examine existing levels of school-level racial

stratification. Without documenting stratification levels in schools prior to the commencement of a choice program or tracking progress towards a community benchmark of desired racial diversity, there is no way to judge the relative harm or benefits of student transfers under a given choice program. For example, Henig (1996) notes that minorities were less likely to participate in a magnet school program in Maryland and that white transfer requests were for schools with high proportions of other white students in the student body. This leads Henig to claim that “choice may exacerbate, rather than ameliorate, racial segregation” (p.105). Willms & Echols (1993) use a similar approach to study a school choice program in Scotland, finding that parents whose children had exercised the school choice option were more likely to have a prestigious occupation and to have attained a higher level of education. Unfortunately, while these measures help describe the types of students who actually access a given program, they do not capture impacts on racial stratification because they fail to examine school-level stratification before and after the program takes effect.

2. Cross-sectional analyses of school racial composition. Studies in the second category also use measures that are cross-sectional in nature but use either the district or core-based statistical area (CBSA) as the racial composition benchmark against which to judge relative levels of racial stratification.

Within-district studies rely on established residential segregation indices in an attempt to model cross-sector differences in integration. With these measures, the standard against which all schools are judged is the racial composition of the district. For example, measures like the dissimilarity index (Burgess, Wilson, & Lupton, 2005; Clotfelter, 1999) and exposure index (Frankenberg & Lee, 2002; Garcia, 2008), which were originally developed to capture geographic segregation, have been used to describe racial dispersion patterns within a school

system. While these measures are intuitively appealing given their background in residential segregation analyses, their focus on strictly within-district comparisons fails to account for existing segregation across school districts (Greene, 2005). This is particularly troubling given the available evidence that school districts themselves tend to be quite segregated (Clotfelter, 1999). The school district simply cannot provide an objective standard against which to judge school-level integration. District boundaries represent political boundaries and there is a strong likelihood that families may make racially-motivated decisions about which side of those boundaries to live on, resulting in racially homogeneous district populations that are unrepresentative of the racial composition of the broader community. For example, a within-district measure would classify a public school that is 100% white in a school district that is 100% white as being perfectly integrated, even if it is adjacent to a district that is 100% African American. In fact, adopting the district as the desired benchmark and actively working to imitate that level of integration would only *exacerbate* school-level segregation. Instead, we must choose an objective standard that can serve as the normative benchmark and hold public and private schools to that same benchmark. This is especially true in Louisiana where the LSP actively allows students to transcend district boundaries.⁷

At least four studies in the second category have used the CBSA to act as a benchmark of the desired racial composition, arguing that it provides a reasonable proxy for the geographical area from which a school could reasonably be expected to draw students in the absence of legal or political boundaries. Forster uses this approach to compare public and private schools in Cleveland (2006a) and Milwaukee (2006b), finding that private schools participating in the Cleveland and Milwaukee voucher programs were less segregated, on average, than neighboring

⁷ Twenty-four percent of voucher users actually crossed their district boundary to attend a private school through the program in its first year.

public schools. The magnitude of this difference was about 18 percentage points in Cleveland and 13 points in Milwaukee. Greene, Mills, and Buck (2010) also use this approach in their study of segregation in Milwaukee, WI. They find that, in some years, the voucher program schools better approximate the metro area and in other years the public schools better approximate this value. Importantly, over a three-year period from 2006-07 through 2008-09, neither sector comes close to approximating the percentage of white students in the metro area. On average, both sectors differ from this target value by more than 40 percentage points. Greene and Winters (2007) also employ this approach in their analysis of the effects of the Washington D.C voucher program, finding that neither the public nor private sector is particularly well integrated. They compute an enrollment- weighted average difference from the percentage of white students in the surrounding metropolitan area and show that private choice schools have a lower difference at 33.8%, compared to 39.5% for public schools.

3. Transfer measures with no racial composition benchmark. The third category of studies improves upon prior work in this area by using dynamic information on individual student transfers to estimate the overall impact of school choice programs on racial stratification. For instance, Zimmer, Gill, Booker, Lavertu, Sass, and Witte (2009) take advantage of a panel dataset to measure charter school segregation across seven locations. They calculate the difference in the proportion of students of each race in the charter school a student switches into and the prior traditional public school the student attended. In the majority of cases, they show that students tend to transfer into schools that do not differ significantly in terms of racial makeup from the schools they left. Without using any racial composition benchmark, however, this approach to measuring integration can only assert that the two schooling environments are

different (or the same), but can't make a normative judgment about whether that change has helped to improve racial integration or not.

4. Transfer measures with a racial composition benchmark. The final category of studies take advantage of panel data to track individual students' migration patterns as they transfer between schools, judging whether these transfers help or hinder integration by whether they move a school towards or away from the racial diversity of some benchmark— eg. the district or the surrounding metropolitan or micropolitan area. Our study of the impact of the LSP on racial stratification in Louisiana's public and private schools belongs in this category.

Bifulco and Ladd (2006) analyze changes in the racial isolation experienced by third through eighth grade students who transfer to charter schools in North Carolina between 1996-97 and 2001-02. Schools in which the proportion of Black students is greater than 20 percentage points away from the district average are classified as "racially unbalanced." The authors then compare the proportion of students in each sector who attend a racially unbalanced school, finding that charter school students are approximately two and a half times more likely to attend one of these schools.

Instead of the school district, a small number of panel studies have used the surrounding metropolitan or micropolitan area as the benchmark for the broader community (Greene, Mills & Buck, 2010; Ritter, Jensen, Kisida, & Bowen, 2012). These CBSAs are characterized by high degrees of social and economic interdependence and provide an appropriate racial composition benchmark against which to judge progress. Thus far, this approach has only been taken in two studies, one study examining racial integration in Milwaukee, WI and the other in Little Rock, AR.

Greene, Mills, and Buck (2010) track student transfer effects on both sending and receiving schools in Milwaukee, WI. They show that in 2007-08, 92% of departing students tended to be a member of a racial/ethnic group that was overrepresented at their originating school, relative to the metro area. The departure of these students positively impacted racial integration efforts. The comparable statistic for 2008-09 is 95%. On the other hand, when they analyze the impact of student transfers on receiving schools, the reverse is true. In 2007-08, 91% of student transfers reduced integration in the receiving schools. The comparable statistic for 2008-09 is 94%.

Finally, Ritter et al. (2012) analyze the effects of charter school transfers in Little Rock, AR between 2004-05 and 2009-10. They show that white student transfers in this time period improved racial integration in the sending schools twice as often as they reduced it (25% compared to 12%). For minority students, student transfers improved racial integration in the schools they left more than three times as often as reducing it (48% compared to 15%). Impacts on receiving schools are not computed.

