THE IMPACTS OF CHARTER SCHOOLS ON STUDENT ACHIEVEMENT: EVIDENCE FROM NORTH CAROLINA

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Abstract
Using an individual panel data set to control for student fixed effects, we estimate the impact of charter schools on students in charter schools and in nearby traditional public schools. We find that students make considerably smaller achievement gains in charter schools than they would have in public schools. The large negative estimates of the effects of attending a charter school are neither substantially biased, nor substantially offset, by positive impacts of charter schools on traditional public schools. Finally, we find suggestive evidence that about 30 percent of the negative effect of charter schools is attributable to high rates of student turnover.

1. INTRODUCTION

Charter schools have been one of the fastest-growing forms of school choice during the past decade. While school voucher programs have faced legal challenges and political opposition, charter school programs have been adopted in thirty-nine states and the District of Columbia. As of January 2004, 2,966 charter schools were in operation, serving approximately 638,000 students nationwide (CER 2005).

Although the provisions of charter school programs vary widely from state to state, most charter schools share several characteristics. They typically have more autonomy than traditional public schools, are exempted from selected state and local regulations, and are schools of choice, which means parents must actively choose to enroll their children in a charter school. They are free and open to all parents within a given jurisdiction and, if oversubscribed, are typically required to select students by lottery. Finally, they are publicly funded, and the amount of their funding is directly linked to the number of students they enroll.

Charter school programs are intended not only to increase student learning but also to promote educational innovation, diversification of educational programs and learning environments, and expanded opportunities for teachers to become more involved in program design and school governance.[1] Nonetheless, improving student learning is among the most important goals of charter school programs, and scholars and policy makers alike have been awaiting evaluations of how charter schools have affected student achievement.

Charter schools might improve academic achievement in several ways. First, they may increase the performance of the students who choose them by providing more effective learning environments than traditional public schools do. Charter schools might achieve this goal by hiring more effective teachers, by using resources more efficiently, or by attracting a more motivated set of students who provide positive spillover benefits to other students. Second, even if charter schools are no more effective than traditional public schools for the typical student, they might benefit some students by providing alternative educational environments and programs. Students at risk of failure in traditional school settings, for example, might do better in charter schools to the extent that those schools offer smaller, more intimate environments, specialized curricula, or targeted support services. Finally, the achievement of students in traditional public schools could rise if the competition from charter schools for students and funding induced traditional public schools to become more productive.

1. Goals similar to these are included in a model charter school law developed by Ted Kolderie, founder of the Charter Friends National Network (Nathan 1996), and similar goals are articulated in many of the charter school laws.

Alternatively, charter school students might achieve at lower levels than they would in traditional public schools if charter schools are less well funded, are operated by less experienced or less qualified officials, provide a peer environment that is less conducive to achievement, or for some other reason are unable to provide an effective educational program. Charter schools might also diminish the quality of traditional public schools by drawing away funding, motivated students, and/or quality teachers.

In this article, we use an extensive, individual-level panel data set to evaluate the impact of charter schools in North Carolina on the math and reading performance of students in grades 4 through 8. We use student-level fixed-effects models together with auxiliary analyses to address three questions: (1) Do students who attend charter schools make larger achievement gains, on average, than they would have in the absence of charter schools? (2) Do students who attend traditional public schools located near charter schools, and thus subject to competition from charter schools, make larger achievement gains than they would have in the absence of charter schools? (3) What accounts for quality differences between charter schools and traditional public schools?

After paying close attention to potential biases in our impact estimates, we find that students make considerably smaller achievement gains in charter schools than they would have in traditional public schools. We also conclude that the large negative estimates of the effects of attending a charter school are neither substantially biased, nor substantially offset, by positive impacts of charter schools on traditional public schools. Finally, we find suggestive evidence that about 30 percent of the negative effect of charter schools is attributable to high rates of student turnover.

The next section provides a brief review of previous efforts to evaluate the impact of charter schools on student performance. Section 3 describes the North Carolina charter school program, and section 4 describes the sample and the data. Sections 5, 6, and 7 examine the impacts on students who attend charter schools, measure the effect of competition from charter schools on students in nearby public schools, and briefly explore the effect of student turnover on the quality of charter schools. A concluding section discusses the implications of our findings.

2. PREVIOUS RESEARCH

In principle, the best way to determine how effective charter schools are in raising student achievement would be to use a random experiment. One could imagine two types of experiments. One type would take the students interested in attending a particular charter school and randomly assign them either to the charter school or to a control group of students who would not have access to that school. One could then measure how effective that charter school is for the types of students who apply to it by comparing the average achievement of the students who were admitted to the school with that of the students in the control group. That approach would be useful for determining whether that particular charter school (or, more generally, the educational program it offered) was effective. A second approach, one that is more similar to the experimental designs for investigating the effect of voucher programs, would be to start with all the students interested in attending any charter school and to randomly assign them either to a treatment group that would give them the right to attend any charter school or to a control group of students who do not have access to charter schools.[2] This latter type of experiment would be useful for determining the average impact of the presence of a whole system of charter schools on student achievement. To the best of our knowledge, no one has attempted to do an experiment of the latter type, and only a few efforts have been made to implement the first strategy.[3]

More common are studies based on administrative data that are designed to determine the effects of a whole system of charter schools. Because these studies are not based on random experiments, users of this approach must be particularly attentive to the issue of selection bias. The most convincing estimates from administrative data of the impact of charter schools on the students who attend them are found in Hanushek, Kain, and Rivkin's (2002) analysis of charter schools in Texas. Drawing on student-level panel data similar to the data employed in this study, they use a model with student fixed effects to isolate the average impact of charter schools on charter school students. The authors find that students in state-sponsored charter schools show significantly smaller test score gains than they would have exhibited had they remained in traditional public schools but that these negative effects diminish as charter schools gain more operating experience. The differences from traditional public schools become statistically insignificant for charter schools operating for three or more years.

Using similar data and methods, Gronberg and Jansen (2001) find a similar pattern of effects for Texas charter schools.[4] These authors also find that

2. These voucher experiments are described in Howell and Peterson (2002).
3. One such study is that by Hoxby and Rockoff (2004), which approximates the random assignment approach by comparing achievement outcomes for students who were accepted or not accepted through a lottery process to three oversubscribed charter schools in Chicago. At best, the positive achievement effects that they report apply to the programs offered by, and to the types of students that applied to, these three oversubscribed schools.
4. One difference between the models estimated by Gronberg and Jansen (2001) and by Hanushek, Kain, and Rivkin (2002) is worth noting. Hanushek, Kain, and Rivkin estimate a model where the change in test scores, A_i, between year t and $t-1$ is on the left-hand side: $A_{it} - A_{i,t-1} = \alpha C_{it} + X_{it}B + \gamma_i + \varepsilon_{it}$, where C_{it} is an indicator of charter school status, X_{it} is a vector of control variables, and γ_i is the effect of unobserved, time-invariant student characteristics. Although it is not entirely clear from their presentation, it appears that Gronberg and Jansen (2001) estimate a

students in charter schools serving mostly at-risk students make slightly larger gains than the average student in traditional public schools. Because charter schools have fewer resources on average than traditional public schools, the authors conclude that charter schools are more efficient than traditional public schools in generating student achievement.

Results comparable to those of Hanushek, Kain, and Rivkin (2002) also emerge from a similar study on the performance of charter schools in Florida (Sass, this issue). Sass finds that student achievement in math and reading is initially lower in charter schools. After operating for four years, however, charter schools generate achievement at levels comparable to those of the traditional public schools.

Additional support for the view that students in charter schools do less well than students in traditional public schools emerges from the experience of Michigan, where Eberts and Hollenbeck (2001) found that students attending charter schools there had lower test scores than other students even after the authors controlled for student, building, and district characteristics, including measures of past achievement levels. In contrast, Solmon, Paark, and Garcia (2001) conclude that students enrolled in charter schools in Arizona for two or more consecutive years made larger gains on standardized tests of reading than students who attended traditional public schools.[5] Unlike the studies in Texas and Florida, however, neither the Michigan study nor the Arizona study controlled for student fixed effects on achievement gains. Thus, whether differences across these studies reflect differences in charter school policy across states, methodological differences, or differences in actual outcomes is not clear. One goal of the present study is to shed light on this issue by replicating with North Carolina data the approach used by Hanushek, Kain, and Rivkin (2002) for Texas.

