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The effect of charter schools on charter students and public schools

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Abstract

This paper estimates the effect of charter schools on both students attending them and students at neighboring public schools. Using school-level data from Michigan's standardized testing program, I compare changes in test scores between charter and public school students. I find that test scores of charter school students do not improve, and may actually decline, relative to those of public school students. The paper also exploits exogenous variation created by Michigan's charter law to identify the effects of charter schools on public schools. The results suggest that charter schools have had no significant effect on test scores in neighboring public schools.

JEL classification: I0; I2

Keywords: Charter schools; Student achievement; Competition

1. Introduction

Charter schools are public schools exempted from most state and local regulations. States, school districts, and other charter-granting organizations often contract these schools out to the private sector. In 1992, two charter schools operated in the United States, both in St. Paul, Minnesota. By September 2001, almost 600,000 students attended 2372 charter schools operating in 34 states.[1] Charter advocates, and to some extent the popular press, have argued that charter schools are more innovative and more responsive to students than public schools. They claim that charter schools not only improve educational outcomes of charter students, but that they also improve student outcomes at neighboring public schools through increased competition. This paper evaluates these claims. Using unique data from Michigan, I attempt to measure the effects of charter schools on both the students who attend them and neighboring public schools.

Besides being of immediate policy interest, understanding the impact of charter schools could shed light on a number of broader issues.[2] For example, economists have long been interested in the relationship between school organization and pupil performance (see e.g., Coleman, Hoffer, & Kilgore, 1982; Evans & Schwab, 1995; Neal, 1997). Since charter schools face

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[1]As of September 1999, 38 states have passed laws allowing charter schools.

[2]To the extent that charter schools facilitate school choice, an investigation on charter schools may also provide insight on other choice programs such as educational vouchers. Gill, Timpane, Ross and Brewer (2001) provide an overview of evidence on the impacts of choice programs.

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fewer state and local regulations than traditional public schools, a study of charter schools may show whether innovations in schooling can generate higher student achievement. In this way, this paper builds on other research investigating the effects of charter schools on student achievement (e.g. Solmon, Paark, & Garcia, 2001; Nelson & Hollenbeck, 2001; Eberts & Hollenbeck, 2002). Economists are also interested in the effects of competition among schools on student achievement (see e.g., Hoxby, 2000, 2001; Borland & Howsen, 1992). The advent of charter schools appears to have led to significant competition among public schools in some districts,[3] suggesting that the growth of charter schools may provide some insight into the effects of competition on student achievement.

This paper begins by evaluating the effects of Michigan charter schools on students attending them, particularly focusing on charter schools opening in the 1996–1997 school year. The 1996–1997 school year was the last year in which Michigan's annual standardized testing took place in October.[4] Presumably the October tests were administered too early in the school year for charter schools to really have had an effect and likely reflect the ability of students prior to entering a charter school. Using these "pre-charter" tests for charter schools opening in Fall 1996, I compare test score gains across repeated cross-sections of 4th graders in charter schools and neighboring public schools. Comparisons of gains may provide a better measure of charter performance than comparisons of levels since Michigan charter schools typically attract students who are performing poorly relative to students at neighboring public schools.

Similar to previous research on Michigan charter schools (Eberts & Hollenbeck, 2002), I find that charter schools do not have strong effects on the academic achievement of students attending them. Simple comparisons provide no evidence that academic achievement of charter students improves more rapidly than in the nearby public schools. However, these simple comparisons might be biased if charter school students have different underlying growth trajectories than non-charter students, if the relative abilities of successive cohorts differ over time, or if charter schools change the neighboring public schools.

To control for the possibility that charter students have different underlying growth trajectories, I estimate more flexible specifications that compare charter students to public school students with similar pre-charter test scores. As before, pupils in charter schools score no higher, on average, and may even be doing worse.

One potential problem with these results is that the composition of successive cohorts of charter school students has changed over time. Each year, many students leave and enter charter schools. I present some evidence on how the test score distributions of new charter students have become increasingly lower over time. Since new students are likely to have lower test scores, the estimated effect of charter school may be biased downward with the inclusion of these individuals. To control for entry and exit from the charter schools, I also estimate the effect of charter schools on students who remain in charter schools for two or more years. The estimates also suggest that charter schools students have not improved relative to nearby public schools.

After estimating the effects of charter schools on charter students, I look at the effects of Michigan charter schools on neighboring public schools. Since charter location may be endogenously determined (Glomm, Harris, & Lo, 2001), simple comparisons of public schools near charter schools to those farther away may be biased. To further explore this relationship, I exploit exogenous variation created by Michigan's charter law, which allows state universities to approve charter schools. In particular, state universities where Governor Engler, an avid charter supporter, appoints the boards have approved 150 of Michigan's 170 charter schools. The proximity of a public school to one of these state universities can be used as an instrument for the likelihood that one or more charter schools were established nearby. The paper provides additional evidence both qualitatively and quantitatively of the validity of this instrument. The resulting instrumental variable (as well as the OLS) estimates suggest that charters have had no significant effect on student achievement in neighboring public schools.

2. Background

2.1. Michigan's charter law

Michigan's charter law is perhaps the most permissive law in the country with respect to charter school formation.[5] The first Michigan charter school opened in 1994, and by 1999, 170 charter schools, 10% of all US charter schools, accounted for 3% of Michigan public school enrollment. This section describes Michigan's

[3]In Inkster, Michigan, for example, after one-fourth of the school district's enrollment transferred to nearby charter schools, public schools began to offer bicycles and video games to parents who enrolled their children in *public schools*.

[4]In subsequent years, annual testing took place in February rather than October. I do not focus on charter schools starting after 1996–1997 because the initial test score available for students is only available after 6 months of school which is likely sufficient time for charter schools to have had an impact on students.

