Competitive Effects of Means-Tested School Vouchers

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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) and new programs have been established or are under debate in a number of states and localities (Cavanagh, 2011). 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 communities that highly value

[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.

educational quality, then the presence of private schools could be positively 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 the Florida Tax Credit Scholarship Program (FTC) to directly study the competitive effects of school vouchers on student outcomes in public schools.

Our identification strategy exploits two differences across schools in the degree to which they might feel competitive pressure from the FTC program and the degree to which they might have incentive to respond to this pressure. We examine (1) 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; and (2) whether schools with a greater financial incentive to retain a small number of low-income students are the schools that respond the most. For the first source of variation, we make use of the introduction of the FTC program as this likely 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. Specifically, we use a difference-in-differences strategy to 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. For the second source of variation, we take advantage of program rules of the federal Title I program, in which extra financial aid to schools is based on a nonlinear function of the fraction low-income in the school. We argue that the schools near the threshold for receiving Title I aid are

the most likely to wish to retain low-income students to increase the likelihood that they qualify for this aid.

We find that public schools subject to more competitive pressure from private schools raise their test scores more following the introduction of Florida's voucher program, and the schools that face the greatest financial incentive to retain low-income students apparently raise their performance the most. Therefore, while the state caps the number of program participants at a small fraction of the overall student body, the program nonetheless appears to generate substantive public school responses.

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. West and Woessmann (2010) conduct a similar analysis using cross-country variation in private school penetration.[2] 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;

[2] 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 Gibbons, Machin and Silva (2008) in England. Hsieh and Urquiola (2006) study the effects of generalized competition in Chile, and Lavy (2009) examines effects of public school choice in Israel.

West and Woessmann, 2010). 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; West and Woessmann, 2010) 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 program analyzed in this paper, 12.08 percent of private schools operating in 2001 were Catholic. By contrast, 13.44 percent were Baptist; 8.58 percent were evangelical, 8.25 percent were reported as Christian with no further identifying information, and 13.93 percent 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 (for work in the United States, see Greene and Marsh, 2009; Chakrabarti, 2008; Hoxby, 2003; Chakrabarti, 2007; Chiang, 2009; Figlio and Rouse, 2006; Rouse et al 2007; West and Peterson, 2006. For work internationally, see Bohlmark and Lindahl, 2008; Gallego, 2006). Overall, this literature finds modest positive effects of vouchers on public schools, but there are often concerns about the identification strategies that these papers use. For instance, several studies have relied on changes in the degree of private school supply over time for identification of competitive effects (Greene and Marsh, 2009; Bohlmark and Lindahl). However, one might be

concerned that private school supply is endogenous to public school performance. Several other papers (e.g., Chakrabarti, 2007; Chakrabarti, 2008; 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. These papers confront the challenge of disentangling the competitive effects of school vouchers versus the performance effects of accountability pressure.

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. Similar to our results, Chan and McMillan 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 several key ways. First, we 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 initial value of the Ontario tuition tax credit was unlikely to attract low-income families, and so was unlikely to lead to a demand shock for that population. Since many programs in the United States are means-tested, it is important to study policies targeted towards this population. Second, Florida's

population is more dispersed 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 suggests that the Florida voucher should work by attracting new private school students rather than subsidizing existing private school students, which should pose a larger threat to public schools. Finally, and perhaps most importantly, we are able to differentiate between schools' incentive to respond to competitive pressure based on financial incentives to retain low-income students.

In addition to employing a stronger identification strategy than most past work and improving on the generalizability to a US context, our analysis makes two other important and unique contributions. First, we test whether responses to competition are stronger for certain types of schools that we argue should be particularly sensitive to the competitive threat posed by the voucher. We find that those schools that we expect to be especially sensitive to the program—schools on the margin of Title I receipt—do in fact respond more strongly to the increased threat of competition induced by the program.

Finally, by exploiting the timing of the roll-out of the program, which was announced spring of 2001 but did not enroll students in private schools until the 2002-2003 school year, we find cleaner estimates than have been obtained in past work of the effect of competitive threats per se—estimates that strip out other effects that vouchers may have on schools, such as changes in school resources or peer group composition due to students taking up vouchers. The fact that we find public school responses to the announcement of a voucher policy before any students actually use the voucher makes us confident that the program has a "true" competitive effect on schools. 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.

Florida Tax Credit Scholarships and the Private School Landscape

The FTC Program, originally called the Corporate Tax Credit Scholarship Program, was signed into law in 2001 and opened to students in the 2002-2003 school year. The program provides corporations with tax credits for donations that they make to scholarship funding 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). Table 1 presents a timeline of the important aspects of this program from the perspective of this analysis.

The program was originally capped to allow $50 million in contributions per year, and originally offered scholarships up to $3,500 for students attending private schools (Florida Statute 220.187, 2001), implying a limit of approximately 14,000 students in the first years of the program if all students received scholarships for the full authorized amount. In practice, this limit was slightly exceeded in the first year of the program, with 15,585 students enrolling (Florida Department of Education, 2009). The program has expanded in both scope and generosity of vouchers over time.

One might question whether a program of this size would have competitive effects on public schools since this represents a relatively small share of Florida's public school students (less than 1% of the overall population, and

between 1-2% of the income-eligible population in public schools as of the 2001-02 school year). However, educators may have been more conscious of the *existence* of a new voucher program than of the *size* of the program relative to the state population, and their responses may have reflected that. Additionally, educators did not know how popular the program would be within their schools, and may have overestimated the extent to which it was likely to affect them. Moreover, schools may have anticipated that the program would expand further in the future (as indeed it did), spurring them to respond even though the cap initially limited the program. At any rate, if educators did recognize that the program could only enroll a relatively small share of students statewide, this should depress any competitive effects associated with the scholarship and diminish the likelihood that we see results.

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 schools 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 (2001-2002); students could only apply for school vouchers during that year. Thus, while there may have been 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 percent microdata sample of the 2000 Census IPUMS (Table 2). Private school attendance was fairly widespread

overall; 11.2 percent of Florida students aged 6-17 attended private schools. Unsurprisingly, given the resource constraints of low-income students, private school attendance rates were lower for this group. Among students in income groups that would become eligible for the FTC program, only 5.4 percent attended private schools on the eve of the voucher.