Similarly, mixed results emerge from existing research on how competition from charter schools has affected the performance of students in traditional public schools.[6] Using school-level data from Michigan and Arizona in

model with the test score from year t on the left side and the test score from $t-1$ on the right side: $A_{it} = \alpha A_{i,t-1} + \beta CH_{it} + X_{it}\Phi + \gamma_i + \varepsilon_{it}$. Controlling for student fixed effects in this model is similar to estimating the following differenced equation:

$A_{it} - \bar{A}_i = \alpha(A_{i,t-1} - \bar{A}_i) + \beta(C_{it} - \bar{C}_i) + (X_{it} - \bar{X}_i)\Phi + (\gamma_i - \gamma_i) + (\varepsilon_{it} - \bar{\varepsilon}_i)$, where each variable value is expressed as a deviation from the individual mean. We can see from this last formulation that in the Gronberg and Jansen (2001) model a component of the differenced error term is correlated with a component of the differenced lagged dependent variable, which biases the fixed-effect estimator (Baltagi 1995).

5. Estimated effects on gains in math were not robust across model specifications, and thus the authors do not draw conclusions about charter school impacts on math.

6. The articles discussed here focus specifically on the effects of competition from charter schools. Belfield and Levin (2002) provide a more comprehensive review of the effect of competition on school performance and find that any positive impacts are either substantively small or subject to question based on subsequent studies.

separate analyses, Hoxby (2001) examines changes in mean test scores before and after the introduction of charter schools. For both states, she finds that schools that face competition from charter schools show larger improvements (by about 1 to 3 percentile points) in average performance levels than schools not facing significant charter school competition.[7] Her analysis does not, however, address the possibility that changes in the student composition of schools might confound the estimated effects of charter school competition. In another study of Michigan schools, Bettinger (1999) uses an instrumental variable strategy to address the possibility that the location of charter schools is influenced by the performance of the public schools and finds no evidence that competition from charter schools improves the performance of students in traditional public schools.

Using school-level panel data from North Carolina and distance from a charter school to measure competition, Holmes, DeSimone, and Rupp (2003) find that average test scores increased about 1 percent more in schools facing competition from charter schools than in other schools. In a companion study, Holmes (2003) uses individual-level data to examine the same question and finds that any gains to students in schools located near charter schools are small at best—no more than one- to two-tenths of a percentile. Because Holmes and his colleagues do not use a full student-level panel, they are not able to account fully for potential differences between students in schools located near charter schools and those in schools located elsewhere. The results we present below are not subject to this limitation.

3. CHARTER SCHOOLS IN NORTH CAROLINA

Legislation authorizing charter schools in North Carolina was passed in 1996, and the first charter schools opened in fall 1997.[8] Figure 1 summarizes charter school policy in North Carolina. Though less permissive than Arizona and Michigan, North Carolina has taken a moderately permissive approach to charter schools compared to most other states.[9] Of particular importance for this study are that the North Carolina charter schools receive operating funding at the same level as the traditional public schools and that the students in

7. Hoxby (2001) counts a school as facing charter school competition if at least 6 percent of the students enrolled in its district (in the case of Michigan) or municipality (in the case of Arizona) are enrolled in charter schools.
8. Except where noted, the description of the North Carolina charter school program that follows is based on the charter school legislation.
9. Annual rankings by the Center of Education Reform (CER) have rated North Carolina among the states with "strong charter laws" and have ranked its law from the seventh strongest to the twelfth strongest in the nation. "Stronger" laws by CER's measures place fewer restrictions on establishing a charter school, place fewer regulations on charter school operations, and provide more funding for charter schools than "weaker" laws. The most recent report by CER ranks the charter school law in North Carolina as "stronger" than those in California, Wisconsin, and Texas, among other states (CER 2003).

Figure 1

Timing and number

First charter schools established in 1997; limit of 100 schools, with no more than 5 new charter schools in any district in a single year.

Sponsors and approval of charters

Eligible sponsors are local school districts, the state university or the State Board of Education, but final approval must come from the State Board of Education; local districts can comment on how the charter school would affect them but their approval is not required; charter is renewable for five-year periods.

Regulations and restrictions on charter schools

No affiliation with a religious institution; subject only to regulations related to the health safety and discipline of students and specific regulations that apply to charter schools; at least 75 percent of teachers in grades K–5 and 50 percent in grades 6–12 must be certified.

Funding

Access to full per pupil state support for schools in the state plus prorated share of locally financed supplements for education; no state-supported access to start-up funding; access to federal start-up funds.

Accountability

Charter schools are subject to the state testing requirements and to the state's accountability program.

Charter School Policy in North Carolina

charter schools are subject to the same state testing requirements as other students. Because North Carolina has been testing all students in grades 3–8 in math and reading since 1992–93, test results can be matched for individual students over a long period of time.

Charters can be revoked for a number of reasons, including poor student performance and financial mismanagement. Between 1997 and 2002 the state board of education revoked seven charters, and seven more relinquished their charter voluntarily or closed due to low enrollment or financial problems. Overall, about 12 percent of the charter schools that have been opened are now closed. However, in no case was the decision to revoke a charter or to close due primarily to low student performance (Manuel 2002).

Table 1 details the growth in charter schools in North Carolina. By 2000–1 there were 90 charter schools and over 15,000 charter school students. Growth in the number of charter schools has slowed since 2000–1, primarily because the state law caps the number of charter schools at 100. Charter schools in North Carolina are more likely to be elementary or middle schools than high schools, and most charter schools serve at least some students between grades 4 and 8, the grades examined in this study. The 93 charter schools in 2001–2 are spread across 46 of North Carolina's 100 counties. During the 2001–2 school year, Wake County (home to the state capital, Raleigh) and nearby Durham County had the highest concentration of charters: 12.4 and 18.2 percent of public schools, respectively. In Charlotte-Mecklenberg, the state's most populous urban area, only 6 of 130 public schools were charters in 2002. As of 2002, only seven states had more charter schools than North Carolina; and of those, only five had a greater concentration of charter schools: Arizona, Florida, Wisconsin, Michigan, and California.

Table 2 shows how the mix of students in North Carolina charter schools differs from that in traditional public schools. Compared to traditional public schools, charter schools have a larger percentage of black students and lower percentages of Hispanic and white students. At the same time, charter schools serve a higher percentage of students whose parents are college educated and a lower percentage of students whose parents are high school dropouts. Despite the higher education level of their parents, these students exhibit lower levels of performance on both end-of-grade (EOG) reading and math tests. The analysis below is designed to determine how much of this difference in student performance can be attributed to the charter schools themselves.

4. THE NORTH CAROLINA DATA

The data for this study come from the North Carolina Education Research Data Center. A collaborative effort involving the North Carolina Department of Public Instruction (DPI), Duke University, and the University of North Carolina, the Data Center collects a wide range of administrative data from the DPI and other sources and prepares the data for use by researchers. The data in this study come primarily from individual-level EOG test score files maintained by the DPI.

For purposes of this study, student-level panels were assembled for five cohorts of students—the cohorts of students in third grade in 1996, 1997, 1998, 1999, and 2000. Each cohort contains the universe of students in third grade in North Carolina during the specified year. Each student has a unique identifier that is consistent over time, allowing us to follow students from third grade through the last year that they remain in North Carolina public schools, the year they complete eighth grade or the 2001–2 school year, whichever comes first. Figure 2 depicts the structure of the data set. We observe most of the students who were in third grade in 1995–96 (cohort 96) each year from 1995–96 through 2000–1 as they move from third grade through eighth grade.[10] Similarly, we follow the cohort in third grade in 1996–97 through eighth grade. We are able to follow subsequent cohorts only through 2001–2, which is before these students reach eighth grade.[11]

10. We are unable to observe all students in all years because we cannot observe those students who leave North Carolina schools before they reach eighth grade. Regardless of whether a student proceeds as expected from third through eighth grade, we observe the student as long as he or she remains in North Carolina public schools.

11. Because not all students progress through grades as expected, some students are part of more than one cohort. For instance, students from the cohort of third graders in 1995–96 who are held back in

Table 1 Number of Charter Schools and Charter School Students in North Carolina, by Grade Level and Year

	GRADES K–8		HIGH SCHOOL		UNITARY		TOTAL	
	Schools	**Students**	**Schools**	**Students**	**Schools**	**Students**	**Schools**	**Students**
1997–98	27		1		6		34	
	(2.0%)		(0.3%)		(5.8%)		(1.9%)	
1998–99	44	7,249	4	270	11	1,036	59	8,555
	(2.6%)	(0.8%)	(1.3%)	(0.1%)	(9.1%)	(3.0%)	(2.8%)	(0.7%)
1999–2000	52	9,667	6	526	19	2,498	77	12,691
	(3.1%)	(1.1%)	(1.9%)	(0.2%)	(15.2%)	(8.1%)	(3.6%)	(1.0%)
2000–1	63	12,371	7	783	20	2,369	90	15,523
	(3.6%)	(1.3%)	(2.1%)	(0.2%)	(19.2%)	(9.8%)	(4.1%)	(1.2%)
2001–2	67	13,517	8	1,263	18	3,455	93	18,235
	(3.7%)	(1.4%)	(2.3%)	(0.4%)	(17.0%)	(11.6%)	(4.1%)	(1.4%)

Notes: Enrollment figures are taken from the NCES Common Core of Data, which does not provide information on charter schools for 1997–98. Enrollment counts are for schools in the identified category, not for students in the grade ranges indicated. Figures in parentheses are the percentages of all North Carolina schools in the category that are charters and the percentage of all students in the category that are in charter schools.