[5]Only Arizona has a higher percentage of student enrollment and a higher number of charter schools than Michigan.

charter law and explains how the law, coupled with the political environment, create unique, exogenous variation that can be used to identify the effects of charter schools on public schools.[6]

In Michigan, charter schools are public schools run by private entities. Any non-religious group, including existing private and public schools, can apply to open a charter school. To gain approval from an authorizing agency, they must submit a "charter," or contract, which establishes academic goals that the charter school will accomplish during the next seven years. These contracts also specify that if the school does not meet these goals, the authorizing agency may close it. Since 1995, authorizing agencies have closed two charter schools that failed to achieve their goals.

When approved, the charter school receives exemptions from most state/local regulations. For example, the charter school is not obligated to hire unionized teachers, and can have more autonomy than public schools in determining disciplinary policies and school curricula. However, to prevent charter schools from "cream-skimming," or selecting only the best students, the law forbids charter schools from discriminating in their enrollment policies. Seventy percent of charter schools are oversubscribed and admit students randomly (Khouri, Kleine, White, Cummings, & Harrison, 1999).[7]

Student enrollment completely determines the annual budget of charter schools. Despite this, charter schools still receive substantially less money than public schools. Charter schools receive 97% of the nearly $6000 of state and federal funding allocated for each student, but they receive no local funding, nor do they receive funds to purchase or rent school buildings. One analysis suggested that, including capital outlays, charter schools receive over $1000 less per student than comparable public schools (Anderson, Watkins, & Cotton, 2003).

Authorizing agencies receive the other 3% of state per student allowances to compensate them for administrative fees and the costs of monitoring charter schools.[8] As in most states, authorizing boards in Michigan include school districts and intermediate school districts.[9] However, unlike most states, the governing boards of community colleges and state universities may also authorize charter schools.

Allowing universities this power of authorization has been the catalyst for Michigan's rapid charter school growth. Of the 170 charter schools existing in 1999, state universities authorized 150, the maximum number that the law permits them to approve. Of the 15 state universities, those ten where the governor appoints the boards approved *all* of the university—authorized charter schools. Miron and Horn (1999) argue that allowing state universities to approve charter schools enabled Michigan's former governor John Engler to exert political pressure. For example, in December 1998, the president of Eastern Michigan University (EMU) announced that EMU would not authorize charter schools. Soon after, the governor threatened EMU with funding cuts, and EMU reversed its policy.

The governor's political pressure forced these ten pubic universities to authorize charter schools. In the initial years, these public universities tried to authorize charter schools in nearby communities. They did this for two reasons. First, oversight was costly. Second, many of these universities wanted to affect students who were more likely to attend their university later in life. EMU, for example, required that charter schools have geographically proximity to the university and "serve 'natural' EMU communities and constituents" (Khouri et al., 1999, p. 31).

While geographic proximity may predict charter school location initially, it has weakened as a predictor of charter schools. One theory for this is that universities may not have continued strict oversight over the schools they authorized. After intensive interviews with charter schools, universities, and state education officials, Khouri et al. (1999) explained, "We believe that neither the [Michigan Department of Education] nor any state authority has much idea as to whether authorizers are exercising quality control over the schools they have authorized." Additionally, the state law limited the number of charter schools that universities could authorize at 150, and as it began to be apparent that this cap would not be lifted or increased, universities, particularly Central Michigan University and Grand Valley State University, began "racing" for the cap. Schools began authorizing charter schools, regardless of geography—even EMU abandoned the "geographic proximity" standard that had once appeared in their application material. Hence, in later years, the proximity of a public school to one of the ten universities where the governor appoints the board may not be as strong of an instrument.

Recent research by Glomm, Harris, and Lo (2001) shows that charter schools may also form in areas with greater diversity, particularly racial diversity. While the instrumental variables estimates in the present study include district fixed effects, which may capture most of the diversity between districts, there may be within district variation in diversity that may also account for

[6] Khouri et al. (1999) and Miron and Horn (1999) describe Michigan's charter school law in detail.

[7] Ideally, one could use the random assignment of charter school admissions to identify the effect of charter schools; however, the state does not monitor the admission lotteries. I contacted several charter schools who were not willing to share such information.

[8] Monitoring is costly and consequently, most authorizing agencies have not directly profited from charter formation.

[9] Intermediate school districts are county-level organizations that oversee local school districts.

the presence of charter schools. Hence, I also use measures of diversity as an additional instrument.

2.2. Data

The primary outcome of interest in this paper is test scores. The test scores I use are from the Michigan Educational Assessment Program (MEAP), created and normed by the Michigan Department of Education (MDE). Specifically, I use annual math and reading tests for 4th graders. I focus completely on 4th grade since 70% of charter schools are elementary schools. I use anonymous student-level data available from the MDE and construct average math and reading scores for each school.[10] Since the scale scores for the MEAP may not be informative to the reader, I normalize the individual test scores to standard deviation units (i.e. Z-scores) within each year. The reading score is actually two separate test scores: ''Story'' which tests reading comprehension on a fictional passage and ''Information'' which tests reading comprehension on a non-fictional passage.[11]

The MDE also makes data available on schools' racial composition, enrollment, pupil–teacher ratios, and free/reduced lunch for both charter and public schools from the 1992–1993 to 1998–1999 school years. Financial data, including average per student expenditures and average teacher salaries, are also available for each school with a one-year lag. The anonymous student-level data also provide limited demographic information for each student.

Using these data, this paper estimates the effects of charter schools opening during the 1996–1997 school year. Although Michigan's first charter school opened prior to this year, little data are available for charter schools opening before 1996–1997. Additionally, starting in the 1997–1998 school year, all MEAP testing took place in spring, and as a result, ''pre-charter'' test scores do not exist for charter schools opening after 1996–1997.

Table 1 reports summary statistics for the math and reading MEAP exams of 4th graders. Column 1 shows the ''pre-charter'' test score distributions for 4th graders in the respective schools. Charter school 4th graders score about .65 standard deviations lower on math tests than students in the nearby public schools. Reading scores are also significantly lower for charter school students as compared to nearby public schools. These large, ''pre-charter'' differences in the test score distributions highlight the fact that Michigan charter schools, on average, attract students who are performing much worse on math and reading exams than the neighboring public schools.

