Competitive Effects of Means-Tested School Vouchers

By David Figlio and Cassandra M. D. Hart

We use the introduction of a means-tested voucher program in Florida to examine whether increased competitive pressure on public schools affects students' test scores. We find greater score improvements in the wake of the program introduction for students attending schools that faced more competitive private school markets prior to the policy announcement, especially those that faced the greatest financial incentives to retain students. These effects suggest modest benefits for public school students from increased competition. The effects are consistent across several geocoded measures of competition and isolate competitive effects from changes in student composition or resource levels in public schools. (JEL H75, I21, I22, I28)

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 biased either upward or downward. 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 biased downward (Hoxby 1994). On the other hand, if private schools locate in communities that highly value educational quality, then the estimated relationship between private school competition and public school performance would be biased upward.

Using microdata on the full population of public school students in Florida, we take advantage of the unique policy variation provided by the announcement of the means-tested Florida Tax Credit (FTC) Scholarship Program\(^1\) in 2001 to provide the most rigorous evidence to date on the competitive effects of means-tested school vouchers on student outcomes in public schools. We use a difference-in-differences strategy that exploits both the timing of the voucher system roll-out and idiosyncratic features of the school finance system to address the endogeneity of private school competition and public school test scores. This identification strategy relies on the intuition that while the introduction of a voucher policy constitutes a shock that should increase the competitive pressure felt by all public schools to some extent, it should be especially salient to public schools that face a more competitive private school landscape prior to the policy announcement. These areas should be ready to more immediately recruit away new voucher-eligible students, while students in less competitive areas may need to wait for acceptable schools to open. The availability of a variety of nearby private school competitors could send a signal to public schools about the potential for losing students under the new voucher program. We exploit the fact that there were considerable differences in private school penetration both between and within metropolitan areas on the eve of the voucher program; many of the within-area differences are due to the unusual physical characteristics of the state of Florida. Our strategy thus allows us 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.

Two features of the program and the school funding environment strengthen our identification strategy. First, by exploiting the timing of the roll-out of the policy, we find cleaner estimates than have been obtained in past work of the effect of competitive threats per se, as we can isolate the effects of competition alone rather than combining competitive effects together with other effects that vouchers may have on schools, such as changes in school resources, student sorting, or peer group composition\(^2\) due to students taking up vouchers. Second, the means-tested nature of the program allows us to test whether effects are especially strong for schools that face funding “cliffs” if they lose a handful of low-income students to the voucher program—schools that are on the border of qualifying for Title I aid. Because the

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\(^1\) The program was initially called the Corporate Tax Credit Scholarship Program because of its funding mechanism. Students received vouchers funded by fully tax-creditable contributions from corporations to scholarship funding organizations that awarded the vouchers based on state rules.

\(^2\) Hoxby (1994) identifies student sorting as a particularly important confounding factor. See Epple and Romano (2011) for a comprehensive review of the peer effects literature. Lavy (2010) suggests one particular avenue through which composition effects may operate. His study of public school choice in Israel indicates improvements in behavioral as well as academic outcomes under school choice, including reduction of violence and disruption in schools.
distribution of these funds has a nonlinear relationship to the fraction of low-income students in the school, we argue that schools on the border of Title I receipt should face the greatest incentives to improve.

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. The effect sizes are precisely estimated but modest, with a one-standard deviation increase in competition raising test scores by less than one-twentieth of a standard deviation. One should note, however, that this is likely an understatement of the overall competitive effect of means-tested vouchers, because it is only a comparison of high-competition to low-competition areas of the state, and not a comparison of competitive areas to the counterfactual of no vouchers at all. We rule out a variety of confounding factors—including the introduction of other policies; preexisting trends; and the concentration of the highest levels of competition in a small number of large, urban districts—that one may worry would drive these results. Consistent with predictions, we also find that the schools that face the greatest financial incentive to retain low-income students raise their performance the most. Our results therefore suggest that 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.

I. Why Might Voucher Introduction Affect Student Performance?

There are three main ways in which the introduction of a school voucher could affect public school performance. These effects are likely to depend in part on the voucher design. Universal vouchers may have different effects than vouchers that are narrowly targeted to certain types of students, such as low-income students.

