# Competitive Effects of Means-Tested School Vouchers

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## 1. Introduction

School choice options—including both voucher and neo-voucher options like tuition tax credit funded scholarship programs—have become increasingly prevalent in recent years (Howell, Peterson, Wolf and Campbell, 2006). One popular argument for school choice policies is that public schools will improve the education they offer when faced with competition for students. Because state funds are tied to student enrollment, losing students to private schools constitutes a financial loss to public schools. If schools face the threat of losing students--and the state funds attached to those students--to private schools, they should be incentivized to cultivate customer (i.e., parental) satisfaction by operating more efficiently and improving on the outcomes valued by students and parents (Friedman, 1962). Alternatively, vouchers may have unintended negative effects on public schools if they draw away the most involved families from public schools and the monitoring of those schools diminishes, allowing schools to reduce effort put into educating students (McMillan, 2004).[1]

It is notoriously difficult to gauge the competitive effects of private schools on public school performance because private school supply and public school performance affect each other dynamically (Dee, 1998; McEwan, 2000). In cross-section, the relationship between private school supply and public school performance could plausibly be either upward-biased or downward-biased. On the one hand, private schools may disproportionately locate in communities with low-quality public schools. In such a case, the estimated relationship between private school penetration and public school performance would be downward-biased. On the other hand, if private schools locate in areas with high valuation of educational quality, then the

[1] Of course, it is also possible that vouchers might attract the students who are the most mismatched to their present education environment. If so, this changing compositional effect could potentially benefit the students remaining in the public sector.

presence of private schools could be correlated with unobservable features of public school quality and public school student performance, and the estimated relationship between private schools and public school performance would be upward-biased.

This paper takes advantage of the introduction of a major new school voucher program -- the largest in the United States -- to directly study the competitive effects of school vouchers on student outcomes in public schools. The Florida Tax Credit Scholarship Program (FTC; formerly called the "Corporate Tax Credit Scholarship Program") offers assistance to students eligible for free or reduced-price lunches at school (that is, those with family incomes below 185 percent of the federal poverty line) to attend private religious or non-religious schools in the state. To qualify for a voucher, students must have spent the entire prior year in a Florida public school (or be entering kindergarten or first grade) in addition to being low-income. Vouchers need not cover all of the costs of attending private schools, and parents are free to send their children to any private school regardless of the share of tuition and fees covered by the voucher. The voucher, however, is quite generous relative to the costs of attending religious private schools; the $3,500 voucher size at the program's introduction[2] was about 90 percent of elementary-grade tuition and fees at a typical Florida religious private school.[3] And the policy change was very large for the target population -- the number of scholarships available was larger than half the size of the low-income population using private schools on the eve of the policy's introduction. For low-income Floridians, the voucher represented a major demand shock for private schooling.

[2] The size of the voucher is now between $3,950 and $4,100 depending on whether the student is new to the program or a returning participant.
[3] In contrast, the size of the voucher is less than two-thirds of the typical tuition and fees at a religious high school in Florida.

We specifically examine whether students in schools that face a greater threat of losing students to private schools due to the introduction of tuition tax credit scholarships improve their test scores more than do students in schools that face less pronounced threats. We use the introduction of the FTC program as a source of exogenous variation that dramatically increased the potential demand for non-public school options after 2001, when the policy was announced, by lowering the effective cost of private school attendance for eligible students. We examine whether test scores improved more in the wake of the new policy for students attending public schools with more (or more varied) nearby private options that suddenly became more affordable for low-income students, than did scores for students attending schools with fewer (or less varied) potential competitors.

This is possible because of the considerable variation in potential competition faced by schools across the state of Florida. Prior to the introduction of the program, some communities in Florida had a much richer and more diverse set of private school options than did other communities. We suspect that public schools in these communities may have experienced more competition at the outset of the program, causing them to respond differently from schools with fewer potential competitors. Because we are identifying off of a policy change, we do not rely on cross-sectional variation to estimate the effects of private school competition on public schools, but rather apply a differences-in-differences modeling strategy. We find evidence that public schools subject to more competitive pressure from private schools raised their test scores the most following the introduction of Florida's voucher program, and that the gains in test scores appear to generalize to students ineligible to participate in the voucher program. Therefore, while the state caps the number of program participants at a small fraction of the

overall student body, the magnitude of the program nonetheless appears to be large enough to generate substantive public school responses.

In addition to employing a stronger identification strategy than most past work, our analysis also uses the timing of the roll-out of the policy to isolate the effect of competitive pressure per se from other potential effects that voucher policies may have on public schools. A simple pre-post comparison of the extent to which private school penetration was associated with public school performance would typically conflate three distinct aspects of the effects of the voucher on public schools. In addition to the competitive effect of the voucher that we seek to identify, there is also a composition effect -- if vouchers change the composition of the public schools, and if peer effects are important,[4] then the voucher effect on public schools would also include the changes in performance associated with the composition of the school changing. Furthermore, if vouchers influence the resources available to a school, then the voucher effect will capture these changes in resources as well.

We are able to separate the competition effect from the other two effects of vouchers because of the timing of the voucher roll-out. For a year following the announcement of the policy, students were applying for vouchers for the *following* school year, but no students had yet left the public school on a voucher. Therefore, any public school changes in this first year of the program, when public school students were applying for vouchers but before they actually used them, can be thought of as a pure competition effect of vouchers.

We are also able to exploit key aspects of the policy environment to obtain further evidence of the competitive effects of school vouchers. First, because the voucher is for a fixed amount regardless of the level of the school, the voucher covers a dramatically larger portion of the tuition and fees at elementary and middle schools than at high schools. For the set of

[4] See Epple and Romano (forthcoming) for a comprehensive review of the peer effects literature.

voucher participants for whom we can observe information on actual tuition and fees charged (those residing in roughly the southern half of Florida), out-of-pocket costs for families using vouchers are typically over three times higher for high school students than for elementary school students. Hence, one would expect larger competitive effects of vouchers at the lower schooling levels than at the high school level.

Second, the federal Title I program provides generous additional cash assistance, levied on a student-by-student basis, as well as substantially increased flexibility as to how to spend it, to schools with relatively high levels of students eligible for free or reduced-price lunches. Title I funds are paid to school districts, which in turn allocate resources to schools, and district policies directing how schools receive those supplemental funds vary dramatically. Moreover, the program's generosity changed from the 2001-02 school year to the 2002-03 school year, leading some school districts to provide Title I assistance to schools higher in the income distribution than before. The overall effect of the funding change was to increase the statewide percentage of schools receiving Title I funds by five percent, with larger changes in some school districts. The schools that stand to become Title I schools and experience a dramatic increase in discretionary revenues if they do not lost many of their free or reduced-price lunch students are likely the schools that would be the most likely to respond to the competitive threats embedded within the voucher policy. We find evidence that the schools facing higher levels of competitive threat or greater financial incentives to retain low-income students were the schools more likely to respond to the competitive threat of school vouchers.

**2. Comparison with the existing literature**

A number of researchers have estimated the relationship between private school penetration and student outcomes -- either test scores, graduation rates, or grade completed -- using effectively cross-sectional variation; examples include Arum (1996); Dee (1998); Hoxby (1994); Jepsen (1999); and Sander (1998) in United States settings, and Andersen and Serritzlew (2005) abroad.[5] Most of these studies have found either modestly positive, or null or inconsistent effects of private school competition on public school students' educational outcomes (Belfield and Levin, 2002; McEwan, 2000). These studies use a variety of estimation techniques to attempt to overcome the simultaneity problem; while some studies rely on OLS with covariates to adjust for possible omitted variables (Arum, 1996), most use some form of instrumental variable analysis (Dee, 1998; Jepsen, 1999; Sander, 1998). Some studies use population-level demographic data as instruments for private school attendance (Couch, Shughart, and Williams, 1993), but most use some measure of the density of the Catholic population in a given area (Dee, 1998; Hoxby, 1994; Jepsen, 1999; Sander, 1998) The rationale is that larger populations of Catholics in a given area increase the likelihood that the Catholic church will expend resources in that area to support a private Catholic school (Dee, 1998; Hoxby, 1994; Jepsen, 1999). However, there are reasons to question the validity of religious concentration as an instrument for private schooling (Altonji, Elder and Taber, 2004), and studies relying on cross-sectional variation in private school concentration in a community are subject to the usual omitted variables and reverse causation problems. In addition, Catholic shares might capture only a small fraction of the private school landscape in large swaths of the country, such as the south. For instance, in Florida, which runs the scholarship tuition program analyzed in

[5] There exists an even larger related literature on the performance effects of the degree of traditional school competition. Well-known examples in this literature include Cullen, Jacob and Levitt (2005), Hoxby (2000) and Rothstein (2006) in the United States, and Clark (2009), and and Gibbons, Machin and Silva (2008) in England. Hsieh and Urquiola (2006) shudy the effects of generalized competition in Chile.

this paper, 12.08% of private schools operating in 2001 were Catholic. By contrast, 13.44% were Baptist; 8.58% were evangelical, 8.25% were reported as Christian with no further identifying information, and 13.93% were non-denominational.

A few papers have taken a different tack and identified the effects of voucher programs directly on student outcomes in the public schools. Greene and Marsh (2009) study the effects of vouchers in Milwaukee, and find evidence of modest positive effects of private school competition. However, the fact that in cross-section the degree of competition facing any given school in Milwaukee is relatively similar and over time could be due to endogenous changes in competition makes interpretation of evidence from Milwaukee more limited. Chakrabarti (2008) and Hoxby (2003) make use of school-level differences in a Milwaukee school's percentage eligible for vouchers, but one might be worried that schools with different levels of student eligibility may be fundamentally different, and there is no arguably exogenous selection mechanism. Other papers (e.g., Chakrabarti, 2007; Chiang, 2009; Figlio and Rouse, 2006; Rouse et al., 2007; and West and Peterson, 2006) estimate the effects of receipt of an "F" grade in Florida's school accountability system, because repeated receipt of the lowest grade triggered voucher eligibility for students under the Opportunity Scholarship Program. But while these papers find evidence that receipt of an "F" grade leads to improved student outcomes, it is unclear to what extent these findings reflect the competitive effects of school vouchers versus the performance effects of accountability pressure. Sandstrom and Bergstrom (2005) study the introduction of a voucher program in Sweden, but their only available outcome is grades, rather than objective measures of test performance. West and Woessmann (2008) make use of cross-country variation to find that countries with more private schools have higher-performing public schools; they find evidence of a positive cross-sectional relationship between the private school

penetration and public school performance using historical Catholic shares as an instrument for private schooling. Gallego (2006) presents similar evidence using a cross-section of Chilean localities in which the historical stock of Catholic priests in a community serve as an instrument for current voucher schools.

The most similar work to ours in the present literature, a working paper by Chan and McMillan (2009) written simultaneously with our paper, studies the effects of a tuition tax credit that was phased in for two years in Ontario and then unexpectedly canceled. The authors take advantage of the fact that some public schools were nearby a larger number of private schools at the time of the voucher's introduction, an identification strategy that is fundamentally similar in key respects to our own. They find that once Ontario began offering its tax credit, initially valued at $700 and set to rise over time, public schools with a larger private school share in their catchment area improved their students' test-passing rates, but these gains were not sustained once the credit was ended.

The Florida Tax Credit Scholarship Program setting provides distinct advantages over studying the Milwaukee voucher program or Florida's Opportunity Scholarship Program. The voucher program being studied is not linked to an accountability system, so we are not confounding voucher threats with accountability pressure, as is the case in studies that measure the effects of "F" receipt under Florida's accountability plan. And unlike studies of the Milwaukee program, we are able to observe data both before and after the introduction of the voucher program, and take advantage of the fact that Florida is not a single educational market like Milwaukee, a relatively small city from a spatial perspective. Our ability to study the introduction of a voucher program at its outset, combined with our study of a state with a vast variation in private school penetration on the eve of the program's introduction, offers

considerable opportunities for causal identification of the effects of a school voucher program in the United States.

As mentioned above, our identification strategy is similar in nature to that of Chan and McMillan's (2009) working paper using data from Ontario. Like Chan and McMillan, we find evidence that the increased competitive pressure associated with school vouchers led to improvements in public school performance. That said, our paper is distinct from theirs in a number of key ways. First, we are able to investigate the effects of a voucher aimed at low-income families, with a level of generosity that approaches the costs of sending a child to a religious elementary school. The Ontario tuition tax credit began at a level sufficiently small that it was unlikely to attract low-income families, and so was unlikely to lead to a demand shock for that population. Our paper looks at the effects of a voucher on the types of schools unlikely to be affected by the Ontario tax credit. Second, Florida's population is dramatically more disperse than is Ontario's, as it has more than twenty major population centers as opposed to six, affording us the ability to exploit a wider variety of cross-market differences in the nature of private school competition. Third, while in Ontario families already enrolled in private schools could collect tax credits, in Florida students must have spent a full year in the public schools to collect a voucher. This fact means that the Florida voucher would work through the attraction of new private school students rather than the subsidization of existing private school students.