By contrast, comparing the ''pre-charter'' distribution of math and reading scores in the public schools within 5 miles of a charter schools to those public schools farther away shows a much smaller difference. Public school students within 5 miles of a charter school score about .13 standard deviations lower on the math exam than public school students living farther away.[12]

The other columns of Table 1 show the test score distributions in subsequent years. In every year, charter school test averages are lower than those of public schools; however, as noted, this may be indicative of the students they attract. Consequently, the gain in relative test scores rather than the actual levels may be a better way to measure the effects of charter schools. From October 1996 to April 1998, charter schools improved their math test scores by about .15 standard deviations. Nearby public schools test scores slightly decline over this same period. Charters also show more rapid improvement after two years in reading scores. Charter advocates have cited these relative improvements as evidence that charter schools outperform public schools (MAPSA, 1999; *Detroit News* Aug 26, 1999). The next part of this paper evaluates this claim.

3. The impact of charter schools on charter students

This paper uses two strategies to identify the effects of charter schools on charter school students. These strategies are similar to those used to evaluate the effects of worker training programs (Ashenfelter, 1978; Card & Sullivan, 1988). The first strategy employs a difference-in-differences estimator to compare successive cohorts of 4th graders. The difference-in-differences estimator is a useful benchmark; however, if students entering charter schools are temporarily doing worse than other students, it may overstate the effect of charter schools. As a check, this paper estimates the effect of charter schools using a lagged dependent variable specification.[13] This estimator compares charter students

[10]The entire universe of 4th grade test scores is available in the 1997–1998 and 1998–1999 school years. A random sample of roughly 1/3 of each school's 4th graders is available for the 1996–1997 school year. The student data are available directly from the MDE.

[11]In earlier versions of this paper, I estimate the charter school effects using coarser test score measures based on the percentage scoring satisfactory, moderate, and low respectively. The results based on these alternative measures are similar to those in the paper.

[12]Glomm, Harris, and Lo (2001) cite these small differences as evidence that charter schools do not systematically locate where test performance is low.

[13]Alternatively, one could estimate a matching estimator (see e.g., Angrist, 1998; Dehejia & Wahba, 1995; Heckman, Ichimura, & Todd, 1997). Earlier drafts of this paper included estimates based on matching. The results were very similar to the lagged dependent variable specifications.

Table 1
Fourth grade MEAP scores

	Charter schools established in 1996–1997 school year		
	Oct 96 Pre-charter (1)	Apr 98 charter year 1 (2)	Apr 99 charter year 2 (3)
A. Charter schools opening in 96–97			
Math test score	−.733	−.597	−.575
	(.380)	(.443)	(.558)
Story test score	−.372	−.311	−.352
	(.391)	(.407)	(.529)
Information test score	−.472	−.433	−.487
	(.412)	(.371)	(.556)
Percent black	28.3	34.9	29.7
	(37.3)	(40.5)	(38.7)
Free/reduced lunch	57.1	42.5	40.0
	(20.9)	(26.5)	(25.5)
N	33	33	33
B. Public schools w/i 5 miles of charter school in 96–97			
Math test score	−.088	−.093	−.124
	(.590)	(.562)	(.564)
Story test score	−.047	−.058	−.101
	(.476)	(.428)	(.440)
Information test score	−.029	−.074	−.155
	(.550)	(.481)	(.508)
Percent black	43.6	44.0	46.6
	(42.9)	(42.9)	(43.2)
Free/reduced lunch	51.6	51.0	50.9
	(29.0)	(28.5)	(27.8)
N	551	555	557
C. All other public schools			
Math test score	.043	.048	.074
	(.446)	(.436)	(.440)
Story test score	.024	.030	.056
	(.353)	(.332)	(.324)
Information test score	.017	.039	.084
	(.408)	(.382)	(.348)
Percent black	8.0	8.2	8.9
	(20.0)	(20.1)	(21.3)
Free/reduced lunch	30.0	30.1	30.5
	(22.1)	(22.0)	(22.6)
N	1315	1321	1321

Notes: Unit of observation is the school. Test scores are in standard deviation units. Standard deviations are in parentheses. Weighted by the number of students taking the exam. The sample size for charter schools reading scores is actually 32 as one school reported math but not reading test scores. Sample sizes vary from columns 1 to 3 because of new school openings. The data appendix contains a comprehensive accounting of the number of school openings (and closings) by year.

to public school students with similar "pre-charter" test scores.

3.1. Difference-in-differences estimator

The first set of results consists of difference-in-differences estimates of the effects of charter schools on charter students. Suppose that a school's educational production function can be represented by

$$E[Y_i|j,t] = a_j + \beta_t + \delta C_i, \quad (1)$$

where $E[Y_i|j,t]$ is the expectation of school i's 4th grade test score (in levels) given that it is of type j (public or charter) at time t. a_j represents the average ability of the students choosing to attend school type j, β_t is a time specific effect common to all schools and C_i is an indicator for whether a charter school has existed for an

Table 2
Difference-in-difference estimates of the effect of charter schools on fourth grade charter students

	Math scores				Reading scores			
			Stayers	Stayers	Story test score		Info test score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Charter*post-yr 1	.074		.079		−.023		−.005	
	(.088)		(.099)		(.088)		(.094)	
Charter*post-yr 2		.138		.109		.003		.023
		(.135)		(.133)		(.105)		(.142)
Charter school	−.529	−.593	−.532	−.588	−.200	−.222	−.318	−.299
	(.100)	(.104)	(.103)	(.102)	(.114)	(.121)	(.117)	(.127)
Post-year 1	−.008		−.007		−.015		−.049	
	(.029)		(.029)		(.020)		(.031)	
Post-year 2		−.035		−.034		−.060		−.130
		(.020)		(.020)		(.053)		(.083)
% Black	.0002	−.001	−.0002	−.0008	.0019	.0011	.0014	.0005
	(.0007)	(.001)	(.0007)	(.0008)	(.0005)	(.0005)	(.0007)	(.0005)
% Hispanic	−.004	−.005	−.004	−.005	−.003	−.003	−.003	−.004
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.003)	(.002)
% Free and reduced lunch	−.007	−.007	−.008	−.008	−.008	−.008	−.008	−.008
	(.002)	(.002)	(.002)	(.002)	(.001)	(.001)	(.002)	(.001)
R^2	.18	.21	.18	.21	.17	.21	.15	.21
N	1172	1174	1157	1166	1170	1172	1170	1172