Table 2 Descriptive Statistics on Charter and Traditional Public Schools, 2001–2

	Charter Schools	Traditional Public Schools
Average enrollment[a]	196	574
% female[a]	48.9	48.8
Ethnic composition[a]		
% black	39.9	31.2
% Hispanic	2.1	5.3
% white	55.5	60.0
Parent education[b]		
% less than high school	3.9	10.6
% high school grad	34.6	43.7
% some college, but did not graduate	4.8	4.1
% two-year college degree	11.6	13.4
% four-year college degree	36.6	22.8
% graduate school degree	8.6	5.3
% that changed schools in last year[b]	47.4	13.2
Avg. performance on EOG reading[b,c]	–0.057	0.001
Avg. performance on EOG math[b,c]	–0.133	0.002

Notes: [a] Figures are calculated using Common Core data and are based on entire population of schools. [b] Figures are computed using individual student end-of-grade (EOG) files maintained by the North Carolina Education Research Data Center and thus are based only on students in grades 3–8. [c] EOG test scores converted to standard scores with mean of zero and standard deviations of one. Grade-specific means and standard deviations were used to make the conversions.

Figure 2

	1995–96	1996–97	1997–98	1998–99	1999–2000	2000–1	2001–2
cohort 96	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	
cohort 97		Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
cohort 98			Grade 3	Grade 4	Grade 5	Grade 6	Grade 7
cohort 99				Grade 3	Grade 4	Grade 5	Grade 6
cohort 00					Grade 3	Grade 4	Grade 5

Cohorts of Students Examined in This Study

The information available for each student in each year includes their scale scores on the EOG reading and math tests, their school, whether their school is a charter, their grade, their gender, their ethnicity, and the highest level of education completed by their parents. EOG reading and math tests are

third grade are also part of the cohort of third graders in 1996–97. In the analyses presented here, the five cohorts are combined. In cases where combining cohorts resulted in duplicate observations for a student in a single year, one of the two observations was eliminated by applying a set of decision rules, which are available from the authors upon request.

multiple-choice tests that measure the achievement of competencies described in the North Carolina *Standard Course of Study* and are administered in the spring of each year to students in grades 3–8. Individual results are reported as developmental scale scores, which are designed to measure growth in reading and math and thus are expected to increase as students move from lower grades to higher grades.[12] In order to ensure comparability of test scores and test score gains for students from different grades, we use grade-by-year-specific averages and standard deviations to convert the developmental scale scores to standard scores with a mean of zero and standard deviation of one.

To distinguish the effects of charter schools themselves from the effects of movement among schools, we created two indicator variables: one to denote whether the student changed schools in the current year, and the other to denote whether the student made a structural school change (i.e., moved from elementary to junior high school). A student is counted as having changed schools if the school identifier in the current year differs from the school identifier for that student in the previous year and the change was not a structural change. A change of schools is considered structural if the student moved to a school in the same district and more than 10 percent of the students in the same grade and school as the student in the previous year are also in the same grade and school as the student in the current year.

The school identifier allows us to link each observation to school-level data from the Common Core of Data. In addition, we have created variables for each school that indicate its distance from the nearest charter school and the number of charter schools within various radii of the school. To calculate these distances we matched addresses from the Common Core[13] to latitude and longitude coordinates using a geocoding service provided by Teleatlas. The distances calculated are straight-line distances (with an adjustment for the curvature of the earth) between the latitude and longitude coordinates.

Table 3 provides information on the number of students and observations in each cohort. Cohorts range in size from 93,349 in 1996 to 106,106 in 2000. The average number of times we observe students in a given cohort is determined by two factors: the number of years between when the students in the cohort are in third grade and either when they are in eighth grade or 2001–2,

12. For more information on the end-of-grade tests see http://www.ncpublicschools.org/reportsstats.html.
13. Addresses for charter schools provided in the Common Core were checked against address information taken from materials on the North Carolina Department of Public Instruction (DPI) web site. In case of a conflict or missing address data in the Common Core, addresses from DPI were used.

Table 3 Number of Students and Observations in Each Cohort

	Cohort 96	Cohort 97	Cohort 98	Cohort 99	Cohort 00	Total[a]
Number of students	93,349	98,404	102,869	105,292	106,106	495,943
Avg. observations/student	4.9	5.3	4.5	3.7	2.8	4.3
Avg. observations/student with valid reading scores	4.8	5.1	4.3	3.5	2.7	4.1
Avg. observations/student with valid math scores	4.8	5.1	4.4	3.6	2.7	4.2
Number of students observed at least once in a charter school	1,145	1,603	2,009	2,181	2,035	8,745
Students with reading gains observed in both charter and traditional public school	1,103	1,360	1,461	1,270	644	5,724
Students with math gains observed in both charter and traditional public school	1,106	1,363	1,467	1,277	645	5,741

Note: [a]Total counts are not equal to the sum of each cohort because a small percentage of students appear in more than one cohort.

and the percentage of missing observations. The percentage of missing observations can be determined as the ratio of the average observations per student to the number of times we would observe a student in the absence of missing observations. This calculation implies that the percentage of missing observations falls from 19 percent in the first cohort to 7 percent in the final cohort and is about 9.5 percent of the total sample.[14] Test scores from a particular year might be missing for a student because that student left the North Carolina public school system, was exempted from taking the test, or has a missing or an invalid test score for some other reason.

The bottom three rows of Table 3 provide information on the number of charter school students in our sample. We observe 8,745 students who spent at least one year in a charter school. As we explain below, our preferred estimates of the impact of charter schools are based on students for whom we observe test score gains at least once in both a traditional public school and a charter school. High percentages of charter school students in the first two cohorts are observed in a traditional public school at some point. For cohorts who were in third grade after the charter school program started, the percentages are smaller. Overall, we observe measures of test score gains in traditional public schools for approximately 65 percent of the charter school students in our sample.

Table 4 compares three groups of students in our sample: students who are observed only in traditional public schools, students observed at least once in a charter school, and students for whom either reading or math gains are observed at least once in a charter school and at least once in a traditional public school. Compared to the students observed only in traditional public schools, the charter school students in our sample exhibit the same patterns as in Table 2: they are more likely to be black, less likely to be white or Hispanic, less likely to have parents with a high school education or less, and more likely to have college-educated parents. They also have lower student test scores, where test scores have been normalized to have a mean of zero and standard deviation of one.

Note that the characteristics of the students observed in both a charter and a traditional public school (last column) are very similar to those for the larger sample of all charter school students (middle column). Thus, we have some assurance that our preferred estimates of charter school impacts reported below are based on a subsample of charter school students that is similar demographically to the larger group of all students in charter schools. The

14. The weighted average of the number of times we would observe a student in the absence of missing observations across all five cohorts is 4.75. The percent of missing observations in the total sample is then $1 - (4.30/4.75) = 0.095$.

Table 4 Descriptive Statistics for Study Sample

	Students observed only in traditional public schools	**Students observed at least once in a charter school**	**Students observed in a charter and traditional public school**[a]
Ethnicity			
% black	30.4	44.0	43.6
% Hispanic	3.4	1.4	1.3
% white	63.0	52.7	53.5
Parent education			
% less than high school	10.5	4.7	4.5
% high school graduate	43.6	37.1	37.2
% some college	5.0	5.5	5.2
% 2-year college degree	13.6	13.6	13.6
% 4-year college degree	21.9	31.5	31.2
% graduate school degree	5.2	7.2	8.2
Average reading score[b]	0.002	–0.122	–0.095
Average math score[b]	0.004	–0.215	–0.181
Average reading gain[b]	–0.010	–0.025	–0.029
Average math gain[b]	–0.011	–0.020	–0.034
Grade first observed in a charter school			
Grade 3[c]		2,224	386
Grade 4		1,642	629
Grade 5		1,827	1,787
Grade 6		2,209	2,149
Grade 7		625	589
Grade 8		218	206

Notes: [a] Students with either reading or math gains observed at least once in a charter school and at least once in a traditional public school. [b] EOG test scores converted to standard scores with mean of zero and standard deviations of one. Grade-specific means and standard deviations were used to make the conversions. [c] Students first observed in a charter school in grade 3 might have first entered a charter school anytime from kindergarten to grade 3.

subsample does, however, differ from the larger group in important ways. The subsample has slightly higher average test scores and shows smaller average gains. The differences in math gains are particularly marked. In addition, students who first entered a charter school before grade 5 are underrepresented. An important question is whether the effects of charter schools are markedly different for students who enter at early grades than those who enter at later grades, an issue we discuss below.