We are also able to identify the effects of school vouchers for sets of schools that experience different levels of incentive to respond to changes in competitive pressure. We consider two specific dimensions – school grade span and district Title I policies—that we believe moderate the extent to which public schools will experience the voucher program as a threat. With regard to grade span, we posit that voucher competition is likely to be more intense

for elementary and middle schools, where the voucher covers a much larger fraction of the total tuition bill, than for high schools. With regard to Title I policies, we use differences in schools' likelihood of receiving Title I funds based on student body composition and district policies to determine whether schools more at risk of losing Title I funds if they lose a few low-income students respond more sharply to the policy than those whose Title I status is not in question.

Finally, and most importantly, there was a year lag between when the policy was announced (and students began actively applying for the school voucher) and when students actually began attending private schools. Therefore, we are uniquely able to identify the pure competitive effects of private school vouchers, absent the composition and resource effects of vouchers. Taken together with the new Chan and McMillan (2009) findings, our results provide strong evidence of the potential effects of school vouchers on the public school system.

**3. Florida Tax Credit Scholarships and the Private School Landscape in Florida**

The FTC Program, signed into law in 2001 and opened to students in the 2002-2003 school year, provides corporations with tax credits for donations that they make to FTC organizations. These organizations, in turn, provide scholarships to students who qualify for free or reduced-price lunch and who either attended a Florida public school for the full school year before program entry, or who are entering kindergarten or first grade. With the exception of these early grade private school students, students already attending private schools in Florida are not eligible for first-time scholarships (though students who enter a private school on a scholarship are eligible to retain their scholarships in future years, so long as their family income remains below twice the federal poverty line.)

We exploit geographic variation in potential private school competition to estimate differential effects of this program. Because we want to employ an identification strategy that is not subject to reverse causation bias, we characterize geographic areas by the amount of private school competition in existence before the program was announced; we have no reason to believe that there was anticipatory entry by private schools, as the program had not been widely discussed for long prior to its announcement, and no students could attend private schools using a voucher in the year following announcement; students could only apply for school vouchers during that year. Thus, while there was increased entry into the private school market following the introduction of the program, our results do not identify program effects off of the entry of these new private schools.

To illustrate the nature of private schooling in the state of Florida on the eve of the program's announcement, we examined parent reports of their children's school attendance in the 5% microdata sample of the 2000 Census IPUMS. As seen in Table 1, private school attendance was fairly widespread overall; 11.4% of Florida students aged 6-17 attended private schools. Unsurprisingly, given the resource constraints of low-income students, private school attendance rates were dramatically lower for this group. Among students in income groups that would become eligible for the FTC program, only 5.4% attended private schools on the eve of the voucher.

One can see from Table 1 that metropolitan areas of Florida had very different levels of private school penetration, as well as very different degrees to which low-income students participated in private schooling prior to the introduction of the program.[6] The share of low-income students attending private schools varied widely in different metropolitan areas, ranging from 1.4% attendance in Punta Gorda to 7.9% attendance in the Melbourne-Titusville-Cocoa-

[6] Metropolitan areas are ordered by school-age population for ease of apples-to-apples comparisons.

Palm Bay area. Similar-sized metropolitan areas had very different levels of private school penetration; for instance, Ocala and Tallahassee were nearly the same size, but Ocala's low-income population share in private schooling was nearly twice that of Tallahassee's. Interestingly, Tallahassee had a larger overall private school attendance share than Ocala, so the variation across metropolitan areas is even more nuanced. We employ both cross-metropolitan area and within-metropolitan area variation in private school penetration in this study, because the concentration of existing private schools is not uniform across a metropolitan area. This difference in penetration of private schools provides us with some degree of variation in the extent to which public schools in different areas face competitive pressure from private schools that could realistically entice away low-income students with FTC scholarships. We present information on within-metropolitan area differences in private school competition in the next section.

## 4. Data and Methods

*4.1 Data*: Our analysis draws on several sources of data from the state of Florida. The Florida Department of Education (FDOE) publishes public and private school addresses, including latitude and longitude measures for the public schools. The FDOE also publishes details on public schools such as the grades that they receive from the FDOE, the grade ranges that they serve, and the percent of their students that are subsidized lunch eligible. Identifying schools as elementary, middle or high school grades is important because we match private school competitors to public schools based on their grade levels served. In the cases in which the FDOE did not report the grade ranges served, we deduced whether the school served elementary, middle

or high school grades based on the grades of observed test-takers in the school. The schools were then classified as elementary, middle, high, K-8, 6-12, or all grades.

The address information was geocoded using ARCGis software to generate the competition measures detailed below. Physical addresses were used to geocode private school locations, yielding valid locations for 85% of private schools. We used latitude and longitude data to locate the public schools, as this generated valid locations for a higher proportion of schools than did using physical addresses. The geocoding process generated valid data for 89% of the public schools, which were then matched to the student data.

Test scores and demographic characteristics for all students in Florida public schools are provided through FDOE's Education Data Warehouse. We also have information on the schools that students attended during the year. Some students attended multiple schools during the school year. Because we lacked information for the proportion of time students spent in each school, we randomly assigned children observed in multiple schools to one of the schools in which they were observed. Our analysis includes test score data from the 1999-2000 school year through the 2006-2007 school year. While we could have also included test score data for the 1998-1999 academic year, the first year of statewide student testing in Florida, we opted to begin our analysis in 1999-2000 for two main reasons. First, the test scores in 1999-2000 are more comparable to future years of data because the state calculated a developmental scale score on the statewide criterion-referenced examination, the Florida Comprehensive Achievement Test (FCAT). Second, 1999 is the first year in which Florida assigned letter grades to schools as part of its school accountability program. Restricting our analysis to the post-accountability years allows us to avoid any contaminating effects from the introduction of the accountability program in the period prior to the introduction of the FTC scholarship program. As an additional check,

we also present additional evidence using a longer panel of older data to evaluate the potential exogeneity of our measure of private school competition for public schools.

Students classified as learning disabled were excluded from the analysis. Disabled students are eligible for a more generous scholarship program, the McKay Scholarship Program, and the new FTC program should therefore have had no additional effect on schools' efforts to retain these students by improving their education. Indeed, applicants for vouchers under the FTC program who were disabled and therefore eligible for a McKay Scholarship were directed to that program instead. The full dataset includes 9,765,799 student-year observations, observed over the 1999-2000 to 2006-2007 school years, for a total of 2,787,158 students. Because scores for students in the same school in the same year are likely to be correlated, we cluster our standard errors at the school-year level.

*4.2 Measures:* Our dependent measure is a student's developmental scale test scores on the Florida Comprehensive Achievement Test, a criterion-referenced test used for school and student accountability. To ease interpretation, test scores are standardized at the grade level.

We use four primary measures to estimate the competitive pressure that public schools face from private competitors. While our measures of competition are all variations on a similar theme, we believe that it is important to report our results using a variety of competition measures in order to help to ensure that our results are not due to a fortuitous choice of competition measure. Our first measure is the crows-flight distance between the physical addresses of each public school and the nearest private competitor. A private school qualifies as a competitor to a public school if it serves any of the grades taught in that public school. We call this the "distance" measure of competition. We find that, as Florida's population is heavily

urban, the overwhelming majority of public schools have a private school within five miles. Therefore, we restrict our analysis in this paper to schools with at least one private school within five miles.

Second, we consider the number of private competitors within a five mile radius of the public school ("local" private competitors). We call this the "density" measure of competition. The distance and density measures gauge whether easier access to a private school of any type increased the competitive pressure on public schools when the new policy lowered the effective price of private school for eligible students.

Third, we consider the number of types of local private schools. A type is defined by religious affiliation; schools self-identify as to their affiliation when reporting to the FDOE. We identify 10 types of private schools, including non-religious; non-denominational or multidenominational; Catholic; Protestant; Evangelical; Baptist; Islamic, Jewish; Christian general (no specific denominational information); and other religious schools. A type is considered to be represented if at least one school of that type is located within a five mile radius of the public school. We call this the "diversity" measure of competition.

In the final specification of the model, we use the counts of different types of schools within a five mile radius to generate a modified Herfindahl index score for each school. We call this the "concentration" measure of competition. The Herfindahl index is a measure of market concentration given by the sum of the squares of the market shares held by each competitor. In our measure, a "competitor" is defined as a religious (or secular) type *r*, and the share is given by: $\frac{Count_r}{\sum_R Count_r}$. Higher values of the Herfindahl index indicate a greater concentration of the share of private schools in the hands of one particular denomination or type of school. Thus, a

Herfindahl index score of 1 indicates a monopoly on the private school market by one denomination or type of school, while scores closer to 0 represent markets that are well-served by a variety of denominations. These last two measures capture the variety of options available to students; public schools in areas with a greater variety of options should feel more competitive pressure in the wake of the policy change. In order to ease interpretation of our results, we report the results of regression models in which we measure negative distance and one minus the Herfindahl measure, so that all four measures' models would generate results in which a positive competition means that more competition is associated with higher levels of student outcomes. We also multiply the coefficients on the competition variables by 100 in our tables to be able to distinguish between relatively small differences in effect sizes; descriptions of the effects in the text convert the coefficients back to their natural metrics.

Additional individual-level controls include demographic characteristics such as the sex and race of the student (Black, Hispanic, Asian, or other race; White is the omitted category), English language learner status, and free or reduced price lunch eligibility. These characteristics are all reported by schools to the Florida Education Data Warehouse.

Until 2000-01, Florida only tested students in a handful of grades; beginning in 2000-01 Florida began to test students in every grade from three through ten. Therefore, we do not observe prior test scores for all students in our analysis. For this reason, and because we would lose another pre-policy year of data were we to include them, our primary models do not include lagged test scores for students, but we have estimated our models including lagged scores. In general, the results that we present are weaker than the results that occur using the same sample when we control for lagged test scores. If anything, therefore, we present estimates that are on the modest side.

We controlled for some time-varying characteristics of schools that affect the degree of competitive pressure they may feel. Specifically, we controlled for the grades that schools received from the FDOE in the prior year; schools with lower FDOE grades may feel particular pressure to increase their scores to avoid accountability sanctions, independent of the effects of the FTC policy. Missing dummy variables were included to preserve information for students in schools for which these data were not reported. We also controlled for the percent of the school's student body that was eligible for free and reduced-price lunch. Finally, a series of dummy variables indicating the student's grade were included to account for developmental trends, and year dummies were included to account for time trends in scores.

*4.3. Models:* We use a series of fixed effects regression models to isolate the effect of competitive pressures from private schools on public school performance. Our basic model is:

$$(1)\ Y_{ist} = \alpha_s + \beta C_s * P_t + \gamma \vec{X}_{it} + \lambda M_{ist} + \mu \vec{S}_{st} + \delta \vec{T} + \varepsilon$$

where $Y_{ist}$ represents the math (or reading) score for student i in school s in year t; $\alpha_s$ represents a fixed effect for school s; $C_s$ represents the measures of the competitive pressure faced by school s; $\vec{S}_{st}$ is a vector of time-varying school characteristics; $P_t$ is an indicator for whether year t is post-policy implementation;[7] $\vec{X}_{it}$ is a vector of student characteristics, including sex, race, English language learner status, and eligibility for free or reduced price lunch, for student i in year t; $M_{ist}$ indicates that a student lacked a test for the prior year; $T$ is a series of year

[7] In our first set of models, post-implementation is simply the year during which students are applying for vouchers but none have left the public sector. We also estimate models with year-by-year post-implementation estimates. In this case P can be thought of as a vector of post-implementation year variables.

dummies; and $\varepsilon$ represents an error term. The coefficient on the competition measures interacted with the post-policy indicator, β, is our parameter of interest. We estimate models with just the first year of the program -- before any students have left the public schools but following the program's announcement -- as well as those with multiple post-implementation years in order to gauge the evolution of the effects of the program over time. Other models reported later in the program interact our competition measures with variables that reflect how strongly schools might respond to the policy. In particular, schools might respond to the policy more when they stand to lose more financial resources were a student to leave the school or when they believe that students are more likely to leave the school. We take up these considerations later in the paper.

We report robust standard errors, clustered at the school-year level, in our regression results. We have also estimated our models in which we have aggregated all data to the school level, and find consistent results.

*4.4. Descriptive statistics:* Table 2 reports descriptive statistics for the dependent and independent measures used in the regressions. Most students in Florida had access to at least some nearby private school options. The average distance from a child's current public school to the nearest private school option was 2.13 miles. Figure 1, Panel A shows the distribution of distance between a child's current public school to the nearest private school that served at least some of the same grades as the public school for all students, and for eligible students specifically (still have to combine these in the same graph). These graphs indicate that most students, regardless of program eligibility, attended public schools with private competitors that were within five miles of the public school, although eligible students tended to attend schools

with a slightly closer nearest competitor than did non-eligible students. Schools that had an above-median proportion of students eligible (46%; "high eligibility concentration schools") tended to be located nearer to a private school competitor than did schools that had a below-median proportion eligible (Figure 1, Panel B).