As this review of the literature reveals, a panel study of student migratory patterns brought about by the introduction of a school voucher program has never been conducted across an entire state. Given the increasing prevalence of large-scale school voucher programs like the LSP, this article provides a timely analysis of a potentially serious unintended consequence of large-scale school choice programs.

Data

The data used in our analysis come from five sources. First, we use unique data obtained from the Louisiana Department of Education on LSP voucher users to track individual-level

school transfers. Data on private school racial compositions come from the Private School Universe Survey (PSS), a biennial survey conducted by the National Center for Education Statistics (NCES). In particular, our analysis relies on school-level data collected in the 2011-12 school year; the year before the voucher program expanded statewide. We collected corresponding data on public schools in the 2011-12 school year from the NCES's Public Elementary/Secondary School Universe Survey. In the case of any missing data in either of these sources, we supplement with data from earlier versions of these same surveys. We rely on 5-year population estimates from the U.S. Census Bureau's American Community Survey to approximate the school-age racial composition of Louisiana's Core Based Statistical Areas (CBSAs) in 2011-12. Finally, we identify public school districts under federal desegregation orders using information from the Justice Department lawsuit filed in August 2013.

Sample selection. Figure 1 describes how we generate the sample for our primary analysis. Starting with a student-level data set that includes all 9,831 eligible applicants for the LSP in its first year of statewide operation, we first narrow the sample to include only the 5,777 voucher winners identified in our data. Because all voucher winners didn't necessarily use their voucher, the next screen reduces the sample to 4,941 students. The third screen only keeps those voucher users who were not participants in the New Orleans pilot program because those students often enroll in the same school as the previous year, and therefore their inclusion would merely contaminate our results. This screen reduces the sample to 3,338 students. The fourth screen excludes those students who were missing a prior school identification code. This includes students entering Kindergarten, for instance, or students moving to Louisiana from out of state. This brings the sample to 2,179 students. Fifth, those students who reside in rural areas that do not fall in a metropolitan or micropolitan area have to be excluded from our sample

because we are unable to calculate the racial composition of the surrounding CBSA to use as the integration benchmark for them. This brings the sample to 2,117. Finally, because our analysis, and the legal and policy debate surrounding the issue is focused on the integration impacts on traditional public schools, we exclude those students who previously attended a public charter school. Once this set of screening rules is employed, our final analysis sample consists of 1,741 students.

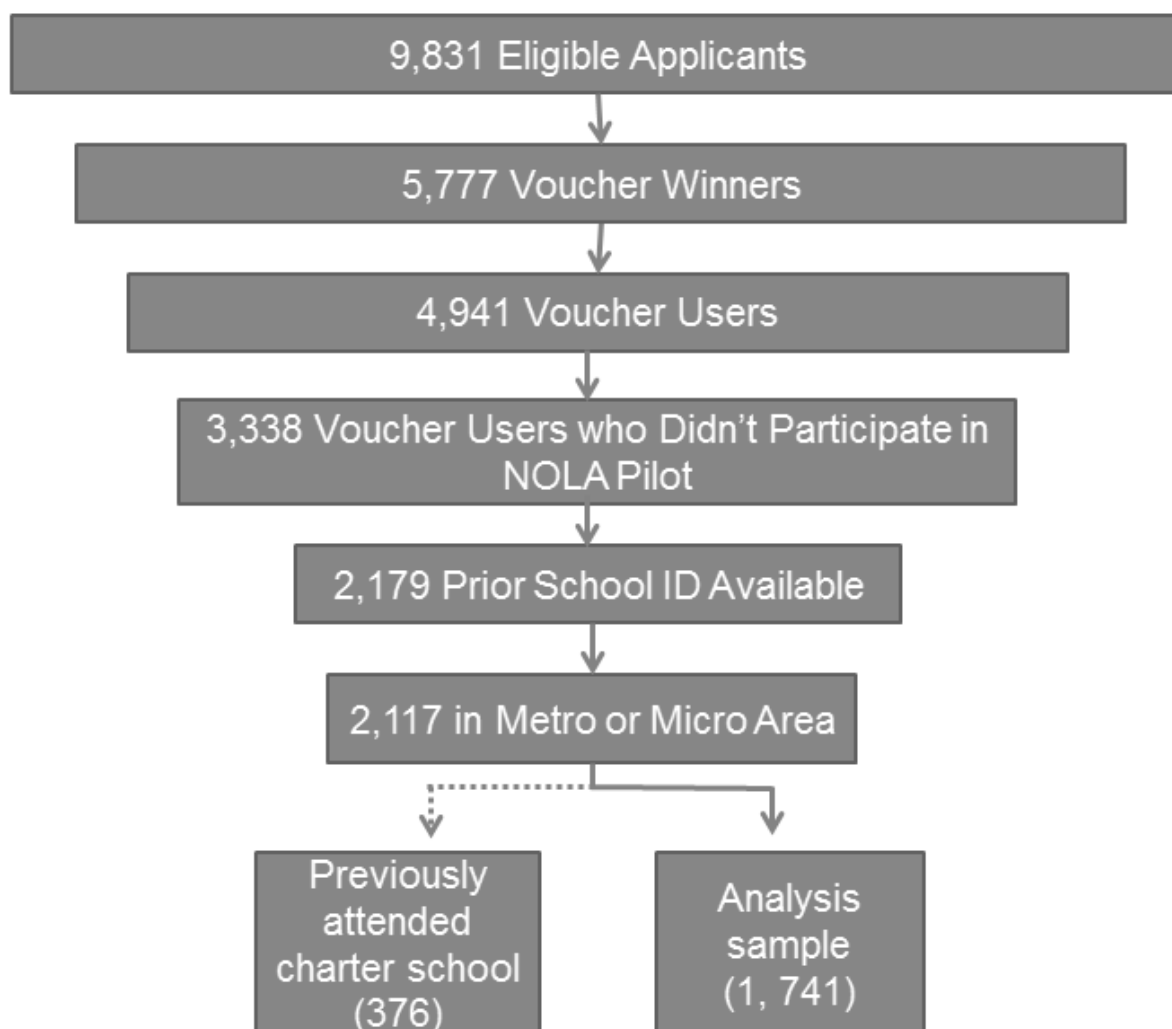


Figure 1. Creation of Student Sample for Primary Analysis of LSP Transfers

Descriptive statistics of students. While the primary analysis examines the effects of all LSP transfers that qualify for our sample, we also identify a subsample of students who are in a traditional public school district that is under an active federal desegregation order. Table 1 presents descriptive statistics for both the primary analysis sample and the desegregation district subsample. There is an approximately even male/female split in both samples. African American students represent an overwhelming majority of LSP voucher users across both samples. Finally, the majority of observations come from the elementary grades of 1 through 5.

Table 1.