5. IMPACTS OF CHARTER SCHOOLS ON CHARTER SCHOOL STUDENTS

In this section we report estimates of the average difference between the achievement of charter school students and what those students would have achieved in the absence of charter schools.

Estimation Strategy

The primary challenge in estimating this charter school effect arises from the fact that charter school students are self-selected and are likely to differ in unobserved ways from otherwise similar students who choose to remain in traditional public schools. To address this challenge, we follow the strategy used by Hanushek, Kain, and Rivkin (2002) and use repeated observations on individual students to control for individual fixed effects. Essentially we are comparing the test score gains of students in charter schools to the test score gains made by the same students in traditional public schools.

Interpretation of our estimates draws on the following model of educational production:[15]

$$Y_{iGT} = \alpha CH_{iGT} + X_{iGT}B_G + \sum_{t=1}^{t=T-1} \lambda_t(\alpha CH_{igt} + X_{igt}B_g) + \gamma_{iG} + \sum_{g=4}^{g=G-1} (\gamma_{ig}) + \eta_{GT} + \varepsilon_{iGT} \quad (1)$$

where Y is a test score for student i in grade G in year T, X is a set of individual student characteristics, including variables indicating whether or not student i made a structural school change, a nonstructural school change, or no change during year t.[16] The variable of interest in this study is CH, which indicates that student i attended a charter school in year t.[17] In this general form of the model, the effects of the control variables on student test scores are allowed to vary by grade, so that parent's education, for instance, might matter more (or less) in later grades than in earlier grades. The effects of school and student characteristics from previous years carry over to year T but degrade at a rate given by $(1 - \lambda_t)$. This form also contains the effects of

15. The discussion here draws on a long-standing literature on educational production. For examples from this literature see Summers and Wolfe (1977), Hanushek (1979), and Ferguson and Ladd (1996). The general form of the production function presented here, and its usefulness in identifying potential biases in our analysis, was suggested to the authors by Robert Kaestner.
16. Typically a production function includes measures of school inputs as well as student characteristics. In this case, however, we are interested in estimating the total charter school effect, including any effect that might be due to input differences between charter and traditional public schools, so it is not appropriate to control for school inputs.
17. Charter school status is a school characteristic. The combination of school-level and individual-level variables in any production function study, including ours, calls for the use of robust standard errors. All of our standard errors are adjusted for clustering within schools using the "cluster" option in STATA, which makes use of a generalization of the Huber/White/Sandwich estimator of variance.

unobserved student characteristics, γ, which are assumed to accumulate in an additive fashion from year to year and which might also vary by grade, and grade-by-year effects, η, which capture systematic differences across exams. The final term represents random error. The coefficient α is the average effect of attending a charter school in year T, and the sum of that effect across all the years a student has attended a charter school is the cumulative charter school effect.

This general form of the production function cannot be estimated because the number of grade-specific individual effects is coincident with the number of observations. In addition, a complete set of explanatory variables from previous years is not available. Consequently, restrictions have to be placed on the general form of the model to obtain estimates. Nonetheless, this general formulation is useful for clarifying the identifying assumptions of and potential sources of bias in the various estimates we present.

We estimate three empirical models, for both reading and math test scores, which place different restrictions on the general model. The third model provides our preferred estimates of charter school impacts. The first model, which we refer to as a "levels model," can be written as:

$$Y_{iGT} = \alpha CH_{iGT} + X_{iGT}\mathrm{B} + \eta_{GT} + \varepsilon_{iGT}. \tag{2}$$

This model, which we estimate using ordinary least squares (OLS) and robust standard errors, yields the difference in levels of performance between charter school students and traditional public school students controlling for observable student characteristics and grade-by-year effects. This formulation places severe restrictions on the general form of the model in equation (1). The effects of the control variables are assumed to be the same across different grades, and the effects of the student's educational experiences in previous years are restricted to zero, as are the effects of other unobserved student characteristics. Because the past educational experiences of the student and other unobserved factors such as the student's motivation are likely to influence both student test scores and the choice to enroll in a charter school, omitting these variables is likely to bias the estimates of the charter school effect.

A second approach restricts the effects of student characteristics to be the same across years and replaces the additive individual effect with a one-time fixed student effect. This yields:

$$Y_{iGT} = \alpha CH_{iGT} + X_{iGT}\mathrm{B} + \sum_{t=1}^{t=T-1}(\alpha CH_{igt} + X_{igt}\mathrm{B}) + \gamma_i + \eta_{GT} + \varepsilon_{iGT}. \tag{1A}$$

Taking the first-difference of (1A) produces our second model, which we estimate using OLS and robust standard errors:

$$\Delta Y_{iGT} = Y_{iGT} - Y_{i(G-1)(T-1)} = \alpha CH_{iGT} + X_{iGT}\mathrm{B} + \lambda_{GT} + \nu_{iGT}$$
$$\lambda_{GT} = \eta_{GT} - \eta_{(G-1)(T-1)} \tag{3}$$
$$\nu_{iGT} = \varepsilon_{iGT} - \varepsilon_{i(G-1)(T-1)}.$$

We call this the "gains model" because it estimates the difference between the average test score gain made by charter school students and traditional public school students controlling for observable student characteristics and grade-by-year effects. By focusing on gains in student achievement during a given year, this model eliminates the need to adjust estimates of the charter school effect for educational experiences prior to year t. However, if unobserved differences between charter school and traditional public school students affect the rate of growth in student performance as well as its level—that is, if unobserved student characteristics have additive effects (as in (1)) rather than a one-time effect (as in (1A))—then estimates of α from the gains model will generate biased estimates of the effect of attending a charter school.

The third model can be derived by restricting λ_t in equation (1) to one, taking the first-difference, and restricting the effects of student characteristics to be the same across grades:[18]

$$\Delta Y_{iGT} = Y_{iGT} - Y_{iG(T-1)} = \alpha CH_{iGT} + X_{iGT}\mathrm{B} + \gamma_i + \lambda_{GT} + \nu_{iGT}$$
$$\lambda_{GT} = \eta_{GT} - \eta_{(G-1)(T-1)} \tag{4}$$
$$\nu_{iGT} = \varepsilon_{iGT} - \varepsilon_{i(G-1)(T-1)}.$$

This is the gains model with an individual fixed effect, γ_i, and the coefficients are estimated using the "within" student estimator (Baltagi 1995) and robust standard errors. Using the first-difference formulation eliminates the need to control for previous educational experiences, and the fixed-effects estimation controls for any unobserved differences between charter school students and traditional public school students that remain constant over time. Estimation of this model requires three or more observations for each student, which, with the exception of the Texas and Florida studies discussed above, has not been available in previous quasi-experimental evaluations of school choice programs.[19]

18. This last restriction is empirically investigated and partially relaxed below.
19. An alternative fixed-effects strategy, used by Rouse (1998) in her evaluation of the Milwaukee voucher program and by Solmon, Paark, and Garcia (2001) in their study of charter schools in

This third model, which we refer to as the fixed-effects model, provides powerful protection against self-selection bias. However, this protection comes at a cost. Note that the estimated effects of charter schools from this model are based on the experiences of only those students who have test scores gains observed at least once in a charter school and at least once in a traditional public school.[20] The estimator could provide biased estimates of the average charter school effect if the subsample of students used to identify the charter school effect is not representative of all charter school students. We discuss this issue further below.

Primary Results

The first three columns of Tables 5A (reading) and 5B (math) present our estimates of the levels, gains, and fixed-effects models. Turning first to the control variables, we find results that are generally consistent with expectations. Females exhibit higher levels of achievement in both math and reading, and larger annual gains, although the difference in gains is significant only for math. Blacks and Hispanics exhibit lower levels of achievement than whites. Hispanics, however, make larger annual gains in both reading and math than either blacks or whites. Both the level of achievement and annual gains in achievement are higher for students with more educated parents. Children of college graduates, for example, score more than one standard deviation higher than children of high school dropouts. Finally, students who change schools, either because of a move or because they are transitioning to middle school, make smaller gains during their transition year than students who remain in the same school.