Moreover, students generally had access to a relatively large number of schools, and a fairly diverse sampling of types of schools, within five miles of their public schools. Students attended schools that had an average of 12.44 private competitors within a five mile radius, representing an average of 4.56 different types of religious (or secular) affiliations (Table 2). Figures 2 and 3 show the distribution of number of schools within a five mile radius of students' public schools and the number of types within that radius, respectively, for all students and for eligible students (Panel A) and by school poverty concentration (Panel B). Eligible students tended to have a greater number of private school competitors within a five mile radius of their public schools and a greater number of types of denominations among those competitors than did the population at large (Panel A, Figures 2 and 3). Students who attended schools with high eligibility concentrations had access to a greater number of local competitors than did students attending schools with low eligibility concentrations, and were more likely to be in schools with a larger number of types of local competitors (Figure 2 and 3, Panel B). Finally, Figure 4 shows the distribution of Herfindahl index scores. As can be seen in the graph, a sizeable fraction of public schools face just one type of private competitor, leading to a Herfindahl index of one. In order to have a higher value of this concentration measure equate to higher degrees of competition, we operationalize this variable as one minus the Herfindahl index in our regression models.

The degree of variation in these figures is somewhat unsurprising since our data draw from the entire state. However, even within metropolitan areas, a high degree of variation in competition measures exists. Figure 5 shows the four competition measures for Miami-Dade county, the largest county in Florida. While students in Dade county have better access to private schools, and to more types of private schools, than students in the state at large, there is still substantial variability in the competition measures among students within Dade county. Figure 6 and 7 supports this analysis by showing the Herfindahl index measures for the districts containing the four MSAs in Table 1 with the largest proportion of low-income students enrolled in private schools as of the 2000 census (Melbourne-Titusville-Cocoa-Palm Bay, Sarasota, Jacksonville, and Ocala; Figure 6) and the smallest number of students (Punta Gorda, Ft. Walton Beach, Tallahassee, and Ft. Pierce; Figure 7). This figure indicates that for both MSAs with large numbers of private school students and those with small numbers of private school students, a fair amount of variation in competition exists within those MSAs. Thus, our analyses are driven not only by between-county effects, but by within-county differences in exposure to competition as well.

## 5. Results

*5.1. Immediate effects of the introduction of the voucher program*: There are three main ways in which the introduction of a school voucher could affect public school performance. Public schools could react to private school competition by altering their policies, practices, or effort -- the direct competitive effect of school vouchers. In addition, school vouchers could affect public schools by changing the set of students who attend the school; if students are positively selected

into private schools with the voucher, this could lead to a reduced-ability clientele remaining in the public schools, or vice versa. Figlio, Hart and Metzger (2009) find that the voucher program led to negative selection into the private schools, indicating that the ability levels of those remaining in the public sector are higher than before. In the presence of positive peer effects, this could mean that part of a positive "competitive" effect of vouchers is the changing of the composition of the public school student body. A third possibility is that, so long as only a few students leave a public school with school vouchers, the vouchers could have a positive resource effect on public schools, as effective per-pupil resources might increase due to the indivisibility of classroom teachers. On the other hand, especially in Title I schools (which comprise a majority of public schools in Florida), losing students eligible for free or reduced-price lunches results in resource reductions that could affect student outcomes as well.

It is possible to eliminate the composition and resource effects of school vouchers and concentrate solely on the competition effects of school vouchers by looking only at the first year of the voucher program -- in the 2001-02 school year, after the program's announcement but before students could actually leave the public schools on a voucher -- in comparison with prior years before the program was announced. During this entire academic year, students in the public schools were applying for private school vouchers but had not yet left for the private schools. We therefore begin with school fixed effects estimates of the effects of competition on student performance in the first year of the program, when students were applying for school vouchers but none had yet left the public schools.

The results of this first analysis are reported in Table 3, in which the point estimates (with standard errors clustered at the school-year level) of the key competition variable for the four alternative models of competition are reported. As can be seen in the table, all four measures of

competition are positively and significantly related to student performance. Every mile the nearest private school moves closer, public school math performance increases by 0.014 of a standard deviation (0.013 in reading.) Likewise, a one standard deviation increase in the number of nearby private schools increases test scores by about 0.02 of a standard deviation in reading and math, as does a one standard deviation increase in the variety of nearby private schools. A one standard deviation increase in the (negative) concentration of private schools nearby is associated with about a 0.01 of a standard deviation increase in reading and math test scores. While these estimated effects are modest in magnitude, they are very precisely estimated and indicate a positive relationship between private school competition and student performance in the public schools even before any students leave the public sector to go to the private sector.[8] That is, these results provide a first piece of evidence that public schools responded to the *threat* of losing students to the private schools via the voucher program.

The final column of Table 3 presents the results of a model in which the dependent variable is the average of a student's standardized reading and standardized math exam (or the single exam score in the case of a student taking just one or the other.) We do this for two reasons: First, in 1999-2000, elementary students took only one test or the other, but not both, in the same year, so aggregating the two tests into the same dependent variable ensures that we include all test-taking students in the same model. Second, we combine the two tests so that we do not need to report two sets of regression results in all subsequent tables. Nonetheless, the three columns yield nearly identical results.

[8] We have also estimated these models aggregated to the school-by-year level and continue to see strong positive and statistically significant estimated effects of private school competition on public school performance. These results are available on request. We therefore conclude that regardless of whether we estimate student-level models with clustered standard errors or aggregated models, the fundamental results remain unaltered.

One potential concern is that any apparent competitive effects of private schools on public school performance picked up in regressions may simply reflect superior performance by schools that have close competitors, regardless of whether or not scholarships are offered to low-income students. For instance, perhaps schools of all different types are more likely to open up in areas with a large concentration of high-income families. This would produce a spurious positive correlation between public school scores (since high-income children tend to outperform their low-income peers on standardized tests, on average) and competition (McEwan, 2000). While the school fixed effect would control for this in a cross-sectional regression, school fixed effects will not remove spurious correlations between competition measures and longitudinal score *gains*, or in trends over time in the performance of public schools in a community. Therefore, it is useful to test how competition and public school performance levels and longitudinal trends were related prior to the introduction of the policy.

One test for whether there were differences in student performance based on the strength of competition prior to the introduction of the FTC scholarship program involves estimating models that include a one-year lead of the policy -- in the 2000-01 school year before the policy was announced. If schools with nearby private schools were improving over time, one would expect to observe positive coefficients on these policy lead variables. Indeed, as seen in Table 4, we do observe modest positive coefficients on the lead of the policy variable -- statistically significant in two of the four models -- suggesting that there may be some evidence that public schools with more nearby private schools (and a larger number of nearby private school types) had been improving prior to the voucher program. That said, the coefficients on the lead of the policy variable are at most one-fifth of the magnitudes of the corresponding coefficients on the first year of the policy, suggesting that a break from trend has been taking place. Unfortunately,

Florida did not collect a long panel of statewide data prior to the policy introduction that would allow us to gauge the presence of long-term trends. The results with the lead of the policy variable indicate that it is also important to see whether the policy had differential estimated effects on the schools for which we would expect to see larger impacts.

Because we have only two years of pre-program data, we cannot rule out the possibility that public schools with a larger amount of private school competition nearby had been on a different growth trajectory during the period of time prior to the policy's introduction. We can, however, use a different data source to investigate the potential presence of longer trend in public school performance. Prior to the policy's introduction, each Florida school district administered its own nationally-normed standardized test (generally the Stanford Achievement Test, Comprehensive Test of Basic Skills or the Iowa Test of Basic Skills), and the three largest school districts in the state (Broward County, Miami-Dade County, and Palm Beach County) have provided us with school average reading and math performance on the relevant standardized tests for the years prior to the policy's introduction.[9] Figures 8 through 11 present graphs showing the relationships between our four private school competition measures and school-level changes in average national percentile rankings in reading and mathematics from 1996-97, five years before the policy introduction to 2000-01, the year before the policy introduction. As can be seen in the graphs, there is no apparent relationship between the level of private school competition present in 2000 (when we measure competition for the purposes of our analyses) and over-time changes in school-level test performance. The correlations between the competition measures and changes in test performance in the long difference are -0.086 for distance, -0.023 for density,

[9] These three counties yield results that are roughly representative of the rest of the state. Were we to restrict our Table 4 analysis to just these three counties, the figures in the first column would be 2.218 (with a standard error of 0.680) for distance; 0.216 (with a standard error of 0.040) for density; 0.955 (with a standard error of 0.256) for diversity; and 4.690 (with a standard error of 3.210) for Herfindahl.

+0.014 for diversity, and +0.006 for Herfindahl. Therefore, while we cannot rule out with absolute certainty the possibility that long-term trends are responsible for our results, the available evidence contradicts that explanation.

Out of concern that results may be driven by particular districts that house a large proportion of the students in the state, we estimate the same analysis excluding, one at a time, each county in the state. We find consistent evidence that, regardless of which county is dropped, the signs and general significance levels of the competition interactions are maintained. That said, the magnitudes of our key findings are notably smaller when we exclude Dade County, home of Miami and the largest county in the state. When Dade County is excluded, the the magnitude of our estimated effects of voucher competition effects fall by between 10 and 20 percent, though remain statistically significant at effectively the same levels as when Dade County is included in the analysis. No other county apparently affects our findings at all, as when we drop any of the other 66 Florida counties, our results remain virtually identical to the full-state analysis. Therefore, it is difficult to believe that some combination of counties is driving the general nature of our results, though the results are clearly stronger in the case of Dade County than in the rest of the state.

*5.3. Differential estimated effects by incentives to respond:* One would expect that some schools would have a greater degree of incentive to respond to potential competition associated with school vouchers aimed at low-income students than would others. We consider two major ways in which schools may face different incentives to react to competitive pressure. First, we suspect that elementary and middle schools will have more of an incentive to respond to competitive pressure than would high schools because the vouchers cover so much more of the share of

private school tuition and fees in the early grades versus in the high school grades. While the differences in the share covered might not be salient for higher-income families (and therefore estimation of differences in responsiveness across school levels might not be as relevant in the case of a universal tuition tax credit like the one studied by Chan and McMillan (2009)) the difference in out-of-pocket expenses between an elementary or middle school and a high school might be strongly prohibitive for many low-income families. Public high schools, knowing this, might not react as strongly to voucher competition as would public elementary and middle schools. Second, we suspect that public schools who stand to gain or lose the largest amounts of revenues depending on how many voucher-eligible students they retain would be more responsive than would those schools less likely to lose large amounts of revenues. We take advantage of plausibly naturally-occurring variation across the 67 Florida school districts in the implementation of the federal Title I program in an attempt to gain purchase on this second question.

There exists strong evidence to believe that the costs of attending a private school on a voucher would be more prohibitive in the case of high schools than in the case of elementary and middle schools, given that the voucher is capped at the same level regardless of the school level. In the case of the two Scholarship Funding Organizations that serve the southern half of Florida (Florida P.R.I.D.E. and the Carrie Meek Foundation) we have access to the actual out-of-pocket costs incurred by families for tuition, fees and books for students who participate in the voucher. As can be seen in Figure 12, the typical out-of-pocket expense is considerably higher for voucher participants in high school than it is for voucher participants in elementary and middle school. While the typical out-of-pocket expense post-voucher for elementary and middle school students is close to zero, it is around two thousand dollars for a high school student. As can be seen in

Figure 13, this is a substantial amount of money for the low-income families eligible for school vouchers in Florida. In order to send a child to a high school using a school voucher, the typical family must spend over one-tenth of its family income per student, more than twice the share of family income necessary to send a child to an elementary or middle school using a voucher.

Table 5 presents the estimated effects of increased private school competition through the school voucher on combined public school test scores, broken down by elementary and middle schools versus high schools. As can be seen, in each of the four model specifications, the estimated effect of voucher competition is sizably and statistically significantly stronger in the case of elementary and middle schools than in the case of high schools. In all four cases, the magnitude of the estimated effect in lower school levels is two to three times the estimated magnitude of the effect in high schools. This provides the first piece of evidence that public high schools were less responsive to private school competition that came about through the voucher program than were public elementary and middle schools.