One can see from Table 2 that metropolitan areas of Florida had very different levels of private school penetration, as well as different degrees to which low-income students participated in private schooling prior to the introduction of the program.[3] The share of low-income students attending private schools varied widely in different metropolitan areas, ranging from 1.4 percent attendance in Punta Gorda to 7.9 percent attendance in the Melbourne-Titusville-Cocoa-Palm Bay area. Similar-sized metropolitan areas had 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. A major reason why private school concentration varies so dramatically within an area involves the physical features of Florida, where the vast majority of the state lives close to the Atlantic Ocean or the Gulf of Mexico, and where the limits of development of many of the largest metropolitan areas are defined by an ocean on one side and the Everglades (or other undevelopable land) on the other. Schools that are located near the Everglades or the Atlantic will naturally face less competition from nearby schools because much of the area nearby is covered by

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

water or swampland. This difference in penetration of private schools provides us with 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.

Data and Methods

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, as well as 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 eligible for subsidized lunch. Identifying schools that serve 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 inferred 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 percent of private schools; locations for the remaining 15 percent were imputed using centroids of the school's zip code. We used latitude and longitude data as the primary method to locate the public schools, supplementing this with geocoding based on physical addresses where necessary. The geocoding process generated valid data for public schools representing nearly 98 percent of the student body population, 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 1998-99 school year through the 2006-07 school year.[4] We also present additional evidence using a longer panel of older data to evaluate the exogeneity of our measure of private school competition for public schools.

Students classified as 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 to the FTC program who were disabled and therefore eligible for a McKay Scholarship were directed to that program instead.

While we exclude disabled students who are unlikely to apply to the program, we retain students who are income-ineligible in our sample. While teachers may make some changes at the margin to specifically tailor instruction to income-eligible students, it may be logistically challenging to target instruction specifically to eligible students without stigmatizing them (e.g., through pull-out instruction or tutoring programs). We therefore suspect that it is more likely that

[4] Florida first assigned school grades under its accountability system following the 1998-99 academic year, so one might believe that it would be best to use only data following the introduction of the accountability system. In previous versions of this paper we have restricted our analysis to the most-comparable period from 1999-2000 onward, and found results that are highly similar to those presented herein. We therefore believe that using all available data -- so that we can have three years of pre-program data rather than only two years -- has advantages that outweigh the disadvantages of there being a changing context in the years prior to the policy's introduction. Other concerns that the accountability program may create a threat to validity are addressed later in the paper.

the types of adaptations that schools might make to boost school scores overall (e.g., modifying instruction, boosting test prep, changing curricula to more closely align with tests, etc.) would be classroom-wide changes that would affect ineligible as well as eligible students.[5] The full dataset includes 9,438,191 potential student-year observations, observed over the 1998-99 to 2006-07 school years, for a total of 2,787,158 students. We restrict our analysis to schools with a private school within five miles, which modestly shrinks our analysis dataset to 9,026,689 student-year observations. We cluster all of our standard errors at the school level; there are 2,592 school clusters in our data.

Measures: Our dependent measure is the average of a student's developmental scale test scores on the math and reading sections of the Florida Comprehensive Achievement Test (or, for 1998-99, the student's scale score on the FCAT). The FCAT is a criterion-referenced test administered in grades 3-10 and used for school and student accountability. We average reading and math scores of students for the purposes of expositional parsimony; the results of all models are consistent when we estimate them using only reading scores or only math scores. To ease interpretation and to facilitate comparisons across different versions of the FCAT, the test scores are standardized at the grade level.[6]

[5] Note that special education students might benefit from improved instruction affecting all students in the schools, if they were mainstreamed. However, because many students with exceptionalities are pulled out from mainstream instruction, they are less likely to see benefits from this program than are other students, including both eligible students that educators may be specifically hoping to target and income-ineligible students who share the same classes with them.

[6] The scores are standardized using student-level standard deviations. This is appropriate since the analyses presented here are all conducted at the student level, but it also produces relatively conservative effects for robustness tests where we use school-level mean scores as the dependent variable, because standard deviations of school mean scores are smaller than standard deviations of student level scores. The average of the standardized reading and standardized math score reported in the summary statistics in Table 3 is not exactly zero because the reading and math scores are standardized separately for the statewide population, and our study population is slightly different from the full set of potential observations.

We use five different types of 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 to bolster confidence that our results are not due to a fortuitous choice of competition measure. First, we measure the crows-flight distance between the physical addresses of each public school and the nearest private competitor in existence prior to the announcement of the voucher program. 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. The distance measure is multiplied by -1 so that a positive coefficient represents a closer competitor having a positive effect on test scores. We find that, as Florida's population is heavily urban, the vast majority (92.4 percent) of public schools have a private school within five miles (see Figure 1). Therefore, as mentioned above, we restrict our analysis in this paper to schools with at least one private school within five miles.[7]

Our second type of measure involves investigating the number and variety of private schools that were operating prior to the announcement of the voucher program in close proximity to the public school in question. We consider two variations on this theme: One variation, which we term the "density" measure of competition, is a simple count of the number of private competitors within a five mile radius of the public school ("local" private competitors). As an alternative, which we call the "diversity" measure of competition, we consider the number of distinct *types* of local private schools. This competition measure is intended to capture the variety of proximate private school options. A type is defined by religious affiliation; schools self-identify as to their affiliation when reporting to

[7] The results are broadly similar if we relax this restriction.

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.[8]

These measures of competition are based on counts of private schools, and weight large and small schools equally. We prefer count-based measures of competition because we believe that it is more plausible for public school educators to know whether there are private schools nearby than how large or small those private schools are, or how many potential slots they have for voucher program participants. However, we also report the results of models in which we measure the total private school enrollment within a five mile radius of the public school, standardized based on the number of grades served. We call this competition measure the "slots per grade" definition of competition.

A final type of competition measure does not focus on the existence of private schools at all, but rather on the number of churches, synagogues and mosques located within five miles of the public school. This competition measure, which we call "churches nearby," captures two facts that may affect the degree of competitive pressure on schools. First, houses of worship are well-positioned to start schools in their existing buildings. Second, the density of churches may capture the underlying religiosity of a community, and therefore the latent demand for religious schooling. Indeed, since ex-post we observe that the overwhelming majority of students participating in the voucher program attend religious private schools (Figlio, Hart and Metzger, 2010), it is reasonable to

[8] We have also considered a measure of competition that measures the concentration of these ten different competitor types using a Herfindahl index of private school competitor types. The results of these models follow the same patterns of signs, magnitudes and statistical significance as those referred to in this version of the paper.

believe that public schools with many nearby houses of worship may feel more potential competitive threat than do those with fewer nearby houses of worship. In summary, while all of these measures of competition are variations on the same basic theme, and all of them could be correlated with other local attributes, they present a variety of signals of potential competition to which public school personnel might respond.