Emerging from all three models and for both subjects are negative and statistically significant coefficients on the charter school indicator variable. Because the dependent variable is expressed as a standard score with a mean of 0 and a standard deviation of 1, the coefficients can be interpreted as proportions of a standard deviation. In the levels models, charter school students score, on average, 0.16 of a standard deviation lower in reading and about 0.25 of a standard deviation lower in math than observationally similar students in traditional public schools. From the gains models, we see that students in

Arizona, regresses test score levels on the treatment indicator controlling for individual fixed effects. The fixed-effects estimates in those studies do not provide as much protection against self-selection bias as this method because they do not control for the additive effects of unobserved characteristics on test score gains, i.e., they do not control for effects of unobserved individual characteristics on test score growth.

20. Only the impacts of variables that change over time can be distinguished from the individual student fixed effects. For the same reason, we cannot obtain estimates of the impacts of gender, ethnicity, and parental education from the fixed-effects models.

Table 5A Estimated Impacts of Attending a Charter School on Reading Test Scores

	Levels	**Gains**	**Fixed Effects**	**Fixed Effects II**
Charter school	–0.158**(0.044)	–0.062**(0.009)	–0.095**(0.014)	
Charter school (for students observed entering charter only)				–0.062**(0.015)
Charter school (for students observed exiting a charter school)				–0.155**(0.021)
Gender (male = 0, female = 1)	0.174**(0.002)	0.001 (0.001)		
Ethnicity[a]				
Black	–0.351**(0.023)	–0.029**(0.004)		
Hispanic	–0.002 (0.025)	0.041**(0.005)		
White	0.235**(0.023)	–0.011**(0.004)		
Parent education[b]				
High school grad	0.444**(0.005)	0.005* (0.002)		
Some college	0.679**(0.006)	0.016**(0.003)		
2-year college degree	0.784**(0.006)	0.016**(0.002)		
4-year college degree	1.130**(0.008)	0.022**(0.002)		
Graduate school degree	1.419**(0.011)	0.027**(0.003)		
Changed schools in last year	–0.133**(0.005)	–0.018**(0.003)	–0.013**(0.004)	–0.013**(0.004)
Made structural change in last year	–0.048**(0.007)	–0.065**(0.006)	–0.056**(0.007)	–0.056**(0.007)
Total observations	1,527,157	1,512,587	1,494,885[c]	1,494,885[c]
Total students	445,562	441,863	424,066[c]	424,066[c]

Notes: All models include grade/year fixed effects. Dependent variable is EOG developmental scale score expressed as a standard score. Figures in parentheses are robust standard errors calculated using generalization of Huber/White/Sandwich estimator and are robust to clustering within schools.

[a] Reference category is Asian and Native American. [b] Reference category is high school dropouts. [c] Sample count includes only those observations of students with at least three valid test score measures, which is the minimum required to identify fixed effects and effect estimates for nonconstant variables.

* statistical significance at .05 level, ** statistical significance at the .01 level.

charter schools also make smaller annual gains, on average, than observationally similar students. In neither case can the lower performance necessarily be attributed to being in a charter school. The estimates from the fixed-effects models, however, indicate that the smaller gains made by charter school students are indeed due to enrolling in a charter school rather than to any fixed, unobserved differences between charter school students and students in traditional public schools.

The negative effects of attending a charter school are large. As shown in column 3 of both tables, charter school students exhibit gains nearly 0.10

Table 5B Estimated Impacts of Attending a Charter School on Math Test Scores

	Levels	Gains	Fixed Effects	Fixed Effects II
Charter school	–0.255**(0.073)	–0.076**(0.021)	–0.160**(0.021)	
Charter school (for students observed entering charter only)				–0.097**(0.022)
Charter school (for students observed exiting a charter school)				–0.272**(0.030)
Gender (male = 0, female = 1)	0.036**(0.002)	0.009**(0.001)		
Ethnicity[a]				
Black	–0.464**(0.023)	–0.019**(0.005)		
Hispanic	–0.046 (0.024)	0.020**(0.006)		
White	0.155**(0.023)	–0.020**(0.005)		
Parent education[b]				
High school grad	0.386**(0.005)	–0.007**(0.002)		
Some college	0.603**(0.006)	0.005 (0.003)		
2-year college degree	0.705**(0.006)	0.004 (0.003)		
4-year college degree	1.076**(0.008)	0.029**(0.003)		
Graduate school degree	1.404**(0.014)	0.058**(0.004)		
Changed schools in last year	–0.160**(0.005)	–0.030**(0.004)	–0.027**(0.005)	–0.028**(0.005)
Made structural change in last year	–0.044**(0.008)	–0.068**(0.008)	–0.061**(0.010)	–0.061**(0.010)
Total observations	1,533,367	1,520,132	1,502,339[c]	1,502,339[c]
Total students	446,855	443,548	425,654[c]	425,654[c]

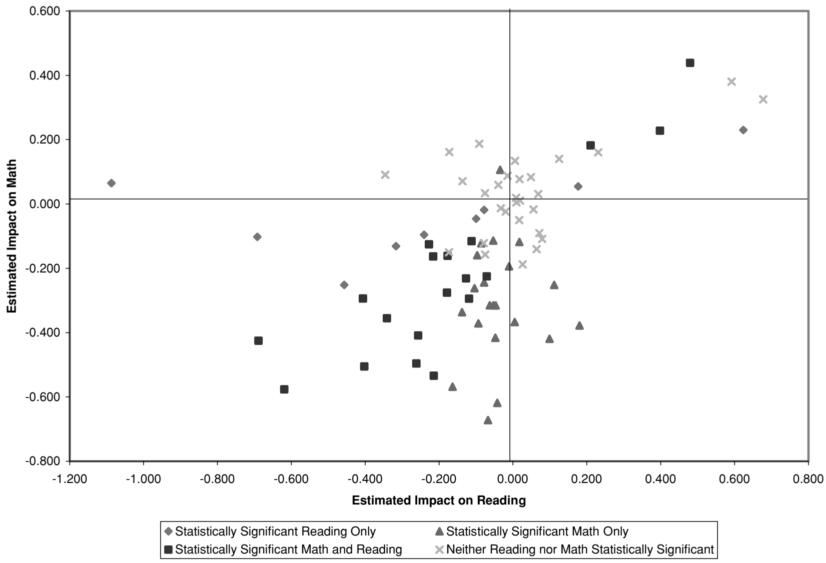
Notes: All models include grade/year fixed effects. Dependent variable is EOG development scale scores expressed as a standard score. Figures in parentheses are robust standard errors calculated using generalization of Huber/White/Sandwich estimator and are robust to clustering within schools.

[a] Reference category is Asian and Native American. [b] Reference category is high school dropouts. [c] Sample count includes only those observations of students with at least three valid test score measures, which is the minimum required to identify fixed effects and effect estimates for nonconstant variables.

* statistical significance at .05 level, ** statistical significance at the .01 level.

standard deviations smaller in reading and 0.16 standard deviations smaller in math, on average, than the gains those same students had when they were enrolled in traditional public schools. If such losses compounded annually, a student enrolled in charter schools for five years would score nearly one-half of a standard deviation lower in reading and eight-tenths of a standard deviation lower in math than they would if they remained in traditional public schools. The difference in achievement growth due to being enrolled in charter schools appears to be considerably larger than differences in growth between children of high school dropouts and the children of parents with graduate

Figure 3



Distribution of Estimated Charter School Impacts across Charter Schools

degrees and between blacks and whites—differences that are the object of considerable concern. The negative impacts of enrolling in a charter school are also substantially larger than the negative impacts of changing schools or making the transition from elementary school to junior high.[21]

This finding of a negative average effect need not mean that all charter schools are unsuccessful in raising the achievement of their students. Nonetheless, as shown in Figure 3, many of them appear to exhibit negative impacts on achievement in both math and reading. The figure depicts our estimates of charter school impacts for each of the charter schools in each of the two subjects. Marks in the southwest quadrant represent schools with negative estimated impacts in both subjects. Those in the northeast quadrant exhibit positive impacts in both subjects. The fact that so many schools are in the southwest quadrant indicates that the negative average impact of charter schools on student achievement is not driven by a few atypical outliers. However, it is also worth noting that a handful of charter schools in North Carolina do appear to provide significant achievement benefits for their students.

