Journal of Public Economics 95 (2011) 850–863

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The effect of charter schools on achievement and behavior of public school students

Scott A. Imberman *

University of Houston, 204 McElhinney Hall, Houston, TX 77204-5019, United States

ARTICLE INFO

Article history:
Received 3 January 2010
Received in revised form 3 December 2010
Accepted 6 February 2011
Available online 17 February 2011

Keywords:
Charter schools
Achievement
Behavior
School choice

ABSTRACT

Charter schools have seen dramatic growth over the last decade. However, we know little about how they affect traditional public schools. I look at how charters affect student outcomes in public schools using data from a large urban school district in the southwest. Unlike prior work that relies on school fixed effects, I address the endogenous location of charter schools using an instrumental variables strategy that relies on plausibly exogenous variation in local building supply. Results show that charters induce modest but statistically significant drops in math and language test scores, particularly for elementary students. However, results for middle and high school students show improvements in discipline.

1. Introduction

Charter schools comprise one of the fastest growing education reforms in the US. While proponents argue that charters provide innovative education to students and spur traditional public schools (TPS) to improve through competitive pressures, opponents argue that charters drain the public school system of funding leaving those who cannot leave worse off.[1] Despite this controversy, since 1997 the number of charter schools in the US has increased more than fivefold, and the number of students has more than doubled since 1999. Today, over a million students attend charters. In some states, the charter population is a substantial portion of the total student population. For example, 10% of students in Arizona attend charters.[2]

Charter schools are publicly funded schools that are given autonomy from local school districts and are subject to fewer regulations than regular public schools. It is theoretically unclear what impact charters have on TPS and only a handful of papers have looked at the empirical evidence of how charter schools affect students in traditional public schools using individual data (Booker et al., July, 2008; Bifulco and Ladd, 2006; Sass, 2006). In this paper I address this question using data from a large urban school district in the southwest (LUSD-SW) to look at charter impacts on test scores and student behavior.

While I consider the reduced form impacts of charters on TPS, it is useful to think about what mechanisms may be at play. The most commonly cited is a competition effect. When a charter school enrolls a student it usually receives money from the chartering authority. Some portion of this funding would have gone to the local public school had the student not left for the charter. Thus there is a financial incentive for public schools to prevent students from attending charter schools.[3] In the long-run a school may lose enough students to the charters so that it is forced to shut-down due to low enrollment. If these two incentives spur public school teachers and administrators to increase effort and efficiency, then charters would generate a positive competition effect on public schools. On the other hand, if charters pull too many students from one school, districts may “give-up” and reallocate funding towards other schools. In addition, some theoretical work by Cardon (2003) suggests that if there are capacity constraints on charters then public schools may not respond to charter competition. Indeed, if public schools are overcrowded, they may welcome the charter schools, since they would serve as a release valve. Another possibility is that charters may induce public schools to shift resources so as to specialize in teaching students who are less attracted to charters.

Even without an explicit response, charters can impact traditional public schools. For example, if charters attract too many students away from a public school, the principal may have to reduce faculty and staff. While this may be beneficial in the long-run, in the short-term it could reduce morale while generating confusion and uncertainty. In addition, new charter schools or expansions in existing

* Tel.: +1 713 743 3839.
E-mail address: simberman@uh.edu.

[1] Throughout this paper the term “traditional” public schools refers to any public non-charter school.

[2] Author's calculation from data provided by Center for Education Reform (a charter advocacy group) and National Center for Education Statistics, US Department of Education.

[3] In the state where LUSD-SW is located, the school district loses state aid for the student. While the amount lost depends on the type of student, i.e. whether the student is economically disadvantaged, special education, gifted, etc., the marginal student from LUSD who is not in a special category would cost the school district \$2843 in 2005, or 46% of average per-student expenditures. In 2002 losing a marginal student would have cost \$4580 or 67% of average per-student expenditures.

0047-2727/\$ – see front matter
doi:10.1016/j.jpubeco.2011.02.003

schools may be disruptive by drawing away a large number of students at once, forcing the local public schools to cope with a sudden drop in funding by cutting costs. Over the long-term the schools will likely adapt, but in the short-run only certain costs may be able to be cut such as teacher and staff hiring, after school programs, or additional instruction. Charters may also attract better teachers from regular public schools, leaving worse teachers behind. Another possibility is that if charters attract only certain types of students, then the characteristics of the peers for students who remain in traditional public schools may change. Thus charters can induce impacts on student outcomes through peer-effects.

Even if we are to abstract away from the mechanism of charter impacts on traditional public schools, identifying the effects of charter schools on TPS students is problematic because neither a student's choice of what school to attend nor a charter school's choice of where to locate are random. Thus, any study of charter school impacts on non-charter students must account for these two potential types of selection bias. Previous work has used student fixed-effects to account for endogenous movements of students and school fixed-effects to account for charter location (Booker et al., 2008; Buddin and Zimmer, 2005b; Bifulco and Ladd, 2006; Sass, 2006).[4] However, we may be concerned that panel data methods insufficiently correct for bias in this context. In particular, while student fixed-effects are likely to be a sufficient correction for student selection (Hanushek et al., 2007; Imberman, forthcoming) selection of charter locations could be based off of local trends in public school quality and thus school-fixed effects will not completely address the bias.

In this paper, I use characteristics of the building stock near traditional public schools as instruments for charter location instead of school fixed effects. The intuition behind this is that when a charter is started, one of the most restrictive constraints is finding space available for rent.[5] In particular, I use the number of properties near a traditional public school with between 20,000 and 50,000 ft^2 of building space. These properties are of sufficient size for a school to operate but are too small for buildings that are generally undesirable locations for charters (e.g. office buildings, warehouses and apartment complexes). I also use the number of properties with shopping centers, in which charters in LUSD commonly locate. I further interact these measures with average charter penetration across the district to add temporal variation to the instruments. I argue that these supply constraints on new charters are plausibly exogenous sources of variation in charter location. In particular, they are unrelated to both the characteristics and trends of the traditional public schools and thus capture variation in charter location that is orthogonal to these potential biases.

I also add to the previous literature by looking at discipline and attendance of TPS students in addition to test scores. Based on findings from Heckman et al. (2006) that non-cognitive skills are correlated with poor behavior, Segal (2009) and Imberman (forthcoming) argue that the combination of student discipline and attendance provides a proxy for non-cognitive skills. Using this strategy, Imberman finds evidence that suggests some charter schools are more effective at improving non-cognitive skills of students than cognitive skills. Thus it seems reasonable that public schools could respond by focusing on student safety and discipline to encourage students to stay in public schools. This may be particularly important since Weiher and Tedin (2002) find that discipline and safety drive many parents' decisions to enroll their children in charters.[6]

I find that charters generate negative and statistically significant impacts on math and language test score growth of 0.05 and 0.04 standard deviations, respectively for a 10 percentage point increase in charter population share within 1.5 miles. There is no statistically significant impact on reading. The negative test score impacts seem to arise largely from students in elementary grades, for whom reading is also significantly negative. Middle and high school students show no statistically significant change in achievement. These results are robust to a series of specification tests that use variations in the instruments, control for school characteristics, control for neighborhood characteristics, or use different distances to define the market area. These estimates differ considerably from prior research and suggest that analyses which rely on school fixed effects to address endogenous location of charter schools may suffer from bias. They also imply that, in the short-term, charter schools can be detrimental to the academic performance of non-charter students. In terms of behavior, on the other hand charters generate some benefits as they induce significant drops in disciplinary infractions in middle and high-school public school students of two-tenths of an infraction per year. However, since I do not find a corresponding improvement in attendance, it is unclear whether this is truly an improvement in non-cognitive skills or a result of changes in enforcement. Additionally, the estimates for behavior are sensitive to specification and thus should be interpreted with caution.

2. Background

2.1. Previous literature

A substantial amount of research has looked at how charter schools affect student outcomes.[7] On the other hand only a handful of papers have considered how charter schools affect non-charter students. Some early work on the topic has used school level data to answer this question. Bettinger (2005) finds little effect of charter schools on public schools while Hoxby (2004) and Holmes et al. (2003) find positive effects of charter schools on public schools.

Recent work on whether charter schools affect non-charter students has instead used individual panel data due to concerns regarding changes in student body composition that school-based studies cannot fully address. Panel data can also be used to account for unobserved heterogeneity of students across different levels of charter penetration, as long as the selection of students into schools near or far from charters is based on time-invariant characteristics. Using school and student fixed-effects strategies Sass (2006) and Booker et al. (2008) find that charter schools have positive impacts on traditional public schools while Bifulco and Ladd (2006) and Buddin and Zimmer (2005a, 2005b) find statistically insignificant impact estimates. Hence, researchers have found that charter schools have, at worst, no significant effect on achievement in non-charter public schools and, at best, a large positive effect.[8]

2.2. Charter schools in LUSD

LUSD-SW was one of the first school districts in the US to face competition from charter schools. Both district and state authorized schools began appearing in 1996. Fig. 1 shows the evolution of charter openings in LUSD's county over time. Each of these types of schools experienced substantial growth over the time period studied in this

[4] Booker et al. (2008) also make use of student-spell fixed effects so that their estimates are identified off of changes in charter penetration while a student is enrolled at a specific school.

[5] While some charters purchase land, since relatively little start-up capital is provided by public sources, most charters in LUSD-SW rent their space or use donated space.

[6] Changes in discipline could also spill over into achievement impacts through peer effects. See e.g. Carrell and Hoekstra (2010), Figlio (2007), Kinsler (2009).

[7] See for example Bifulco and Ladd (2006), Booker et al. (2007), Hanushek et al. (2007), Hoxby and Murarka (2009), Hoxby and Rockoff (2004), Imberman (forthcoming), Sass (2006) and Zimmer and Buddin (2003).

[8] Clark (2009) considers competition from schools similar to charters in the UK called "grant managed schools" and uses whether having a school that just barely became a GM school in parent elections nearby had a different effect from having a school that barely missed becoming GM nearby. He finds little competitive impact on regular public schools.