First, public schools could react to private school competition by altering their policies, practices, or effort—the direct competitive effect of school vouchers. The most common argument is that competition should incentivize schools to operate more productively, improving the educational quality that they offer in order to maintain their clientele (Friedman 1962; Manski 1992). When vouchers are universally available, the opposite result may occur. McMillan (2004) shows that some types of schools will find it advantageous to reduce their (costly) efforts to provide high-quality schooling, and that school quality will fall as a result. When vouchers are means-targeted, however, we do not expect the perverse result of diminished effort in the face of a voucher (McMillan 2004). At the same time, the positive competitive effects may be weaker than under a universal voucher because restricting eligibility implies that fewer households fulfill the function of acting as marginal consumers putting pressure on public schools to improve. That said, means-targeted vouchers may be especially interesting because the schools serving low-income students may have faced smaller amounts of latent competition before the voucher program was put into place.

Second, school vouchers could affect public schools by changing the set of students who attend the school (and relatedly, the set of parents acting as monitors of school effort). If more academically capable students, or those with more involved parents, disproportionately select into private schools with the voucher,
this could lead to a reduced-ability clientele remaining in the public schools (Epple and Romano 1998). However, the effects might be reversed if vouchers primarily attract students who are low-ability or are struggling in their current school. The direct effects of a higher- or lower-ability student body on aggregate test scores may be magnified by spillover effects on peer performance as well (Epple and Romano 2011). To the extent that student ability and parental involvement are positively correlated with income, means-targeted vouchers again may limit the potential negative compositional effects of vouchers since they do not facilitate the exit of more affluent students.

Third, providing students with vouchers may affect the financial resources allocated to public schools (Caucutt 2002). So long as only a few students leave a public school with school vouchers, the vouchers could plausibly 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, losing students eligible for subsidized lunches could result in resource reductions that affect student outcomes as well.

In practice, these three channels are often difficult to disentangle. While our paper takes advantage of a unique element of Florida’s voucher policy to isolate the direct competitive effects—the first of the three channels—most existing empirical literature should be read as encompassing the net effects of all three channels.

II. Prior Literature on Voucher Competition

A number of researchers have empirically 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 Hoxby (1994), Arum (1996), Dee (1998), Sander (1999), and Jepsen (1999) 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. Most of these studies have found either modestly positive, or null or inconsistent effects of private school competition on public school students’ educational outcomes (McEwan 2000; Belfield and Levin 2002). 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; Sander 1998; Jepsen 1999; 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 (Hoxby 1994; Dee 1998; Sander 1998; Jepsen 1999; Card, Dooley, and Payne 2010; West and Woessmann 2010). That said, there are reasons to question the validity of using

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cross-sectional differences in religious concentration as an instrument for private schooling (Altonji, Elder, and Taber 2005).

A few papers have taken a different tack and identified the effects of voucher programs directly on student outcomes in the public schools (Hoxby 2003; Figlio and Rouse 2006; Gallego 2006; West and Peterson 2006; Chakrabarti 2007; Böhlmark and Lindahl 2008; Chakrabarti 2008; Chiang 2009; Greene and Marsh 2009; Rouse et al. 2013). Overall, this literature finds modest positive effects of vouchers on public schools, but while these papers are novel and thoughtful, there are often concerns about their identification strategies. Studies that rely on changes in the degree of private school supply over time for identification are subject to the concern that private school supply is endogenous to public school performance. Those that identify competitive effects through the fact that repeat “F” school grades in Florida once triggered vouchers 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, Chan and McMillan (2009), 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. 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.

While their estimation strategy is quite similar to ours in many ways, the program that Chan and McMillan (2009) analyze features a universal voucher rather than the means-tested variety that is more commonly offered in the United States. Our results also have the added advantage of being able to isolate the pure competitive effects of the vouchers. In addition, we can exploit idiosyncratic features of the public school finance regime to further promote identification. Taken together with the results from Chan and McMillan, our results provide strong evidence for the competitive effects of vouchers.