21. Each of the models presented in Tables 5A and 5B was also estimated with fixed effects for school districts. Including the district fixed effects had negligible impacts on estimates of charter school impacts and the other coefficients.

Possible Biases Related to Peculiarities of the Identifying Sample

Despite its advantages in addressing self-selection bias, the fixed-effects estimator could provide biased estimates if the sample of students used to identify the charter school effect is not representative of all charter school students.[22] As we have already noted (see Table 4), the identifying sample is demographically similar to the larger group of all charter school students in the tested grades. Nonetheless, two differences between the full sample of charter school students and the subsample that is used to identify charter school effects in the third columns of Tables 5A and 5B are worth noting.

One concern is that students who leave charter schools to return to public schools may be overrepresented in the identifying sample. This overrepresentation of exiters would bias downward the effect of charter schools (that is, make it more negative than the true effect for all charter school students) to the extent that these students leave because of an unsatisfactory academic experience in charter schools. The size of this bias depends both on the extent to which exiters are overrepresented in the sample and on the size of the difference in outcomes between charter school students who exit and those who do not.[23] Among students whose test score gains are observed at least once in both sectors, 37.1 percent are exiters (i.e., are observed in a traditional public school after they were in a charter school). This percentage exceeds the 30.4 percent exit rate for all charter school students in our sample. Thus, exiters are overrepresented in the identifying sample of switchers by nearly 25 percent. This overrepresentation matters because, as shown in the final columns of Tables 5A and 5B, the negative effect of attending a charter school is larger for exiters than for those students who are observed only entering charter schools. Note, however, that even for the students who are not observed exiting a charter school, the estimated impacts of charter schools are still negative, statistically significant, and substantial. Thus, the overrepresentation of exiters cannot explain away the estimated negative effect of charter schools.

22. Hanushek, Kain, and Rivkin (2002) acknowledge that their fixed-effects estimates cannot be interpreted as how enrollment in a charter school would affect the achievement of the average student. Because the benefits of charter school enrollment are likely to be different for those who choose to enroll than they would be for a randomly selected student, any study that estimates charter school impacts based on the experience of students who choose charter schools cannot be generalized to the broader school population (Heckman, LaLonde, and Smith 1999). The issue here is different. If the set of students on whom we base our estimates of charter school impacts is not representative of the larger charter school population, then we might not be able to generalize to the population of charter school students, let alone the broader population of all public school students.

23. This issue is counterpart to the issue faced by past evaluations of voucher programs in Milwaukee and elsewhere, in which impact estimates were based on stayers. Since those who remain in voucher schools are likely to have had a more positive experience than the average voucher recipient, these earlier evaluations have had to address the possibility that impact estimates are biased upward (Rouse 1998; Howell and Peterson 2002).

To determine the size of the bias created by overrepresentation of exiters, we calculated "true" effect estimates as weighted averages of the estimated effects for students only observed entering charter schools and those observed exiting charters, with the weights set equal to the proportion of each group in the overall sample of charter school students (0.696 for enterers only and 0.304 for exiters). The weighted effect estimates are −0.090 for reading and −0.150 for math, which implies that our original fixed-effects estimates are biased downward by only 5.5 and 6.5 percent of the "true" effect.

A second difference between our identifying subsample and the larger group of charter school students is that students who entered charter schools in the younger grades are underrepresented in the identifying subsample. In contrast to the case for exiters, there is no clear a priori reason for charter school effects to differ between students who enter charter schools in different grades. The data suggest, however, that the gains of students who entered charter schools in the later grades are smaller than those of students who entered in earlier grades. These differences emerge from a modified form of the gains model (not the fixed-effects model, for which such an estimation is not possible) in which we allow the effects of attending a charter school to vary by the grade that the student first entered the charter school. Specifically, for students entering a charter school in third or fourth grade, the estimated effect is −0.052 for reading and −0.041 for math, and for those entering a charter in a later grade the estimated effects are −0.069 for reading and −0.103 for math.[24]

Given the underrepresentation of students who enter during early grades, the smaller negative effects for early entrants, particularly for math, suggest that the true average effects of attending a charter school across all charter school students might be less negative than indicated by the fixed-effects estimates. Nonetheless, the true effects are almost certainly negative. We are quite confident in making this assertion, for the following reasons. First, the estimated charter school effects obtained from the gains model are negative and statistically significant even for students entering a charter school in the early grades. Second, we have reason to believe that those estimates are biased toward zero because they emerge from a model that does not control for student fixed effects.[25]

24. The estimated effects for early grade entrants are statistically significantly at the 0.01 level for reading and at the 0.10 level for math; and for later grade entrants both reading and math estimates are statistically significant at the 0.01 level. The difference between estimated effects for the early grade entrants and for the later grade entrants is not statistically significant in the case of reading but is statistically significant at the 0.05 level in the case of math.

25. The estimates of charter school impacts from the models that control for individual fixed effects are substantially larger (more negative) than those obtained from the gains model without individual fixed effects. The difference between the two sets of estimates reported in Tables 5A and 5B might reflect either self-selection bias in the estimates from the gains model without fixed effects or differences in the sample of charter school students on which the effect estimates are based. When we reestimate the gains model using only those charter school students that help to identify charter

Other Potential Biases

Another potential source of bias is the possibility that competition from charter schools may improve the performance of traditional public schools. To the extent that this occurs, the estimates from the fixed-effects approach would understate the true impact of the charter schools. We address this issue empirically below and conclude that this form of downward bias is inconsequential.

In addition, the fixed-effects estimates in Tables 5A and 5B would be biased downward if students with declining test score gains were more likely than other students to transfer to a charter school. The gains of those students while they were in traditional public schools would systematically overestimate the gains they would have made in subsequent years in the absence of charter schools.[26] To assess the likelihood of this source of bias, we estimated the following equation for math and reading using observations on charter school students while they were in public schools but before they transferred to a charter school:

$$\Delta A_{iGT} = A_{iGT} - A_{i(G-1)(T-1)} = \alpha t + \eta_{GT} + \varepsilon_{iGT}. \qquad (5)$$

ΔA_{iGT} is, as before, the achievement gain of student i in year T; t is a year counter, taking a value of zero in 1997 and increasing by one for each year after; η_{GT} represents grade by year fixed effects; and ϵ is a random error term. Because the test scores have been scaled to have a mean of zero in each grade and year, if gains for all students were observed they would have a mean of zero each year and thus no time trend. The estimated value of α indicates whether the trend in test score gains is significantly different for students

school effects in the fixed-effects model, we get statistically significant impact estimates of −0.068 for reading and −0.104 for math. Both of these estimates are significantly less negative than the estimates provided by the fixed-effects model, which provides evidence that the estimates from the gains model do suffer from self-selection bias and that the differences between the gains models and fixed-effects models in Tables 5A and 5B cannot be attributed solely, or even primarily, to differences in samples.

26. This argument assumes that our estimates are based primarily on charter school students who are observed in a traditional public school prior to being observed in a charter school. In fact, of the students for whom we observe test score gains in both charter and traditional public schools 62.9 percent are observed first in traditional public schools, then in charters; 20.4 percent are observed first in charter schools, then in traditional public schools; and 16.8 percent are observed first in traditional public schools, then in charter schools, and then again in traditional public schools. Less than 1 percent are observed in charter schools, then in traditional public schools, and then again in charter schools.

Hanushek, Kain, and Rivkin (2002) consider the possibility that students experiencing a temporary dip in test score gains in a given year might be more likely to transfer to a charter school. In this case, test score gains of charter school students while they were in traditional public schools would underestimate what would have been observed for those students in the absence of charter schools, and the fixed-effects estimator would overestimate the positive impacts of charter schools (Ashenfelter 1975). However, this type of selection cannot provide an alternative explanation for findings of negative charter school impacts.

who subsequently enroll in a charter school. For reading, the estimate of α is very small, negative (−0.002), and statistically insignificant. For math, α is a larger, positive number (0.022), but still statistically insignificant.[27] These results indicate virtually no trends, on average, in the test score gains of students who subsequently enroll in a charter school, which suggests that the estimates in the last column of Tables 5A and 5B are not biased.

Finally, our estimates might be biased downward if the characteristics that distinguish charter school students from traditional public school students have more negative impacts on student achievement in later grades than in earlier grades. This possibility led us to relax the restriction in our fixed-effects model that the effects of observed student characteristics are constant over time. In regressions that include interactions between student characteristics and grade levels (not shown), we find that the achievement effects of individual characteristics do in some cases differ across grades. However, the variation in effects across grades for a given student characteristic often does not follow an obvious pattern; and, more important, allowing the effect of observed characteristics to vary by grade has virtually no impact on the estimated charter school effect. Thus, our estimates of charter school impacts are robust to assumptions about variation across grades in the influence of student characteristics on achievement.