Descriptive Statistics for Analysis Sample and Subsample of Students in Desegregation Districts

	Analysis Sample		Desegregation District Subsample	
	(1)	(2)	(1)	(2)
	N	%	N	%
Count	1,741	100%	493	100%
Male	839	48%	238	48%
Race/Ethnicity				
African American	1,395	80%	367	74%
Hispanic	75	4%	13	3%
White	218	13%	93	19%
Other	53	3%	20	4%
Grade				
Grades 1-5	1,070	61%	313	63%
Grades 6-8	436	25%	119	24%
Grades 9-12	235	13%	61	12%

Source: Authors' calculations.

Note: The desegregation districts subsample is composed of public schools in the 34 public school districts that are currently under desegregation orders (see Table A1 in the appendix for a full list)

Descriptive statistics of schools. In order to provide context for this study, we also present descriptive statistics of public and private schools in Louisiana at baseline using two widely-used segregation measures. The first measure is the segregation index, which is computed

by calculating the absolute value of the difference between each school's percentage of minority students from the percentage of minorities in the school-aged population of the broader community (the CBSA). We then use these school-level figures to generate a sector-specific, enrollment-weighted, average distance from the community average.

Table 2 examines the existing differences in school-level segregation across both the public and private school environments using the segregation index. When comparing public schools to private schools on this measure, we find that both sectors are segregated and that the private schools are slightly more segregated, on average, than the public schools. Private schools are 27.9 percentage points from the community average racial demographic, whereas public schools are 25.5 percentage points from the community average. We can also break out the data to compare private and public schools within CBSA classifications — metro and micro areas. While we observe no statistically significant differences between sectors in metropolitan areas, private schools are significantly more segregated than public schools in micropolitan areas, with a difference of about 6 percentage points between the two sectors.

Table 2.

Enrollment-Weighted Average Distance from the Percentage Minority of the CBSA, by sector

	Private Schools		Public Schools		Comparison	
	n	Distance from CBSA	n	Distance from CBSA	Difference	p-value
TOTAL	332	27.92	1,278	25.46	2.46***	.01
Metro Areas	282	28.32	953	27.06	1.25	.24
Micro Areas	50	25.40	325	19.75	5.65***	.01

Source: Authors' calculations using private school data from the Private School Universe Survey, 2011-12 and public school data from the Common Core of Data's "Public Elementary/Secondary School Universe Survey", 2011-12; CBSA values from the 5-year American Community Survey estimates, 2008 through 2012

Note. *** indicates significance at the 1% level. Distance from the CBSA is an absolute value.

We also use a second segregation measure to assess the private and public school context before the LSP was expanded. Frankenberg, Siegel-Hawley, and Wang (2010) suggest that schools where 90% or more of the population belongs to the same race/ethnicity are “hyper-segregated”; thus, we create the homogeneity index. This is a binary measure that takes on a value of 1 if 90% of a school’s population belongs to the same race/ethnicity and 0 otherwise. Table 3 examines the prevalence of school-level racial homogeneity across sectors at baseline. Private schools are significantly less likely to be racially homogeneous, as judged by this measure. Just 14 % of private schools are identified as racially homogeneous, compared to 26% of public schools, a difference that is highly statistically significant. In addition, when we provide separate comparisons by CBSA classification, we see that private schools in metropolitan areas are, once again, significantly less likely to be identified as racially homogenous than public schools – 14% compared to 29%. In micro areas, where there are far fewer schools, there is no difference between the two sectors in terms of the proportion of racially homogeneous schools.

Table 3.

Percentage of Schools that are Racially Homogeneous, by Sector and CBSA Type

	Private Schools		Public Schools		Comparison	
	n	Percent Racially Homogeneous	n	Percent Racially Homogeneous	Difference	p-value
TOTAL	332	.14	1,278	.26	-.12***	0.00
Metro Areas	282	.14	953	.29	-.15***	0.00
Micro Areas	50	.16	325	.18	.02	0.79

Source: Authors’ calculations using private school data from the Private School Universe Survey, 2011-12 and public school data from the Common Core of Data’s “Public Elementary/Secondary School Universe Survey”, 2011-12; CBSA values from the 5-year American Community Survey estimates, 2008 through 2012

Note. *** indicates significance at the 1% level;

The school-level descriptive statistics presented here reveal that both public and private schools in Louisiana are segregated. Students in private schools are significantly more likely to attend a school whose percentage of minority students is lower than that of the surrounding metropolitan area. Students in public schools, meanwhile, are more likely to be enrolled in schooling environments where 90% or more of a school's population belongs to the same race or ethnicity. Given that 80% of voucher users in the first year of the program were African American, this suggests that the desegregating potential of the voucher program is high. By using a state-funded voucher, African American LSP voucher users are accessing private schools that, up to this point, have had a low representation of minority students.

In the next section, we present the results of our analysis of transfers brought about by the LSP, which explicitly models the direct effects of the program on racial stratification.

Research Design

While the previous section provided a general overview of the racial makeup of Louisiana's schools before the LSP expanded statewide, we turn now to an analysis of how the LSP changes racial stratification levels within these schools. We first define a benchmark representing the racial composition goal a school could reasonably achieve given the broader community. Without such a benchmark, we would have no standard against which to judge progress. For our analysis, we use as our benchmark the racial composition of the CBSA, as defined by the U.S. Census Bureau.⁸ In total, 25 CBSAs are represented in our sample. The

⁸ Core based statistical areas (CBSA) are defined by the U.S. Census Bureau; and are broken into two types. Metropolitan statistical areas represent geographical areas with populations of at least 50,000. Micropolitan statistical areas contain populations of between 10,000 and 50,000. By restricting our analysis to CBSAs, we exclude 62 students from our sample who live in rural counties that fall outside of metropolitan or micropolitan areas.

percentage of the school-age population that is white in these areas ranges from 26 percent to 78 percent, with a mean value of 56 percent. The largest CBSA is the New Orleans-Metairie-Kenner metro area, which has a population of approximately 226,000. The median population for a CBSA in our sample is 13,047.

Having defined the CBSA as our community benchmark, we can now answer our primary research question, “Have LSP transfers reduced or increased racial stratification in sending and receiving schools?” We identify student transfers that move a school’s racial composition closer to the racial composition of the relevant CBSA as stratification-reducing transfers, while transfers that move a school’s racial composition further from this benchmark are identified as stratification-increasing transfers. For example, if an African American student leaves a public school in which African Americans are over-represented relative to the broader community, we would identify this transfer as stratification-reducing. On the other hand, if the school has a lower percentage of African American students than the broader community, that transfer is counted as contributing to the increased racial stratification of the sending school. In cases, where an African American student leaves a school that is 100% African American, this transfer is neither coded as stratification-reducing or stratification-increasing as it is considered a null impact. The same logic is applied to the analysis of the transfers of students who are white or Hispanic. This measure takes an intuitive approach to studying the racial stratification effects of a school choice policy and has been previously used by Greene, Mills, & Buck (2010), Jensen & Ritter (2009, 2010), and Ritter, Jensen, Kisida, & Bowen (2012).