Notes: Unit of observation is the school. Standard errors are corrected for correlation within districts. Weighted by the number of students taking the exam. Treatment group includes charter schools opening in the 1996–1997 school year. Control group includes all public schools in a 5-mile radius of the treatment group. In columns 4 and 5, the dependent variable mean is computed to be the average test score among students who attended the same public or charter school for two or more years. The sample changes due to schools where data that identify "stayers" are unavailable.

entire year. The effects of charter schools, δ, is identifiable with difference-in-differences techniques:

$$\{E[Y_i|j = \text{charter}, t = 1997-98] \\ - E[Y_i|j = \text{public}, t = 1997-98]\} \\ - \{E[Y_i|j = \text{charter}, t = 1996-97] \\ - E[Y_i|j = \text{public}, t = 1996-97]\} = \delta. \quad (2)$$

δ can also be computed in a regression using stacked micro data for schools and years. The regression-adjusted version of the difference-in-differences estimator is

$$Y_{it} = \beta_t + \alpha_j + \delta C_{it} + \gamma X_{it} + \varepsilon_{it}, \quad (3)$$

where X_{it} includes controls for race and the proportion of students on free/reduced lunch and C_{it} is the interaction between an indicator for a charter school and an indicator for observations occurring in 1998. The coefficient δ is interpretable as the difference in test score gains between public and charter schools.

Table 2 shows the difference-in-differences estimates from Eq. (3) for math and reading scores. The sample includes all charter schools starting in 1996–1997 as well as all public schools within a five-mile radius of one of these charter schools.[14] The standard errors allow for within-district correlation in test scores. All of the regressions are weighted by student enrollment although the results are not sensitive to such weighting.

The results for 4th grade math and reading scores suggest that math and reading scores have not increased significantly relative to nearby public schools. After controlling for covariates, the estimated relative change in math scores between charter and public schools is only .07 standard deviations and the change in reading scores is even smaller. All of the estimates are statistically insignificant. The estimated relative changes in reading test scores remains small when comparing changes after two years, and as before, the estimated effects are insignificant for both math and reading scores.

Table 2 also reports estimates of the baseline difference between charter and public schools. The

[14]Although the estimates become weaker as the distance increases, the results are similar when the control groups includes public schools within a 10-, 20-, or 40-mile radii or when the control group includes public schools within the same county (i.e. intermediate school district—see footnote 9).

coefficient on charter schools reflects the "pre-charter" difference between charter and nearby public schools. After controlling for covariates, charter schools attract students whose initial test scores in both math and reading are significantly worse than students in the neighboring public schools.

One criticism of the results in Table 2 is that the failure to find significant estimates may be the result of too many restrictions on the standard errors. The standard errors in these tables correct for within district correlation. This correction tends to inflate standard errors when compared to standard corrections for heteroskedasticity (Moulton, 1986). However, the results in Table 2 are still insignificant if I assume that there is no within district correlation and estimate heteroskedasticity-consistent standard errors.

The causal interpretation of the estimates in Table 2 hinges on whether the assumption of a fixed difference between charter schools and public schools is plausible. There are two possible threats to this assumption. First, if the average ability of charter students changes over time, then comparisons of successive cohorts of 4th graders may give misleading results. The 4th grade cohort in later years may differ systematically from previous cohorts. For example, if charter schools attract better (or worse) students over time, the average ability of 4th grade students may change for reasons unassociated with the charter school itself. The other potential threat to the difference-in-differences estimates is whether the charter school and public school students have similar test score trajectories. I discuss these two threats in turn.

The ideal way to identify the effect would be to use longitudinal data, but, in Michigan, such data are not available to researchers. However, student-level data help discern whether the estimates are biased upward or downward. If over time charter schools attract students with lower and lower abilities than existing charter students, then the test scores of new charter school attendees are likely to be lower than the test scores of existing students and the estimated effects of charter schools may be biased downward. Similarly, if the test scores of new charter school attendees are higher than that of existing students, then the estimated effects of charter schools may be biased upward.

The Michigan data provide some evidence that incoming students have lower test scores. In the anonymous student-level data, students complete a question about whether or not they are new to that specific school in that year. Not only do new charter students score about $\frac{1}{3}$ of a standard deviation lower than the existing students, but the distribution of new charter students test scores steadily declines between 1997 and 2000. In each year, the mean, the 25th percentile, the median, and the 75th percentile of the distribution of new student test scores is lower than the year before. There are a couple of plausible explanations for this decline. First, charter schools are attracting worse and worse students over time. Second, there are a number of individual students, presumably students from disadvantaged backgrounds, who transfer schools almost *every* year. Over time, the transfer students represent more and more these disadvantaged students. Regardless of the explanation, the inclusion of these transfer students likely biases the estimated effect downward.

Identifying the new attendees at a school also allows us to identify students who were 4th graders in the 1997–1998 school year and who had attended the same charter school in the 1996–1997 school year. I can exclude students who identified themselves as being new to the school, and after making the same exclusion with the public schools, I can compare student progress among these "stayers" (i.e. students who enrolled in the same school for two or more years).[15]

While this strategy is appealing, it may also be problematic. First, this strategy controls for entry into the charter school but not exit. If attrition in charter schools differs from that in the public schools, then the results may be invalid. In other words, if poorly performing charter students are more likely to exit charter schools than poorly performing public school students—a fact verified in Texas (see Hanushek, Kain, & Rivkin, 2002), then comparisons of "stayers" may be biased. Another potential problem with comparing "stayers" is the ability to isolate "stayers" beyond one year. The Michigan data only allow identification of students who transferred into the school during the previous year. This does not affect comparisons after one year, but it may affect comparisons after two years of a charter's opening. These latter comparisons may include some students who attended second grade in a public school and third grade in a charter (or similarly a student who attended a charter in second grade and a public school in third grade).