We next consider whether public schools that face increased financial incentives to respond to voucher competition do so. As mentioned above, all public schools may experience resource effects as a consequence of losing students to private schools on the voucher. However, no schools have as large of an incentive to retain free or reduced-price lunch eligible students as those who are on the margin of receiving federal Title I aid. These federal resources, which average around $500 per pupil, are directed to school districts, which then allocate them to the elementary and middle schools where low-income students attend.[10] Not every public school with low-income students receives Title I aid; indeed, in 2001-02, 61 percent of elementary schools and 31 percent of middle schools statewide received Title I aid. Title I aid is allocated

[10] In Florida, high schools do not receive Title I funding. The potential loss of Title I funding is therefore another possible reason for the differences in estimated effects of voucher competition for elementary and middle schools versus high schools.

based on where schools rank within the school district; the highest-poverty schools receive Title I aid while the lower-poverty schools do not, and the poverty threshold that generates Title I funding differs considerably from district to district. In some school districts, all elementary or middle schools are Title I schools, and school districts also have discretion to limit Title I funding to elementary schools. In Florida, the overwhelming majority (92 percent) of Title I schools are considered "Schoolwide" Title I schools, where the Title I aid is not required to follow individual students per se but can be spent anywhere in the school (as the school is considered to be sufficiently low-income that all uses of the money would likely serve low-income students.) The remaining eight percent of Title I schools are considered "targeted assistance" schools, where the school's allocation for Title I must be spent on the low-income students themselves.[11] In either case, there is a large discrete jump in funding for a school that comes with being considered a Title I school versus not being a Title I school.

Between 2001-02 and 2002-03 Title I funding increased dramatically as a result of the reauthorization of the Elementary and Secondary Education act on January 8, 2002, permitting schools that did not receive Title I funding but were very close to the threshold for Title I funding to receive Title I funding in 2002-03. The likely expansion of Title I funds, which enjoyed strong bipartisan support in Congress, was well-known to Florida schools for all or most of the 2001-02 school year, according to conversations with school officials. While we do not know when exactly schools knew what the Title I cutoff for the next year in their school district would be, it is reasonable to believe that schools already receiving Title I funding in 2001-02 were relatively secure in the knowledge that they would likely continue to receive funding in 2002-03, and the poorest schools not yet receiving Title I funding in 2001-02 might expect that,

[11] This differs from the nation as a whole. Nationally, just over half of all Title I schools were schoolwide Title I schools in 2001-02.

should they not lose many low-income students to private schools via the voucher, they might themselves become Title I schools in the next year.

We envision that, in 2001-02, during which time public school students were applying for school vouchers, there were five different groups of public elementary and middle schools: (1) schools in districts where every school of a given level receives Title I funding; (2) schools in districts where no school in that given level receives Title I funding; (3) schools that received Title I funding in 2001-02 and, we suspect, expected to continue to receive funding in 2002-03; (4) schools on the margin of Title I funding in 2001-02 who might have expected to receive funding in 2002-03 were they to not lose many low-income students; and (5) schools below this margin who likely expected not to receive funding in 2002-2003. Of the 67 school districts, 30 districts identified all elementary schools as Title I schools, and 15 districts identified all middle schools as Title I schools in 2001-02 (group 1). All school districts had at least one Title I elementary school in 2001-02, but in 24 districts no middle schools were Title I schools (group 2). Of schools receiving Title I in 2001-02 (group 3), nearly 96 percent continued to receive Title I funding in 2002-03, indicating that ex post the presumption that with increased generosity would come some measure of Title I security appears to be accurate. It is not obvious how to draw the dividing line between groups 4 and 5, so we choose to be agnostic and look instead at how the school districts redrew their Title I allocation thresholds between 2001-02 and 2002-03. Specifically, we consider a school to be in group 4 if their 2001-02 student free/reduced-price lunch share is below the 2001-02 threshold for inclusion but above what would be the district's 2002-03 threshold for inclusion, and we consider a school in group 5 if its 2001-02 free/reduced-price lunch share is below what would be the district's 2002-03 threshold for inclusion. This distinction appears to be discerning: 40 percent of our group 4 schools ultimately received Title I

funding in 2002-03, while only 5 percent of group 5 schools did. In 17 school districts, the threshold for elementary school Title I inclusion was lower in 2002-03 than it was in 2001-02, as compared with one district in which the threshold increased. In five school districts, the threshold for middle school Title I inclusion was lower in 2002-03 than it was in 2001-02, while it increased in two districts. Hence, while we do not know how much schools knew about their prospects for receiving Title I aid the next year, this definition at least allows us to speculate as to which schools might have been on the margin of Title I receipt. We are particularly interested in the differential effects of voucher competition for group 4, these schools that we consider to be on the margin.

We estimate separate effects of voucher competition for three groups -- groups 1 (always-receivers in district), 2 (never-receivers in district), and 4 (on the margin of Title I receipt) -- relative to groups 3 (almost sure receivers) and 5 (almost sure non-receivers), which we lump together for now. Table 6 reports the differential estimated effects of voucher competition for these different groups. In all four specifications, we see that the estimated interaction between private school competition and schools always receiving Title I aid is negative, though it is only statistically significantly different from zero in the case of the concentration (Herfindahl) measure of competition. The estimated interaction between private school competition and schools never receiving Title I aid is not statistically significantly different from zero in any of the four specifications, though it too is at its most significant in the concentration measure of competition case. The interaction between private school competition and our measure of being in group 4, on the other hand, is always positive and strongly statistically significantly different from zero. This result indicates that public schools that are likely to receive Title I aid in the

next year if they retain their low-income students, but not if they don't, tend to disproportionately improve in the voucher-threat year.

Title I assistance is not the only way in which program-eligible students might plausibly bring resources to a school. Schools on average tend to receive more funding from their districts the more students they have in the school, and therefore these students -- like all other students -- could generate resources for the school. We therefore repeat the same exercise, but now also include the 2001-02 percentage eligible for free or reduced-price lunch (interacted with a dummy variable for post-policy and with that dummy for post-policy times the competition measure, as is the case with the other interacted variables. This interaction allows us to consider both ways in which more program-eligible students might bring more revenues to a school. There is no need to interact the 2001-02 percentage free or reduced-price lunch with the competition measure, as this interaction is subsumed into the school fixed effect.) The results of this specification are reported in Table 7. We find the findings from Table 6 remain basically unchanged, and that in two of the four specifications there is a positive and significant relationship between the estimated effects of the degree of competition and the percentage in the school eligible to receive a school voucher. These results again indicate that the schools on the margin of becoming Title I eligible -- the schools that would gain considerably were they to become Title I eligible -- are also the schools that apparently respond the most to the competitive threats of school vouchers. There is consistent evidence that the schools with the biggest incentives to improve as a result of the voucher program were the schools that improved.

*5.4. Longer-term estimates of the effects of school vouchers:* We also investigate whether the estimated effects of the voucher program persist to later years. After the first year of the

program, in addition to the competitive effects of the program there are also resource and composition effects as students leave the public schools for private schools under the voucher program. These results can be seen in Table 8, which presents results of models that include year-by-year estimates of the effects of the voucher program competition as well as leads of the policy. Table 8 shows that the estimated effects of the voucher program grow stronger over time; this could be due to increased knowledge of the program which might contribute to greater competitive pressure or to composition and resource effects. It is difficult to disentangle the reasons for this strengthening over time in the estimated effects of the voucher program.

**6. Discussion**

Our results indicate that the increased competitive pressure faced by public schools associated with the introduction of Florida's FTC Scholarship Program led to general improvements in public school performance. Both greater ease of access to private school options (measured by the distance and density measures) and the variety of options that students have in terms of the religious (or secular) affiliations of private schools (measured by the diversity and Herfindahl index measures) are positively associated with public school students' test scores following the introduction of the FTC policy. The gains occur immediately, before students left the public schools to use a voucher, implying that competitive threats are responsible for at least some of the estimated effects of the voucher program. The gains appear to be much more pronounced in the schools most at risk to lose students (elementary and middle schools, where the price of private school attendance with a voucher is much lower) and in the schools that are on the margin of Title I funding (with the attendant increases in revenues that might accrue.)

That said, our study has several limitations. First, our measures of competition reflect the state of the private school market in 2001, before private schools had a chance to respond to the FTC scholarship program. Although that ensures that the competition measure is not endogenous to post-policy test scores, it does give a less accurate view of the competitive pressures faced by schools as more time passes following the introduction of the FTC program. However, since we view this measure of competition as an instrument for the true degree of competition faced by public schools, these are likely to be conservative estimates of the effects of competitive pressures on public school students' test scores.

Second, our study includes only Florida data. It is possible that the dynamics between competitive pressures and public students' test scores are systematically different in Florida than in the rest of the nation. In particular, over 90 percent of Florida's students live in the top 20 most populous metropolitan areas represented in Table 1. In states with a greater share of the population in rural areas, the effects of a voucher program may not exert the same degree of competitive pressure on public schools. (That said, in sensitivity testing we do find evidence that rural schools with nearby private alternatives respond similarly to urban and suburban schools with similar levels of measured competition.) It may also be the case that Florida's diverse range of private school options may mean that Florida has a larger amount of private school competition in existence relative to other places. To the extent to which this is true, it limits the study's generalizability. Nonetheless, this study indicates that private school competition, brought about by the infusion of means-tested scholarships aimed at low-income families, could have sizeable effects on the performance of traditional public schools.

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**Table 1: Private school shares of total Florida student population aged 6-17, from 2000 Census**

| | Share of state student population | Percent of students in private schools | Percent of s below 185% poverty in p schools |
|---|---|---|---|
| Statewide | 100% | 11.2% | 5.4% |
| Miami-Hialeah | 14.4 | 12.4 | 4.7 |
| Tampa-St. Petersburg-Clearwater | 13.7 | 12.3 | 6.5 |
| Orlando | 11.0 | 11.6 | 5.9 |
| Fort Lauderdale-Hollywood-Pompano Beach | 9.8 | 12.4 | 5.5 |
| Jacksonville | 7.8 | 13.0 | 6.9 |
| West Palm Beach-Boca Raton-Delray Beach | 6.5 | 13.5 | 5.3 |
| Lakeland-Winter Haven | 3.2 | 8.7 | 4.6 |
| Melbourne-Titusville-Cocoa-Palm Bay | 3.1 | 11.5 | 7.9 |
| Pensacola | 2.9 | 10.9 | 6.5 |
| Sarasota | 2.8 | 11.6 | 7.0 |
| Daytona Beach | 2.5 | 9.7 | 5.7 |
| Fort Myers-Cape Coral | 2.4 | 10.7 | 6.0 |
| Fort Pierce | 1.9 | 11.0 | 4.5 |
| Tallahassee | 1.8 | 11.1 | 3.8 |
| Ocala | 1.5 | 10.1 | 6.7 |
| Gainesville | 1.4 | 10.5 | 6.4 |
| Naples | 1.2 | 9.4 | 4.7 |
| Fort Walton Beach | 1.2 | 7.3 | 3.2 |
| Panama City | 0.9 | 7.6 | 4.9 |
| Punta Gorda | 0.6 | 5.2 | 1.4 |
| Other areas of Florida | 9.1 | 5.7 | 3.7 |

Notes: Data are collected from the 5 percent public microdata sample from the 2000 Census IPUMS files. Extrapolating to the state as a whole, there would be approximately 226,000 students enrolled in private schools statewide, with approximately 50,000 coming from families with incomes below 185 percent of the poverty line.

**Table 2: Descriptive statistics**

| | Mean | Standard deviation |
|---|---|---|
| **Test performance** | | |
| State scale math score | 0 | 1 |
| State scale reading score | 0 | 1 |
| **Competition measures** | | |
| Miles to nearest private school competitor | 2.13 | 3.28 |
| Number of local private schools | 12.44 | 11.57 |
| Number of denominational types represented in 5 mile radius | 4.56 | 2.58 |
| Herfindahl index | .36 | .25 |
| Specific denominational measures | | |
| Number of local Baptist schools | 1.52 | 1.58 |
| Number of local Catholic schools | 1.93 | 2.56 |
| Number of local "Christian" (general) schools | 1.03 | 1.33 |
| Number of local Evangelical schools | 1.02 | 1.45 |
| Number of local Protestant schools | 1.07 | 1.48 |
| Number of local Islamic schools | .10 | .35 |
| Number of local Jewish schools | .40 | 1.19 |
| Number of local schools of other denominations | .07 | .27 |
| Number of local non-denominational schools | 1.63 | 1.74 |
| Number of local non-religious schools | 3.66 | 4.16 |
| **Demographic measures** | | |
| Black | .22 | |
| Hispanic | .23 | |
| Asian | .02 | |
| White | .50 | |
| Other race | .03 | |
| Male | .48 | |
| English language learner | .18 | |
| Free lunch eligible | .36 | |
| Reduced lunch eligible | .10 | |
| Observations | 9,767,275 | |

Notes: Data from the Florida Education Data Warehouse, the Florida Department of Education's Florida School Indicators Reports, and the Florida Department of Education. Herfindahl index means include only children in schools for which at least one local competitor existed (92.4% of the sample); the Herfindahl index is undefined if there are no local competitors.