Results

Using student-level panel data, we document all LSP-related transfers and record the impact of the moves on school-level racial stratification. Figure 2 summarizes our primary analysis of the effects of LSP transfers on racial stratification in both sending and receiving schools. For sending schools, we identify transfers as “stratification-reducing” when a student of a given race leaves a school that is disproportionately composed of students of his same race relative to the greater CBSA. Conversely, outcomes that increase racial stratification occur when a student leaves an “integrated” school in which the proportion of his race is less than the proportion of individuals of that race in the greater CBSA. As indicated in figure 2, the overwhelming majority—82%—of LSP student transfers reduced racial stratification in sending schools. Meanwhile, less than a fifth of transfers increased racial stratification in the former public schools of LSP students.

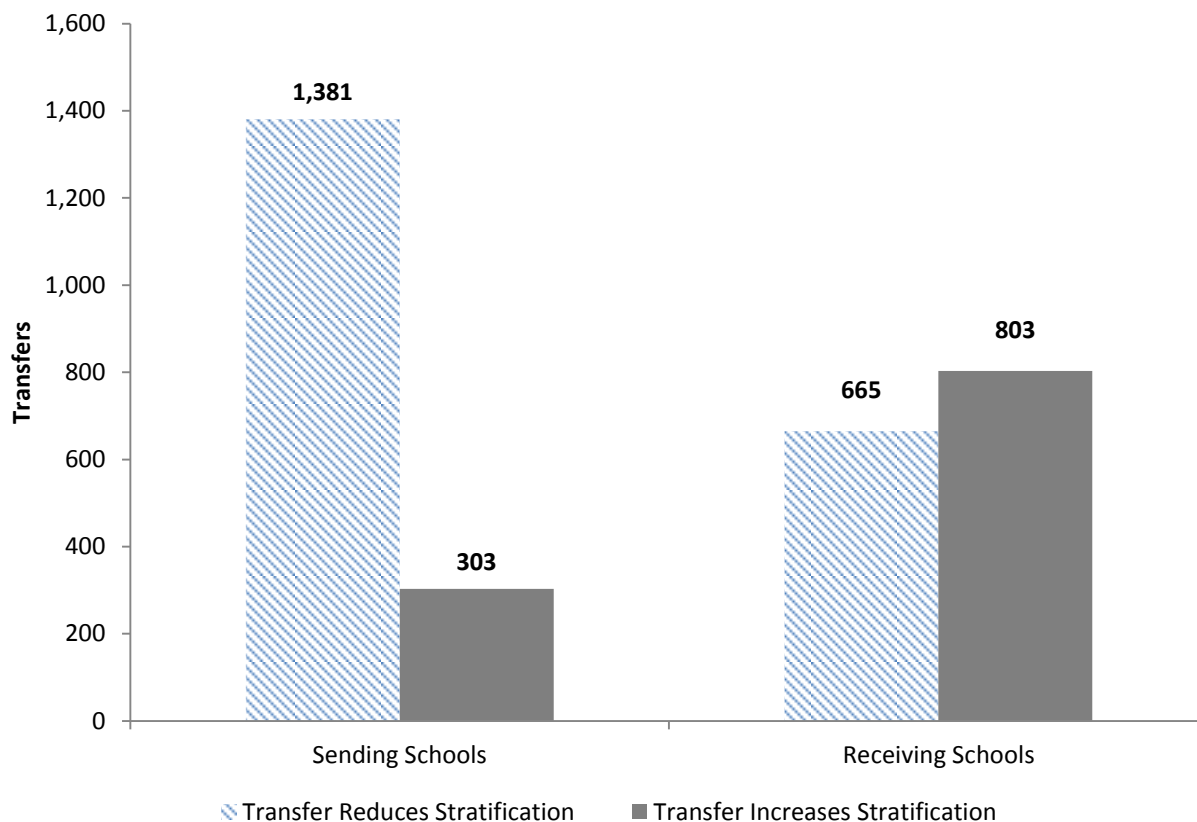


Figure 2. Impacts of Voucher Transfers on Racial Stratification. Sending schools are traditional public schools only- excludes private New Orleans schools that were already participating in the voucher program and charter schools. Impacts on receiving schools are based on student transfers from traditional public schools only. Transfers from sending schools come close (1,684) but don't completely sum to the size of the full analysis sample (1,741) because this figure only examines transfers for the three largest racial categories. The numbers of transfers from sending and into receiving schools don't match because a small number of private schools don't appear in the Private School Universe Study, which is a voluntary NCES survey. Number of transfers excluded because sending school was 100% same race = 4 (Black), 0 (Hispanic), and 0 (White). Number of transfers excluded because receiving school was 100% same race = 32 (Black), 0 (Hispanic), and 7 (White). Chi-square tests for goodness-of-fit indicate the observed differences are significant for sending schools ($p < 0.01$) and significant for receiving schools ($p = .0003$).

Racial stratification in receiving schools may be affected by student transfers too. We identify transfers that bring the school's racial proportions closer in line with those of the greater CBSA as reducing racial stratification and those transfers that bring the racial proportions further from those of the greater CBSA as increasing racial stratification. As Figure 2 shows, LSP

transfers result in slightly more negative outcomes for receiving schools: 803 student transfers increase racial stratification compared to 665 transfers that reduce stratification, a difference that is statistically significant. Thus, while our analysis indicates large positive impacts of the LSP vouchers for traditional public schools, the effect on private receiving schools is small and negative.

In Table 4, we examine transfer impacts for three major student subgroups – White, African American, and Hispanic. Given that 80% of voucher users are African American, it is unsurprising that the majority of student transfers are for African American students. Within this group, 92% of transfers reduce stratification at the sending school, compared to 24% of white student transfers and 56% of Hispanic student transfers. In receiving schools, 45% of African American student transfers reduce stratification, compared to 28% for white students and 96% for Hispanic students.

Table 4.