With these potential biases in mind, I present estimates of the effect of charter schools on math scores of "stayers" in columns 3 and 4 of Table 2. The estimated effects are similar to those in columns 1 and 2. The magnitude of the estimated effect after two years drops by 20 percent. These estimates are similar to those in Hollenbeck and Eberts (2002). Hollenbeck and Eberts

[15]An alternative strategy is to make distributional assumptions about the students who enter and exit from charter schools. For example, a reasonable lower bound to the estimates would assume that students transferring into charter schools have low test scores and that students transferring out of charter schools have high test scores. Other distributional assumptions can identify a corresponding upper bound. Unfortunately, these upper and lower bounds are uninformative as they generous range of both positive and negative effects.

(2002) matches fourth grade students' math and reading test scores to the students' fifth grade science and writing test scores. Since the data are anonymous, they link students according to the demographic characteristics they report each year. They successfully match 1/3 of the students. Their estimates are subject to the same problems described above. They find that being in a charter school reduces test scores by 2 percent in science and 6–7 percent in writing.

Another potential problem with the difference-in-differences estimates in Table 2 is that they rely on the assumption that charter school students' test scores would have had similar trajectories as public school students in the absence of charter schools. However, if charter school attendance is conditional on past performance, this assumption would be violated. For example, in the worker training literature, Ashenfelter (1978) shows that applicants to training programs experienced a dip in their earnings just *prior* to their application. If earnings follow a mean-reverting process, then comparing applicants and non-applicants, without controlling for the earnings dip, will show a spurious, positive effect of the training on participants (Heckman & Robb, 1985; Manski, 1989). Similarly, the difference-in-differences estimates from Table 2 will overstate the effect of charter schools if charters attract students that are *temporarily* performing worse than their public school counterparts. If the likelihood that parents send their children to charter schools is conditional on past performance, comparisons that control for "pre-charter" test scores will give the effect of the intervention (Rubin, 1977).

3.2. Lagged dependent variable

The next set of results, reported in Table 3, consists of regression estimates that control for lagged outcomes. The motivation for this approach is a model where charter status is determined by lagged test scores, instead of permanent school-specific effects. The estimated equation in this case is

$$Y_{it} = \gamma Y_{it-1} + \beta_t + \delta C_{it} + \varepsilon_{it}. \quad (4)$$

As long as the residual is not serially correlated, least-squares will give a consistent estimate of δ, the effects of the charter school conditional on pre-treatment scores. The regression compares fourth grade test scores in later years to fourth grade test scores in 1996–1997. Each regression in Table 3 also controls for racial composition and the proportion of the student body on free/reduced lunch.

Using the lagged dependent variable specification, the estimated effects of charter schools on fourth grade charter students are negative for both math and reading. In column 1, the estimated coefficient implies that the proportion of charter school enrollment that scored satisfactory in math declined .12 standard deviations relative to similar public schools. After two-years, the estimated effect is larger (.23 standard deviations) and marginally significant. In the reading scores, the point estimates of the effects of charter schools are always negative. They are larger in magnitude and more significant for the Information (non-fictional reading comprehension) test than for the Story (fictional reading comprehension) test. These estimates, based on a specification with a lagged dependent variable, have a causal interpretation if charter school attendance is "as good as randomly assigned" conditional on past outcomes.

In summary, the difference-in-differences estimates do not provide evidence that charter schools' test scores improve relative to nearby public schools. While the point estimates are positive, the estimates are all insignificant. The lagged dependent variable specifica-

Table 3
Estimates of the effect of charter schools on charter students controlling for lagged dependent variable

	Math scores		Reading scores			
			Story		Information	
	1998	1999	1998	1999	1998	1999
Charter school	−.120	−.233	−.053	−.142	−.186	−.174
	(.106)	(.120)	(.079)	(.114)	(.076)	(.098)
1996–1997 avg. test score	.524	.400	.410	.325	.403	.354
	(.053)	(.032)	(.044)	(.018)	(.037)	(.036)
R^2	.43	.38	.40	.40	.39	.45
N	579	579	578	578		

Notes: Unit of observation is the school. Standard errors are corrected for correlation within districts. Weighted by the number of students taking the exam. Treatment group includes charter schools opening in the 1996–1997 school year. Control group includes all public schools in a 5-mile radius of the treatment group. The regressions also include controls for percentage of enrollment that is black, percentage of enrollment that is Hispanic, and percentage of enrollment that is on free/reduced lunch.

Table 4
Difference-in-difference estimates of the effect of charter schools on fourth graders in public schools

	Math scores			Reading scores			
				Story		Information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Treatment effects							
Diff-in-diff: number of charters yr 1	−.010	−.011		−.013		−.017	
	(.005)	(.005)		(.005)		(.005)	
Diff-in-diff: number of charters yr 2			−.020		−.021		.030
			(.004)		(.010)		(.009)
B. Main effects							
Near charter school	.016	.017	.024	.015	.021	.012	.023
	(.006)	(.005)	(.005)	(.008)	(.011)	(.007)	(.011)
Post-year 1	.005	.009		.013		.026	
	(.010)	(.010)		(.010)		(.012)	
Post-year 2			.034		.038		.066
			(.014)		(.013)		(.022)
District FE	No	Yes	Yes	Yes	Yes	Yes	Yes
C. Building covariates							
% Black	.0001	−.003	−.005	−.002	−.003	−.003	−.004
	(.0006)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
% Hispanic	−.00 1	−.005	−.005	−.005	−.007	−.005	−.008
	(.002)	(.001)	(.001)	(.001)	(.001)	(.002)	(.002)
% Free and reduced lunch	−.010	−.006	−.009	−.006	−.006	−.006	−.006
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
R^2	.25	.50	.51	.48	.56	.49	.49
N	3742	3742	3744	3742	3744	3741	3743

Notes: Unit of observation is the school. Standard errors are corrected for correlation within districts. Weighted by the number of students taking the exam. Treatment group includes public schools within 5-miles of a charter school. Control group includes all other public schools in the state.

tions suggest that charter school fourth grade test scores declined significantly relative to public schools.