**Table 3: Fixed effects regression estimates of the effects of the introduction of voucher competition on public schools: first year program estimates only (data through 2001-02)**

| Competition measure | Estimated effect on math scores | Estimated effect on reading scores | Estimated effect on average of reading and math scores |
|---|---|---|---|
| Distance | 1.415*** | 1.317*** | 1.438*** |
| | (0.170) | (0.154) | (0.155) |
| | [4.641] | [4.320] | [4.717] |
| Density | 0.216*** | 0.193*** | 0.216*** |
| | (0.019) | (0.015) | (0.016) |
| | [2.499] | [2.233] | [2.499] |
| Diversity | 0.732*** | 0.717*** | 0.766*** |
| | (0.087) | (0.075) | (0.077) |
| | [1.889] | [1.850] | [1.976] |
| Herfindahl | 4.681*** | 4.608*** | 4.982*** |
| | (0.740) | (0.641) | (0.656) |
| | [1.170] | [1.152] | [1.246] |
| Number of school-by-year clusters | 7748 | 7745 | 7751 |
| Observations (all 4 models) | 2,604,746 | 2,610,177 | 2,761,350 |
| R-squared (all 4 models) | 0.255 | 0.247 | 0.281 |

Notes: Each cell represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a post-policy indicator) from a separate regression model. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school-year level are beneath parameter estimates. Estimated effects of a one-standard-deviation change in the competition variables are presented in square brackets beneath standard errors. The dependent variable is a student's standardized test score in reading or math (or the average of the standardized reading+math scores.) Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, percent of student body eligible for free or reduced price lunch and the school's prior year grade from the Florida Department of Education, as well as school fixed effects. Data come from 1999-2000 through 2001-02 years only. There are more observations for the average of reading and math scores because in 1999-2000 elementary-aged students took reading and math examinations in different grades. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively.

**Table 4: Fixed effects regression estimates of the effects of the introduction of voucher competition on public schools: first year program estimates only, including program leads (data through 2001-02)**

| Competition measure | Estimated effect on average reading+math scores | |
|---|---|---|
| | First year program effect (2001-02) | Lead of program (2000-01) |
| Distance | 1.591*** | 0.225 |
| | (0.239) | (0.230) |
| Density | 0.261*** | 0.067** |
| | (0.024) | (0.023) |
| Diversity | 0.908*** | 0.211+ |
| | (0.114) | (0.111) |
| Herfindahl | 5.659*** | 0.997 |
| | (1.011) | (0.983) |
| Number of school-by-year clusters | 7751 | |
| Observations (all 4 models) | 2,761,350 | |
| R-squared (all 4 models) | 0.281 | |

Notes: Each pair of cells represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a post-policy indicator) as well as the coefficient estimate on the lead of the same variable from a separate regression model. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school-year level are beneath parameter estimates. The dependent variable is a student's standardized test score in reading or math. Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, percent of student body eligible for free or reduced price lunch and the school's prior year grade from the Florida Department of Education, as well as school fixed effects. Data come from 1999-2000 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively.

**Table 5: Fixed effects regression estimates of the effects of the introduction of voucher competition on public schools: differences by elementary or middle versus high school, first year program estimates only (data through 2001-02)**

| Specification | Estimated effect on elementary and middle schools | Estimated effect on high schools | p-value of difference |
|---|---|---|---|
| Distance | 1.738*** | 0.835** | 0.009 |
| | (0.177) | (0.300) | |
| Density | 0.263*** | 0.107 | 0.024 |
| | (0.017) | (0.067) | |
| Diversity | 1.046*** | 0.346+ | 0.001 |
| | (0.085) | (0.183) | |
| Herfindahl | 7.304*** | 1.685 | 0.000 |
| | (0.735) | (1.252) | |

Notes: Each row represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a post-policy indicator) broken down by elementary/middle versus high school status. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school-year level are beneath parameter estimates. The dependent variable is a student's standardized test score in reading or math. Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, percent of student body eligible for free or reduced price lunch and the school's prior year grade from the Florida Department of Education, as well as school fixed effects. All relevant two-way interactions are included as control variables. Data come from 1999-2000 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. All models have 2,761,350 observations in 7751 school-by-year clusters, an a R-squared of 0.281.

**Table 6: Fixed effects regression estimates of the effects of the introduction of voucher competition on public schools: differences by likely Title I status in 2002-03 (data through 2001-02)**

| Competition measure | Estimated effect for "almost sure Title I" and "almost sure not Titlel I" schools | Additional effect for schools where all schools at this level receive Title I | Additional effect for schools where no schools at this level receive Title I | Additional effect for schools on margin of Title I |
|---|---|---|---|---|
| Distance | 1.127*** | -0.763 | 0.072 | 2.691*** |
| | (0.222) | (0.669) | (0.484) | (0.626) |
| Density | 0.205*** | -1.083 | -0.068 | 0.333*** |
| | (0.021) | (1.367) | (0.076) | (0.053) |
| Diversity | 0.785*** | -2.666 | -0.279 | 2.195*** |
| | (0.105) | (1.795) | (0.279) | (0.298) |
| Herfindahl | 6.072*** | -13.273** | -3.278 | 8.330*** |
| | (1.008) | (4.283) | (2.017) | (2.431) |

Notes: Each row represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a post-policy indicator) as well as the coefficient estimates of the interaction of this variable with indicators of potential incentives for retaining students to receive Title I funding. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school-year level are beneath parameter estimates. The dependent variable is a student's standardized test score in reading or math. Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, percent of student body eligible for free or reduced price lunch and the school's prior year grade from the Florida Department of Education, as well as school fixed effects. All relevant two-way interactions are included as controls. Data come from 1999-2000 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. All models have 1,975,505 observations in 5525 school-by-year clusters (high school students are excluded, as they do not receive Title I aid), an a R-squared of 0.272.

**Table 7: Fixed effects regression estimates of the effects of the introduction of voucher competition on public schools: differences by likely Title I status in 2002-03, models also including interactions with percent free/reduced-price lunch (data through 2001-02)**

| Competition measure | Estimated effect for "almost sure Title I" and "almost sure not Titlel I" schools | Additional effect for schools where all schools at this level receive Title I | Additional effect for schools where no schools at this level receive Title I | Additional effect for schools on margin of Title I | Interaction between competition measure x post and percent free/reduced-price lunch |
|---|---|---|---|---|---|
| Distance | -0.808+ | -1.398* | 0.508 | 2.914*** | 0.044*** |
| | (0.445) | (0.708) | (0.484) | (0.646) | (0.009) |
| Density | 0.056 | -1.115 | -0.029 | 0.360*** | 0.003*** |
| | (0.056) | (1.383) | (0.079) | (0.056) | (0.001) |
| Diversity | 0.466*** | -2.760 | -0.208 | 2.250*** | 0.006 |
| | (0.233) | (1.800) | (0.285) | (0.306) | (0.004) |
| Herfindahl | 5.334* | -12.838** | -2.819 | 8.477*** | 0.007 |
| | (2.123) | (4.290) | (2.052) | (2.502) | (0.040) |

Notes: Each row represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a post-policy indicator) as well as the coefficient estimates of the interaction of this variable with indicators of potential incentives for retaining students to receive Title I funding. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school-year level are beneath parameter estimates. The dependent variable is a student's standardized test score in reading or math. Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, percent of student body eligible for free or reduced price lunch and the school's prior year grade from the Florida Department of Education, as well as school fixed effects. All relevant two-way interactions are included as controls. Data come from 1999-2000 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. All models have 1,975,505 observations in 5525 school-by-year clusters (high school students are excluded, as they do not receive Title I aid), an a R-squared of 0.272.

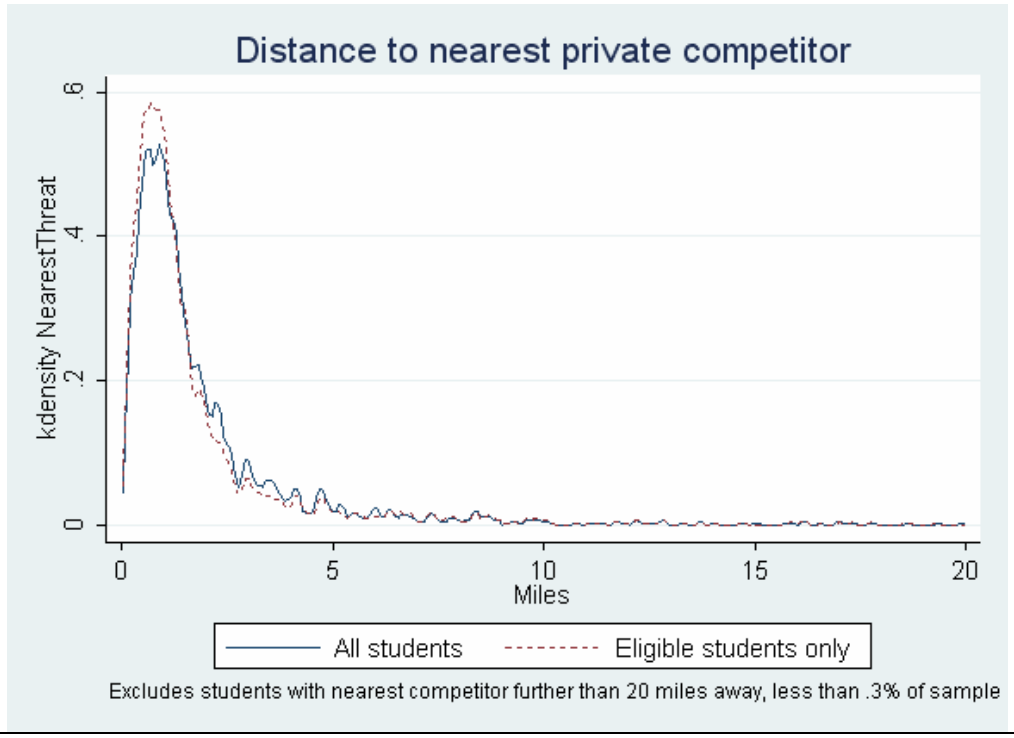
**Table 8: Fixed effects regression estimates of the effects of the introduction of voucher competition on public schools: year-by-year program estimates, including leads (data through 2006-07), estimates using average of reading plus math test scores**

| Competition measure (pre-policy) | Lead of program (2000-01) | First year (2001-02) | 2002-03 | 2003-04 | 2004-05 | 2005-06 | 2006-07 |
|---|---|---|---|---|---|---|---|
| Distance | 0.096 | 1.435*** | 1.207*** | 1.580*** | 2.130*** | 2.549*** | 1.969*** |
| | (0.334) | (0.305) | (0.296) | (0.294) | (0.300) | (0.302) | (0.315) |
| Density | 0.048 | 0.242*** | 0.239*** | 0.272*** | 0.371*** | 0.460*** | 0.395*** |
| | (0.034) | (0.031) | (0.031) | (0.030) | (0.031) | (0.032) | (0.033) |
| Diversity | 0.084 | 0.779*** | 0.700*** | 0.862*** | 1.270*** | 1.619*** | 1.342*** |
| | (0.168) | (0.150) | (0.146) | (0.144) | (0.148) | (0.151) | (0.155) |
| Herfindahl | 0.071 | 4.646*** | 3.834** | 5.496*** | 8.556*** | 10.740*** | 8.859*** |
| | (1.491) | (1.332) | (1.279) | (1.281) | (1.305) | (1.314) | (1.353) |

Notes: Each cell represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and year indicators) from a separate regression model. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school-year level are beneath parameter estimates. The dependent variable is a student's standardized test score in reading or math. Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, percent of student body eligible for free or reduced price lunch and the school's prior year grade from the Florida Department of Education, as well as school fixed effects. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. All models have 8,979,603 observations spread across 22,247 school-by-year clusters and a r-squared of 0.26.