Impact on Racial Stratification in Sending and Receiving Schools across the state of Louisiana

Type of Transfer	Sending		Receiving	
	N	%	N	%
African American Students				
Reduce Stratification	1,286	92	542	45
Increase Stratification	105	8	659	55
White Students				
Reduce Stratification	53	24	56	28
Increase Stratification	165	76	141	72
Hispanic Students				
Reduce Stratification	42	56	67	96
Increase Stratification	33	44	3	4
Percent of overall transfers that reduce racial stratification		82%	45%	

Note: Sending schools are traditional public schools only- excludes private New Orleans schools that were already participating in the voucher program and charter schools. Impacts on receiving schools are based on student transfers from traditional public schools only. Transfers from sending schools don't sum to the size of the full analysis sample (1,741) because this table only examines transfers for the three largest racial categories. The numbers of transfers from sending and into receiving schools don't match because a small number of private schools don't appear in the Private School Universe Study, which is a voluntary NCES survey. Number of transfers excluded because sending school was 100% same race = 4 (Black), 0 (Hispanic), and 0 (White). Number of transfers excluded because receiving school was 100% same race = 32 (Black), 0 (Hispanic), and 7 (White). Chi-square tests for goodness-of-fit indicate the observed differences are significant for sending schools ($p < 0.01$) and significant for receiving schools ($p = .0003$).

Subgroup Analysis

While the prior analysis focused on LSP transfers in general, it is also relevant to examine how these transfers are differentially impacting public schools in districts under federal desegregation orders. In particular, we can examine this question by restricting the primary analysis to LSP schools in the 34 public school districts that are currently under desegregation orders.⁹ When we restrict our analysis to this subgroup, we find that, once again, transfers significantly reduce stratification in sending schools and have null impacts on receiving schools.

⁹ See Table A1 in the appendix for the list of school districts under federal desegregation orders

As Figure 3 shows, 354 LSP transfers (75% of all transfers) reduce stratification in the sending schools. In receiving schools meanwhile, there is no statistically significant difference between the number of stratification-reducing and stratification-increasing transfers.

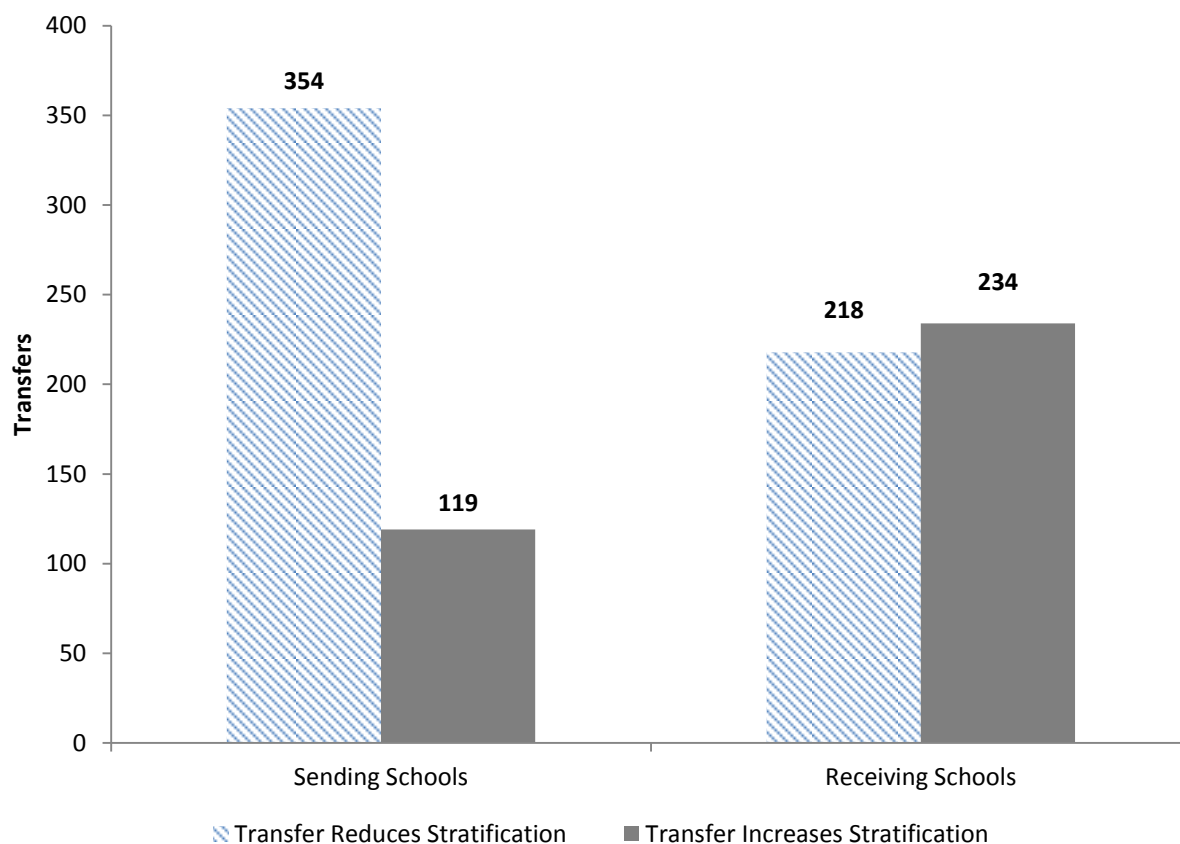


Figure 3. Impacts of Voucher Transfers on Racial Stratification in Districts under Desegregation Orders. Sending schools are traditional public schools under federal desegregation orders only. Impacts on receiving schools are based on student transfers from traditional public schools under federal desegregation orders. Number of transfers excluded because sending school was 100% same race =0 (Black), 0 (Hispanic), and 0 (White). Number of transfers excluded because receiving school was 100% same race =0 (Black), 0 (Hispanic), and 0 (White). Chi-square tests for goodness-of-fit indicate the observed differences are significant for sending schools ($p < 0.01$) and insignificant for receiving schools ($p = .4517$).

Table 5 breaks out these results by race. The same general patterns hold as before. For African American students, 87% of transfers reduce stratification at the sending school, compared to 33% of white student transfers and 38% of Hispanic student transfers. In receiving

schools, 57% of African American student transfers reduce stratification, compared to just 4% for white students and 100% for Hispanic students.

Table 5.

Impact on Racial Stratification in Schools under Federal Desegregation Orders

Type of Transfer	Sending		Receiving	
	N	%	N	%
African American Students				
Reduce Stratification	318	87	204	57
Increase Stratification	49	13	154	43
White Students				
Reduce Stratification	31	33	3	4
Increase Stratification	62	67	80	96
Hispanic Students				
Reduce Stratification	5	38	11	100
Increase Stratification	8	62	0	0
Percent of overall transfers that reduce racial stratification		75%	48%	

Note: Sending schools are traditional public schools under federal desegregation orders only. Impacts on receiving schools are based on student transfers from traditional public schools under federal desegregation orders. Number of transfers excluded because sending school is 100% same-race is zero. Number of transfers excluded because receiving school is 100% same-race is zero. Chi-square tests for goodness-of-fit indicate the observed differences are significant for sending schools ($p < 0.01$) and insignificant for receiving schools ($p = .4517$).