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 (grades four, eight and ten in reading, and five, eight and ten in math); beginning in 2000-01 Florida began to test students in every grade from three through ten.[9] Therefore, we do not observe prior test scores for all students in our analysis. For this reason, and because we would lose pre-policy years 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 norm-referenced test scores (observed beginning in 1999-2000) or lagged FCAT scores. The results that we present are substantively similar to, although generally somewhat weaker than, the results (available on request) 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 also controlled for some time-varying characteristics of schools that may affect the degree of competitive pressure they feel. Specifically, we

[9] Students in previously-untested grades received field tests of the FCAT in 1999-2000, but these results were not used for accountability purposes (Florida Department of Education, 2011) and we do not have access to these scores. Our results are consistent if we restrict our analysis to just the grades and subjects consistently observed over the entire study period.

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 year dummies were included to account for time trends in scores.

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:

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

where Y_{ist} represents the average of the standardized math and reading scores for student *i* in school *s* in year *t*; α_s represents a fixed effect for school *s*; C_s represents the measures of pre-policy competition faced by school *s*; P_t is an indicator for whether year *t* is post-policy implementation,[10] $\vec{X}_{it}$ 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*; $\vec{S}_{st}$ is a vector of time-varying school characteristics, including the school's prior-year grade assigned by the Florida Department of Education and the share of students in the school eligible for the program; T is a series of year dummies; and ε 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

[10] 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.

the first year of the program—before any students have left the public schools but following the program's announcement—as well as models 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 paper 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. We take up this consideration later in the paper.

We report robust standard errors, clustered at the school level, in our regression results.

Descriptive statistics

Table 3 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 1.35 miles. 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 15.37 private competitors within a five mile radius, representing an average of 5.22 different types of religious (or secular) affiliations. The typical public school has 305 potential slots per grade in surrounding private schools, and has 151 houses of worship within a five mile radius. The degree of variation in these measures is somewhat unsurprising since our data draw from the entire state. However, even within metropolitan areas, we see a high degree of variation in competition measures. Table A1 in the appendix presents means and standard deviations on all five competition measures for the eight most populous districts in the state.

It is useful to know the degree to which our competition measures are correlated with other attributes of the public schools and their student bodies. Table 4 presents simple correlations of the relationships between these measures and school characteristics. Public schools with higher measures of local competition tend to be in more urban settings. They serve populations that are more heavily minority, with larger fractions of students in poverty and English language learners. They also tended to score lower on Florida's school grading system in 2001, a fact that is unsurprising given the populations served since Florida's initial school grading system was based nearly entirely on average test score levels and school grades were therefore highly correlated with student demographics. These correlations are very strong, and are particularly pronounced in the case of our house of worship-based measure of competition. The fact that schools facing higher degrees of competition serve substantially different populations than do schools facing lower degrees of competition highlights the importance of estimating school fixed effects models to observe whether a substantial differential change in performance occurred following the policy announcement in high-competition areas, of evaluating trends in school performance prior to the voucher program's announcement, and of exploiting programmatic rules to aid in the identification of causal estimates of the effects of voucher competition.

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 (2010) find that the voucher program led to negative selection the private schools, indicating that the ability levels of those remaining in the public sector are higher than before. In the presence of peer effects, this could mean that part of a positive 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 subsidized lunches could result in resource reductions that affect student outcomes 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 (the 2001-02 school year), 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. [11] We therefore begin with school fixed effects estimates of the effects of

[11] It is possible that the results could be driven by low-income students working harder in the "pure competition" year to help to attain a place in a private school. However, since most private schools do not have high academic criteria for admission, it is unlikely that this is the driving factor. Moreover, we find little evidence of differential effects for low-income versus higher-income students, indicating that our estimates are best considered to be generalized performance effects of the program. We also observe little evidence of strategic entry into the public schools of families seeking to "game the system" by attending public schools for a single year. Among students observed selecting out of the public schools to attend a private school using the voucher program, only about five percent had spent just one year in Florida prior to using the voucher. Given that some of these students may have come from out of state, this is an upper bound of the fraction of students strategically selecting into public schools in the year before students could leave the public schools using a voucher.

competition on student performance using post-policy data only from 2001-02 rather than all post-policy years.

The results of this first analysis are reported in the first column of Table 5. Each cell represents the coefficient on the Post-policy x Competition interaction for separate regressions that use each of the five measures of pre-policy competition in turn. As can be seen in the table, all five measures of competition are positively and significantly related to student performance. Every mile the nearest private school moves closer, public school student test score performance in the post-policy period increases by 0.015 of a standard deviation.[12] Adding ten nearby private schools (just shy of a standard deviation increase in this measure) increases test scores by 0.021 of a standard deviation. Each additional type of nearby private school is associated with an increase of 0.008 of a standard deviation. Adding an additional 100 churches in a five mile radius (a nearly one standard deviation increase) is associated with a .02 standard deviation rise in scores, and adding an additional 300 slots in each grade level in a five mile radius (just over a one standard deviation increase in this measure) increases scores by .03 standard deviations. Overall, a one standard deviation increase in a given measure of competition is associated with an increase of approximately 0.015 to 0.027 standard deviations in 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.[13][14] These results provide a first piece of evidence that public schools

[12] Coefficients in the table are multiplied by 100 to ease the interpretation of very small effects.
[13] 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.

responded to the *threat* of losing students to the private schools via the voucher program.

We posited that there may be a non-linear relationship between distance to nearest competitor and test score responses. We therefore investigated how sensitive schools are to varying levels of distances to their nearest competitors by categorizing the distance measure into quarter-mile bins. Figure 2 presents the estimated post-announcement effects for each of these groups, along with 95 percent confidence bands. As can be seen in the figure, the farther away the nearest private competitor, the smaller the estimated effect of the voucher program announcement. These results indicate that the strongest effects are experienced when the nearest private school is less than about two miles from the public school.