III. The Florida Tax Credit Scholarship Program

The FTC Program was signed into law in spring 2001, but, crucially to our analysis, it only began to actually provide scholarships to students in the 2002–2003 school year. The program provides scholarships, by way of scholarship funding organizations, to students who qualify for free or reduced-price lunch (i.e., those with verified family incomes below 185 percent of the federal poverty line) 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

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*Families have to submit tax returns to verify income eligibility.*
schools in Florida are not eligible for first-time scholarships (though students who enter a private school on a scholarship were 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.6 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. By the 2011–2012 school year, 40,248 students eligible to receive up to $4,335 participated in the program (Florida Department of Education 2012).

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 percent of the overall population, and between 1–2 percent of the income-eligible population in public schools as of the 2001–2002 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.

There were two other publicly financed school voucher systems put in place around the same time as the FTC program. The Florida Opportunity Scholarship Program offered vouchers to students attending chronically failing public schools but very few students were eligible and it never served more than a few hundred students before being struck down by the Florida Supreme Court. The McKay Scholarship Program offered vouchers to students with disabilities; we therefore concentrate our analysis on nondisabled students.

Scholarships were allotted on a “first come, first served” basis, with applications accepted until funds ran out.
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 students could only apply for, but not use, a voucher in the year following announcement (2001–2002). Thus, while the program may have induced entry into the private school market, our results do not identify program effects off of the entry of these new private schools.

IV. Data and Methods

A. Data

We draw on several sources of data from the Florida Department of Education (FDOE), which maintains an Education Data Warehouse containing test scores, demographic characteristics, and schooling histories for all students in Florida. Our analysis includes test score data from the 1998–1999 school year through the 2006–2007 school year. 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. FDOE provided us with lists of public and private schools in the state prior to the program, and we geocoded these schools’ locations using ArcGIS.

We excluded students classified as disabled 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.

On the other hand, we included 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 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. The full dataset includes 9,438,191 potential student-year observations, observed over the 1998–1999 to 2006–2007 school year.

7 When a student is observed in multiple schools in a given year, we randomly assign the student to one of these schools.

8 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.
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 (and, as a check, sometimes at the school district level); there are 2,592 school clusters in 67 school districts in our data.

Our dependent variable is the student’s developmental scale test scores (the state scale score in 1998–1999) on the Florida Comprehensive Achievement Test (FCAT), a criterion-referenced test administered in grades 3–10 and used for school and student accountability. For most analyses, we average reading and math scores of students for the purposes of expository 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, we standardize test scores at the grade level.

B. Competition Measures

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 multiply our “distance” measure 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 online Appendix Figure A1). Therefore, as mentioned above, we restrict our analysis in this paper to schools with at least one private school within five miles.

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. In one variation (our “density” measure) we count the number of private schools within a five-mile radius of the public school. In the “diversity” measure of competition, we count the number of distinct types of private schools within five miles. 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 the FDOE. We identify ten types

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9 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.

10 The results are broadly similar if we relax this restriction.
of private schools, including nonreligious; nondenominational or multidenomina-
tional; Catholic; Protestant; Evangelical; Baptist; Islamic; Jewish; Christian general
(no specific denominational information); and other religious schools.

These measures of competition are based on counts of private schools, and
weight large and small schools equally. We prefer count-based measures of com-
petition 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 factors 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
the overwhelming majority of students participating in the voucher program attend
religious private schools (Figlio, Hart, and Metzger 2010), it is reasonable to believe
that public schools with many nearby houses of worship may have felt more potential
competitive threat than did 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.

C. Models

We use a series of fixed effects regression models to isolate the effect of competi-
tive pressures from private schools on public school performance. Our basic model is:

\[ Y_{ist} = \alpha_s + \beta C_s \times P_t + \lambda X_{it} + \mu S_{st} + \delta T_t + \varepsilon_{ist}, \]

where \( Y_{ist} \) typically represents the average of the standardized math and reading
scores (though sometimes reading and math scores separately) for student \( i \) in
school \( s \) in year \( t \); \( \alpha_s \) 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; \( X_{it} \) 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 \( t \); \( S_{st} \) is a vector of time-varying school characteristics
(school grades and the share of students eligible for subsidized lunches); and \( T_t \) is a

\( ^{11} \) 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.
series of year dummies. In some specifications we control for lagged test scores as well.\footnote{We do not control for lagged test scores in our primary specifications because we would lose a large number of observations since, in the early years FDOE, did not administer the FCAT in every grade, and because we would lose an entire preprogram year of observations.} The coefficient on the competition measures interacted with the post-policy indicator, $\beta$, 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 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 cluster our standard errors at the school level. Our results are robust to clustering at higher levels of geography as well.