In estimating the effects of charter school on charter students, an implicit assumption was that charter schools do not affect public schools nearby. Section 4 investigates the plausibility of this assumption.

4. The impact of charter schools on the public schools

This section estimates the effects of charter schools on neighboring public schools. Besides being of policy interest, these estimates shed light on the interpretation of the estimates in the previous section. Depending on how charter schools affect student achievement in public schools, the estimates from the previous section could be biased upward or downward.

4.1. *Difference-in-differences estimates*

Table 4 reports differences-in-differences estimates of the effects of charter schools on public schools. The estimated equation is

$$Y_{it} = \beta_t + \alpha_j + \delta C_{it} + \gamma X_{it} + \varepsilon_{it}, \quad (5)$$

where C_{it} is the number of charters within a 5-mile radius of public school i at time t. This equation is identical to Eq. (3), except now I allow the treatment effects to vary linearly with the number of charters.

This model differs from the traditional difference-in-differences setup. Eq. (5) assumes that there is a state effect for having *any* charter school while allowing a different effect by the number of charter schools. In a true difference-in-differences, there would be separate "state" effect for having one, two, three, or more charter schools. There would also be a separate treatment effect for each number of charter schools. In early versions of this paper, I used such a specification and could not reject the hypothesis that the state effects were similar. Allowing the treatment parameter to vary linearly is meant to capture the idea that if a competition effect exists, it may be strongest when there are more schools. Hoxby (2001) argues that charter school competition is likely observable once charter schools account for over 6 percent of district enrollment. I get similar results if I respecify the model to a traditional difference-in-differences model where "treated" schools are those with at least 6 percent of district enrollment in charter schools.

Table 4 reports estimates for fourth graders based on Eq. (5).[16] Column 1 estimates the effects of charter schools on public schools' math scores by comparing, after one year, public schools near charter schools to public schools farther away with a basic set of covariates. The other columns estimate the effects after including district fixed-effects.

In each specification, the estimated effect of charter schools is negative, significant, and small. For example, in columns 1 and 2, the students in public schools near charters score about .01 standard deviations lower per charter school relative to other public schools after one year. After two years, the students appear to score about .02 standard deviations lower per charter school relative to the other public schools. In public schools near charter schools, schools on average had 2 charter schools within a 5-mile radius. After one year, this implies that public schools near charter schools, on average, scored between .02 and .03 standard deviations lower than other public schools in both reading and math. After two years, there were, on average, 3 charter schools. Hence, on average, the relative decline in test scores is even greater—between .06 and .09 standard deviations lower than other public schools. These estimates are all statistically significant.

Table 4 also shows that small but significant pretreatment differences existed between public schools with and without charters. The row "Near Charter School" shows the pre-charter differences between public schools near and away from charters. Public schools near charter schools have test scores between .01 and .02 standard deviations higher than other public schools. This supports the conclusion in Glomm, Harris, and Lo (2001) that after controlling for community characteristics, charter schools do not necessarily locate in areas where public school performance is worse. In fact, these "pre-charter" differences suggest that public schools near charters had higher test scores than other public schools. As discussed in the previous section of the paper, if the "pre-charter" differences reflect *temporary* differences between public schools near charter and other public schools, then the difference-in-differences estimate may overstate the effects of charter schools.

[16]Since charter schools attract students who are performing low relative to nearby public schools, nearby public schools should have higher averages already. This will bias all of my coefficients upward in this section.

4.2. Lagged dependent variable specifications

The next set of estimates controls for lagged dependent variables as in Eq. (4). Using Eq. (6), Table 5 compares test scores in public schools near charter schools to those of other public schools with similar "pre-charter" test scores

$$Y_{it} = \gamma Y_{it-1} + \beta_t + \delta C_{it} + \lambda X_{it} + \varepsilon_{it}. \quad (6)$$

Eq. (6) is identical to Eq. (4) except that C_{it} is the number of charter schools within a 5-mile radius of the public school i at time t.

The estimates in column 1 of Table 5 suggest that each charter school within a five-mile radius helped increase math scores about .001 standard deviations over similar public schools located further away. Since the coefficient is measured imprecisely, it does not provide conclusive evidence of whether charter schools benefit or hurt neighboring public schools. Using a 95% confidence interval, I can, however, estimate the range of possible effects. A 95% confidence interval for the treatment effect in column 1 is from −.007 to .008. Although the confidence interval does not exclude positive or negative effects, it suggests that the estimated effect is extremely small, measuring, at the extreme points, less than one one-hundredth of a standard deviation movement in math scores for public schools near charter schools.

Column 2 shows the effect measured after two-years of having charter schools located nearby. Public schools near charter schools perform about .007 standard deviations better than similar public schools farther away. The effect is even significant, but as before, the effect size is very small in magnitude. The estimated effects of reading scores are all positive. The effects after two years are always significant. The reading scores, in particular, imply that public schools near charters increase by about .01 standard deviations per nearby charter school, or since there were an average number of 3 charter schools near a public school in 1998, the estimated effect is about a .03 standard deviation movement in test scores.

4.3. Instrumental variable estimates

The results in Table 5 control for spurious mean-reversion effects by comparing schools with similar "pre-charter" test scores. However, if charter location is endogenously determined (e.g., charter schools forming in areas which are always performing poorly), these estimates will also be biased. Instrumental variable estimation will provide a consistent estimate of the effect of charter schools on public schools so long as an instrument can be found that is correlated with charter school location and uncorrelated with residual test scores.