Given these results, we repeat our analyses, now estimating the effects of voucher competition measured at the five mile radius level as well as the *additional* competitive effect of those schools within two miles of the public school. The results of these specifications are reported in the second and third columns of Table 5. As can be seen in the table, there is limited evidence that the effects of competitors within two miles are substantially larger than those of competitors farther away, as there are economically meaningful differences between the two-mile and five-mile definitions of competition for only two of the four (non-distance) competition measures, and the difference is only statistically significant at conventional levels for one of these. Therefore, although there is substantial evidence that schools respond the most when there is a private school

[14] Another concern is that, in this data set as in many others that use developmental scale scores to measure achievement, there is more compression of test scores in the higher grades (see, e.g., Ballou, 2009 for a discussion of this problem). We address this in two ways. First, we check these results using state scale scores provided in the Florida data; these scores suffer somewhat less from compression of variance at higher grades, and produce similar results. In addition, we examine whether the results differ by grade level in follow-on analyses; these results are reported in the paper.

very nearby, the interaction between this distance and the other competition measures is not particularly powerful. Throughout the remainder of the paper, we present results for the five-mile radius measure of private competition.

It may be that elementary and middle schools have a greater incentive to respond to potential competition than would high schools. Nationwide, private high schools are more expensive than are elementary schools (National Center for Education Statistics, 2009). Because the voucher is capped at the same amount regardless of students' grade level, this implies that, on average, families will have to pay more out of pocket for high school students than for students in lower grades. Data on the actual out-of-pocket costs paid by families for tuition, fees and books for students who receive vouchers from the two Scholarship Funding Organizations that serve the southern half of Florida (Florida P.R.I.D.E. and the Carrie Meek Foundation) confirm this intuition. While the typical out-of-pocket expense for elementary and middle school students is about ten percent of tuition and fees, it is three times that for high school students participating in the program. 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. Public high schools, knowing this, might be less motivated to respond to competitive pressure. Of course, it could also be that high schools, with a much more diffuse mission than primary schools, might have responded less than would primary schools for other reasons as well. Nonetheless, it is important to investigate the degree to which estimated competitive responses vary by school level.

Table 6 presents the estimated effects of increased private school competition through the school voucher on average public school test scores, broken down by elementary and middle schools versus high schools. There is limited evidence that elementary schools are more responsive than are high

schools: While their point estimates are meaningfully larger, they are not statistically distinct at conventional levels in most cases. Therefore, while public elementary and middle schools appear to be somewhat more responsive than high schools, we cannot rule out the possibility that their responses were similar. In the rest of the paper, we will report the results that pool together all school levels; these results are more conservative than the results that we find when we limit our analysis to the elementary and middle schools only.

Threats to validity

One potential concern is that results may be driven by particular districts that house a large proportion of the students in the state. We therefore estimate the main analysis presented in Table 5 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. However, 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 magnitude of the estimated effects of voucher competition effects fall by between 10 and 20 percent, though they 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: 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.

A second serious 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 trends were related prior to the introduction of the policy.

One test for whether there were differences in student performance trends based on the strength of competition prior to the introduction of the FTC scholarship program involves estimating models that include interactions of the competition measures with year dummies for the two lead years before of the policy announcement (the 1999-2000 and 2000-2001 school years). Coefficients on all *Year x Competition* interaction terms are then interpreted in relation to the omitted interaction between 1998-99 and the competition measures. If schools with nearby private schools were improving over time, one would expect to observe positive and increasing coefficients on these policy lead variables.

However, as seen in Table 7, there is very little evidence of a positive trend in school performance in the lead years preceding the voucher introduction. The coefficients on the 2001-02 *x Competition* interactions are significant and positive for all measures of competition (Column 1), echoing the results presented in Table 5. However, the coefficients for the 2000-01 x *Competition* interactions (Column 2) and 1999-2000 x *Competition* interactions (Column 3) are generally not significantly different from zero. The only exception involves our measure of potential competition based on nearby churches; in this measure, the coefficient on the first lead of the program is positive and statistically significant. That said,

even there the gap in coefficients between the two-year lead and the one-year lead is only about one-third the size of the gap in coefficients between the one-year lead and the first year of the policy. Therefore, even in this case, there is evidence to suggest that the voucher program at least accelerates a trend toward increasing test scores in areas with greater degrees of pre-policy competition.

Because Florida did not collect a long panel of statewide data prior to the policy introduction, we were still concerned that public schools with more competitive private school landscapes in 2000 may have been on a different growth trajectory prior to the policy's introduction. We therefore drew on a different data source to investigate the potential presence of longer trends in public school performance. Prior to 2001, 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 most populous 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 five years prior to the policy's introduction.[15] While unfortunately these testing programs had ended before the first year of the voucher program, making it impossible to directly compare the five-year pre-policy period to the first year of potential competition, it is still possible to observe whether there exist any pre-period trends across schools based on their measures of competitive pressure. There is no apparent relationship between the level of private school competition present in 2000 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.

[15] These three counties yield results that are roughly representative of the rest of the state on the regressions reported in Table 6, although the estimated effects of voucher competition are modestly larger in Miami-Dade County than in the state as a whole.

Moreover, year-by-year comparisons for these districts demonstrate that there was no general trend of improvement in the schools with more local private school competitors. Results of school fixed effect regressions of school-year average test scores in the three school districts on year dummies and year-specific leads of the competition measures indicate that there is no consistent pattern in the relationship between 2000 levels of competition measures and the leads of the policy over the longer time horizon, and that the magnitudes of the estimated coefficients are extremely small.[16] 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.

Finally, one might be concerned that other policy innovations besides the voucher program may be driving these results. Most obviously, the national No Child Left Behind Act was under discussion at the same time that the FTC program was passed, and NCLB was passed in January 2002, during the year we argue schools were exposed to "pure competitive effects" from the FTC program. Since schools with greater competition were lower-performing, on average, than schools that faced less competition, one might be concerned that this legislation, which was intended to put pressure on low-performing schools, may be driving these results to some extent.