**D. Descriptive Statistics**

Table \textit{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, with the average distance from a child’s current public school to the nearest private school option being 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 averaging 15.37 local private competitors, representing an average of 5.22 different types of religious (or secular) affiliations. These private schools averaged 305 potential slots per grade. On average, a public school had 151 houses of worship within a five-mile radius. The degree of variability in these measures is somewhat unsurprising since our data draw from the entire state and one would expect a fair amount of variation between metropolitan areas. However, even within metropolitan areas, we see a high degree of variability in competition measures: online Appendix Table A1 presents means and standard deviations on all five competition measures for the eight most populous districts in the state.\footnote{Online Appendix Table A2 demonstrates that our competition measures are correlated in cross section with a variety of school characteristics. The fact that schools facing different levels of measured competition serve substantially different populations of students highlights the importance of estimating models with school fixed effects.}

**V. Immediate Effects of the Introduction of the Voucher Program**

We noted above that there were three channels through which vouchers might affect public schools: direct competitive effects, composition effects, and financial effects. 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 2001–2002 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...
can be thought of as a pure competition effect of vouchers. We therefore begin with school fixed effects estimates of the effects of competition on student performance using post-policy data only from 2001–2002 rather than all post-policy years.

The results of this first analysis are reported in the first column of Table 3: Each cell represents the coefficient on the \( \text{Post-policy} \times \text{Competition} \) interaction for separate regressions that use each of the five measures of prepolicy 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. Adding 10 nearby private schools (just shy of a standard deviation increase in this measure) increases test scores by 0.021 of

| Observation: 3,103,993 (2,264 schools)

Note: Means include only children in schools with at least one local competitor (92.4 percent of the potential sample).

Source: Data from the Florida Education Data Warehouse, the Florida Department of Education’s Florida School Indicators Reports, and the Florida Department of Education.
Table 3—Fixed Effects Regression Estimates of the Effects of the Introduction of Voucher Competition on Public Schools: First-Year Program Estimates Only  

<table>
<thead>
<tr>
<th>Competition measure</th>
<th>Reading + math</th>
<th>Math only</th>
<th>Reading only</th>
<th>Reading + math (control for lagged scores)</th>
<th>Reading + math (with MSA × year FEs)</th>
<th>Effect of competition within five miles</th>
<th>Additional effect of schools &lt;2mi</th>
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<tr>
<td>Panel A. Five-mile definition of competition</td>
<td></td>
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<td>Distance</td>
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<td>1.513***</td>
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<td>(0.251)</td>
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Panel B. Five- and two-mile definitions

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<th>Reading + math</th>
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<th>Reading only</th>
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<th>Reading + math (with MSA × year FEs)</th>
<th>Effect of competition within five miles</th>
<th>Additional effect of schools &lt;2mi</th>
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<td>0.241***</td>
<td>0.189***</td>
<td>0.149***</td>
<td>0.098***</td>
<td>0.190***</td>
<td>0.079</td>
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<tr>
<td></td>
<td>(0.022)</td>
<td>(0.022)</td>
<td>(0.019)</td>
<td>(0.020)</td>
<td>(0.020)</td>
<td>(0.032)</td>
<td>(0.137)</td>
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<tr>
<td>Diversity</td>
<td>0.779***</td>
<td>0.781***</td>
<td>0.676***</td>
<td>0.487***</td>
<td>0.308***</td>
<td>0.469***</td>
<td>0.596***</td>
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<tr>
<td></td>
<td>(0.110)</td>
<td>(0.127)</td>
<td>(0.102)</td>
<td>(0.116)</td>
<td>(0.117)</td>
<td>(0.148)</td>
<td>(0.243)</td>
</tr>
<tr>
<td>Slots per grade</td>
<td>0.009***</td>
<td>0.009***</td>
<td>0.008***</td>
<td>0.005***</td>
<td>0.004***</td>
<td>0.009***</td>
<td>−0.001</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Churches nearby</td>
<td>0.020***</td>
<td>0.021***</td>
<td>0.017***</td>
<td>0.016***</td>
<td>0.010***</td>
<td>0.020***</td>
<td>0.018</td>
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<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.015)</td>
</tr>
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</table>