In addition, these patterns of findings generally hold when we further restrict our analysis to just include the 24 districts in which the U.S. is listed as a party in the original desegregation cases. Specifically, LSP transfers in this subsample reduce racial stratification in sending schools 80% of the time ($p < .01$) and increase racial stratification in receiving schools 66% of the time ($p = .0248$).

The results presented here reveal large positive impacts of the LSP vouchers for traditional public schools, which have long been the focus of federal efforts to desegregate.

These positive findings hold when we restrict the sample to include only the districts under active federal desegregation orders and again when we further restrict the sample to those districts where the U.S. is a party to the suit. It is important to keep in mind, however, that although the effect on private receiving schools is small, it is negative and statistically significant in the overall sample but not significant in the samples limited to areas under desegregation orders.

Sensitivity Test: Choosing Between Two Potential Panel Measures

The racial stratification measure used in this analysis uses panel data to assess the impacts of the LSP on racial stratification, judging the direction of impacts by comparing against a racial composition benchmark. Zimmer et al. (2009) also employ a panel approach to assess the impacts of a school choice program on racial stratification levels but compare the racial composition of the receiving school to that of the sending school instead of an external benchmark. There is one scenario in which the Zimmer et al. (2009) panel approach could be regarded as superior to the panel approach used here that employs a racial integration benchmark. That situation arises when a student leaves a public school in which his race is over-represented for a private school in which his race is also over-represented (eg. in the case of “white flight”). The panel approach that uses a benchmark would rate such a move as reducing racial stratification for the sending school and increasing racial stratification for the receiving school. A transfer measure without a racial composition benchmark (eg. Zimmer et al., 2009), however, would assign a net rating to that move, judging it to increase racial stratification, which is perhaps more intuitive to many people. Because readers may disagree over which approach is superior and to ensure transparency regarding our choice of measure, we provide Table A2 in the

appendix breaking apart all potential scenarios in which a student transfer is rated as “stratification-reducing” in the sending school. The “white flight” example is Scenario 6, when the student departs a sending school in which his race is over-represented (thus, being rated as stratification-reducing by our panel measure) and arrives in a private school in which his race is even more over-represented (thus, being rated as stratification-increasing by our panel measure). This scenario explains only 16% of all transfers, thus reducing any concerns that the choice of measure is driving our results.

Limitations

There are at least two limitations that restrict the generalizability of the methods and findings presented here. First, the measure employed in this paper to calculate the racial stratification impacts of the voucher on sending schools includes all students who depart a public school. Technically, it would be possible for this sample to include students who drop out of school or move out of the state entirely. We avoid such an error in this study by limiting the sample to those students who actually used an LSP voucher and, thus, arrived in a participating private school in the fall of 2012. Researchers seeking to imitate our methods should beware of this limitation of the integration measure used here and restrict their sample appropriately. Second, integration is measured in this paper using a binary measure that rates transfers in a binary fashion— as either stratification-reducing or stratification-increasing. The benefit of this approach is that it is easy to understand and interpret but it could be criticized for equally weighting a transfer from a school in which the student’s race is only slightly under-represented and a transfer from a school in which the student’s race is dramatically under-represented. It is possible that a more sophisticated measure could be employed that would weight transfers and

express the overall stratification impact on a continuous scale, although it is not clear how one would interpret the numbers produced by such a measure.

Conclusion

This article presents an analysis of the impacts of the Louisiana Scholarship Program (LSP) on racial stratification in Louisiana public and private schools. Overall, we find large, positive improvements to racial stratification in public schools that are consistent across our samples and small increases in racial stratification in private schools that are not consistent across our samples as a result of this school voucher program.

Our primary analysis uses student-level panel data to track individual student transfers as they switch from the public to the private sector. Outcomes that reduce racial stratification occur when a student of a given race leaves a school in which his race is over-represented relative to the greater CBSA. Conversely, outcomes that increase racial stratification occur when a student leaves a school in which his race is under-represented relative to the CBSA. This analysis reveals that the vouchers used by the low-income, mostly minority recipients have impacted public school desegregation efforts in a positive manner. By leaving schools in which they were racially overrepresented, 82% of voucher users reduced racial stratification in Louisiana public schools, bringing those public school racial populations closer in line with those of the broader communities. Positive impacts are particularly sizeable for African American students, who constitute the majority of voucher recipients. Ninety-two percent of LSP transfers for African American students resulted in positive outcomes for sending schools in the overall transfer sample. At the same time, student transfers have, in general, a small, negative impact on the the

schools they transfer to. Just 45% of all transfers reduced racial stratification in the receiving schools.

In addition, an analysis of the subgroup of students leaving districts under active federal desegregation orders demonstrates that transfers significantly reduce racial stratification in these 34 public school districts, the very districts that have been the subject of the greatest segregation concerns. In total, 75% of transfers reduce racial stratification in the sending schools in this subgroup. Meanwhile, the impact on receiving schools in this subgroup is statistically equivalent to zero.

While acknowledging that LSP transfers have resulted in a small, negative impact on private school racial stratification, the results of this study allow us to confidently conclude that the LSP has not reduced desegregation efforts in Louisiana public schools. To the contrary, public schools in Louisiana, including those public schools under active desegregation orders, are significantly less racially stratified as a direct result of the statewide school voucher program.

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Appendices

Table A1.

Public school districts under Federal Desegregation Orders

District Name	U.S. is a Party to the Desegregation Order
Avoyelles Parish	YES
Bienville Parish	YES
Bossier Parish	YES
Caddo Parish	YES
Catahoula Parish	YES
Claiborne Parish	YES
Concordia Parish	YES
Desoto Parish	YES
Franklin Parish	YES
Jackson Parish	YES
Lasalle Parish	YES
Lincoln Parish	YES
City Of Monroe School District	YES
Plaquemines Parish	YES
Pointe Coupee Parish	YES
Richland Parish	YES
Sabine Parish	YES
St. Helena Parish	YES
St. James Parish	YES
St. John The Baptist Parish	YES
St. Martin Parish	YES
St. Mary Parish	YES
St. Tammany Parish	YES
West Carroll Parish	YES
Acadia Parish	NO
Allen Parish	NO
Assumption Parish	NO
Iberia Parish	NO
Jefferson Davis Parish	NO
Lafourche Parish	NO
Madison Parish	NO
Ouachita Parish	NO
Tangipahoa Parish	NO
Winn Parish	NO

Source: United States' Memorandum in Support of its Motion for Further Relief, *Brumfield v. Dodd*, Civ. A. No. 71-1316, (p.4)

Table A2.