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[16] The dependent variable in these comparisons is the average reading plus math *national percentile rank* in the school, the only measure that is directly comparable across school districts using these more historical data. For ease of interpretation, we can express the estimated effects on national percentile ranks to effect sizes: In the case of distance, the estimated effect sizes, relative to 1996-97, for 1997-98, 1998-99, 1999-2000 and 2000-01 are 0.002, 0.001, -0.002 and -0.000, respectively (with standard errors of 0.004.) For the density measure, the estimated effect sizes are 0.001, 0.000, -0.002 and -0.001 (with standard errors of 0.003.) For the diversity measure, the estimated effect sizes are -0.004, -0.004, -0.004 and -0.002 (with standard errors of 0.004.) For the slots measure, the estimated effect sizes are -0.001, 0.002, -0.004 and 0.001 (with standard errors of 0.003.) For the churches measure, the estimated effect sizes are 0.003, 0.001, 0.004 and 0.009 (with standard errors of 0.004.) As with the statewide measure, the only statistically significant lead term is the 2000-01 lead for the churches measure of competition.

However, this is unlikely because Florida had a separate, comparably stringent accountability law in place prior to NCLB, the A+ Accountability Plan. Schools had received publicized grades from the Department of Education under the A+ Plan since the summer of 1999 (see Table 1), and students at persistently failing schools were eligible for scholarships to public or private schools. Specifically, the A+ Plan offered vouchers to students in schools that received two F designations within four years. However, in the first three years of the program (Summer 1999-Summer 2001, the grades that were available to schools through the periods covered by our main results), only 74 of Florida's approximately 2300 schools had received even one F grade (Rouse, Hannaway, Goldhaber & Figlio, 2007). This suggests that relatively few schools felt a pronounced threat of vouchers from the A+ Plan in the time period covered by our main results.

Moreover, the timing of the results suggests that accountability pressures do not drive results. For accountability policies to confound our results, we would need to see some distinct policy change in the 2001-2002 school-year that heightened the salience of nearby competitors for public schools. The sole change during the 2001-02 school-year of which we are aware—a change to the formula used to calculate grades—was not fully unveiled to schools until the midst of the testing period (Rouse, Hannaway, Goldhaber, & Figlio, 2007), making it relatively unlikely that this change accounts for the results.[17] Moreover, it is not clear that this change should have elicited disproportionate reactions from schools facing a greater number of competitors nearby, as would have to be the case to account for our results.

Finally, additional analyses suggest that even when we include interactions with schools' summer 2000 grades from the Department of

[17] Prior to that, they knew only a few general parts of the plan, such as that students in all grades were to be tested and that standards were to be raised somewhat (Rouse et al., 2007).

Education, the main results hold. There is a significant marginal effect for students in "F" schools; however, given that only four schools received "F" grades in 2000, these results are relatively unstable. There are no other interaction effects for any other prior school grade ("A" schools constituted the reference group).

Differential Estimated Effects by Incentives to Respond

Not all public schools face the same incentives to respond to competitive pressure from the FTC program. While all public schools may experience resource effects as a consequence of losing students to private schools on the voucher, no schools have as large of an incentive to retain free or reduced-price lunch eligible students as those on the margin of receiving federal Title I aid. These federal resources, which average more than $500 per pupil, are directed to school districts, which then allocate them to the elementary and middle schools attended by low-income students.[18]

Not every public school with low-income students receives Title I aid.[19] In 2001-02, 61 percent of elementary schools and 31 percent of middle schools statewide received Title I aid. Title I aid is allocated based on where schools rank within the school district with respect to concentration of low-income students; the highest-poverty schools receive Title I aid while the lower-poverty schools do

[18] 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.

[19] While we treat Title I as a unitary program in the remainder of the paper, it is worth noting that there are two types of Title I schools. The first is "Schoolwide" Title I schools, where the Title I aid is not required to follow individual students per sé 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 second type is "Targeted assistance" Title I schools, where the school's Title I funds must be spent directly on the low-income students. In either case, there is a large discrete jump in funding for a school that comes with Title I school status. In Florida, the overwhelming majority (92 percent) of Title I schools are considered schoolwide Title I schools. By contrast, just over half of all Title I schools were schoolwide Title I schools for the nation as a whole in 2001-02.

not, and the poverty threshold that generates Title I funding varies from district to district. In some school districts (generally very small, rural districts), all elementary or middle schools are Title I schools. In other school districts, Title I funding is limited only to elementary schools.

Title I funding, and the number of schools receiving Title I aid in Florida, began rising every year starting in 1999, and schools that were just below the 2001-02 cutoff for Title I aid were likely to believe that they stood a good chance of receiving Title I aid 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, and while the precise Title I cutoffs for 2002-03 were not known to schools, they were known to a first approximation by schools during the 2001-02 school year.

We seek to identify the effects of voucher competition for schools on the margin of Title I receipt. Because schools in 2001-02 had reasonably but not completely precise information about the school district's threshold for Title I receipt in 2002-03 and also were unsure of what their fraction of eligible students would be in 2002-03 -- this is a highly mobile population so it's hard to know exactly how many students would leave or come into the school from year to year -- we cannot conduct a regression-discontinuity design for likely Title I receipt. However, we can flexibly estimate the effects of competition in the vicinity of the Title I thresholds to see whether schools most likely to be on the bubble of Title I receipt behaved differently from other schools. We operationalize this by identifying schools in three basic groups: (1) those who were receiving Title I aid in 2001-02 and would continue to receive aid in 2002-03 so long as their low-income percentage did not fall by much; (2) those who were not receiving Title I funding in 2001-02 but *would* due to Title I expansion in 2002-03 so long as their low-income percentage did not fall; and (3) those who would not be predicted to

receive Title I aid in 2002-03 but might if their low-income percentage increased by a small amount. Schools in group 2 are in our "focal range". We define this focal range as those schools with 2001-02 shares of students on subsidized lunch that fell between two policy thresholds: the 2001-02 Title I eligibility cutoff and the realized 2002-03 Title I eligibility cutoff. We would expect that the schools most likely to respond to competitive pressure will be the schools in the focal range—the schools who would stand to lose Title I funding if only a few low-income students left the school—or those in the areas just adjacent to this range.