Extensions

As we discussed above, Hanushek, Kain, and Rivkin (2002) and Gronberg and Jansen (2001) find that the negative impacts of charter schools in Texas disappear for charter schools that have been operating for three or more years. To examine whether that pattern also emerges in North Carolina, we report in the first and fourth columns of Table 6 (Model 1) fixed-effects estimates that allow the estimated impact of attending a charter school to vary with the number of years the charter school has been open. As was the case for Texas, we find that the negative effects of charter schools are larger for newly opened charter schools than for more established charter schools. However, in contrast to the Texas studies, the negative effects of charter schools in North Carolina remain statistically significant and large even for schools that have been operating for five years.[28]

27. The results of these regressions as well as those referenced in the next two paragraphs are available from the authors upon request.

28. The dip in charter school performance during the fifth year is anomalous. There is some support for the explanation that schools that opened during the 1997–98 school year, and thus observed into their fifth year, are of lower quality than the charter schools opened subsequently. Specifically, the fifteen charter schools that we observe into their fifth year of operation, on average, have more negative impacts on student test score gains than the other charter schools, even when observations during the fifth year of operation are excluded. However, when we allow the impact of the fifteen schools observed into their fifth year to vary by year of operation, we find a similar dip

Table 6 Variation in Estimated Impacts of Attending a Charter School by Years of Operation

	READING		MATH	
	Model 1	**Model 2**	**Model 1**	**Model 2**
First year of charter school	–0.184**(0.027)	[–0.174**][a]	–0.312**(0.051)	[–0.284**][a]
Students obs. entering only		–0.144**(0.044)		–0.233**(0.058)
Students obs. exiting		–0.243**(0.027)		–0.401**(0.048)
Second year of charter school	–0.064**(0.019)	[-0.071**][a]	–0.131**(0.028)	[–0.117**][a]
Students obs. entering only		–0.061**(0.025)		–0.081**(0.035)
Students obs. exiting		–0.093**(0.032)		–0.200**(0.039)
Third year of charter school	–0.056**(0.021)	[–0.039*][a]	–0.081**(0.037)	[–0.079**][a]
Students obs. entering only		–0.020 (0.022)		–0.050 (0.045)
Students obs. exiting		–0.084**(0.039)		–0.147**(0.051)
Fourth year of charter school	–0.064**(0.021)	[–0.056][a]	–0.092**(0.030)	[–0.093**][a]
Students obs. entering only		–0.040* (0.024)		–0.067**(0.024)
Students obs. exiting		–0.094 (0.097)		–0.152**(0.073)
Fifth year of charter school	–0.159**(0.050)	–0.110**(0.053)	–0.198**(0.060)	–0.123**(0.053)
Change in schools	–0.011**(0.001)	–0.012**(0.001)	–0.025**(0.002)	–0.027**(0.003)
Structural change in schools	–0.044**(0.001)	–0.044**(0.001)	–0.035**(0.001)	–0.034**(0.002)

Notes: Both estimates include grade/year and individual student fixed effects. Dependent variables are EOG scale scores converted to a standard score with a mean of zero and standard deviation of one. Figures in parentheses are robust standard errors calculated using generalization of Huber/White/Sandwich estimator.

[a] Coefficient marked by (a) are not directly estimated in the model but, rather, weighted averages of the coefficients for students observed entering only and students observed exiting. Note that the weighted average of these coefficients is a linear combination; inferences for these figures are based on Wald tests (Griffiths, Hill, and Judge 1993, p. 453).

* statistical significance at 0.10 level, ** statistical significance at the 0.05 level.

More refined estimates of the effects by year are presented in the second and fourth columns of Table 6 (Model 2). These estimates account for the potential bias that arises from the overrepresentation of students who exit charter schools.[29] We also report the weighted average effects, where the weights, as above, are the proportions of the exiters and the nonexiters observed in the entire sample of charter school students. Consistent with our earlier analysis, the estimated weighted impacts from Model 2 for each year in both math and reading are slightly less negative than those for Model 1. The weighted impact

in impact estimates during the fifth year. For some reason, it appears that these schools performed substantially worse during 2002 than during 2001.

29. All observations of students in a fifth-year charter are from 2002, which is the last year we observe students. Consequently, none of the students observed in a charter during its fifth year are subsequently observed in traditional public schools, and thus we cannot separate the effects on charter school exiters from the effects on those observed entering only.

estimates are still negative, however, in all eight cases and are statistically significant in seven. Thus, even after several years of operation, charter schools apparently continue to reduce student learning relative to what it would have been in the traditional public schools.

A second extension involves disaggregating the average charter school impacts not only by the age of the charter school but also by whether it is the first year a student has been in the school and by whether the student is observed exiting a charter school at some point. Analytically, this disaggregation is achieved by replacing the single charter school indicator in equation (4) with a separate indicator variable for each type of charter school experience. The key results are shown in Tables 7A and 7B. First, the large negative impacts on average appear to be driven largely, but not entirely, by students during their first year in a charter regardless of the age of the school. Remember that these estimates control for the generic effect of changing schools, which is identified primarily by transfers between traditional public schools. For some reason, the year a student newly transfers into a charter has much more negative impacts than transferring into a traditional public school.[30] Second, as indicated by the generally nonsignificant coefficients in the next-to-last column, students who choose to remain in charter schools do not continue to accumulate negative impacts after their initial year in a charter school. This finding is reassuring in that it justifies the decision of many parents to keep their children in charter schools once they are there. However, it is also clear that the initial hit these students take is not offset by gains in subsequent years, so that even this group, which is harmed least by their choice to attend a charter school, still has lower levels of achievement as a result of that choice. Third, the students who ultimately leave charter schools typically exhibit poorer performance in math relative to what they would have done in a traditional public school, both during their first year in a charter school and in subsequent years.

6. IMPACTS OF CHARTER SCHOOLS ON TRADITIONAL PUBLIC SCHOOL STUDENTS

Although the charter school sector has grown rapidly over the last decade, it is still a marginal share of the public school system and is likely to remain so for a number of years. Even so, charter school programs have the potential to have broader impacts on student achievement if traditional public schools respond to the threat of losing students by improving the quality of their own educational programs. To the extent, however, that charter schools draw more motivated students away from traditional public schools and that peer

30. We also ran the models presented in Tables 7A and 7B with controls for whether or not the student's school change interacted with his or her grade, and we obtained virtually identical results.

Table 7A Disaggregated Effects of Charter Schools on Reading

Age of Charter	STUDENTS' FIRST YEAR IN CHARTER SCHOOL: Nonexiter	STUDENTS' FIRST YEAR IN CHARTER SCHOOL: Exiter	STUDENTS' SUBSEQUENT YEARS IN THE SCHOOL: Nonexiter	STUDENTS' SUBSEQUENT YEARS IN THE SCHOOL: Exiter
First year	–0.127**	–0.243**		
	(0.042)	(0.052)		
Second year	–0.074**	–0.176**	–0.015	–0.038
	(0.033)	(0.087)	(0.040)	(0.053)
Third year	–0.061**	–0.537**	0.025	–0.114
	(0.030)	(0.114)	(0.033)	(0.081)
Fourth year	–0.105**	–0.388**	0.000	–0.108
	(0.036)	(0.171)	(0.024)	(0.112)
Fifth year	–0.140**		–0.195	
	(0.062)		(0.130)	

Notes: Estimates are from models that include grade/year and individual student fixed effects, as well as controls for whether or not student made a structural change or other change of school in the current year. Figures in parentheses are robust standard errors calculated using generalization of Huber/White/Sandwich estimator with correction for clustering within schools.

* statistically significant at 0.10 level, ** statistically significant at 0.05 level.

Table 7B Disaggregated Effects of Charter Schools on Math

Age of Charter	STUDENTS' FIRST YEAR IN CHARTER SCHOOL: Nonexiter	STUDENTS' FIRST YEAR IN CHARTER SCHOOL: Exiter	STUDENTS' SUBSEQUENT YEARS IN THE SCHOOL: Nonexiter	STUDENTS' SUBSEQUENT YEARS IN THE SCHOOL: Exiter
First year	–0.209**	–0.475**		
	(0.048)	(0.069)		
Second year	–0.106**	–0.294**	–0.011	–0.193**
	(0.047)	(0.074)	(0.058)	(0.064)
Third year	–0.090	–0.294**	0.003	–0.218**
	(0.065)	(0.134)	(0.042)	(0.075)
Fourth year	–0.156**	–0.185	–0.005	–0.135
	(0.038)	(0.255)	(0.034)	(0.129)
Fifth year	–0.189**		–0.117*	
	(0.049)		(0.063)	

Notes: Estimates are from models that include grade/year and individual student fixed effects, as well as controls for whether or not student made a structural change or other change of school in the current year. Figures in parentheses are robust standard errors calculated using generalization of Huber/White/Sandwich estimator with correction for clustering within schools.