**Figure 1: Distribution of distance between students' public schools and the public school's nearest private competitor.**

Panel A




Panel B

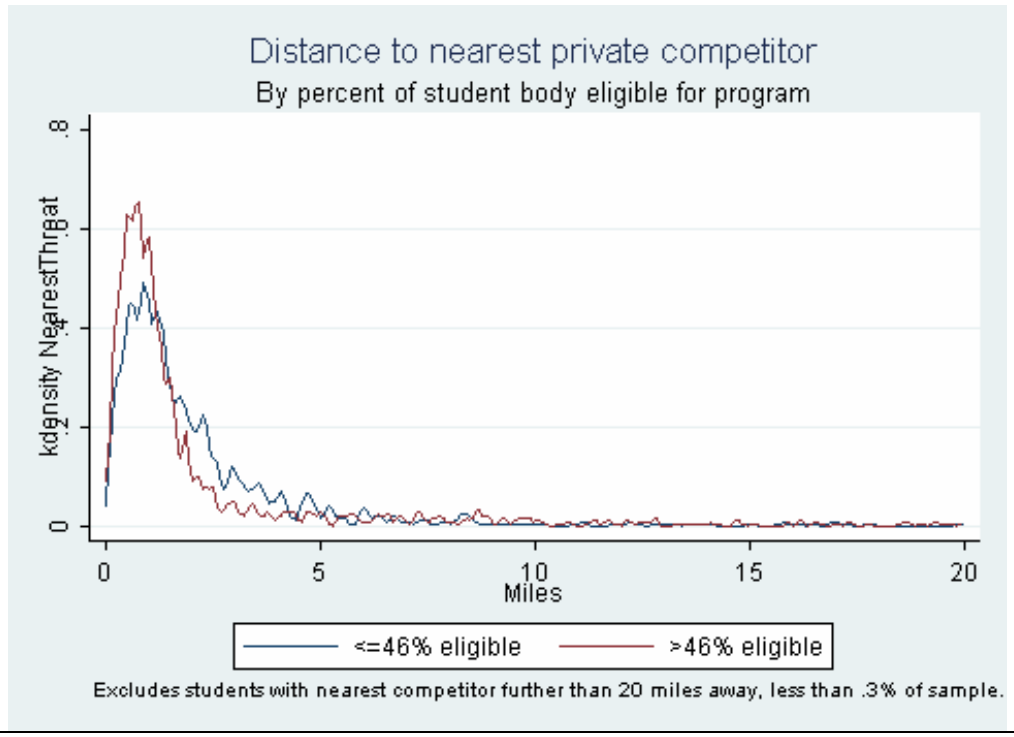
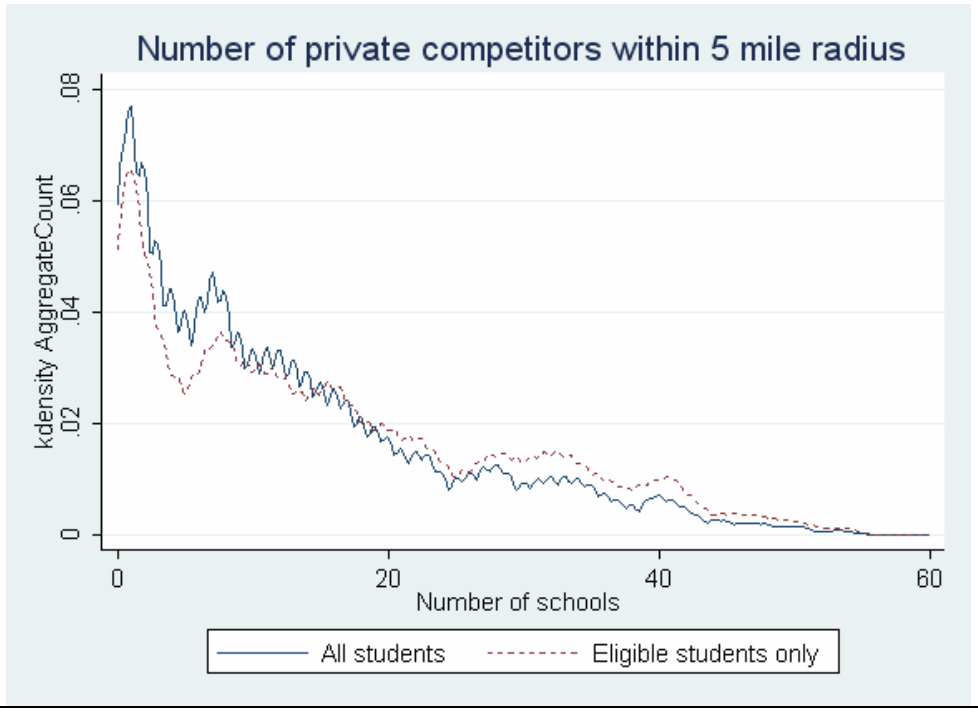


**Figure 2. Distribution of number of private school competitors within five miles of students' public schools.**

Panel A




Panel B

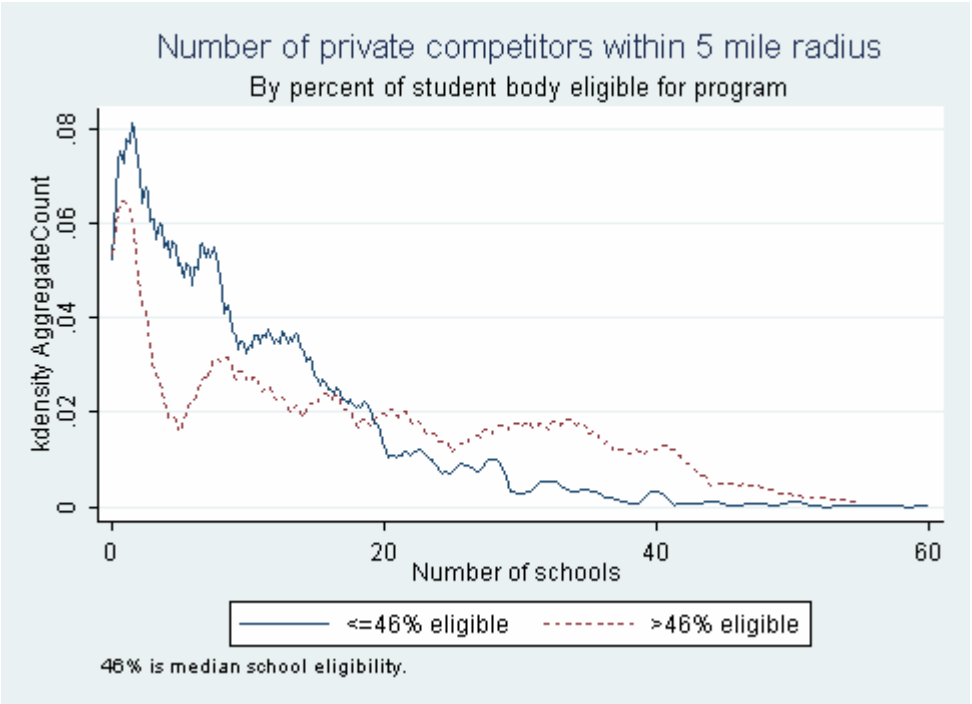


**Figure 3. Distribution of diversity measures**

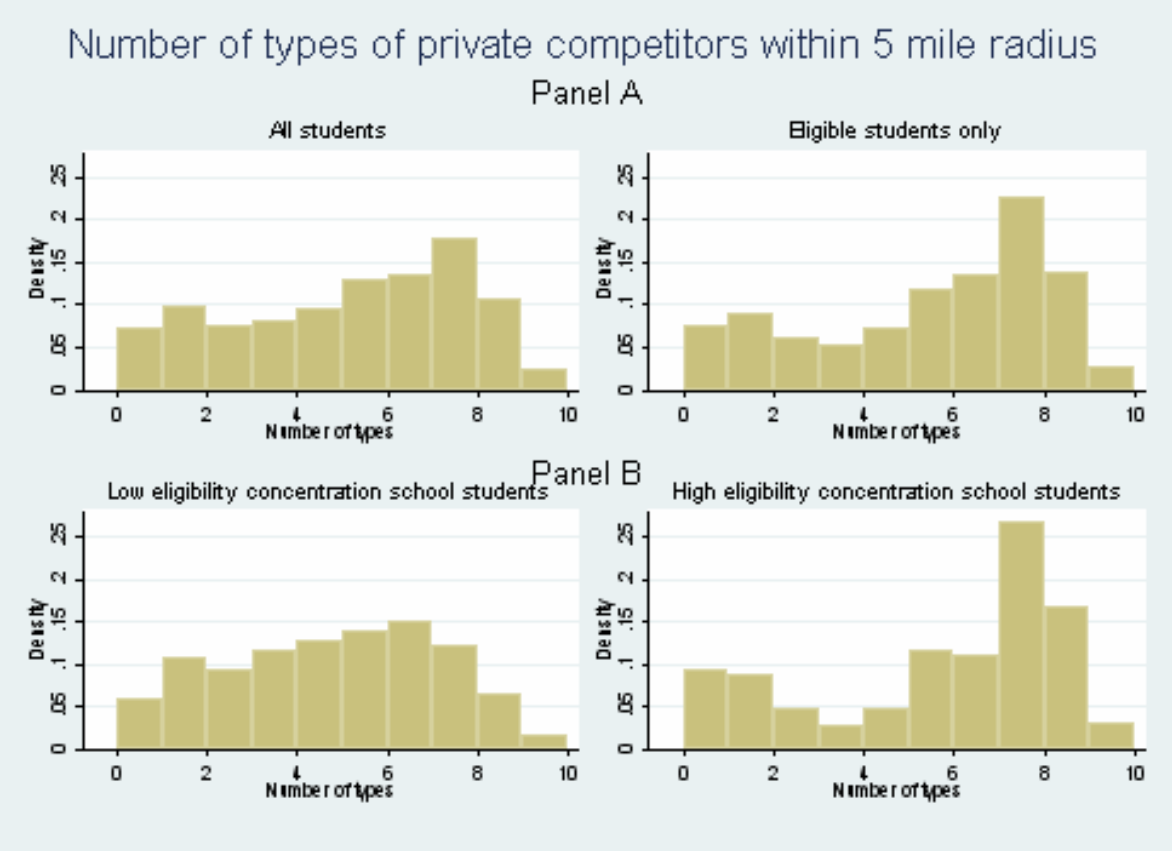
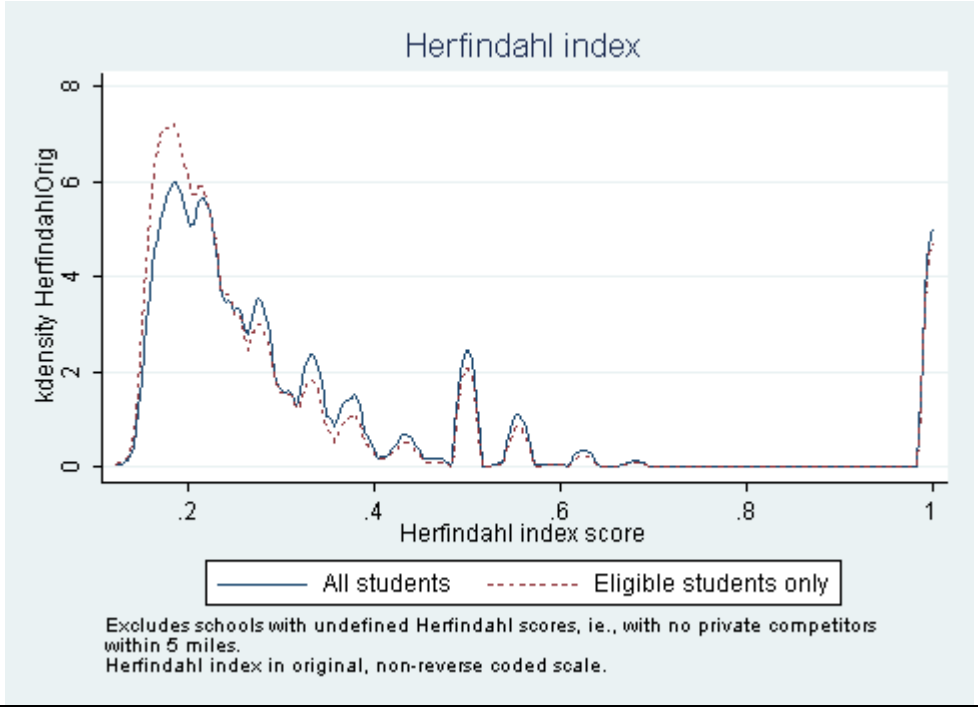