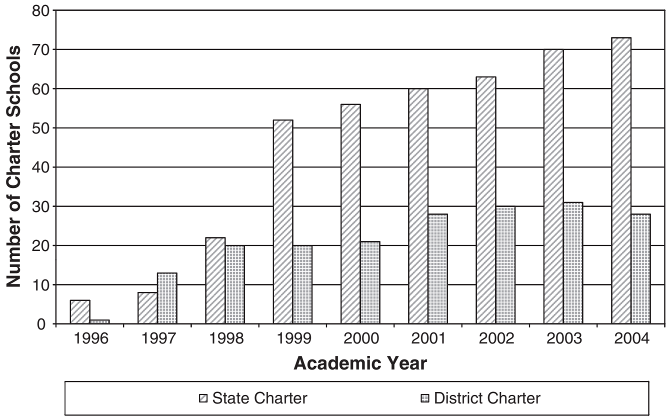

Fig. 1. Charter schools in LUSD area (dates refers to the calendar year of the fall semester).

paper. By 2004–05 there were 99 charter schools in operation of which 71 were chartered by the state while 26 were chartered by LUSD-SW directly. Two more schools were chartered by local universities. Since these schools are completely independent of LUSD I consider them “state charters” in my estimations. In all three cases schools must apply directly to the chartering agency which then decides whether or not to grant the charters and the conditions under which it is granted. Given the substantial number of charter schools in LUSD, it is not surprising that they account for a large portion of the student population. Enrollment in district and state charters in 2004–05 equals 6% and 9% of non-charter LUSD enrollment, respectively.

My measure of charter penetration, which I explain in more detail below, includes both district and state charters, with the exception of district conversion charters. These eight charters are regular public schools that switch to charter status but largely keep the same staff and stay in the same building.[9] Hence, my instrument will not be appropriate for these schools.[10] Since funding for district charters remain nominally in the district, we may be concerned that these schools exert less pressure on public schools than state charters. Hence I also provide specifications that use penetration from state charters only.

Table 1 provides summary information on traditional public schools and charters for the years 1998–99 to 2004–05, as prior to 1998 there were very few charters in LUSD. The first column includes all non-charter schools in LUSD-SW, the second shows district charter schools, and the third provides the characteristics of state charter schools. Stars denote whether a measure is significantly different from the non-charter public schools. District charter students are generally similar to other LUSD students, although they are less likely to be gifted or require special education. State charter students, on the other hand, differ considerably from LUSD students. They are less likely to be economically disadvantaged and to have special needs, as shown by the lower rates of limited English proficiency, special education, and gifted and talented.[11] State charters also attract disproportionately fewer African-Americans and more Hispanics. The white population of charters, while slightly lower, does not statistically significantly differ from non-charter schools. However, despite the higher wealth status of charter students, their test score performance is lower than non-charter students. Thus, these results suggest that state charter students tend to be low-performers who do not have special needs.

Table 1
Charter and non-charter student characteristics, 1998–2004.

	LUSD-SW traditional schools	LUSD-SW charters	State charters
% Economically disadvantaged	77.8	77.5	65.4***
	(24.4)	(25.3)	(27.4)
% Limited English proficient	27.8	19.4	10.0***
	(22.5)	(23.0)	(21.0)
% Special education	17.9	5.6***	12.0*
	(27.5)	(12.9)	(22.5)
% Gifted	25.6	15.2**	8.8***
	(21.6)	(21.6)	(20.4)
% White	9.7	9.9	6.7
	(15.2)	(17.6)	(9.7)
% African-American	30.9	41.1	55.0***
	(29.4)	(34.7)	(38.2)
% Hispanic	56.3	46.9	37.0***
	(30.9)	(32.8)	(35.8)
Observations	1928	166	358
% Passing state math exam at 2004 level (2002–2004 only)	56.8	61.8	45.8**
	(21.2)	(26.4)	(28.6)
Observations	780	74	152
% Passing state reading exam at 2004 level (2002–2004 only)	70.8	74.4	64.0**
	(13.5)	(20.5)	(21.4)
Observations	780	76	155

Observations are school level aggregates in each year. Standard deviations are provided in parentheses. Results are weighted by enrollment. ***, **, and * denote that a t-test of the difference in weighted means between charter and non-charter schools with standard errors clustered by school is significant at the 1%, 5%, and 10% levels, respectively.

3. Data

I utilize administrative records from a large urban school district in the southwest with information on disciplinary infractions warranting an in-school suspension or harsher punishment, attendance rates, scores from the Stanford Achievement Test versions 9 and 10, and some student characteristics. The data cover the 1993–1994 to 2004–2005 academic years and I am able to follow individual students for as long as they attend school in LUSD, providing a long time-series on many students. After dropping students in pre-school or kindergarten, with missing data, 57% of students who are first observed in the data prior to ninth grade have at least four observations.

Since not all students take the Stanford Achievement Test, which is a norm-referenced examination, and test data are only available starting in 1998, I generate two samples.[12] I call the first sample the “behavior sample.” This sample is used when analyzing discipline and attendance. It includes students in grades 2–12 who are enrolled for two consecutive years as of the end of October of each year, since this is when demographic information is collected by the district. The restriction to having two consecutive years is to allow for regressions that use a value-added framework. The demographic files identify the school a student attends and thus I use this as the student's school for the year. Some observations (<0.1%) are excluded due to missing attendance data.

In order to reduce the likelihood of sample attrition bias, I keep district charters, for whom I have data, in the sample. One can also make the argument that district charters face competition from other charters and thus should be included in the sample. On the other hand, generally policy-makers are concerned about the impact of

[9] Imberman (forthcoming) finds that these charters generate little impact overall on student outcomes in LUSD.

[10] I find no statistically significant relationship between whether a school is a conversion charter and the instruments.

[11] A student is economically disadvantaged if he or she satisfies one of the following conditions: (1) is eligible to receive free or reduced price lunch, (2) has family income at or below the Federal poverty line, (3) is eligible for TANF or other public assistance, (4) is eligible for programs under Title II of the Job-Training Partnership Act, (5) is eligible for food stamps, or (6) receives a Federal Pell Grant.

[12] Norm-referenced examinations are tests which are scaled to match a representative sample of students in the same grade. Some papers use criterion-referenced examinations instead, which are exams where the student's score is based on a set of absolute standards.

charter schools on traditional public schools rather than on other charter schools. Hence it is unclear which strategy is more appropriate. Nonetheless, as I will show later, with the exception of attendance rate impacts my results are robust to the inclusion or exclusion of these charters.

I call the second sample the "test sample," which includes all students in the behavior sample from 1999–00 to 2004–05 who have scores recorded for the mathematics, reading, and language portions of the Stanford 9/10 examination in both the current and previous years so that I can generate measures of test-score growth. I standardize scale scores within grade and year so that test scores are measured in standard deviation units. If any one of these tests are missing I drop the observation so that all three test scores are analyzed using the same sample. Stanford 9/10 was given to all English-speaking students in grades 1–8 and all students in grades 9–11. This provides wider coverage of grades than previous work on charter schools, since most districts and states do not start testing until third grade and often stop testing by eighth grade. Students who were not proficient enough in English in grades 1–8 took the Spanish language Aprenda exam.[13] While I have data on these exam results, the scores are not directly comparable to those of students taking the English exam. Nonetheless, estimates of the baseline model that include Aprenda scores when Stanford 9/10 are missing along with dummies for year interacted with whether the student took the Aprenda test provide similar results.[14] This leaves 1,543,969 student–year observations in the base sample and 602,864 student–year observations in the test sample.[15]

School addresses were identified from the US Department of Education's Common Core of Data. Any missing addresses were filled in using school directories acquired directly from LUSD-SW. These addresses were then converted to latitude and longitude using the geocoder.us website. If an address could not be matched using geocoder.us then I used Google Earth™ to find the latitude and longitude.[16] Afterwards, distances between schools were derived using the sphdist command in Stata™. Data on local building stock comes from LUSD-SW's county tax appraisal district. Schools were matched to nearby properties using ArcGIS™. Economic characteristics of census tracts were obtained from the 2000 Census Summary Files while school characteristics were acquired from the state education agency.[17]

Table 2 provides summary statistics from 1998 to 2004 for schools that are between the 0th and 64th, 65th and 74th, 75th and 89th, and 90th and 99th percentiles of charter penetration within 1.5 miles, which is the distance I use to calculate charter penetration. Charter penetration is defined as the fraction of students in schools within 1.5 miles and in grades covered by the observed school who attend charters. A more detailed description of how this variable is constructed and an explanation for why I choose a distance metric of 1.5 miles is provided in the empirical strategy section below. Schools with charters nearby tend to enroll more students who are free-lunch eligible and are classified as at-risk of dropping out but differ little in terms of racial composition. In terms of student outcomes, schools with charters nearby have lower test scores than schools without charters nearby as well as lower attendance rates. Finally, in panel C, I provide means and standard deviations for the instruments, which I will describe in more detail below. All of the instruments increase as charter share increases.

Table 2
Demographic characteristics of LUSD schools by charter penetration.