In Table 8, we present the estimated effects of increased competition for eight groups of schools based on their 2001-02 share of low-income students: those with percentage low-income more than 10 percent below the bottom of the focal range; those 5-10 percentage points below the bottom; those 0-5 percentage points below the bottom; those in the lower half (below the midpoint) of the focal range; those in the upper half (above the midpoint) of the focal range; those 0-5 percentage points above the top of the focal range; those 5-10 percentage points above the top; and those more than 10 percentage points above the top of the focal range. The precise patterns of results vary across the five competition measures: In the density and diversity measures of competition, schools across the spectrum improved with more competitive pressure; in others, schools with high percentages of low-income students experienced bigger gains than those with low percentages of low-income students; and in others the estimated improvement is more concentrated. However, in all five measures of voucher competition, the largest estimated gains are observed in the "focal range" groups of schools. Therefore, the schools with the most to lose financially when they lose low-income students appear to have responded the most to the voucher competition aimed at low-income students. These results are consistent with a story that voucher competition is responsible for the gains that we attribute to the FTC program.

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. Table 9 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.[20] These results show 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. However, past work has shown that students who participate in the program are lower achieving on average than their peers in the same school (Figlio, Hart, and Metzger, 2010), suggesting that composition effects may be at play. The loss of these low-achieving students over time may magnify the "effects" of competition over time.

Discussion

We find that the increased competitive pressure faced by public schools associated with the introduction of Florida's FTC Scholarship Program led to modest general improvements in public school performance. 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 are more pronounced in the schools most at risk to lose students; specifically, schools on the margin of Title I funding were

[20] In these models, we do not control for lagged measures of school performance because changes in school performance associated with the voucher program would be embedded within these variables. Therefore, the coefficients on the leads of the policy measure and the first year estimates of policy effects differ from those reported in Table 6.

the ones that appear to have been particularly responsive to voucher competition. The fact that we observed generalized improvements in school performance in response to the competitive threats of school vouchers, even in a state with rapid population growth, suggests that voucher competition may have effects elsewhere.

That said, our study has several limitations. First, our measures of competition reflect the state of the private school market in 2000, before private schools had a chance to respond to the FTC scholarship program. Although this ensures that the competition measure is exogenous 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. The dynamics between competitive pressures and public students' test scores may be 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 find that rural schools with nearby private alternatives respond similarly to urban and suburban schools with similar levels of measured competition.) It may also be that Florida's diverse range of private school options provides Florida with a larger amount of private school competition relative to other states. To the extent that this is true, it limits the study's generalizability. In addition, Florida's large county-level school districts mean that Florida public schools face less Tiebout competition than do those in other states; perhaps schools with more public school competition would

respond less to the introduction of a voucher program. (However, in earlier versions of this paper we investigated whether schools in districts with more charter school penetration responded less to the voucher program and found no evidence of that.) Nonetheless, this study indicates that private school competition induced by scholarships aimed at low-income families could have positive effects on the performance of traditional public schools.

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Table 1. Timeline of events regarding Florida's Corporate Tax Credit Scholarship Program and relevant other dates

Spring 1999	First administration and public reporting of FCAT scores
Summer 1999	Public schools graded from "A" through "F" by Florida Department of Education
Spring 2000	Private school data used to generate competition measures for this paper
Spring 2001	Corporate Tax Credit Scholarship Program conceived, signed into law
2001-2002	Families with existing public school students, and those with students entering kindergarten or first grade in 2002-03, apply to use vouchers for 2002-03
Summer 2002	New public school grading regime implemented by Florida Department of Education
Fall 2002	First voucher students enroll in private schools

Table 2. Private school shares of total Florida student population aged 6-17, from 2000 Census

	Share of state student population	percent students in private schools	percent students below 185% of poverty in private 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 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, including 50,000 with family income below 185% of the poverty line.

Table 3. Descriptive statistics for students represented in 1998-99 through 2001-2002 school years and in schools with at least one private competitor within a five mile radius

	Mean	Standard deviation
Test performance		
State scale average reading+math score	-0.01	0.95
Competition measures		
Miles to nearest private school competitor	1.35	1.06
Number of local private schools within 5 miles	15.37	12.64
Number of denominational types represented in 5 mile radius	5.22	2.29
Number of private school slots per grade within 5 miles	305	295
Number of churches, synagogues and mosques within 5 miles	151	118
Demographic measures		
Black	.24	
Hispanic	.19	
Asian	.02	
White	.53	
Other race	.01	
Male	.48	
English language learner	.16	
Free lunch eligible	.35	
Reduced lunch eligible	.10	
Observations	3,103,993 (2,264 schools)	

Notes: Data from the Florida Education Data Warehouse, the Florida Department of Education's Florida School Indicators Reports, and the Florida Department of Education. Means include only children in schools with at least one local competitor (92.4% of the potential sample).

Table 4. Correlations between pre-policy measures of competition and school-level attributes

School attribute	Correlation with distance measure	Correlation with density measure	Correlation with diversity measure	Correlation with slots measure	Correlation with churches measure
Percent male	-0.001	0.026	0.030	0.029	-0.026
Percent black	0.227***	0.356***	0.361***	0.311***	0.613***
Percent Latino	0.141***	0.372***	0.213***	0.374***	0.142***
Percent English language learner	0.191***	0.430***	0.274***	0.434***	0.281***
Percent free/reduced price lunch	0.217***	0.399***	0.285***	0.285***	0.478***
School grade in 2001 (A=4, F=0)	-0.121***	-0.186***	-0.128***	-0.167***	-0.305***

Notes: Correlations marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. Distance is reverse-coded, so that a positive coefficient represents a positive correlation between competition and the school characteristics in question.

Table 5. 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)

	(1)	(2)	
Competition measure	Estimated effect on reading+math scores (5-mile definition of competition)	Estimated effect on reading+math scores (5-mile definition of competition)	Additional estimated effect of schools within 2 miles
Distance	1.455***	n/a	
	(0.239)		
Density	0.209***	0.193***	0.077
	(0.022)	(0.032)	(0.137)
Diversity	0.773***	0.467***	0.595***
	(0.110)	(0.148)	(0.242)
Slots per grade	0.009***	0.009***	-0.001
	(0.001)	(0.001)	(0.004)
Churches nearby	0.020***	0.02***	0.018
	(0.002)	(0.003)	(0.015)

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. The dependent variable is the student's average reading+math standardized score. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school level are beneath parameter estimates. 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 1998-99 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. There are 3,103,993 observations in 2,264 school clusters. The R-squared in each model is 0.27.