Comparing Methodologies: Transfer Measures of Integration, With and Without a Racial Benchmark

		Jacob et al. Approach: Using a Benchmark			Zimmer et al. (2009) Approach: No Benchmark
		Impact on Sending School			Net Impact
		Transfer Rating	Count	Proportion	Transfer Rating
1.	$R^* > R_0 > R_1$	Bad	74	.05	Good
2.	$R_0 > R^* > R_1$	Good	536	.34	Good
3.	$R_0 > R_1 > R^*$	Good	504	.32	Good
4.	$R^* > R_1 > R_0$	Bad	70	.04	Bad
5.	$R_1 > R^* > R_0$	Bad	141	.09	Bad
6.	$R_1 > R_0 > R^*$	Good	255	.16	Bad
TOTAL			1,580	1.00	

Note: R_0 denotes percent of race R in sending school; R_1 denotes percent of race R in receiving school; R^* denotes benchmark. Only the subset of students who were present in the analyses of impacts on *both* sending and receiving schools are included. Scenario 6 represents the example of “white flight”- when a student leaves a public school in which his race is over-represented for a private school in which his race is also over-represented.



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June 16, 2014

To: The Graduate School

Re: Confirmation of Authorship

This letter is to confirm that Anna Jacob is the lead author of the article, “The Impact of Targeted School Vouchers on Racial Integration in Louisiana.” She completed more than 51% of the work that went into this paper.

Sincerely,

Patrick J. Wolf, Ph.D.

Conclusion

As market-based school reforms have become an increasingly important part of the American educational landscape, policymakers, advocates, and a growing segment of the general public have expressed hope that these programs will stimulate improvements in conventional public schools. There are currently 39 private school choice programs in operation in 18 U.S. states plus the District of Columbia and Douglas County, Colorado; yet less than 1% of all public school students in the U.S. attend a private school through these programs. The impact of choice reforms is most widely felt, therefore, through second-level effects on those students who remain in district-run public schools. Even as growth in the number of private school choice programs accelerates, the vast majority of students will continue to be educated in traditional public school districts so there is a particular need for research that examines the impact of expanded educational choice on student achievement in these institutions. Furthermore, opponents of educational choice have raised concerns about the potential racial stratification that might occur as a result of such programs. This dissertation examines both of these questions in the context of two statewide, means-tested school voucher programs— the Indiana Choice Scholarship Program and the Louisiana Scholarship Program.

Research Question 1: Is public school achievement influenced by competition from a private school choice program?

Milton Friedman claimed that competition from school choice would lead to “the development and improvement of all schools” (1962, p.93). Articles one and two of this dissertation directly address that assertion. In Louisiana, we show that the lowest-graded Louisiana public schools exposed to the threat of the LSP had a modest, statistically significant

response to the injection of completion, with positive impacts ranging from .001 to .06 SD. These impacts may seem modest but it is important to recall that the voucher program only served approximately 5,000 students in its first year, which is less than 1% of all public school students in Louisiana.

In Indiana, the evidence is slightly weaker. We employ a school fixed effects model to test for evidence of a competitive response to the first year of the ICSP. In math, none of the four competition measures are significantly related to school-average performance whereas in English Language Arts, three out of eight results provide evidence of a statistically significant, positive competitive effect, which is more than would be expected by statistical chance but not particularly robust. Depending on the radius selected, a one-unit increase in the concentration measure (a modified Herfindahl Index) is associated with a .04 to .05 SD increase in school-average ELA achievement.

Overall, the takeaway from these complementary studies is clear. The competitive threat of these voucher programs ranges from negligible to modestly positive but has certainly not *decreased* student achievement in the public schools exposed to the threat of competition.

Research Question 2: How do student transfers through a private school voucher program impact racial stratification in Louisiana's public and private schools?

To promote better racial integration in America's educational institutions, states have experimented with various policies since *Brown v. Board of Education* first ordered the desegregation of all U.S. public schools. The 1964 Civil Rights Act and the Title 1 of the 1965 Elementary and Secondary Education Act, for instance, combined a heightened threat of

litigation with the financial incentives for elementary and secondary schools to desegregate, particularly in poor districts, which relied most heavily on federal grants (Cascio, Gordon, Lewis & Reber, 2008). Almost a decade later, the Supreme Court sanctioned the use of busing to achieve desegregation goals (*Swann v. Charlotte-Mecklenburg, 1971*). In Louisiana in particular, school districts have experimented with strategies such as student reassignment, busing, facility upgrades for schools with large minority populations, and extensive monitoring of district compliance and reporting requirements to reduce racial stratification across its public schools, many of which operate in districts under court supervision. Article three of this dissertation describes a voluntary school choice program whose primary goal was not to reduce racial stratification in the public schools but that actually had overwhelmingly positive impacts in this regard. This encouraging finding is tempered, however, by the small, negative impacts we observe on racial stratification levels in private schools.

Using a student-level panel data set to track individuals as they switch schools, we are able to measure the impact of the LSP on racial stratification in both the public and private schools affected by the program. Overall, we show that LSP transfers reduce racial stratification in the voucher students' former public schools, but these transfers have marginally increased racial stratification in the private schools. Eighty-two percent of all student transfers reduce racial stratification in the traditional public schools, compared to 45% in private schools. When we narrow the sample to just the districts under federal desegregation orders—arguably, the highest priority schools—75% of transfers are stratification-reducing in the public schools and there are null effects in private schools.

Limitations

There are some noteworthy limitations to this set of studies. First, both school voucher programs examined were just in their first year of operation at the time these competitive effects studies were conducted. Enrollment in these programs grew every year after that first year as more students learned about the programs and applied for a private school voucher. As such, the competition generated by these programs was likely at its weakest at the time these studies were conducted. Once later years of data become available, it will be interesting to test how the coefficient on the competition variables changes— whether it grows in magnitude, changes direction, or loses statistical significance.

Second, both of these voucher programs are means-tested, meaning they are designed to target low-income students, not the universe of public school students. This feature of program design may significantly shape the public schools' perception of the program. It is even possible that public schools could be supportive of a program that attracts their poorest and possibly hardest-to-educate students. Instead of viewing these targeted voucher programs as a threat to which they must respond, public schools may actually view them as a release valve and welcome the programs as a positive outlet to which they can direct struggling students.

Third, it is important to acknowledge the limited set of outcomes examined in articles one and two, which measure the effect of private school competition on math and English language arts test scores only. These two tested subjects certainly do not reflect the sum total of a public school experience. It could be the case that public schools focused on science, art, music, or social studies as a mechanism to retain students who would be eligible for the voucher program. Alternatively, they could have responded to family preferences by offering more diverse

electives, for example, by conducting renovations on school facilities, or by taking active steps to better market their school (Holley, Egalite, & Lueken, 2013). Furthermore, it might also be possible that the effects of competition might be felt in long-term outcomes such as improvements in student graduation rates or college enrollment if public schools respond to the competitive threat by sharpening their focus on attainment goals for students and cultivating an environment that prepares students for long-term success.