Notes: Each cell represents the coefficient estimate on the interaction between the competition measure and a postpolicy 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 dummies for sex, race, subsidized lunch eligibility, English language learner, and year, 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. There are 3,103,993 observations in 2,264 school clusters. The $R^2$ in each model is 0.27 (0.24 in reading-only and math-only regressions; 0.66 when controlling for lagged test scores).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Data come from 1998–1999 through 2001–2002 years only.

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 5 mile radius (a nearly one standard deviation increase) is associated with a 0.02 standard deviation rise in scores, and adding an additional 300 slots in each grade level in a 5 mile radius (just over a 1 standard deviation increase in this measure) increases scores by 0.027 standard deviations. Overall, a 1 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 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. 

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16 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. For instance, the equivalent estimates to those reported in the first column of Table 3, when we collapse the observations to the school-by-year level, are 1.485 (se = 0.319) for distance; 0.233 (se = 0.030) for density; 0.927 (se = 0.160) for diversity; 0.011 (se = 0.001) for slots; and 0.022 (se = 0.003) for churches nearby. We therefore conclude that regardless of whether we estimate student-level models with clustered standard errors or aggregated models, the fundamental results remain unaltered.

17 One might be concerned that school-level clustering is too fine a geographic area at which to cluster the standard errors. If we cluster at the county level, rather than at the school level, the standard error in the distance specification...
The next two columns in Table 3 show the estimated effects on math and reading scores, respectively; the results are highly similar across the two different test scores. These results provide a first piece of evidence that public schools responded to the threat of losing students via the voucher program.

As mentioned above, we face a tradeoff between including more prepolicy years in the analysis and controlling for lagged student test scores. When we control for prior test scores in the fourth column of Table 3, we observe that the estimated effects are modestly smaller in magnitude but of the same level of statistical significance as were our other findings.

One might also wonder whether the results are driven by within-area versus between-area variation in the levels of voucher competition. Therefore, in the fifth column of Table 3 we estimate models in which we control for metropolitan area \times year fixed effects so that we can isolate only the estimated effects of competition coming from within-area variation in our competition measures. The estimates are one-third to one-half the size of those reported in the first column of Table 3 (though they are still strongly statistically significant). This indicates that our results are driven both by between-area and within-area variation in competition faced by schools.

We posited that there may be a nonlinear 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 and interacting these distance indicators with the post-policy indicator. Figure 1 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 announcement.

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 panel B of Table 3. 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 one of the four (nondistance) competition measures. Therefore, although there is substantial evidence that schools respond the most when there is a private school 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 measures of private competition, in addition to the distance measure.

VI. 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 3 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 falls 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 may be driven by spurious correlations between competition measures and longitudinal score gains, or in trends over time in the performance of public schools in a community. That is, areas with more competitive private school landscapes may have been experiencing public school test score gains even prior to the program announcement. 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 the policy announcement (the 1999–2000 and 2000–2001 school years). Coefficients on all Year × Competition interaction terms are then interpreted in relation to the omitted interaction between
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1998–1999 and the competition measures. If schools with nearby private competitors were improving over time, one would expect to observe positive and increasing coefficients on these policy lead variables.

However, as seen in Table 4, there is very little evidence of a positive trend in school performance in the lead years preceding the voucher introduction. The coefficients on the 2001–2002 × Competition interactions are significant and positive for all measures of competition (column 1), echoing the results presented in Table 3. However, the coefficients for the 2000–2001 × Competition interactions (column 2) and 1999–2000 × 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-half 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 prepolicy 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.\footnote{These three counties yield results that are roughly representative of the rest of the state on the regressions reported in Table 3, although the estimated effects of voucher competition are modestly larger in Miami-Dade County than in the state as a whole.}

While unfortunately these testing programs had ended before the first year of the voucher program, making it impossible to directly compare the five-year prepolicy period to the first year of potential competition, it is still possible to observe whether there exist any preperiod 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 1996–1997, five years before the policy introduction, to 2000–2001, the year before the policy introduction.