Percentiles of charter penetration	0–64	65–74	75–89	90–99
Range of charter penetration rates	0.0%–2.8%	2.8%–6.2%	6.2%–15.6%	15.6%–60.0%
A. Demographics				
Female	0.49	0.48	0.48	0.51
	(0.09)	(0.07)	(0.06)	(0.09)
White, non-Hispanic	0.09	0.06	0.06*	0.10
	(0.15)	(0.13)	(0.12)	(0.17)
African American, non-Hispanic	0.36	0.35	0.37	0.36
	(0.33)	(0.31)	(0.31)	(0.28)
Hispanic	0.53	0.56	0.55	0.50
	(0.32)	(0.31)	(0.31)	(0.30)
Free lunch eligible	0.65	0.72**	0.72***	0.64
	(0.24)	(0.24)	(0.21)	(0.26)
Reduced-price lunch eligible	0.08	0.08*	0.08	0.08
	(0.04)	(0.03)	(0.04)	(0.04)
At risk	0.57	0.62**	0.65***	0.65**
	(0.20)	(0.18)	(0.19)	(0.23)
B. Outcomes				
Stanford 9/10 — math	−0.06	−0.11	−0.11	−0.10
	(0.45)	(0.39)	(0.41)	(0.52)
Stanford 9/10 — reading	−0.05	−0.15*	−0.15**	−0.10
	(0.47)	(0.42)	(0.41)	(0.53)
Stanford 9/10 — language	−0.06	−0.14	−0.15*	−0.10
	(0.46)	(0.40)	(0.41)	(0.52)
# of disciplinary infractions	0.34	0.26	0.38	0.43
	(0.55)	(0.64)	(0.57)	(0.58)
Attendance rate (%)	94.65	94.87	94.33	93.02*
	(5.45)	(5.17)	(5.41)	(7.67)
C. Instruments				
# of buildings with 20k to 50k square feet within 1.5 miles	58.3	78.5***	77.3***	89.8***
	(47.9)	(52.3)	(45.9)	(59.3)
# of shopping centers within 1.5 miles	17.9	22.2**	26.3***	34.4***
	(17.2)	(19.9)	(20.2)	(23.9)
Average district-wide charter share	0.052	0.058***	0.062***	0.068***
	(0.024)	(0.020)	(0.020)	(0.016)
Observations	1271	195	293	195

Charter penetration is defined the fraction of students who attend schools within 1.5 miles of and are in grades covered by the school being observed who attend state charters. Standard deviations shown in parentheses. All test scores are measured as standard deviations from the mean scale score. ***, **, and * denote that the mean is statistically significantly different from column one using standard errors clustered by school at the 1%, 5%, and 10% levels, respectively. Covers 1998–2004 only, so that only years with a large number of charter schools are considered. Each observation is the school-year mean.

4. Empirical strategy

4.1. Endogenous student movements and charter location

Estimates of the impacts of charter schools on traditional public schools are potentially biased by two types of selection. First, parents may select into or out of schools near charters for unobservable reasons that are correlated with student ability and behavior. It is also likely that parents respond to observed changes in traditional public schools that result from charter competition. For example, suppose charters generate positive competition effects in non-charter schools. Some parents with high achieving students who planned to send their children to magnet or private schools may now decide to keep their children in their newly improved neighborhood school, thus increasing the estimated charter impact. To address this problem I

[13] Twenty-four percent of elementary student–year observations in the base sample have no test score because they take the Spanish language exam, but by the time students reach middle school, almost all are taking the English language exam. In high school, 23% of student–years in the base sample are missing test scores. This is mostly due to students dropping out of school or moving out of the district between October and the testing period in late winter. Some students also are missing test scores due to illness or suspension during the testing period.

[14] Unfortunately, regressions looking at Aprenda scores alone provide estimates that were too imprecise to draw any conclusions.

[15] Estimates restricting the behavior regressions to the test sample provide similar results.

[16] For a small number of schools addresses could not be matched to a location. Students in these schools were dropped from the analysis.

[17] Researchers wishing to conduct replication studies may contact the author for district identification and instructions for requesting permission from LUSD for data access.

follow the previous literature by incorporating student fixed-effects into the regressions. This will sufficiently correct for student selection based on time-invariant characteristics of the students, such as their parents' motivation.

Second, the location of charter schools is non-random. Charter location may be affected by space availability, transportation options, economic conditions, and the quality of nearby public schools. This is not a problem if these factors are uncorrelated with student and traditional public school characteristics. However there may be higher demand for alternative schooling options in areas with low-performing schools. Indeed, many charters are created through grass roots organizing in a community, often in response to the poor quality of the local schools.

Depending on the nature of this selection, the bias in the charter impact estimates could be positive or negative. If charters locate near low-performing schools based on time-invariant characteristics of the public schools (i.e. the charters locate near schools which have been low performing for many years and have shown little improvement or worsening), then simple OLS regressions would underestimate the effects of charters. Researchers have addressed this type of selection by including school fixed-effects in OLS regressions along with student fixed-effects. However, if location is, at least partially, based on time-variant characteristics of non-charter schools then this strategy will not eliminate, and in fact may exacerbate, the bias. One possible way this can occur is if charters locate in areas where performance is worsening on the belief that this will generate higher demand in the future. Since many charters face high start up costs and credit constraints and thus open with few students and expand later, having an anticipated increase in demand could be desirable. Another mechanism for this selection would be if parents and community leaders start charter schools when they see performance in the public schools worsening. The direction of this type of bias depends on the school's counterfactual outcome trajectory. If the public schools were to continue on their downward trends, then the estimated charter impact with school fixed effects will account for this trend as part of the treatment effect. Hence, the charter impact estimate will be biased downward. If, on the other hand, public schools that attract charters are temporarily on downward trends which then undergo mean reversion, the charter impacts will be over-estimated, and in fact we could even estimate positive charter impacts when the true effect is negative.

Table 3 presents some suggestive evidence that charters tend to locate near schools with worsening performance. The model presented here regresses the number of charter schools within 1.5 miles with grade coverage that overlaps the public school's grade coverage on passing rates on the state accountability exam lagged one, two, three, and four years along with some school characteristics. The estimates show that there is no significant relationship between the number of charters nearby and passing rates four years prior. However, having a 10 percentage point lower passing rate 3 yr and 2 yr prior is associated with having 0.3 and 0.6 more charters nearby, respectively. This correlation falls to statistical insignificance in year t − 1, but note that charters usually take more than one year to set up. Hence, these results provide suggestive evidence that charters location is correlated with trends in local TPS quality.

One way to address this issue is to instrument for charter penetration. I argue that characteristics of the pre-existing stock of buildings, in particular the availability of appropriately sized building space on nearby properties and the location of both large and small shopping centers are plausibly exogenous instruments for charter school location. I use these supply characteristics as instruments in lieu of school fixed effects. While ideally one would like to include both the instruments and school FEs, I must rely on pre-existing building supply rather than concurrent supply as this could pick up local economic trends. Although I interact the instrument with average charter share to gain time variation, it is still undesirable to include school-fixed effects as this would remove variation from the building supply variables. Nonetheless, given that the instruments are valid, they should address selection of charter location based on both time-variant and time-invariant unobservable characteristics. Hence, the IV estimates will be consistent regardless of whether the school fixed-effects model is consistent or not, although in the absence of time-variant selection bias the fixed-effects model may be more efficient.

Table 3
Trends in state exam passing rates in non-charter schools prior to charter entry.

	Number of charters with overlapping grades within 1.5 miles
Passing rate in year t − 1	− 0.0024
	(0.0024)
Passing rate in year t − 2	− 0.0060***
	(0.0021)
Passing rate in year t − 3	− 0.0034**
	(0.0016)
Passing rate in year t − 4	− 0.0007
	(0.0017)
% of enrollment African-American	− 0.0087
	(0.0058)
% of enrollment Hispanic	− 0.0080
	(0.0062)
% of enrollment economically disadvantaged	0.0015
	(0.0054)
Observations	1455
R-squared	0.13

Regressions are at the school level and cover 1993–2001. After 2001, the state switched to a new exam. Regressions also include the share of enrollment in each grade level and year dummies. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

4.2. Defining charter penetration

In order to assess the impact of charter schools on non-charter students, I first need to identify which charters are geographically close enough to affect the traditional public school. The extent of concentration of charters nearby can be referred to as "charter penetration."[18] There are two issues to consider when measuring charter penetration at the school level. First, for a given geographic area, what is the proper measure of charter penetration? Previous work has used the number of charters near a traditional public school and the share of total enrollment in charter schools (Booker et al., 2008; Bifulco and Ladd, 2006; Sass, 2006; Holmes et al., 2003). I use a modification of the second measure which uses enrollment only in the grades covered by the regular public school. I believe this measurement best reflects the pressures that traditional public schools face from charters. Thus, I define charter penetration as follows.

Define a set of schools within a distance (d) of school j, including j as $J = 1, 2, \ldots, N_T^d, N_T^d + 1, N_T^d + 2, \ldots, N_T^d + N_C^d$ where N_T^d is the total number of traditional public schools and N_C^d is the total number of charter schools. Charter penetration is calculated prior to dropping students with missing data from the sample as

$$ChartPen_{jt}^{d} = \frac{\sum_{g = Gmin_j}^{Gmax_j} \sum_{l = N_T^d + 1}^{N_T^d + N_C^d} Enroll_{glt}}{\sum_{g = Gmin_j}^{Gmax_j} \sum_{l = 1}^{N_T^d + N_C^d} Enroll_{glt}} \tag{1}$$

where $Gmin$ and $Gmax$ are the lowest and highest grades, respectively for school j and $Enroll_{gnt}$ is enrollment in grade g, school l and year t. For example, suppose I am measuring charter penetration within

[18] Hoxby (2001) proposes using district-wide share of enrollment in charter schools as a measure. However, such a definition is infeasible when considering school level penetration.

1.5 miles of a school, j, that serves grades kindergarten through five. In this case, the denominator is the total number of students attending public and charter schools within 1.5 miles (including those in j) who are in grades kindergarten through five. For the numerator, I do the same calculation, but limit only to charter schools. Note that if school j is a district charter, its enrollment is excluded from the numerator. Hence, my charter penetration measure is the fraction of all public school and charter school students in overlapping grades who attend a charter school within a geographic radius around the observed school.[19]

The second issue is how geographically disperse is a school's "market area" within which it is subject to competitive pressures from charters. A necessary condition for this pressure to exist is that there is the potential for charters to draw students away from regular public schools. While I cannot directly test this potential, I can investigate whether increases in charter enrollment are associated with reductions in enrollment in nearby regular public schools. Table 4, columns (1) and (2) show results from regressions of the form

$$Enroll_{jt} = \alpha + \beta ChartEnroll_{jt}^{d} + \mathbf{X_{jt}}\Psi + \epsilon_{jt} \tag{2}$$

where $Enroll_{jt}$ is enrollment in a regular public school j at time t, $ChartEnroll_{jt}^{d}$ is total enrollment in charter schools within d miles of the regular public school and X includes year effects and school fixed-effects. Column (1) uses total enrollment in any charters and column (2) uses total enrollment in only state charters. Column (3) modifies Eq. (2) to use the measure of charter share described above. Each specification consistently shows a negative association between charter penetration and enrollment which phases out completely by a distance of 2 miles from the school.[20]

This suggests that in LUSD, any regular public school would likely only be affected by charters which open within 2 miles of their boundaries. Thus, for the purposes of this paper, I focus my attention on schools where charters open within relatively short distances. In particular I use 1.5 miles from the regular public schools as distance measures.[21] Given the size of school zones in LUSD, this is a reasonable distance measure. The average elementary school zone has an area of 1.77 square miles, implying that if school zones were circular, the radii would average 0.75 miles. Hence, a market area of twice this distance from a school would be reasonable. Since the corresponding radii measures for middle and high schools are 1.6 miles and 2.0 miles, respectively, I also provide models using 2 mile radii.