Table 5
Lagged dependent variable estimates of the effect of charter schools on 4th graders in public schools

	Math scores		Reading scores			
			Story		Information	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Treatment effects						
Number of charters yr1	.0009		.002		.002	
	(.0038)		(.002)		(.003)	
Number of charters yr 2		.007		.009		.012
		(.002)		(.001)		(.001)
B. Covariates						
1996–1997 test score	.430	.347	.290	.238	.316	.316
	(.080)	(.033)	(.078)	(.043)	(.041)	(.041)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.64	.64	.63	.64	.65	.66
N	1840	1820	1840	1820	1840	1818

Notes: Unit of observation is the school. Standard errors are corrected for correlation within districts. Weighted by the number of students taking the exam. Treatment group includes public schools within 5-miles of a charter school. Control group includes all other public schools in the state. The regressions also include fixed effects for time and controls for percentage of enrollment that is black, percentage of enrollment that is Hispanic, and percentage of enrollment that is on free/reduced lunch.

In this paper I use two instruments. The first is the distance of a public school from a state university where the governor appoints the board as a predictor for the number of charter schools establishing nearby. Since many of these ten public universities, particularly in the first years that charter schools were established, sought to authorize charter schools with geographic proximity, the distance between public schools and these ten universities is likely to be correlated with charter school location. One might rightly worry, however, that proximity to a university may be correlated with residual test scores. The instrument in this paper is the proximity of a public school to one of the *ten* universities where the governor appoints the board. There are 60 private and public universities and 29 community colleges in Michigan. Only 10 of these—the ten where the governor appoints the board—are relevant for the instrument in this paper. Most schools in the sample are located with a 75-mile radius of at least one private/public university.[17] For proximity to these ten universities to be problematic, the proximity to these ten universities must have different effects than the proximity to the other 50 private/public universities. The second instrument is a Herfindahl index on racial shares.[18] Glomm, Harris, and Lo (2001) shows that charter schools tend to form in districts with more racial diversity. While my regressions include district fixed effects, I also include the racial Herfindahl to see if charter schools form within a given district closer to places with greater diversity.

The first stage for this problem is

$$C_{it} = \theta X_{it} + \phi Z_{it} + \varepsilon_{it}, \quad (7)$$

where C is the number of charter schools; Z represents both the distance from the nearest university where the governor appoints the board and the Herfindahl index on racial shares; and X are covariates included in Eq. (6). I also include district fixed effects. In order to allow time for charter schools to have affected nearby public schools, I only report estimates of the effects after two years.

Panel A of Table 6 reports the first stage results. Even after controlling for district fixed effects, the farther that a public school is from one of the ten governor-influenced colleges, the less likely they are to have a charter school nearby. Although not reported, the first-stage relationship between distance to colleges and the number of charter schools is actually stronger in the first year. Given the "race" to the imposed cap and possible breakdowns in charter school oversight, it is not surprising that the relationship weakens over time.

The first stage regression also shows a significant relationship between test scores and charter formation. Within a school district, charter schools are more likely to establish themselves near schools with higher test scores. The results in Table 6 also show that charter schools are more likely to form near less diverse schools within a school district. This last result contrasts the

[17] I can also restrict the sample to public schools located within a 75-mile radius of any public/private university. The results are similar to those presented here.

[18] The race Herfindahl in this paper is one minus the traditional Herfindahl index of racial shares. Hence, higher values of the Herfindahl index in this paper reflect greater diversity. Racial diversity may not be a valid instrument if it directly affects test scores.

Table 6
First-stage and IV regressions of the effects of charter schools on public schools treating number of charter schools w/i 5 miles as endogenous

	Math scores sample (1)	Reading scores sample (Story) (2)
A. First stage estimates (Dep var=number of charters 98–99)		
Minimum distance from state university where Gov appoints	−.110	−.110
	(.049)	(.049)
1996–1997 Test score	.807	.593
	(.215)	(.264)
Racial Herfindahl[a]	−2.52	−2.65
	(.97)	(.98)
% Black	.038	.036
	(.009)	(.009)
% Hispanic	.085	.084
	(.018)	(.018)
% Free and reduced lunch	−.006	−.005
	(.008)	(.008)
District FE	Yes	Yes
R^2	.82	.82
B. IV estimates		
(Dep var = test scores)		
Number of charters yr 2	.013	.028
	(.027)	(.020)
1996–1997 test score	.342	.228
	(.036)	(.037)
District FE	Yes	Yes
N	1820	1820

Notes: Unit of observation is the school. White standard errors are reported. Weighted by the number of students taking the exam. Treatment group includes public schools within 5-miles of a charter school. Control group includes all other public schools in the state.
[a]The racial Herfindahl is defined as one minus traditional Herfindahl index on race, so the higher the racial Herfindahl, the more diversity in the school.

result in Glomm, Harris, and Lo (2001). The key difference between the results in Table 6 and the findings in Glomm, Harris, and Lo (2001) is the unit of observation. This paper relies on building-level data, and with the inclusion of district fixed-effects, this paper looks at differences in charter location within districts. The Glomm, Harris, and Lo (2001) paper focuses on district-level data and estimates the number of charter schools forming in a given district. This distinction may lead to contrasting results.[19]

Is it reasonable to think that charter school competition may have had an effect after the two years? Perhaps. The charter schools may be too new, and public schools may not have anticipated what type of growth charter schools would have had. However, there may be reasons to believe that there could be a discernible effect. Public schools were aware when and where a charter school was going to open. In some cases, public schools knew as much as 1.5 years before the charter school opened. Public schools may have started "competing" in anticipation of the charter school opening. While the estimated effect focuses on the effect two years after the charter school opened, it may actually represent public schools' responses multiple years after the announcement of a new charter school.