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 (online Appendix Table A3).\footnote{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.} 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.

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 FDOE 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 2,300 schools had received even one “F” grade (Rouse et al. 2013). 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 our 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 2001–2002 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 et al. 2013), making it relatively unlikely that this change accounts for the results. 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.

VII. 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.

Not every public school with low-income students receives Title I aid. In 2001–2002, 61 percent of elementary schools and 31 percent of middle schools statewide received Title I funds. Title I aid is allocated based on where schools rank within their school districts with respect to concentration of low-income students, with the highest-poverty schools in each district receiving Title I aid. The poverty threshold that determines 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.

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. 2013).

Additional analyses suggest that even when we include interactions with schools’ summer 2000 grades from the FDOE, 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.

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 consists of “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 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 2001–2002.
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 2001–2002 cutoff for Title I aid were likely to believe that they stood a good chance of receiving Title I aid in 2002–2003. 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 2001–2002 school year, according to conversations with school officials, and Title I cutoffs for 2002–2003 were anticipated to a first approximation by schools during the 2001–2002 school year.

We seek to identify the effects of voucher competition for schools on the margin of Title I receipt. Because schools in 2001–2002 had reasonably but not completely precise information about the school district’s threshold for Title I receipt in 2002–2003, and also were unsure of what their fraction of eligible students would be in 2002–2003—this is a highly mobile population so it is 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 that were receiving Title I aid in 2001–2002 and would continue to receive aid in 2002–2003 so long as their low-income percentage did not fall by much; (2) those that were not receiving Title I funding in 2001–2002 but would due to Title I expansion in 2002–2003 so long as their low-income percentage did not fall; and (3) those that would not be predicted to receive Title I aid in 2002–2003 but might if their low-income percentage increased sufficiently. Schools in group 2 are in our “focal range.” We define this focal range as those schools with 2001–2002 shares of students on subsidized lunch that fell between two policy thresholds: the 2001–2002 Title I eligibility cutoff and the realized 2002–2003 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—schools that would stand to lose Title I funding if only a few low-income students left the school—or those near this range.

In Table 5, we present the estimated effects of increased competition for eight groups of schools based on their 2001–2002 share of low-income students: those with a percentage of 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 competition is responsible for the gains that we attribute to the FTC program.

VIII. Longer-Term Estimates of the Effects of School Vouchers

We also investigate whether the estimated effects of the voucher program persist in 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 6 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. 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

23 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.

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<td>−0.202 (0.336)</td>
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<td>0.237***</td>
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<td>0.380***</td>
<td>0.479***</td>
<td>0.410***</td>
</tr>
<tr>
<td>Diversity</td>
<td>0.022 (0.145)</td>
<td>0.061 (0.161)</td>
<td>0.772***</td>
<td>0.725***</td>
<td>0.923***</td>
<td>1.367***</td>
<td>1.759***</td>
<td>1.450***</td>
</tr>
<tr>
<td>Slots per grade</td>
<td>0.000 (0.001)</td>
<td>0.002 (0.002)</td>
<td>0.011***</td>
<td>0.011***</td>
<td>0.013***</td>
<td>0.016***</td>
<td>0.021***</td>
<td>0.019***</td>
</tr>
<tr>
<td>Churches nearby</td>
<td>0.012*** (0.003)</td>
<td>0.016*** (0.003)</td>
<td>0.032***</td>
<td>0.034***</td>
<td>0.040***</td>
<td>0.045***</td>
<td>0.055***</td>
<td>0.044***</td>
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</table>

Notes: Each cell represents the key coefficient estimate (on the interaction between the measure of prepolicy 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, average years of experience, 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. All models have 9,026,689 observations spread across 2,592 school clusters and a $R^2$ of 0.26.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

(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.