Table 6. 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	1.719***	0.662	0.114
	(0.252)	(0.619)	
Density	0.258***	0.090	0.124
	(0.022)	(0.106)	
Diversity	1.038***	0.478	0.123
	(0.117)	(0.343)	
Slots per grade	0.011***	0.004*	0.004
	(0.001)	(0.002)	
Churches nearby	0.023***	0.010	0.085
	(0.003)	(0.007)	

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. The dependent variable is the student's average reading+math standardized score. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school level are beneath parameter estimates. 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 1998-99 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. There are 3,103,993 observations in 2,264 school clusters. The R-squared in each model is 0.28.

Table 7. 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		
	(1)	(2)	(3)
	First year program effect (2001-02)	Lead of program (2000-01)	Second lead of program (1999-2000)
Distance	1.087***	-0.476	-0.447
	(0.374)	(0.323)	(0.300)
Density	0.208***	0.012	-0.028
	(0.033)	(0.030)	(0.029)
Diversity	0.737***	-0.001	-0.142
	(0.166)	(0.154)	(0.139)
Slots per grade	0.009***	0.0010	-0.0016
	(0.002)	(0.0015)	(0.0012)
Churches nearby	0.028***	0.012***	0.005
	(0.004)	(0.003)	(0.003)

Notes: Each row represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a set of year indicators) from a separate regression model. The dependent variable is the student's average reading+math standardized score. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school level are beneath parameter estimates. 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 1998-99 through 2001-02 years only. Coefficients marked ***, **, * and + are statistically significant at the 0.001, 0.01, 0.05 and 0.10 levels, respectively. There are 3,103,993 observations in 2,264 school clusters.

Table 8. 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)

	School's 2001-02 percentage free-or-reduced-price lunch falls into the following range:							
Competition measure (pre-policy)	>10 pct pts below focal range	5-10 pct pts below focal range	0-5 pct pts below focal range	Bottom half of focal range	Top half of focal range	0-5 pct pts above focal range	5-10 pct pts above focal range	>10 pct pts above focal range
Distance	0.700	0.774	1.478	2.465**	4.572***	0.701	0.056	1.932***
	(0.469)	(0.771)	(1.128)	(1.164)	(0.946)	(1.018)	(1.221)	(0.575)
Density	0.207***	0.282***	0.269***	0.611***	0.433***	0.201+	0.183***	0.219***
	(0.059)	(0.111)	(0.112)	(0.02)	(0.071)	(0.111)	(0.073)	(0.039)
Diversity	0.794***	1.280***	0.815+	2.818***	2.732***	0.881	0.678	0.719***
	(0.250)	(0.574)	(0.408)	(0.694)	(0.462)	(0.517)	(0.433)	(0.243)
Slots per grade	0.008***	-0.001	0.001	0.020***	0.013***	-0.001	0.003	0.003
	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)	(0.003)
Churches nearby	0.012**	0.010	0.019	0.097***	0.021**	0.009	0.025***	0.024***
	(0.006)	(0.011)	(0.016)	(0.019)	(0.012)	(0.013)	(0.008)	(0.004)

Notes: Each row represents the key coefficient estimate (on the interaction between the measure of pre-policy private school penetration and a set of year indicators) from a separate regression model. The dependent variable is the student's average reading+math standardized score. Coefficients are multiplied by 100 for interpretability. Standard errors that adjust for clustering at the school level are beneath parameter estimates. 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 1998-99 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 9. 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)	Second lead of program (1999-00)	Lead of program (2000-01)	First year (2001-02)	2002-03	2003-04	2004-05	2005-06	2006-07
Distance	-0.094	-0.202	1.362***	1.131***	1.477***	1.985***	2.437***	1.907***
	(0.309)	(0.336)	(0.381)	(0.379)	(0.395)	(0.409)	(0.427)	(0.424)
Density	0.019	0.040	0.228***	0.237***	0.280***	0.380***	0.479***	0.410***
	(0.030)	(0.031)	(0.034)	(0.034)	(0.035)	(0.038)	(0.038)	(0.039)
Diversity	0.022	0.061	0.772***	0.725***	0.923***	1.367***	1.759***	1.450***
	(0.145)	(0.161)	(0.171)	(0.174)	(0.183)	(0.194)	(0.199)	(0.198)
Slots per grade	0.000	0.002	0.011***	0.011***	0.013***	0.016***	0.021***	0.019***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Churches nearby	0.012***	0.016***	0.032***	0.034***	0.040***	0.045***	0.055***	0.044***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)

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 level are beneath parameter estimates. The dependent variable is a student's average standardized test score in reading and math. Controls include sex, race dummies, subsidized lunch eligibility dummies, English language learner dummies, year dummies, and percent of student body eligible for free or reduced price lunch, 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 9,026,689 observations spread across 2,592 school 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