There are also some important limitations to the racial stratification study described in article three. First, the racial composition benchmark that we choose is relative in that it allows us to reasonably measure how well or how poorly a school is integrated relative to some realistic expectation. It is not an absolute measure however, that dictates where the benefits of integration might be optimally realized. Second, this analysis is designed to give a bird's eye view of the general direction of the impacts of the LSP on school-level racial stratification. Thus, all transfers are pooled into just two broad categories— either reducing or increasing racial stratification. Observing that 82% of all transfers are stratification-reducing is certainly encouraging from a bird's eye perspective, but that statistic doesn't take into account how diffuse those transfers might be between schools. In a particular school, for example, a single student transfer might be rated as reducing racial stratification but if it is the only transfer to occur in that whole school, it will not have any real effect on the day-to-day experiences of the students attending that school. Put simply, if the stratification-reducing transfers we observe are dispersed rather sparsely across Louisiana's public schools, the net effect of the program could appear positive on paper without actually altering students' lives in any meaningful way.

Furthermore, it is likely that whatever benefits are associated with reductions in racial stratification do not accrue in a linear fashion. Those benefits may be attitudinal (eg. students

gain an improved respect for other cultures and a reduction in prejudice); academic (eg. students of minority races gain access to higher-achieving peers by being placed in a more diverse school environment); or they may result in the eradication of stereotype threat; but our analysis implicitly treats all progress towards the attainment of these benefits equally. In reality, it is more likely that the benefits of a reduction in racial stratification follow a more complex functional form. Perhaps early progress boosts all of these benefits significantly, but the returns may diminish at a certain point and only pick up again later as full attainment of the racial composition goals comes within arm's reach. Take for example a community that is 20% African American. In our analysis, a stratification-reducing transfer that shifts the racial composition of a school in that community from 100% African American to 90% African American is weighted exactly the same as a transfer that shifts another school in that community from 30% African American to 20% African American. In reality, any actual benefits associated with these two transfers are unlikely to be equivalent. While the move from a completely racially homogenous school to one that includes students from different races is certainly a significant event, the move from 30% to 20% may go unnoticed by most students.

Despite the limitations outlined in this section, these articles have important implications for legislators and activists designing and promoting school voucher programs in the United States. In the next section, I outline the key takeaways for policymakers.

Implications

As large-scale school voucher programs continue to expand across the country, policymakers who are fearful of negative implications for traditional public schools should be heartened by these findings. Overall, the articles presented in this dissertation demonstrate the

private school choice programs have null to modest, statistically significant, positive impacts on the math and ELA achievement of non-participants who remain in their assigned public schools. Given that traditional public schools are and will continue to be the primary provider of educational services in K-12, it is good news that their achievement outcomes are not negatively impacted by the expansion of private school choice programs.

Second, contrary to fears of the segregating effects of large-scale choice programs, article three of this dissertation shows that there is little basis for these fears. Although we find evidence of small increases in racial stratification in private schools, the effects for public schools are overwhelmingly positive. Particularly in school districts where desegregation concerns are to the forefront of administrators' minds, the Louisiana Scholarship Program should be viewed as a positive force to help reduce racial stratification in the public schools. It will, of course, be important to continue monitoring the impact of this program on racial stratification in Louisiana schools, especially in private schools, as enrollment in the program continues to grow.

Directions for Further Research

There are still many unanswered questions regarding the systemic effects of large-scale private school choice programs. For instance, we do not have a lot of insight into how school leaders identify competitors. Survey work in New Orleans by Jabbar (2014) has taken an initial step towards addressing this question by asking public school principals to list the schools they perceive as rivals and using network analysis to identify ties between perceptual rivals. It would be fascinating to repeat her study, incorporating principals from both the public *and* private sector. Such a study would add dimensionality to our currently limited understanding of the

social, political, interpersonal, and economic contexts in which public and private school leaders identify and respond to competitors.

Jabbar's work also reveals the predictive power of principal gender— male leaders were more 2.76 times more likely to name schools as competitors. This finding is unsurprising given the economics literature on the relationship between gender and competition. In laboratory work exploring this issue, Niederle and Vesterlund (2007) show that women are more likely to shy away from competition than their male counterparts. It would be valuable to explore differences in the demographic characteristics (eg. gender, race, age) of school leadership teams in the public and private sectors and to analyze how these differences interact with principals' perceptions of competition and what steps they take to respond to threats for students.

Another branch of research that would help us to develop more informed, multi-dimensional competition models would be an analysis of private school branding. Preliminary work on this subject by Trivitt and Wolf (2011) examined the Catholic school "brand," showing that even non-Catholics are attracted to this brand and attrit from a voucher program when the school they select doesn't live up to the expectations associated with that brand. As private school choice programs grow within the 18 states that already have established programs and the new states and regions that are considering introducing legislation proposing such programs, we can expect to see the development of new private schools that won't have a religious affiliation. What mechanism will these schools use to brand themselves with? Will websites like www.greatschools.org play a bigger role in advertising, promoting, and sharing information about secular private school brands? How will this affect the potential for racial or economic stratification within schools? Will public schools respond by creating more differentiated public-school brands?

Many of the research questions I have proposed thus far will require more nuanced datasets than the administrative datasets that the articles in this dissertation rely upon. Given the constantly increasing prevalence and reduction in cost associated with using technology to assist with school administration, it will be incredibly informative if researchers can persuade schools to conduct climate surveys and collect data on teacher and administrator perceptions of competition so that we can answer behavioral questions about how school leaders act in a competitive environment and how their strategies change based on who they perceive to be their competitors on a large scale.

The final suggestion I will offer for future research on the systemic effects of private school choice programs is an examination of how competitive networks are influenced by other networks that exist simultaneously between schools. For instance, in a given Catholic diocese, school leaders may collaborate to develop standards and curricula. Similar cooperative networks exist between public schools. This is not an uncommon phenomenon in major cities such as New York City, where public schools share professional development resources. Less common, are public/private collaborations such as the Private Schools with Public Purpose consortium, which encourages private schools to collaborate with public school students and teachers. Will new injections of competition hurt such cooperative collaborations or will it facilitate their growth as school leaders become more motivated to improve school practices and performance?

Clearly, many questions on the systemic impacts of large-scale school choice programs remain. I concede that the direct impacts of school choice programs are easier to measure but I hope that the articles presented here provide evidence that it is possible to study systemic impacts and that we should be motivated to do so because the number of students indirectly impacted by these large-scale reforms is much larger than the number of students who directly

participate in these programs. At a minimum, I hope that these three articles can provide empirical evidence to inform political and legal debates around these issues.

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