IX. Potential Mechanisms

Our results indicate that the increased competition generated by the FTC program improved the test scores of neighboring public schools. We next to seek to investigate the potential mechanisms used by public schools to generate these improvements. In separate work, Hart (2011) used data from the FDOE’s Florida School Indicator Reports and surveys administered by Rouse et al. (2013) to the universe of Florida school principals in 2000, 2002, and 2004 to study several possible mechanisms, including both changes in staffing practices (average years of experience of teachers, share of teachers with advanced degrees, and share of staff devoted to instruction), and changes in organizational structure and instructional practices. Candidate mechanisms in the latter two categories included rewarding teachers whose students perform well, mandating professional development activities for teachers whose students perform poorly, providing additional opportunities to learn for students, changing the minimum mandated amounts of time spent in various academic subjects, imposing sanctions on failing students, introducing novel types of scheduling systems, and reducing class sizes. Hart characterized the extent to which schools implemented reforms in each domain based on whether they newly reported using a variety of subreforms during the study period. For instance, reforms in the teacher reward domain included offering exemplary teachers monetary rewards,
comp/release time, a choice of classes, the opportunity to attend conferences and workshops, special leadership posts, and “other” teacher incentives.

Hart’s results suggest that schools faced with greater competitive threats were increasingly likely to adopt certain reforms. In particular, schools under competitive threat were more likely to adopt new forms of scheduling systems, particularly block schedules and the use of subject matter specialist teachers (Hart 2011). This finding echoes Rouse et al.’s (2013) evidence that schools faced with accountability pressures similarly engaged in reforms of scheduling systems.

Schools faced with increased competition also became less likely to employ teachers with stronger traditional qualifications. That is, schools with increased competitive pressure responded by shifting their teaching forces to include teachers with less experience and less formal education. Past research has suggested that the links between formal teacher qualifications and student achievements are relatively weak (Hanushek 1997; Rivkin, Hanushek, and Kain 2005; but see Greenwald, Hedges, and Laine 1996). This finding could therefore suggest that schools faced with increased competition shift their resources away from unproductive uses (paying for teacher qualifications that have little bearing on achievement) to more productive uses. However, we lack budget data to examine this hypothesis in more detail. Furthermore, this result was limited to the first two years after the voucher program was announced; these estimated effects of competition on average teacher qualifications did not persist.

However, neither of these results seems to account well for the relationship between increased competition and increased test scores for public school students. When the organizational changes associated with increased competition (changes in scheduling systems and staffing changes) were included in Equation 1 as potential explanatory variables, they did little to diminish the relationship between the Post × Competition variable and public school test scores (Hart 2011). We have several theories about why this may be.

First, it is likely that different schools establish different ideas about how best to respond to competition. That is, some schools may react to increased competition by cutting class sizes; others may offer teacher rewards, and others still may provide increased learning time for struggling students in the form of after-school tutoring, Saturday school options, or summer school. If different schools try different innovations in response to competition, we may not see sufficient response in any individual domain to account for our main results, even though experimentation generally is occurring.

Second, we are limited by the lack of availability of detailed data on school practices in the time period during which the FTC program was introduced. While the principal surveys used in this analysis represent the best data available, data are only available for one prepolicy year and for only three years overall. This may simply not be enough data to tease out causal patterns in trends in organizational practices.

A similar concern plays into a third reason that we may be unable to uncover mechanisms. Specifically, the survey may simply miss the most common responses to competition. Perhaps principals responded to competition by making changes in curricula, or by exhorting teachers to better monitor student understanding and to adjust their instruction accordingly. Very fine-grained changes such as these would not be
captured by the instruments used in the mediation analysis, but represent important school responses that could have payoffs in terms of public school test scores.

We argue that the best interpretation of this analysis is that schools do seem to be experimenting with new innovations in response to competition, but that the specific responses are insufficiently consistent from school-to-school for us to confidently identify one single mechanism responsible for the improvements in test scores observed in our main results. Uncovering the mechanisms that account for these improvements remains an important area for future research.

X. Discussion

We find that the increased competitive pressure faced by public schools associated with the introduction of Florida’s FTC 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 facing large financial consequences to losing students; specifically, schools on the margin of Title I funding 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 program. Although this ensures that the competition measure is not affected by 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, voucher programs 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, sensitivity checks not presented here yield no evidence that voucher effects are mitigated in
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.

REFERENCES