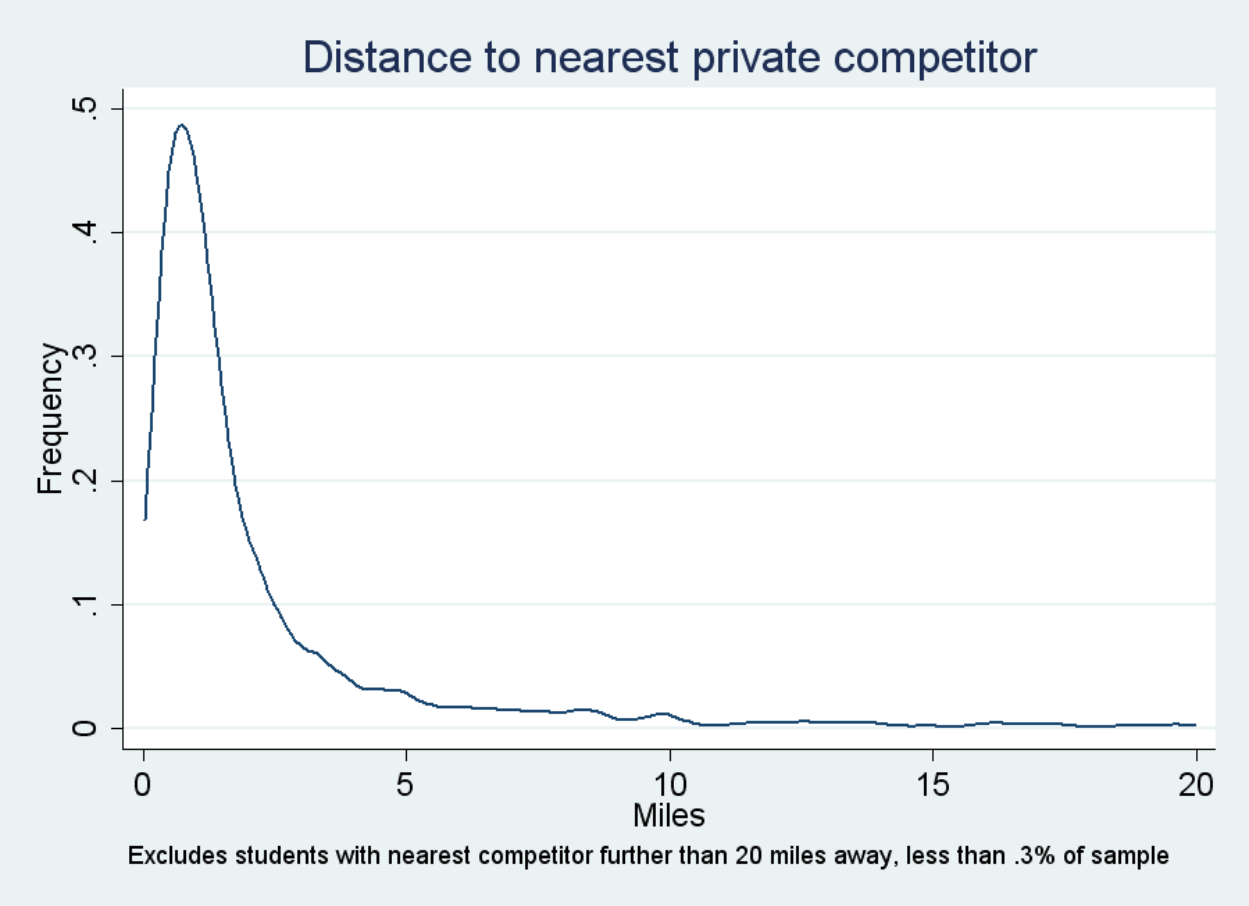
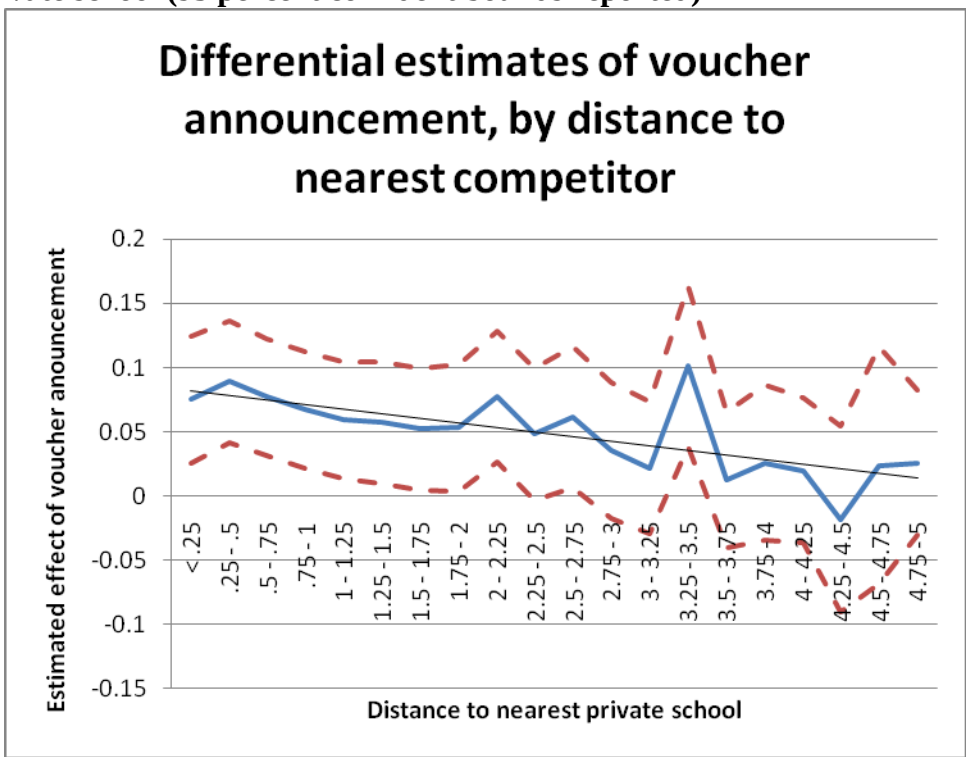


Figure 2. Estimated effects of voucher program announcement based on proximity of nearest private school (95 percent confident bounds reported)



Appendix A.

Table A1. Descriptive statistics on competition measures for eight most populous districts in schools with at least one private competitor within a five mile radius

	Distance	Density	Diversity	Slots per grade	Churches nearby
Statewide	1.35	15.37	5.22	304.67	151.41
	(1.06)	(12.64)	(2.29)	(295.02)	(117.61)
Broward	1.18	20.12	6.62	511.90	206.00
	(0.73)	(10.97)	(1.73)	(254.90)	(107.93)
Miami-Dade	0.82	32.67	6.82	653.39	264.32
	(0.55)	(13.06)	(1.06)	(329.62)	(149.89)
Duval	1.05	18.29	6.64	406.05	239.16
	(0.74)	(7.90)	(1.52)	(255.34)	(147.51)
Hillsborough	1.17	18.34	6.10	309.54	176.70
	(1.02)	(11.57)	(2.28)	(221.96)	(119.69)
Orange	1.29	18.24	5.88	411.69	179.17
	(0.92)	(12.33)	(1.99)	(326.86)	(97.97)
Palm Beach	1.29	10.97	4.68	238.62	109.89
	(0.85)	(5.68)	(2.04)	(1.51)	(61.15)
Pinellas	1.08	19.27	6.27	320.86	152.82
	(0.92)	(9.25)	(1.75)	(175.25)	(63.02)
Seminole	1.45	14.35	5.46	220.03	119.16
	(0.97)	(9.67)	(1.90)	(198.42)	(43.87)

Notes: Data from the Florida Education Data Warehouse, the Florida Department of Education's Florida School Indicators Reports, and the Florida Department of Education. Means include only children in schools with at least one local competitor (92.4% of the potential sample).