**Figure 4. Distribution of Herfindahl Index**

Panel A




Panel B

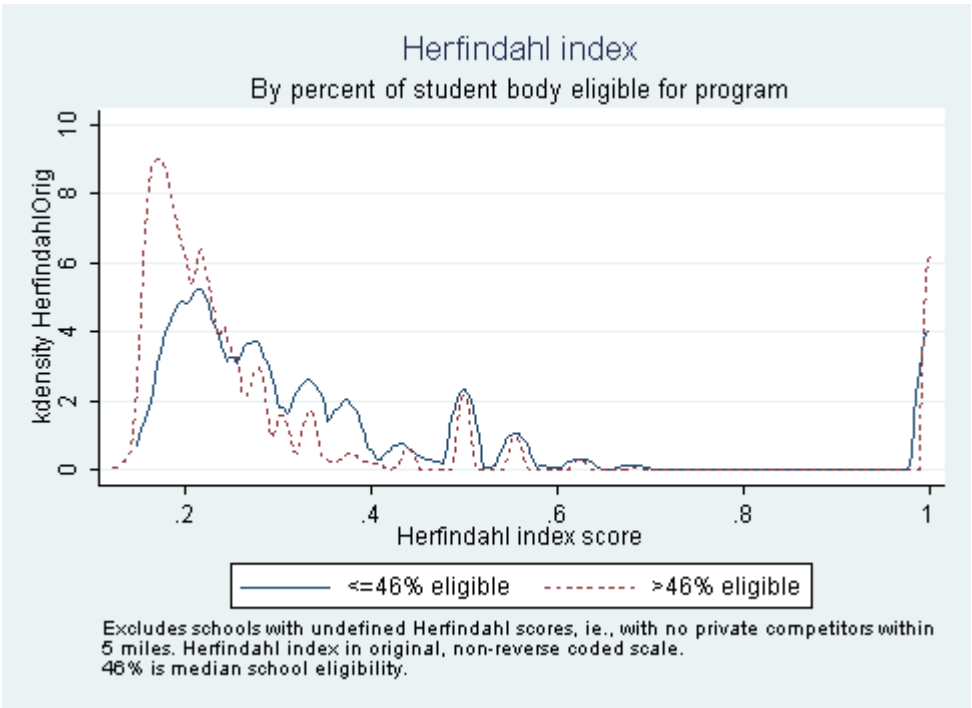


**Figure 5. Competition measures for Miami-Dade county**

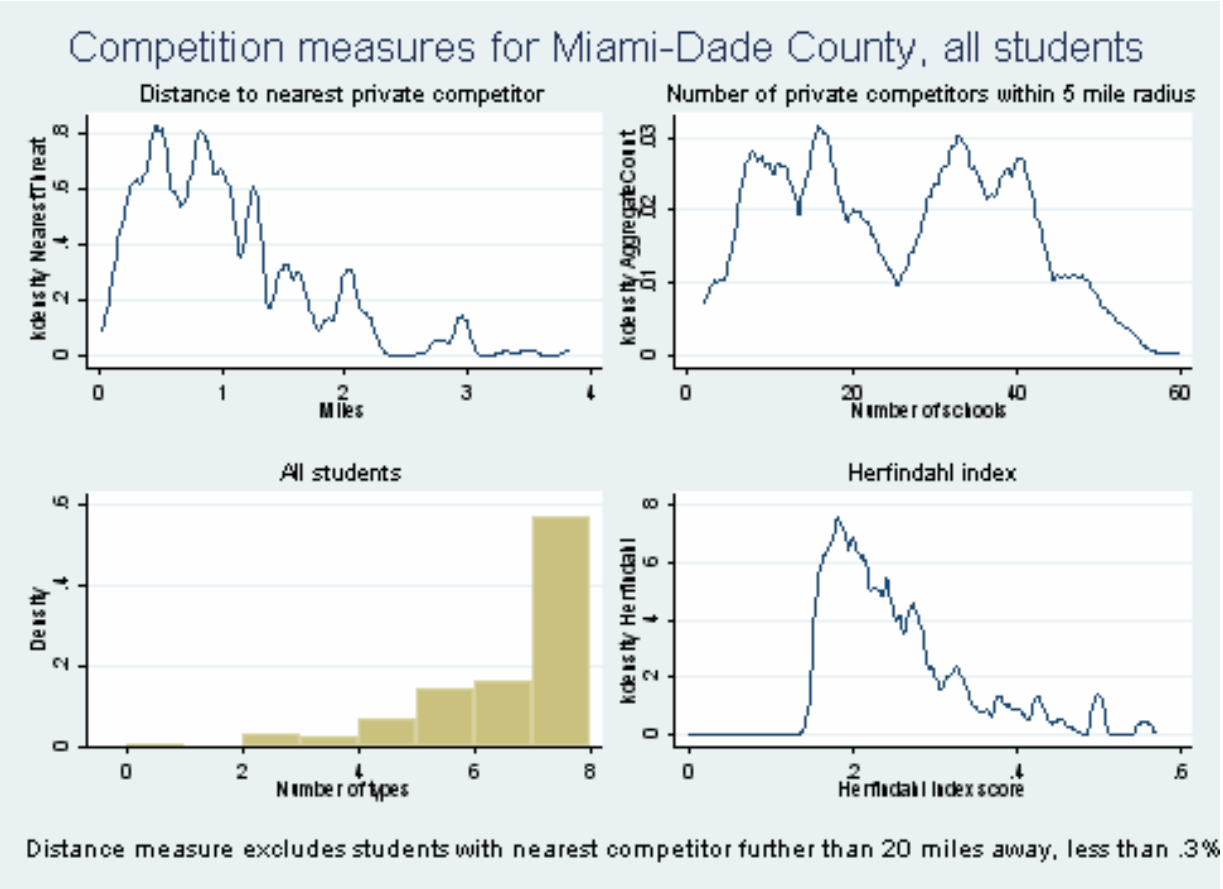


**Figure 6. Competition measures for four MSAs with highest degree of private school enrollment of low-income students in 2000 census**

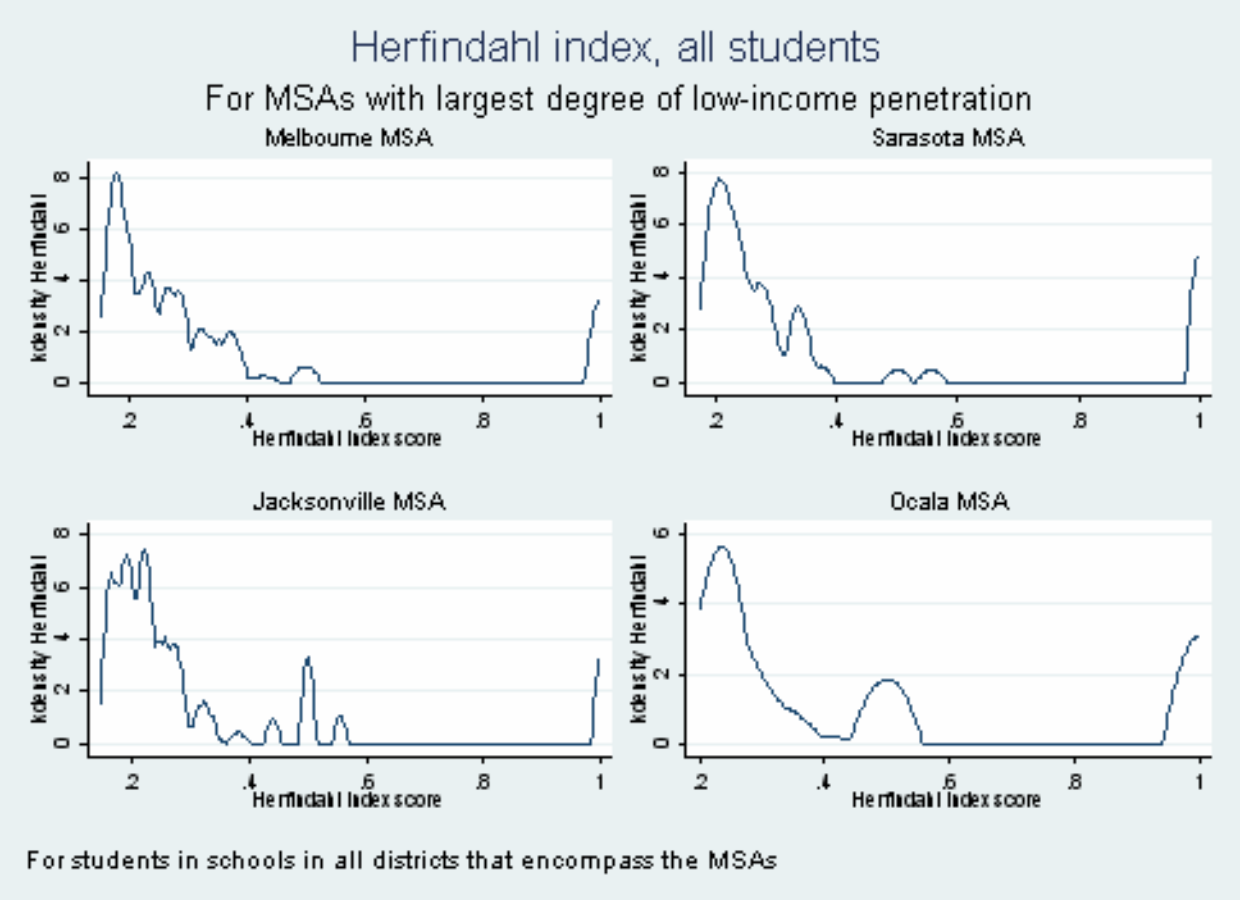


**Figure 7. Competition measures for four MSAs with smallest degree of private school enrollment of low-income students in 2000 census**

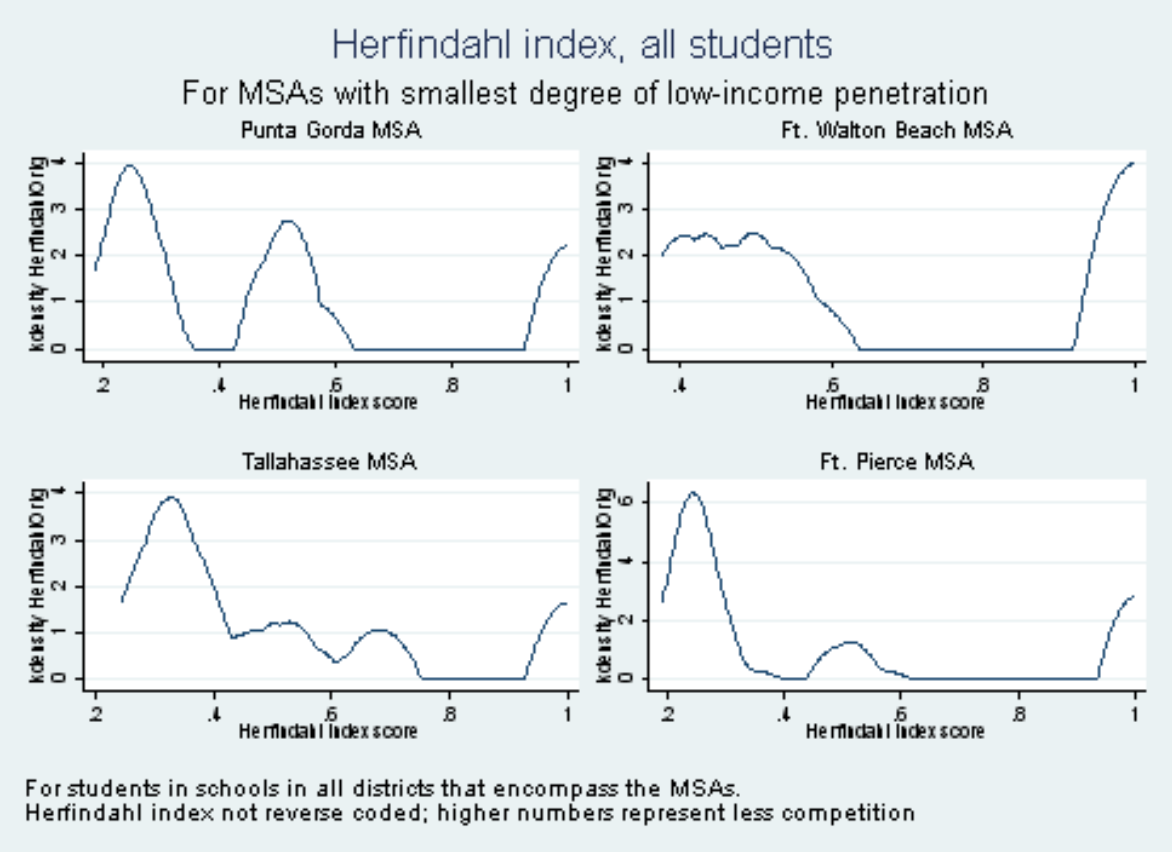


Figure 8: Relationship between distance to nearest competitor in 2000 and pre-policy test score trends, 1996-97 to 2000-01

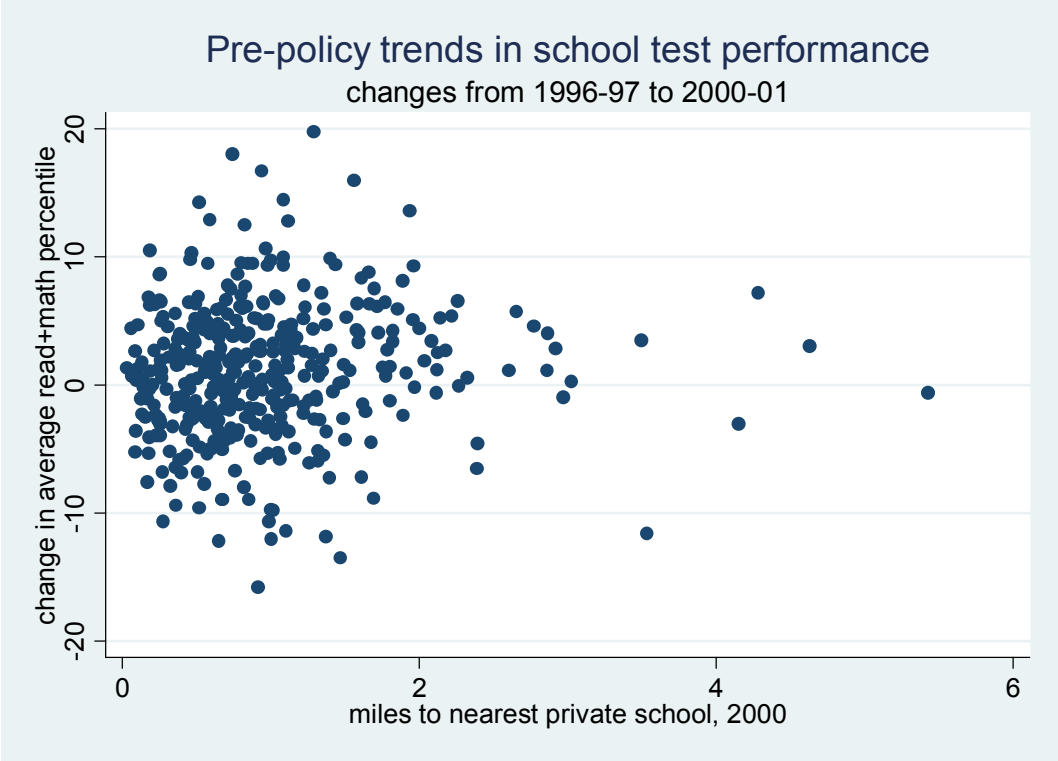


Figure 9: Relationship between number of nearby competitors in 2000 and pre-policy test score trends, 1996-97 to 2000-01