Panel B of Table 6 reports instrumental variables that provide a check on the basic lagged dependent variable specification. For both math and reading scores, the estimated relationships are positive and insignificant. For example, in column 1, each additional charter school causes math test scores to increase by .01 standard deviations in nearby public schools relative to other public schools. This point estimate is imprecisely measured; however, as above, a 95% confidence interval around this point estimate provides evidence on the magnitude of the effect of charter schools. A 95%

[19]Moreover, the results may differ in how we measure charter intensity. Glomm, Harris, and Lo (2001) focuses on the number of charter schools in a district while this paper focuses on the number of charter schools within a five-mile radius of a given public school. Since charter school students can attend charter schools in other districts, this measure in this paper may more accurately identify the influence of charter schools that form on the border of one district yet target nearby public schools in other school districts.

confidence interval implies that the effect of charter schools is between −.03 and .06 standard deviations after one year for math. For "Information" reading scores, the effect of charter schools is between −.01 and .07 standard deviations. The estimated effects are small and may be biased upward since charter schools attract students who are performing worse than the neighboring schools. These students have already left public schools located near the charter schools automatically increasing the average test scores for these public schools. As a result, the test scores of public schools near charters are already higher for mechanical reasons (exit of poor students) than they would have been in absence of charter schools.

These results are not as strong as the conclusions presented in Hoxby (2000) and (2001). Those papers find that test scores significantly increase in areas where there are a greater number of school districts or charter schools, respectively, concluding that competition improves student achievement. The point estimates and confidence intervals in Table 6 are not inconsistent with this conclusion, but the estimates in Table 6 are insignificant and likely biased upward. Additionally, Hoxby's papers estimate the long-run effects of school district competition whereas this paper focuses on the effects of newly created schools.

5. Conclusion

Using school- and student-level data from Michigan, I find that charter schools do not improve test scores or passage rates as rapidly as public schools with similar "pre-charter" test scores. The estimates suggest that charter school fourth graders may score as much as .2 standard deviations lower on the reading and math exams than students from similar public schools. The analysis also highlights that charter schools attract students who have lower "pre-charter" test scores than neighboring public schools. On "pre-charter" tests, charter school students score about .5 standard deviations lower than students in neighboring public schools.

Despite the fact that public school test scores mechanically increase as charter schools draw away underperforming public school students, there is no robust, significant evidence that test scores increase or decrease in neighboring public schools as the number of charters increases. Moreover, the magnitude of the point estimates and corresponding confidence intervals is extremely small, suggesting a small effect if any of charter schools.

The results reported here raise a number of interesting questions. First, why do charter schools have lower academic achievement than public schools? Some possible mechanisms include differences in financial resources, teacher experience, or institutional immaturity. Second, why are the effects of charter schools on student achievement in neighboring public schools so small? As the charter school movement continues to grow, researchers will have more data to estimate these effects more precisely. Future research can also identify the specific mechanisms by which charter schools induce competition. Finally, what are the long-run effects of charter schools? The results in this paper are estimated in the midst of rapid growth and flux of charter schools. All of the charter schools in the study opened in Fall 1996. The short-run effects of these effects may differ substantially from the long-run equilibrium with charter schools. Additionally, once the charter school movement is old enough to generate long-term data, other outcomes, such as dropout rates, college attendance, and future wage and employment status, will also be interesting to study.

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Data Appendix

MEAP test scores

Thirty-five charter schools reported test scores for the first time in October 1996. Of these 35 charter schools, one never reported test scores again (Northland Math and Science Academy) and one reported testing results in March 1998 and closed shortly thereafter (New School for Creative Learning). I exclude both of these charter schools from the analysis. Two charter schools reported test scores in subsequent years but failed to report scores in either 1998 or 1999 (Academy of Detroit in 1998 and Warwick Point Academy in 1999). For both of these schools, I imputed the missing test score by taking the average of the test scores in adjacent years (e.g. the average of 1997 and 1999 test scores for the missing 1998 value). The analysis is robust to the exclusion of these two charter schools. In the reading analysis, one additional charter school (Oasis Academy) is excluded because individual test score data are not available. Aggregate school level data are available for satisfactory, moderate, and low rates but not for the normalized test scores.

The sample size of public schools within a 5-mile radius increases by five schools in 1998 and by another two schools in 1999 because of an increase in the number of schools. The net increase in schools can be broke down as follows: 545 schools were open from 1997 to 1999; four schools converted to early elementary schools and no longer taught fourth graders after 1997; one school either closed or converted to an early elementary school after the 1998 school year; nine new schools opened in 1998 although one of these closed after 1998; three other schools opened in 1999; and one school reported test scores in 1997 and 1999 but not in 1998. All of the new and converted schools are not excluded from the sample since the population attending those schools is included in other schools' statistics either before the respective school opened (or after it closed in the case of the four converted schools). Since the regressions are weighted by enrollment, these schools (and their consolidated counterparts in subsequent years) are properly represented in each time period. Data for one school were miscoded after 1997 (building id 5741) and required correcting the data directly with the school.

The sample sizes for schools outside a 5-mile radius range are 1315 schools in 1997, 1321 schools in 1998, and 1321 in 1999. As before, these sample sizes represent the same population of students. Changes are reflected primarily from consolidations and conversions. The specific breakdown by year is as follows: 1275 schools were open from 1997 to 1999. Twenty schools administered fourth grade tests in 1997 but not in subsequent years because of either school consolidation or conversion to early elementary school status. Another 20 schools had fourth graders during 1997 and 1998 but subsequently closed or converted to early elementary status. 26 schools either opened or began teaching fourth graders in 1998, but two of these closed after 1998. An additional 22 schools began teaching fourth graders in 1999. I deleted an additional 22 schools that had taught fourth graders between 1997 and 1999 but had not participated in the standardized testing program in one or more of these years. Most of these schools were special education centers with relatively few students.

Distances between schools

I computed distances by taking the distance between the population centroids of the respective zip codes. There were 46 schools for whom data on zip codes were incorrect or missing (e.g. the zip code of a closed school was no longer in use). I was able to correct the data for 33 schools by tracking down new addresses. I imputed distance data for the remaining schools by taking the average distances reported in the school district.

Missing values

A number of schools, including many of the charter schools, did not report values for the percentage of students on free/reduced lunch programs. All of the regression specifications included indicators for observations where missing values were present. If missing, the observation was assigned the sample mean.

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