4.3. Initial OLS model

I begin with an equation of the form

$$y_{ijt} = \alpha + \beta C_{jt}^{d} + \mathbf{X}_{ijt}\Omega + \mathbf{G}_{it}\Theta + \epsilon_{ijt} \tag{3}$$

where y_{ijt} is an outcome measure for student i in school j during academic year t, C^d is the a measure of charter penetration within a radius d of the regular public school j, $\mathbf{X}$ is a set of observable student characteristics, G_{it} is a set of grade-by-year indicators, and ε is an error term.

Table 4
Relationship between charter penetration and school enrollment (1993–2004).

	Any charter enrollment	State charter enrollment	Any charter share
0–1 mile	−0.054*	−0.101**	−112.4*
	(0.028)	(0.051)	(57.7)
1–2 miles	−0.023*	−0.071**	−163.4**
	(0.013)	(0.029)	(71.5)
2–3 miles	0.016	0.010	40.4
	(0.012)	(0.023)	(86.2)
Observations	3423	3423	3423

Dependent variable is total school enrollment. Observations are school-year aggregates. Regressions also contain year and school fixed effects. Robust standard errors clustered by school are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

I follow Booker et al. (2008) and Sass (2006) in using a restricted value-added model where achievement is measured as annual changes of test scores standardized within grade and year while behavior and attendance are raw year-to-year changes.[22]

ϵ_{ijt} can be broken down into student and school error components

$$\epsilon_{ijt} = \gamma_{ijt} + \eta_{jt}. \tag{4}$$

An important concern is that both γ_{ijt} and η_{jt} could be correlated with C_{jt}^{d} through some unobserved factors.

4.4. Student selection into schools

One part of this problem is the potential that $cov(\gamma_{ijt}, C_{jt}^{d}) \neq 0$, i.e. that something unobservable is driving student selection into schools facing more or less charter competition. The most obvious type of selection is that only certain types of students may leave non-charters for charter schools. As was shown in Table 1, students who attend charter schools appear to differ considerably from LUSD-SW traditional public school students. Thus the loss of these students from schools with a large amount of charter penetration could bias the results. Another type of selection is that students may remain in LUSD-SW but change schools in response to charter competition.

I use a student-fixed effects strategy to address this problem as in Booker et al. (2008), Bifulco and Ladd (2006), and Sass (2006). More precisely, I assume that

$$\gamma_{ijt} = \lambda_i + \nu_{ijt} \tag{5}$$

where $cov(\lambda_i, C_{jt}^{d}) \neq 0$ but $cov(\nu_{ijt}, C_{jt}^{d}) = 0$. Under this assumption we can remove λ from Eq. (3) by demeaning the model with respect to the individual as such

$$\tilde{y}_{ijt} = \tilde{\alpha} + \beta \tilde{C}_{jt}^{d} + \tilde{\mathbf{X}}_{ijt}\Omega + \tilde{\mathbf{G}}_{it}\Theta + \tilde{\nu}_{ijt} + \tilde{\eta}_{jt}. \tag{6}$$

where $\tilde{B} = B_{ijgt} - B_i + \bar{B}$.[23]

4.5. Endogenous charter school location

While student fixed-effects correct for student selection under the assumption stated above, if charter location is endogenous then $cov\left(\tilde{\eta}_{jt}, \tilde{C}_{jt}^{d}\right) \neq 0$. One way to address this type of selection is to use school fixed-effects. For this strategy to be valid it must be that

$$\tilde{\eta}_{jt} = \tilde{\zeta}_j + \tilde{\theta}_{jt} \tag{7}$$

[19] I exclude from this measure five residential treatment facilities for substance abusers or juvenile detention centers since enrollment in these is not voluntary. I also exclude one internet based charter and one school that helps adults with credit repair.

[20] Regressions using years 1996–2004 and 1998–2004 provide qualitatively similar results.

[21] Previous papers which look at charter impacts on non-charter schools use considerably varying distances. Bifulco and Ladd (2006) and Sass (2006) use 2.5, 5, and 10 miles, while Holmes et al. (2003) use distances ranging from 5 to 20 km (3.1 to 12.4 miles) and also use the county as the local education market. These longer distances are more appropriate in the context of these papers, since their data include many suburban and rural areas where school attendance zones are larger. However, my results do suggest that the proper distance should vary with urbanicity.

[22] Sass also uses an unrestricted model that has lagged test scores as an explanatory variable. Estimates using this model, as well as models using a levels framework are similar.

[23] This is the equivalent of the method used by the "xtreg, fe" command in Stata™.

where $cov\left(\tilde{\zeta}_j, \tilde{C}_{jt}^d\right) \neq 0$ and $cov\left(\tilde{\theta}_{jt}, \tilde{C}_{jt}^d\right) = 0$. Under this assumption we can add school indicator dummies to the regression which will eliminate ζ_j. Thus, our regression equation becomes

$$\tilde{y}_{igjt} = \beta\tilde{C}_{jt}^d + \tilde{\mathbf{X}}_{ijt}\Omega + \tilde{\mathbf{G}}_{it}\Theta + \tilde{\mathbf{S}}_{jt}\Gamma + \tilde{\nu}_{ijt} + \tilde{\theta}_{jt}. \tag{8}$$

where **S** is the vector of school indicators. However, if charters select locations based on trends in local school performance, or, similarly, if grass root efforts to create charters are spurred by trends in local schooling conditions as suggested by the results in Table 3, then Eq. (7) will be incorrect and including school indicators will not correct for selection. One possible solution to this problem is to use an instrumental variables strategy.

I propose using characteristics of building stock near non-charter public schools as an instrument for charter share. The idea behind this instrument is that certain types of buildings are better suited for a school then others. Often charters need to rent space or use donated space because they do not have funds available to build their own buildings. In particular, charters in LUSD often locate in shopping centers because they have a lot of space available, can be easily renovated to accommodate building classrooms, and are easy to rent. These shopping centers range in size from small strips of five units to large multi-unit commercial and retail complexes. In fact 14% of state charters in LUSD borders are located in current or former shopping centers.[24] In addition charter schools are more likely to locate on plots of land with certain amounts of building space. If the building space is too small, then the charter will not have enough room to operate. If it is too large, then much of the space goes unused and the charter is unlikely to be willing to pay the rent premium.[25] In addition, properties with particularly large amounts of building space are often hi-rise office buildings, warehouses and apartment complexes, properties that are ill-suited for charter schools. Also, since charter schools tend to rent or use donated space as little funding is available to build new structures, the availability of existing building space is particularly important.

My data on building supply comes from the county tax appraisal office and is based on their 1995 tax records. I use the year 1995 to address the concern that building supply could be correlated with local economic trends. Since 1995 is prior to the opening of charters in LUSD-SW, I avoid concurrent changes in building supply and charter share as local economic conditions vary over time.

For the buildings space variable, I use properties with between 20,000 and 50,000 ft^2 of building space as this closely corresponds to the 25th to 75th percentile of building space for charter schools whose current address I could match to property records.[26] After removing one outlier that is housed in a large apartment complex, average building space for properties housing charters is 42,367 ft^2 and the median is 34,647 ft^2.[27] Additionally, this range corresponds well to the middle of the distribution for public schools in general. In LUSD properties that I am able to identify as housing public schools have a median size of 26,851 ft^2 and an inter-quartile range of 7390 to 59,942 ft^2.[28] Later, I provide specification tests that use different square-footage ranges.[29] These results are similar to baseline.

[24] Schools' 2008 addresses were matched, when possible, to property records available from 1995 to 2001. If schools could not be matched then a visual inspection was done using Google Street View™. One charter in LUSD's boundaries could not be matched via property records or visually to a building type.

[25] Discussions with a superintendent in charge of more than 25 charter schools in LUSD's state suggest that the availability of a suitable location with enough space is a constraint on siting.

[26] The 25th percentile is 18,252 ft^2 and the 75th percentile is 48,750 ft^2.

[27] I use the number of properties rather than percentage since the each available property will increase the options available to a new charter and thus increase the likelihood that they will locate near the public school. A percentage measure does not capture this increase in probability as well as a count measure.

[28] I identify public schools by using properties that are listed as being for school use, owned by tax-exempt government entities, and have non-zero building space.

[29] Histograms of the distributions of building space for charters and public schools are provided in the online appendix.

Table 5
Relationship between instruments and existing school characteristics.

School characteristics in 1994–95	Instruments: # properties with 20k–50k ft^2 of building space in 1995 within 1.5 miles	Instruments: # of shopping centers or strip malls in 1995 within 1.5 miles
% of enrollment African-American	−0.102	−0.525***
	(0.300)	(0.136)
% of enrollment Hispanic	0.360	−0.473***
	(0.334)	(0.151)
% of enrollment economically disadvantaged	0.101	0.097
	(0.283)	(0.120)
Per-Student Total Operating Expenditures ($100)	0.260	−0.036
	(0.484)	(0.143)
Passing rate on state exam	0.067	−0.011
	(0.218)	(0.070)
Student–teacher ratio	−0.838	0.069
	(2.300)	(0.672)
Average teacher experience	−1.00	−0.544
	(1.07)	(0.391)
Observations	248	248
R-squared	0.21	0.39
Instrument mean	66.5	20.3

Regressions are at the school level and also include the share of enrollment in each grade level as controls. Robust Huber/White standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

My second instrument is the number of shopping centers within a specified distance radius.[30] Shopping centers are useful as instruments as they are attractive to charters due to their flexible interior design options and can easily be rented. Indeed, discussions with a superintendent of a large charter school district in LUSD revealed that they revealed that they like to use abandoned grocery or large retail stores that often reside in shopping centers for their schools. It is also important to note that it is unlikely that shopping centers open with the intention of drawing charters as anchor tenants.