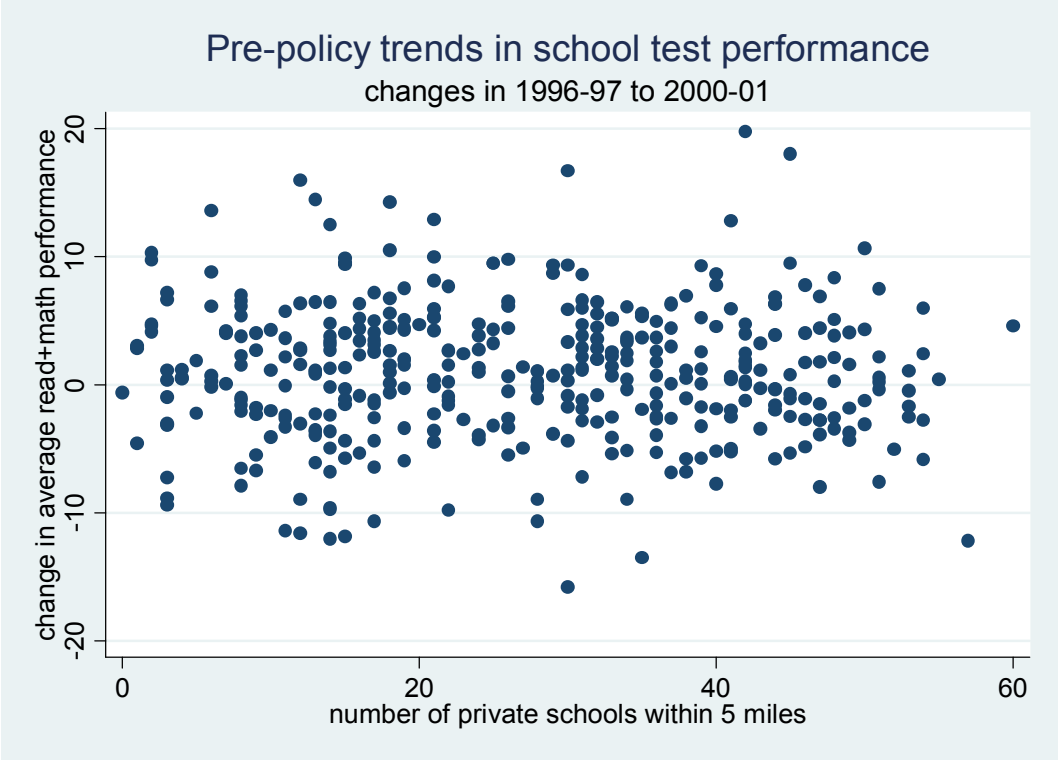


Figure 10: Relationship between number of local competitor types in 2000 and pre-policy test score trends, 1996-97 to 2000-01

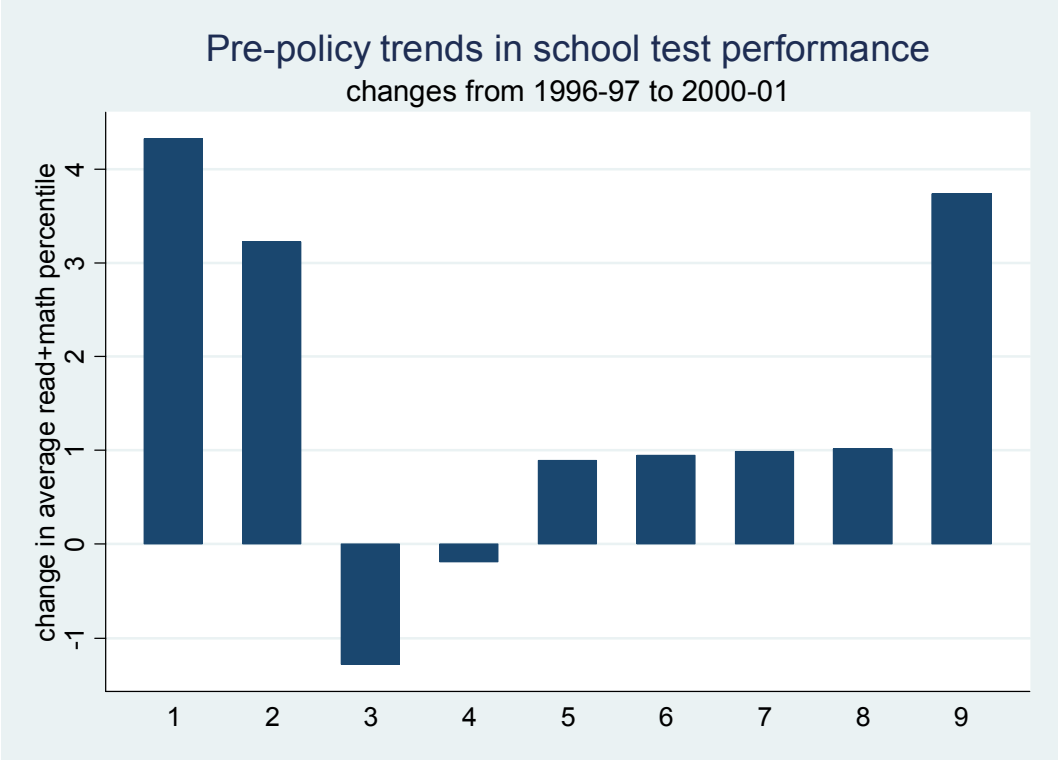


Figure 11: Relationship between distance to nearest competitor in 2000 and pre-policy test score trends, 1996-97 to 2000-01

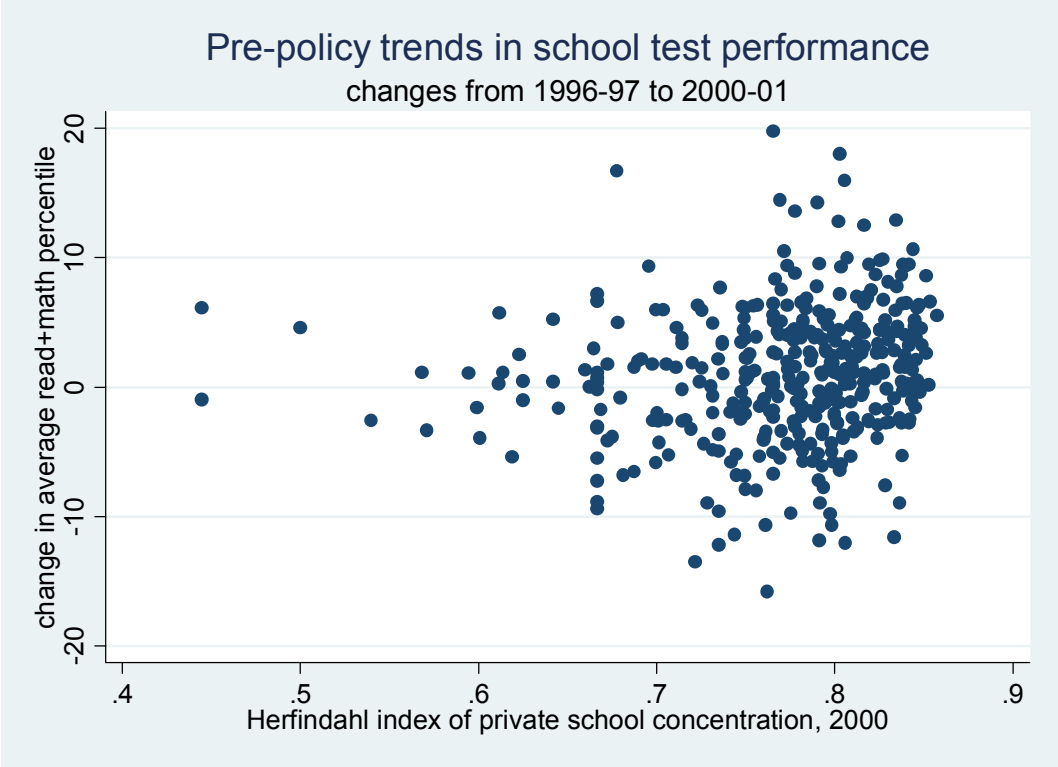


**Figure 12: Distribution of out-of-pocket expenses for voucher participants**

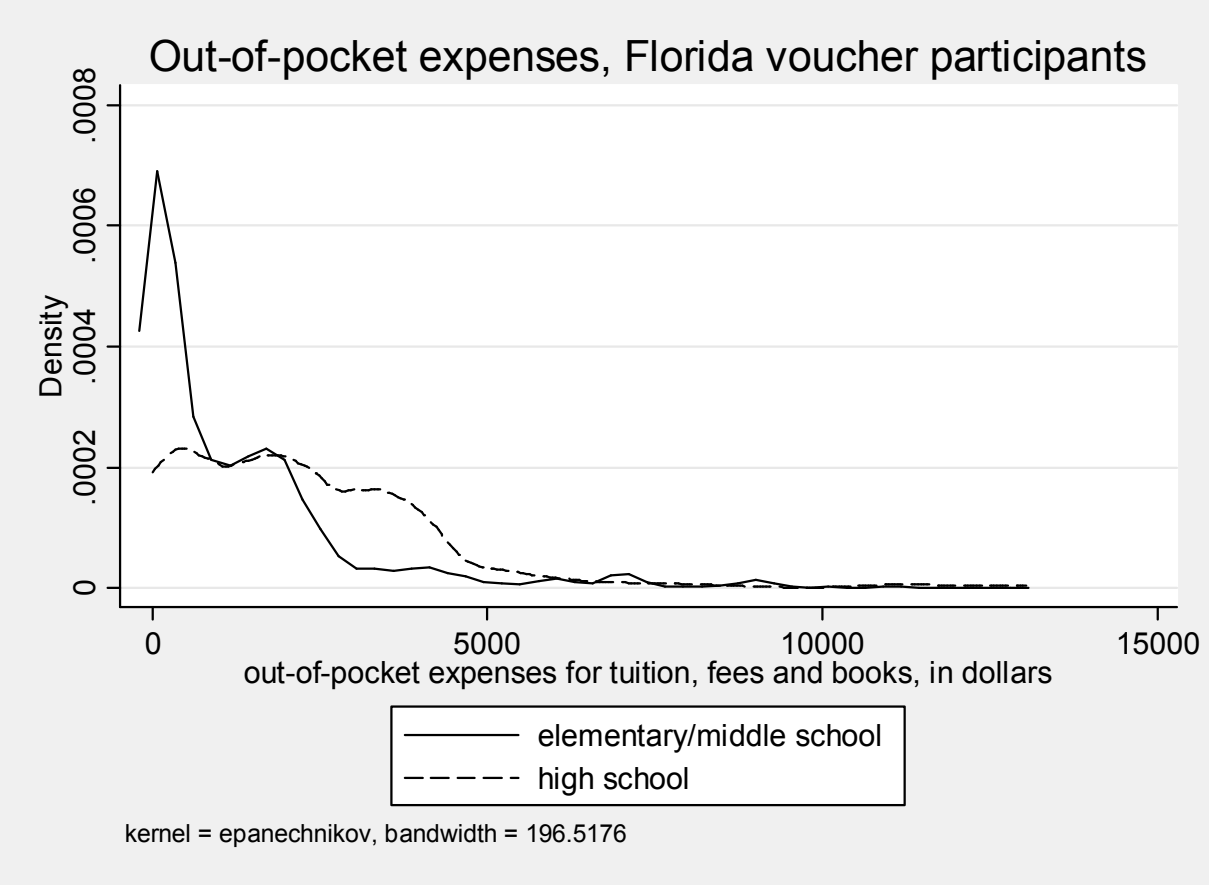


**Figure 13: Out-of-pocket expenses per voucher participant as a share of family income**