One concern regarding these instruments is that they are time-invariant. Hence all of the variation in a model using these instruments is cross-sectional. To address this, I fully interact the instruments with the average charter share across the district in each year.[31] This allows the impact of the building supply to vary with the amount of charter penetration overall. Hence, denoting B_j^d as number of properties with 20,000 to 50,000 ft^2 of building space within d miles of school j, S_j^d as the number of shopping centers and strip malls within d miles of school j, and A_t as the average charter share across all schools in LUSD in year t, my total set of instruments includes B_j^d, S_j^d, $B_j^d \times S_j^d$, $B_j^d \times A_t$, and $S_j^d \times A_t$. Note that A_t itself drops out due to the year fixed-effects in the regressions.

Table 5 provides regressions that show the relationship between my proposed instruments and characteristics of the local schools prior to the opening of the first charters in LUSD. For the instruments to be valid, we would expect to see little relationship between these characteristics and the instruments. For the availability of buildings between 20k and 50k ft^2 of space this is clearly the case. There is no statistically significant relationship with racial composition of the school, the size of the disadvantaged population, expenditures, test scores, teacher–student ratios, or teacher experience. In addition, in all

[30] LUSD has the unique characteristic that it is located in a city with few zoning restrictions. Thus commercial and residential areas are often integrated and thus there are is considerable variation in the instruments across schools. For the 272 traditional public schools in operation in 2004 the average number of properties within 1.5 miles with 20,000 to 50,000 ft^2 of building space is 64.5 with a 10th percentile of 16, median of 58, and a 90th percentile of 127. For shopping centers the average is 21.2 with a 10th percentile of 5, median of 16, and a 90th percentile of 46.

[31] To make the point estimates easier to read, in estimations the average charter share is normalized to be equal to one in 2004. Note that this is simply dividing the average charter share by a constant across the sample and hence has no impact on the other estimates.

Table 6
2SLS first stage.

Instrument(s)	(1)	(2)	(3)	(4)
A. Test score regressions				
# properties with 20k–50k ft^2 of building space in 1995 within 1.5 miles	0.00040***		0.00028***	−0.00093***
	(0.00007)		(0.00008)	(0.00025)
# of shopping centers or strip malls in 1995 within 1.5 miles		0.00092***	0.00053**	0.00274***
		(0.00019)	(0.00021)	(0.00049)
Building space*district-wide average charter share				0.00152***
				(0.00034)
Shopping centers*district-wide average charter share				−0.00281***
				(0.00062)
Observations	602,864	602,864	602,864	602,864
F-test of joint significance	32.0***	24.2***	17.8***	14.3***
B. Behavior regressions				
# properties with 20k–50k ft^2 of building space in 1995 within 1.5 miles	0.00030***		0.00024***	−0.00010
	(0.00005)		(0.00006)	(0.00008)
# of shopping centers or strip malls in 1995 within 1.5 miles		0.00058***	0.00025*	0.00029**
		(0.00012)	(0.00014)	(0.00014)
Building space*average charter share				0 00074***
				(0.00018)
Shopping centers*average charter share				−0.00023
				(0.00040)
Observations	1,543,969	1,543,969	1,543,969	1,543,969
F-test of joint significance	34.1***	22.2***	19.0***	11.9***

Charter penetration measure is share of enrollment in overlapping grades within 1.5 miles All regressions include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Each regression is also demeaned within individuals to remove student fixed-effects. Note that "average charter share" main effects are subsumed by year dummies. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

but one of these cases – % Hispanic – the t-statistics are below one. For the second instrument – the number of shopping centers – the only statistically significant relationship is with racial composition. Schools with more African-American or Hispanic students are found to have fewer shopping centers nearby. Nonetheless, there are a few reasons why this is unlikely to substantially affect the estimates. First, LUSD is a heavily minority district — 87% of students are black or Hispanic. Hence, very few schools have substantial non-minority populations. Second, the shopping centers instrument has no significant relationship with the economically disadvantaged population or test scores and in both of these cases t-statistics are considerably lower than one. Third, I provide a series of specification tests that limit only to schools that are heavily minority, control for race and disadvantaged share in schools, control for local economic conditions, or use only the building space instrument. In all of these cases I find results that are qualitatively similar to the baseline model.[32]

Defining $\mathbf{I}_{\mathbf{jt}}^{\mathbf{d}}$ as the full set of instruments described above, the first and second stages of the baseline 2SLS model can hence be expressed as

$$\tilde{C}_{jt}^{d} = \tilde{\mathbf{I}}_{\mathbf{jt}}^{\mathbf{d}}\Gamma + \tilde{\mathbf{X}}_{\mathbf{ijt}}\Omega + \tilde{\mathbf{G}}_{\mathbf{it}}\Theta + \tilde{\mu}_{\mathbf{ijt}}. \tag{9}$$

$$\tilde{y}_{ijt} = \beta\widehat{\tilde{C}_{jt}^{d}} + \tilde{\mathbf{X}}_{ijt}\Omega + \tilde{\mathbf{G}}_{it}\Theta + \tilde{\upsilon}_{ijt}. \tag{10}$$

where a tilde reflects demeaning within individuals.

Table 6 shows the first-stage results for the 2SLS estimates using the building supply variables on their own in columns (1) through (3) and the fully interacted model in column (4). Panel A provides estimates using the test sample and panel B shows estimates in the behavior sample. For both samples building space and shopping centers are statistically significant at the 1% level when each is used on its own, showing that there is a strong positive relationship between charter location and building supply. Even when both are used in the same regression (column 3), each instrument remains statistically significant. Using both the instrument levels and interactions with average charter share in column (4) the instruments provide significant F-tests of 14.3 in the test sample and 11.9 in the behavior sample.[33]

5. Results

5.1. Test scores

Table 7 provides the main set of estimates for this paper. All regressions include student fixed-effects. I also control for free lunch eligibility, reduced price lunch eligibility, whether the student is otherwise identified as economically disadvantaged, whether the student's parents are currently classified as migrant workers, and grade-by-year indicators. The estimates shown here are interpreted as a ten percentage point increase in charter share changing outcomes by the coefficient divided by ten.

Panel A shows the estimates for Stanford 9/10 growth rates in standard deviation units. In column (1) I provide OLS estimates without school fixed-effects. All three exam subjects – math, reading, and language – show no statistically significant relationship with charter penetration. Column (2) adds school dummies to account for school fixed-effects. This is the type of model that has been commonly used in the previous literature.[34] As such, the results are broadly consistent with

[32] Sargon–Hansen over-identification tests for all of the baseline regressions are statistically insignificant at the 5% level in all but one specification.

[33] A potential concern here is that the estimate on the interaction for shopping centers is negative. While this may seem odd, it is consistent with charters moving from shopping centers to other buildings over time. Indeed, I find that of the charters operating in 2004 that I can match to a property, 17% were in shopping centers in 1999 but only 10% were in those locations in 2003 (this number increases to 14% in 2004). This could be due to charters gradually finding shopping centers less desirable or from charters that initially locate in shopping centers moving to more permanent locations. I also note that the marginal effect of adding a shopping center on charter share never becomes significantly negative over the course of the sample. Nonetheless, one way to address this concern is to estimate the model using only the building space instrument and its interaction with average charter share. Estimates from this model are similar to the main model and are provided in the online appendix.

[34] Estimates using school and student fixed effects may not be comparable to other research if students rarely switch schools in the data. In the LUSD data, this does not appear to be a major concern. 52% of students in the behavior sample and 44% of students in the test sample are observed in two or more schools.

Table 7
Estimates of charter school impacts on public school students.

Instrument(s)	(1)	(2)	(3)	(4)	(5)	(6)
	OLS with student fixed effects	OLS with student and school fixed effects	2SLS	2SLS	2SLS	2SLS
			# properties with 20k–50k ft^2 of building space in 1995 within 1.5 miles	# of shopping centers or strip malls in 1995 within 1.5 miles	Building space and shopping centers	Building space, shopping centers, district-wide avg charter share and interactions
A. Stanford 9/10 (standard deviation units)						
Math	0.006	0.088	−0.641**	−0.752***	−0.688***	−0.469***
	(0.053)	(0.079)	(0.289)	(0.240)	(0.235)	(0.158)
Reading	−0.031	0.026	−0.219	−0.180	−0.203	−0.044
	(0.035)	(0.058)	(0.174)	(0.155)	(0.142)	(0.118)
Language	0.012	0.088	−0.565***	−0.673***	−0.610***	−0.404***
	(0.037)	(0.060)	(0.199)	(0.203)	(0.179)	(0.141)
Observations	602,864	602,864	602,864	602,864	602,864	602,864
B. Behavior measures						
# disciplinary infractions	−0.15	0.31	−4.96***	−8.59***	−5.92***	−2.01**
	(0.34)	(0.25)	(1.42)	(3.28)	(1.64)	(0.83)
Attendance	−1.60**	−0.77*	15.68**	18.44*	16.41**	3.70
	(0.67)	(0.46)	(7.83)	(9.45)	(7.82)	(2.89)
Observations	1,543,969	1,543,969	1,543,969	1,543,969	1,543,969	1,543,969

Charter penetration measure is share of enrollment in overlapping grades within 1.5 miles All models are restricted value-added and instruments are set to equal zero in 1995 and earlier since the first charters opened in 1996. All regressions include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Each regression is also demeaned within individuals to remove student fixed-effects. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

what has been found in those papers. All three exams show small and statistically insignificant, but positive, estimates of less than 0.01 standard deviations for a 10 pp increase in charter share.

Columns (3)–(5) provide estimates using only the building supply variables as instruments. Column (3) uses the building space instrument, column (4) uses shopping centers, and column (5) uses both. In all three cases the estimates are similar, showing a drop in both math and language scores of 0.06 to 0.07 sd for a 10 pp increase in charter share. There is no statistically significant effect on reading scores. The similarity of these estimates show that the results are not sensitive to the choice of building supply instruments, and thus enhances the validity of my econometric framework as one only one of the two instruments needs to be valid for these results to be consistent.

Column (6) shows my preferred estimates using the fully interacted set of instruments.[35] These estimates are smaller than the estimates in columns (3)–(5) but they are also more precise. Thus the math and language estimates are significant at the 1% level and show reductions of 0.05 and 0.04 standard deviations, respectively, for a 10 percentage point increase in charter share. The impact on reading scores is not statistically significant and close to zero. Hence, if a school increases from no charter penetration within 1.5 miles to the 2004–05 average of 7% then math and language score growth drops by 0.04 and 0.03 standard deviations, respectively. This is a relatively modest reduction, but nonetheless differs substantially from the positive estimates in the school fixed effects analysis.[36]

5.2. Behavior

In panel B of Table 7 I provide results for discipline and attendance impacts. The discipline measure I use is the number of times a student is given an in-school suspension or more severe punishment over the course of the year while attendance is the percent of enrolled days a student is in school. These two measures are important indicators of non-cognitive skill formation. A potential problem, however, is that disciplinary infractions is a measure of both outcomes and enforcement. As a result, Imberman (forthcoming) argues that while impacts on disciplinary infractions alone may not indicate improved behavior, if it is concurrent with improvements in attendance than we can interpret the results as improvements in behavior due to non-cognitive skill formation.

In columns (1) and (2) OLS estimates show essentially no relationship between charter share and disciplinary infractions regardless of whether I include school fixed-effects in the regressions. However there is a statistically significant and negative relationship with attendance rates. In columns (3)–(5) we again see similar, albeit somewhat imprecise, estimates regardless of whether I use building space, shopping centers, or both as the instruments. The results suggest that behavior and attendance improve in schools after charters move in. Nonetheless, the estimates become closer to zero when I use my preferred set of instrument interactions, in part because of increased precision. Disciplinary infractions remain negative and statistically significant at 0.2 fewer infractions per year for a 10 pp increase in charter share. The attendance estimate, while still positive, is statistically insignificant. Thus, it is possible that the discipline improvement only reflects adjustments in enforcement. While it may seem odd that charters induce improvements in behavior but reduce test scores, later I show that the test score impacts are coming almost entirely from elementary schools while the discipline improvements are from middle and high schools.

5.3. Specification checks

In Tables 8 and 9, I provide some specification checks to test potential concerns with the 2SLS strategy. Table 8 shows models where I modify the instruments. Column (1) provides results where I include interactions of the instruments with a linear time-trend as a control. This tests the sensitivity of the IV estimates to accounting for local trends in building supply. The results are similar to baseline.

While the results in Table 5 show the instruments to be largely uncorrelated with pre-existing school characteristics, one might still be concerned that the estimates are more likely to be biased if they rely primarily on cross-sectional variation in the instruments. To address

[35] Reduced form results are available upon request.
[36] Tests of exogeneity of charter share fail at the 1% level for math and language scores as well as for discipline, supporting the argument that OLS estimates suffer from selection bias.

Table 8
2SLS specification checks — instrument variations.

	Control for building space and shopping centers interacted with linear time trends	Control for levels of building space and shopping centers	Control for building space and shopping centers interacted with linear time trends and levels of building space and shopping centers	Use district-wide share in overlapping grades instead of average district-wide charter share	Building space instrument uses 10k–60k ft^2	Building space instrument uses 30k–40k ft^2	Divide building space by # of commercial properties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Stanford 9/10 (standard deviation units)							
Math	−0.841***	−0.318	−0.302	−0.313**	−0.350**	−0.500***	−0.676***
	(0.310)	(0.234)	(0.689)	(0.146)	(0.171)	(0.172)	(0.181)
Reading	−0.327*	0.067	−0.104	0.069	0.019	−0.034	−0.134
	(0.193)	(0.180)	(0.478)	(0.115)	(0.120)	(0.118)	(0.107)
Language	−0.547**	−0.261	−0.392	−0.194*	−0.369**	−0.450***	−0.149
	(0.214)	(0.207)	(0.484)	(0.115)	(0.156)	(0.142)	(0.121)
Observations	602,864	602,864	602,864	602,864	602,864	602,864	602,864
B. Behavior measures							
# disciplinary infractions	−3.74**	1.55*	2.10	−1.47*	−2.60***	−2.78**	−6.38**
	(1.69)	(0.91)	(1.66)	(0.87)	(0.90)	(1.28)	(2.83)
Attendance	3.89	−7.85**	−7.64	1.40	6.17	3.00	6.58*
	(3.22)	(3.60)	(4.77)	(2.42)	(3.80)	(2.95)	(3.760)
Observations	1,543,969	1,543,969	1,543,969	1,543,969	1,543,969	1,543,969	1,278,375

Charter penetration measure is share of enrollment in overlapping grades within 1.5 miles All regressions include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Each regression is also demeaned within individuals to remove student fixed-effects. Instruments are the full interaction of the building space, shopping center, and average charter share variables. See text for details. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

this, I estimate models in column (2) that control for the levels of the building space and shopping center instruments in both stages. In column (3) I also control for the interactions of the instruments with time trends. This ensures that the estimates are identified only off of non-linear temporal variation in the instruments. In both cases, although they become somewhat smaller and lose significance, the math and language estimates remain negative and are of the same order of magnitude as baseline, while reading estimates remain close to zero. Hence when I limit to using only temporal variation in the instruments, the achievement results are qualitatively similar. For discipline and attendance on the other hand, the estimates change sign, showing that both cross-sectional and temporal variation are contributing to the baseline results. Nonetheless, even if one is concerned about the variation contributing to the behavior results, the achievement results – which are the primary focus of this paper – are robust.

In column (4) I provide estimates that interact the instruments with the district-wide charter share in overlapping grades rather than the overall district-wide charter share. This has the benefit of potentially improving precision. Nonetheless, a potential drawback is that charters may choose their grade structure in response to demand patterns, and hence this could introduce endogeneity into the estimates. Even so, although the math and language estimates shrink, they remain significant and other estimates are similar to baseline. Columns (5) and (6) test the sensitivity of the estimates to

Table 9
2SLS specification checks — school and neighborhood controls, sample restrictions.

	75% or higher minority schools only	60% or higher minority schools only	Control for Black, Hisp, disadv share	Control for quadratic in comm property	Control for zip code FE, area characteristics	Combine controls in (4) and (5)	Combine controls in (4) and (5) using only penetration from state charters	Include school fixed-effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Stanford 9/10 (standard deviation units)								
Math	−0.245	−0.307**	−0.404**	−0.486***	−0.366	−0.261	−0.920*	0.028
	(0.161)	(0.153)	(0.164)	(0.170)	(0.240)	(0.274)	(0.483)	(0.419)
Reading	0.054	0.001	0.019	0.042	0.199	0.255	0.416	0.077
	(0.135)	(0.128)	(0.118)	(0.128)	(0.172)	(0.213)	(0.354)	(0.281)
Language	−0.237*	−0.319**	−0.374***	−0.286**	−0.378*	−0.347	−0.981**	−0.440
	(0.139)	(0.135)	(0.137)	(0.145)	(0.201)	(0.240)	(0.461)	(0.344)
Observations	459,865	498,754	602,864	602,864	602,864	602,864	602,864	602,864
B. Behavior measures								
# Disciplinary infractions	−2.57**	−2.94***	−2.55***	−2.32**	−2.67***	−0.66	−2.92*	−0.87
	(1.14)	(1.11)	(0.88)	(1.14)	(0.93)	(0.74)	(1.71)	(0.86)
Attendance	1.48	3.49	3.53	1.15	6.09**	0.85	12.7**	0.94
	(2.34)	(2.83)	(2.92)	(2.74)	(3.09)	(2.82)	(5.76)	(2.83)
Observations	1,097,612	1,209,357	1,543,969	1,543,969	1,543,969	1,543,969	1,543,969	1,543,969

Charter penetration measure is share of enrollment in overlapping grades within 1.5 miles All regressions include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Each regression is also demeaned within individuals to remove student fixed-effects. Instruments are the full interaction of the building space, shopping center, and average charter share variables. See text for details. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 10
2SLS estimates of charter school impacts on traditional public school students — by grade, race and gender.

	Grade Level		Gender		Race		
	Elementary	Middle/High	Boys	Girls	Black	Hispanic	White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Stanford 9/10 gains (standard deviation units)							
Math	−0.571*	−0.134	−0.335**	−0.614***	−0.404**	−0.307	−0.986
	(0.294)	(0.186)	(0.145)	(0.197)	(0.200)	(0.194)	(0.754)
Reading	−0.518**	0.013	0.062	−0.154	−0.059	0.164	−0.621
	(0.219)	(0.133)	(0.122)	(0.140)	(0.177)	(0.141)	(0.404)
Language	−0.725***	0.024	−0.372**	−0.441***	−0.399**	−0.068	−0.904**
	(0.225)	(0.135)	(0.161)	(0.152)	(0.197)	(0.117)	(0.443)
Observations	242,355	360,509	301,968	300,896	218,825	291,921	70,274
B. Behavior measures							
# Disciplinary infractions	0.16	−1.75**	−2.59**	−1.38**	−3.86***	−0.82	−1.81*
	(0.21)	(0.88)	(1.05)	(0.62)	(1.23)	(0.95)	(0.96)
Attendance	−1.48	5.06	3.10	4.32	1.58	5.69**	8.40*
	(0.90)	(3.79)	(3.06)	(2.81)	(4.38)	(2.87)	(4.72)
Observations	674,741	869,228	782,548	761,421	509,748	829,899	159,756

Charter penetration measure is share of enrollment in overlapping grades within 1.5 miles. Elementary includes all students in grades 1–5 and middle\high includes all students in grades 6–12. All regressions include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade * year dummies as covariates. Each regression is also demeaned within individuals to remove student fixed-effects. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

using a wider and narrower range of building space, respectively. In all both cases the results are similar to the main model.

Another potential concern is that the building space instrument may be picking up whether the school is in a commercial or residential area, as being in a commercial area may be correlated with economic conditions. Hence, in column (7) I use the baseline model but divide the building space instrument by the number of commercial properties within 1.5 miles. The results are generally robust to the instrument modification. The one exception is that while math and discipline remain significant and negative, language scores drop to statistical insignificance, although the estimate remains negative.

Table 9 provides an additional set of specification checks where I use the fully interacted set of instruments but change control variables or the sample. Columns (1), (2) and (3) attempt to address the correlation of the shopping center instrument with racial makeup of the TPS seen in Table 5. In column (1) I limit to schools that are at least 75% black and Hispanic so that the sample only includes heavily minority schools. In column (2) I weaken this restriction to 60% minority. While the estimate for math falls to statistical insignificance in column (1) language and discipline remain significant, albeit with language only significant at the 10% level. In column (2) both math and language are significant at the 5% level. In addition, I note that a drop in the estimates also consistent with heterogeneous impacts by race, for which I show some evidence below. In column (3) I control directly for the share of each school that is black, Hispanic, or economically disadvantaged. These estimates are similar to the baseline model with statistically significant and negative estimates for math, language, and disciplinary infractions. Hence, the results are robust to the racial composition of the school.

Columns (4), (5), and (6) provide models that account for local neighborhood characteristics. One concern with the instruments is that they may be picking up neighborhood quality which could spill over into student outcomes. Hence in column (3) I control for a quadratic in the number of commercial properties within 1.5 miles to account for whether the school is in a more heavily commercial area. In column (4) I control for zip-code fixed effects and characteristics of the school's census tract.[37] Both of these models show results similar to the baseline model. I combine these covariates in the model provided in column (5). While math and language drop to statistical insignificance, this is partly due to an increase in the standard error, particularly in the case of language. The language point estimate is −0.35 compared to −0.40 in the main model, while for math the estimate drops to −0.26. Nonetheless, in column (7) I show that if we ignore district charters in the charter penetration measure, we still get negative and statistically significant estimates at the 10% level for both math and language. Discipline and attendance results are similar across all specifications. Hence the baseline model is generally robust to controls for neighborhood characteristics.[38] Finally, in column (8) I provide estimates that include school fixed-effects. Since these estimates remove a substantial amount of variation, they are less precise than the baseline estimates.[39] Nonetheless, with the exception of math achievement, the estimates are qualitatively similar to baseline.

5.4. Heterogeneity by grade, gender and race

In Table 10, I look at what happens to the estimates when the sample is split by grade, gender and race. Columns (1) and (2) consider how the estimates vary by grade level. Virtually all of the reduction in test-score growth occurs in elementary grades, where math, reading, and language fall by 0.06, 0.05, and 0.07 standard deviations, respectively for a 10 percentage point increase in charter share. Middle and high school show no statistically significant

[37] These include the fraction of residents who are black, Hispanic, born in another country, have a high-school degree or some college, and have a college degree, the labor force participation rate for males 25 years or older, and the log of annual average household income.

[38] Another potential concern is endogenous attrition as charters may induce enough students to leave the public schools in a non-random fashion to affect the estimates. While student fixed-effects helps address this, it may be insufficient. Hence, I run 2SLS models of attrition from the data on the share of students within 1.5 miles attending state charters along with student fixed-effects, grade-year fixed effects, and covariates. I focus on state charters here as district charters are in the sample and hence any movement to these schools would not generate attrition bias. In the test sample the estimate is a statistically insignificant −0.043 (s.e. 0.060), suggesting that attrition is not a substantial problem. Nonetheless, in the behavior sample the estimate is a statistically significant 0.102 (s.e. 0.051). This is likely due to the fact that many charters opened in 1997 and 1998 and thus many students would have been drawn from nearby schools during these years. These years are in the behavior sample but are too early for the test sample. Hence, I also run the behavior models on the test sample where attrition is less of a concern and find results that are similar to the baseline models.

[39] Note that the levels of the instruments drop out of this model, leaving only the interactions with average charter share for identification.

Table 11
2SLS alternative models.

	Separate state and district penetration measures		Drop district charters from sample	Levels models	Lagged dependent variable models	1 mile distance radius	2 mile distance radius
	State charters	District charters					
	(1)		(2)	(3)	(4)	(5)	(6)
A. Stanford 9/10 gains (standard deviation units)							
Math	−0.880***	0.050	−0.614***	−0.660***	−0.675***	−0.633***	−0.501***
	(0.324)	(0.475)	(0.158)	(0.243)	(0.258)	(0.223)	(0.170)
Reading	−0.258	0.228	−0.103	−0.126	−0.133	−0.112	−0.024
	(0.219)	(0.357)	(0.105)	(0.145)	(0.155)	(0.197)	(0.110)
Language	−0.766***	0.053	−0.420***	−0.453***	−0.458***	−0.683***	−0.441***
	(0.278)	(0.440)	(0.129)	(0.158)	(0.170)	(0.194)	(0.153)
Observations	602,864		576,522	602,864	602,864	602,864	602,864
B. Behavior measures							
# Disciplinary infractions	−8.38***	5.40**	−1.32*	−2.08*	−2.08*	−2.51**	−2.99**
	(3.07)	(2.32)	(0.78)	(1.21)	(1.16)	(0.99)	(1.46)
Attendance	22.4*	−20.2*	−7.63***	−3.34	−1.24	6.12	4.22
	(12.8)	(11.1)	(2.36)	(3.65)	(3.25)	(3.88)	(2.83)
Observations	1,543,969		1,507,502	1,543,969	1,543,969	1,543,969	1,543,969

Charter penetration measure is share of enrollment in overlapping grades within 1.5 miles. All regressions include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Each regression is also demeaned within individuals to remove student fixed-effects. Huber/White standard errors clustered by school in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

changes in achievement, and in fact the estimates for reading and language are close to zero.[40]

Consistent with the lack of test score reductions, discipline improvements occur almost exclusively in middle and high school while elementary students show a marginally significant reduction in attendance that is consistent with their test score drops.[41] This suggests that schools in elementary grades react differently to charters than secondary schools. One possible explanation for this is that the impacts work through peer effects where elementary charters tend to attract higher ability students and secondary charters tend to attract students with behavior problems and low achievement. However, while the evidence for secondary schools are consistent with a peer effects explanation, students who enter elementary charters also tend to have lower test scores. Hence, it appears unlikely that the achievement impacts are the results of negative peer effects through charters cream-skimming.

Another possible explanation is that the discipline results in middle/high are due to enforcement policy changes rather than actual behavioral improvements. The lack of improvement in attendance is consistent with this. Nonetheless it seems unlikely that weaker enforcement policies would lead to higher test scores, and thus is unlikely to explain the test score differences across grade levels. A third possibility is that elementary schools may be more sensitive to funding losses than secondary schools. For example, middle and high schools may be more able to take lost funds out of non-instructional activities such as extra-curricular activities or athletics while elementary schools, which tend to have less ancillary expenditure, might be forced to take funds out of instruction.

In columns (3) and (4) of Table 10, I split the sample by gender. Charter impacts are similar across boys and girls. In columns (5) through (7) I split the sample by race. Black students tend to be more affected by charter schools than Hispanic students. While blacks show reductions of math and language scores of 0.04 sd per 10 pp of charter share, for Hispanics these estimates fall to statistically insignificant −0.03 and −0.01 for math and language, respectively. Black students also exhibit more improvements in behavior than Hispanic students although Hispanics show marginally significant improvements in attendance. For white students, the test score estimates are imprecise due to small sample sizes, but are overall negative, with language significantly so. Both discipline and attendance improve marginally.

5.5. Alternative models

In Table 11, I provide some alternative models. Column (1) allows for state and district charters to have different impacts on LUSD schools. Most of the negative test score impacts come from state charters, whereby a 10 pp increase in state charter share reduces math and language scores by 0.09 and 0.08 sd, respectively. There is no significant impact from district charters. For behavior, on the other hand, state charters appear to generate improvements in discipline and attendance while district charters induce worse behavior.[42] In column (2) I drop district charters from the sample. The results are similar to baseline except that attendance now has a negative and statistically significant effect. Column (3) provides models in levels and column (4) uses an unrestricted value-added model where the dependent variable is in levels and a lagged dependent variable is included as a control. Both models provide results that are similar to my baseline restricted value-added models. Finally, in columns (5) and (6) I provide estimates that use a 1 mile and 2 mile radius around the schools, respectively. These are also similar to the baseline models.

6. Conclusion

Charter schools have the potential to generate strong incentives for public school administrators and teachers to increase effort and

[40] OLS estimates by grade level are provided in the online appendix. They generally show little significant impact of charter schools on any outcome, with the exception of discipline in elementary schools, which is significantly lower at the 10% level. Estimates that include school fixed-effects are similar. The difference between OLS, school fixed-effects and IV results suggest that elementary charters may locate in areas where schools are temporarily worsening and then undergo improvement due to mean reversion. Such a phenomenon would lead OLS and school fixed-effects estimates to overstate the charter impact. Secondary charters, on the other hand, may locate near schools where discipline is slightly better than average but on a permanently downward path. This would lead to an under-estimate of the charter impacts.

[41] Given that middle and high schools have larger attendance areas, longer distances may be more appropriate. Nonetheless, estimates using 2 miles instead of 1.5 provide similar results.

[42] Another potentially interesting model is allowing charter impacts to vary with capacity constraints since schools that are over-capacity may see charters as a release valve rather than a competitor and hence may have a different response to charters than under-capacity schools. Nonetheless, regressions that include an interaction of charter share with the ratio of enrollment to capacity in 1995 show no statistically significant difference in impacts by overcapacity rates.

improve student performance. Charter advocates argue that traditional public schools will work harder to prevent students from leaving so as to avoid losing funding and enrollment. In addition, charters could improve student outcomes by serving as "release valves" for overcrowded schools or by changing peer composition. On the other hand, charters could also lead to worsening student outcomes if traditional public schools cannot adjust easily to funding losses, peer groups change in a negative way, or charters attract good teachers from nearby traditional public schools.

Using data from a large urban school district in the southwest (LUSD-SW) I analyze how charter schools affect students in traditional public schools using an instrumental variables strategy to address potentially endogenous charter location and student fixed-effects to address endogenous movement of students across schools. My instruments utilize constraints on charter location choices determined by the availability of appropriate locations to house a school. I argue that charters in LUSD need to locate in properties that have sufficient building space. However, properties that are very large (e.g. office buildings, warehouses and apartment complexes) are generally undesirable locations for charters. Hence, I utilize as an instrument the number of properties with 20,000 to 50,000 ft^2 of building space within 1.5 miles, which closely corresponds to the inter-quartile range of building space for properties housing charters in LUSD in 2004. In addition, 14% of charters in LUSD as of 2004 locate in current or former shopping centers. These structures are often ideal locations for charters since they are easy to rent, are usually on main roads, and provide space that can easily be modified. Thus I also use the number of shopping centers within 1.5 miles of a regular public school as an instrument.

I find evidence that charter schools have a negative impact on math and language test score growth in public schools. Average math growth rates fall by 0.05 standard deviations from a 10 percentage point increase in charter share within 1.5 miles, while language growth rates fall by 0.04 standard deviations. Reading scores show no statistically significant effect from charter penetration overall, but do fall for elementary students. This is in sharp contrast to models using school fixed-effects that show positive but insignificant impacts similar to those found in previous research. These impacts are heavily concentrated in elementary schools as middle and high schools exhibit no statistically significant effects of charters on achievement.

In addition to test scores, I look at disciplinary infractions – measured by the number of in-school suspensions or more severe punishments a student incurs over the course of an academic year – and attendance rates. I find that there are statistically significant drops in disciplinary infractions of 0.2 instances per student with an increase in charter share of 10 percentage points. These improvements occur entirely in middle and high schools. However, I find little evidence of improvement for attendance. Thus, it is unclear whether the discipline results reflect changes in enforcement or real behavioral improvements. Additionally, sensitivity analysis shows that while achievement results are similar if we limit to using temporal-variation in the instruments, the discipline results are sensitive to this specification. Hence if one is concerned about the identification coming from cross-sectional variation, these results should be interpreted with caution.

While I cannot be sure what mechanisms drive these results, as the analysis is reduced-form in nature, there are a few possibilities. One is that the schools lose substantial funding in LUSD of up to 67% of average expenditures from a loss of a student to a charter that may not be recoverable in the short-term. While such a loss of funding would tend to increase average student expenditures, in the short-run many education inputs, particularly teacher and administrator salaries, are fixed. Hence the loss of revenue on the margin may have to be compensated by inefficient cuts. Given that the test score impacts are small, it is feasible that cuts to services such as extra tutoring, counseling, or school maintenance could have a deleterious effect. In addition, the fact that achievement losses occur primarily in elementary schools suggests that their budgets may be less flexible if, for example, middle and high school can cut extra-curricular activities and athletics, which elementary schools tend not to have. Cost cutting may also have a detrimental impact on staff and teacher morale, which could, in turn, spill over into achievement.

A second possibility is that charters change the peers students face in the non-charter schools by attracting specific types of students. Secondary charters appear to attract students with worse behavior and lower test scores, making this theory consistent with the discipline improvements seen in the public schools. However, elementary charters also tend to attract lower achieving students as measured by passing rates on state exams. Hence it seems unlikely that peer-effects are the dominant factor in the achievement reductions.

A third possibility may be that charters attract high-quality teachers from the regular public schools. Carruthers (2008) finds that charter schools in North Carolina attract a mix of high and low quality teachers. Hence, it is possible that the LUSD charters attract high quality teachers from nearby public schools. In LUSD, data on charter teacher quality is limited to measures of experience. Charter teachers are considerably less experienced than non-charter teachers. On average teachers in charters have six years of experience while average non-charter teachers have twelve. However, experience is at best a weak predictor of teacher quality and the marginal impact of additional experience is heavily focused in a teacher's first year, after which there are few experience gains (Hanushek et al., 2005; Hanushek and Rivkin, 2006). Thus, while charters attract less experienced teachers, their quality may be enhanced by unobservable factors such as motivation.

I should note that there are some limitations to consider when interpreting these estimates. First, since I am looking within a single, albeit very large, school district, the treatment effects are for highly localized insertions of charter schools. Thus, this analysis does not consider what happens to the district overall as charter programs grow. In fact, one could potentially have a situation where while charters may be detrimental to individual schools close by, they could improve outcomes in a school district as a whole. Second, LUSD is somewhat unique in the sense that there already exist substantial choice options for students. The district has a wide range of magnet programs and its own charter program.[43] Hence, whereas the marginal benefit from additional choice in LUSD may be small, it could be substantially larger in other urban school districts. Finally, due to the limitations of my instruments, I can only consider short-term impacts of charters and thus, over the long-term, charters could be more beneficial.

Nonetheless, these results show that charters can generate detrimental impacts in regular public schools. Hence, it is important for policy makers and researchers to watch charter programs so that we can determine whether they're having the desired effect on regular public schools and to modify such programs if the impacts become undesirable.

Acknowledgements

I would like to thank the Maryland Population Research Center for their financial support for this project. I extend my sincerest gratitude to employees and administrators at an anonymous school district and county tax-appraisal office for providing me with data and assistance and for making this project possible. I'd especially like to thank Mark Duggan for his many years of help with this work. In addition, I am grateful for guidance and assistance provided by Aimee Chin, Steve

[43] While one may be concerned that magnets can be used as a response to charters, the vast majority of magnets started prior to when the first charter opened in 1996. In addition, I find no statistically significant relationship between a school having a magnet program and charter penetration in 2004.

Craig, Julie Berry Cullen, Dennis Epple, Bill Evans, Judy Hellerstein, Ginger Zhe Jin, Chinhui Juhn, Jennifer King Rice, and two anonymous referees along with seminar participants at NYU, University of Houston, the AEFA Annual Meetings, and the Public Choice Society Annual Meeting. Finally, thanks to Mikailo Situk for excellent research assistance. Portions of this project were conducted at the University of Maryland as part of my doctoral dissertation. All errors remain my own.

Appendix A. Supplementary data

Supplementary data to this article can be found online at doi:10.1016/j.jpubeco.2011.02.003.

References

Bettinger, Eric P., 2005. The effect of charter schools on charter students and public schools. Economics of Education Review 24, 113–147.
Bifulco, Robert, Ladd, Helen F., 2006. The impacts of charter schools on student achievement: evidence from North Carolina. Education Finance and Policy 1 (1), 123–138.
Booker, Kevin, Gilpatric, Scott M., Gronberg, Timothy, Jansen, Dennis, 2007. The impact of charter school attendance on student performance. Journal of Public Economics 91 (5/6), 849–876.
Booker, Kevin, Gilpatric, Scott M., Gronberg, Timothy, Jansen, Dennis, 2008. The effect of charter schools on traditional public school students in Texas: are children who stay behind left behind? Journal of Urban Economics 64 (1), 123–145 July.
Buddin, Richard, Zimmer, Ron, 2005a. Is charter school competition in California improving the performance of traditional public schools? RAND Working Paper.
Buddin, Richard, Zimmer, Ron, 2005b. Student achievement in charter schools: a complex picture. Journal of Policy Analysis and Management 24 (2), 351–371.
Cardon, James H., 2003. Strategic quality choice and charter schools. Journal of Public Economics 87 (3), 729–737.
Carrell, Scott, Hoekstra, Mark, 2010. Externalities in the classroom: how children exposed to domestic violence affect everyone's kids. American Economic Journal: Applied Economics 2 (1), 211–228 January.
Carruthers, Celeste K., 2008. The qualifications and classroom performance of teachers moving to charter schools. NCPSE Occasional Paper No. 173.
Clark, Damon, 2009. The performance and competitive effects of school autonomy. Journal of Political Economy 117 (4), 745–783 August.
Figlio, David, 2007. Boys named Sue; disruptive children and their peers. Education Finance and Policy 2 (4), 376–394 Fall.
Hanushek, Eric A., Rivkin, Steven G., 2006. Teacher quality. In: Hanushek, Eric A., Welch, Finis (Eds.), Handbook of the Economics of Education, vol. 2. North Holland.
Hanushek, Eric A., Kain, John F., O'Brien, Daniel M., Rivkin, Steven G., 2005. The market for teacher quality. NBER Working Paper No. 11154.
Hanushek, Eric A., Kain, John F., Rivkin, Steven G., Branch, Gregory F., 2007. Charter school quality and parental decision making with school choice. Journal of Public Economics 91 (5/6), 823–848.
Heckman, James J., Stixrud, Jora, Urzua, Sergio, 2006. The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior. Journal of Labor Economics 24 (3), 411–482.
Holmes, George M., DeSimone, Jeff, Rupp, Nicholas G., 2003. Does school choice increase school quality? NBER Working Paper 9683.
Hoxby, Caroline M., 2001. Rising tides. Education Next, 4.
Hoxby, Caroline M., Achievement in Charter Schools and Regular Public Schools in the United States: Understanding the Differences, *Department of Economics, Harvard University, mimeo*, 2004.
Hoxby, Caroline M., Murarka, Sonali, 2009. Charter schools in New York City: who enrolls and how they affect their students' achievement. NBER Working Paper No.14852.
Hoxby, Caroline M., Rockoff, Jonah E., The impact of charter schools on student achievement, *Department of Economics, Harvard University, mimeo*, 2004.
Imberman, Scott A., forthcoming. Achievement and behavior of charter students: drawing a more complete picture. The Review of Economics and Statistics.
Kinsler, Joshua, School Policy and student outcomes in equilibrium: determining the price of delinquency, *University of Rochester, mimeo*, 2009.
Sass, Tim R., 2006. Charter schools and student achievement in Florida. Education Finance and Policy 1 (1), 123–138.
Segal, Carmit, 2009. Classroom behavior. The Journal of Human Resources 43 (4), 783–814 Fall.
Weiher, Gregory R., Tedin, Kent L., 2002. Does choice lead to racially distinctive schools? Charter schools and household preferences. Journal of Policy Analysis and Management 21 (1), 79.
Zimmer, Ron, Buddin, Richard, 2003. Academic outcomes. Charter School Operations and Performance: RAND, pp. 37–62.