* statistically significant at 0.10 level, ** statistically significant at 0.05 level.

effects matter, the quality of education at those traditional public schools may suffer.

Measuring Charter School Competition

To estimate the competitive effects of charter schools, we must first measure the amount of competition provided by charter schools. Two approaches appear in the literature. Hoxby (2001) identifies schools located in districts that have at least 6 percent of their students enrolled in charter schools as facing charter school competition. This measure is not appropriate for North Carolina, where most districts cover relatively large geographic areas. That measure would miss the competition that occurs for some schools when charter schools are concentrated in one area within a district, and it would overstate competition in other parts of the district. Holmes, DeSimone, and Rupp (2003) and Bettinger (1999) both use distance from a charter school to develop indicators of whether or not schools face competition from charter schools. This approach has the advantage of capturing within-district variation in the amount of charter school competition schools face.

How close does a charter school have to be located to a traditional public school to provide substantial competition for students? We observe 6,576 transfers from traditional public schools to charter schools in our data. For 89.7 percent of these transfers the distance between the charter school where the student enrolled and the school the student attended the previous year is fewer than 10 miles. If the threat of losing students is what motivates traditional public schools to respond to charter schools, then only those charter schools located within 10 miles of a given school are likely to exert much effect on the school.

Table 8 helps us further assess the intensity of competition from charter schools. This table summarizes the distribution of the percentages of students who transfer to a charter school in a given year for schools that are various distances from charter schools. Even among schools within 2.5 miles of a charter school, only slightly more than 1 percent of students are lost to charter schools each year, and only a small percentage of schools have lost as many as 5 percent of their students in any year. If the likelihood of losing students to charter schools indicates the intensity of the competition, the amount of competition provided by charter schools in North Carolina is small.

Table 8 also indicates that whatever competition there is varies reasonably systematically with distance up to the 10-mile radius. In particular, schools within 2.5 miles of a charter school lose a higher percentage of students to charter schools and hence appear to face more competition, on average, than do schools 2.5 to 5 miles from the nearest charter, and so forth. Thus, in this

Table 8 Annual Transfers to Charter Schools by Distance to the Nearest Charter School

Miles to Nearest Charter School	Avg. Annual % of Students Lost to Charter Schools	% of Schools Losing More Than 1% Annually	% of Schools Losing More Than 2% Annually	% of Schools Losing More Than 5% Annually
0–1	1.02%*	38.1%	17.2%	0.9%
1–2.5	1.23%*	35.9%	18.3%	3.8%
2.5–5	0.87%*	26.1%	10.6%	2.6%
5–7.5	0.58%*	17.7%	6.8%	1.7%
7.5–10	0.46%*	12.4%	6.4%	0.9%
10–12.5	0.33%	8.4%	4.6%	0.8%
12.5–15	0.23%	6.9%	2.0%	0.4%
15–20	0.28%	7.2%	3.9%	1.2%
>20	0.24%	6.4%	2.9%	0.8%

Note: * The reported average is significantly different at a 0.05 significance level than the average for schools located more than 20 miles from any charter school (the last column).

section we estimate the separate effects of being within 2.5 miles, between 2.5 and 5 miles, and between 5 and 10 miles of a charter school.

Table 9 indicates that the threat of losing students to a charter school depends not only on the distance to the nearest charter school but also on the number of charter schools within a given radius of the school. For instance, the average percent of students lost to charter schools is twice as high for schools with more than two charter schools located within 5 miles than it is for schools with only one charter school within 5 miles. A school with more than two charters within 5 miles is also more than twice as likely to lose more than 2 percent of its students to a charter school in a given year as a school with only one charter school within 5 miles. Thus, we investigate how the effect of charter schools on traditional public schools varies with the number of nearby charter schools as well as with the distance to the nearest charter.

Estimation Strategy

The location of charter schools is not randomly determined. If charter schools were primarily established in response to dissatisfaction with traditional public schools, charter schools would tend to be located in areas with low-quality traditional public schools where students would tend to make below-average test score gains. Alternatively, charter schools might be more likely to attract students in areas where parents tend to be more motivated and better informed. In those areas, gains in student test scores might be higher than in other areas, even in the absence of charter schools.

Table 9 Annual Transfers to Charter Schools by Number of Charter Schools within Various Distances

	Avg. Annual % of Students Lost to Charter School	% Losing More Than 1% Annually	% Losing More Than 2% Annually	% Losing More Than 5% Annually
# of charters within 2.5 miles				
1	1.15%	35.5%	16.4%	2.7%
2	1.33%	40.9%	23.7%	4.3%
>2	1.03%	39.0%	22.0%	1.7%
# of charters within 5 miles				
1	0.79%	26.7%	10.9%	2.1%
2	1.34%	36.4%	19.0%	5.0%
>2	1.63%	47.2%	24.2%	2.8%
# of charters within 10 miles				
1	0.51%	17.5%	6.8%	1.1%
2	0.95%	23.4%	12.1%	3.0%
>2	1.40%	41.4%	20.2%	3.9%

To protect against potential bias created by the selection of charter school locations, we rely again on individual student fixed effects. Specifically, we estimate equations similar to equations (2), (3), and (4), with two key differences. First, we compute our estimates using observations only of the students in traditional public schools. Second, we replace the variable indicating charter school status with three dichotomous variables indicating whether or not the school attended by the student is within 2.5 miles of a charter school, between 2.5 and 5 miles of a charter school, and between 5 and 10 miles of a charter school. Estimates from the modified versions of equations 2 and 3 are susceptible to selection bias, while the fixed-effects estimates, which are based on within-student comparisons, effectively control for any unobserved student characteristics that remain constant over time.

The estimates from the fixed-effects equations are identified primarily by students who attend schools located within the specified distance of a charter school and whose test score gains we observe in that school both before and after the nearby charter school opens. However, students who move from a traditional public school not located near a charter school to a school that is located near a charter school (and vice versa) also contribute to the identification. If charter schools tend to locate near low-quality schools, then we would expect to see a drop in the test score gains of students moving into schools located near charter schools, regardless of any charter school impacts.

Thus, estimates from student fixed-effects models, which are based in part on the change in test score gains of students moving into or out of schools located near charters, might be biased downward. To address this possibility, we estimate a fourth equation, for both math and reading, which controls for school fixed effects as well as student fixed effects.

More specifically, we begin with the following model:

$$\Delta Y_{ijGT} = Y_{ijGT} - Y_{ijG(T-1)} = \alpha C_{jGT} + X_{iGT}\mathrm{B} + \gamma_i + \theta_j + \lambda_{GT} + \nu_{ijGT} \qquad (6)$$

where each term is defined as in equation (4), except subscript j indexes schools, C_{jGT} represents measures of charter school competition, and θ_j is a school fixed effect. Unbiased estimates of α and β in equation (6) can be obtained by differencing each variable from its individual student mean and by including a set of explicit school dummy variables. For our sample this would require 1,885 school dummy variables, which creates computational challenges.

To avoid these difficulties, we define an individual spell as the set of observations on a particular student in a particular school. Let s(i,j) index individual spells, and set $\eta_{\mathrm{s(i,j)}} = \gamma_{\mathrm{i}} + \phi_{\mathrm{j}}$. Note that $\eta_{\mathrm{s(i,j)}}$ is the same for each observation within the same spell. Consequently, substituting $\eta_{\mathrm{s(i,j)}}$ into equation (6) and differencing each variable in the resulting equation from that variable's within-spell mean effectively sweeps out the sum of the effect of unobserved individual and school heterogeneity ($\gamma_i + \phi_j$). OLS estimates of the resulting equation identify the effect of charter school competition using students who remain in the same school (and thus within the same individual spell) as the extent of charter school competition faced by the school changes over time. The OLS estimates of α effectively control for both school and individual fixed effects.[31]

Results

The results of our estimations are presented in Tables 10A and 10B. For reading, estimates from the student fixed-effects models suggest that charter school competition reduces student test score gains in schools located within 2.5 miles of a charter school and has no effect on gains in schools located between 2.5 and 10 miles from a charter school. For math, none of the estimates from the student fixed-effects model are significantly different from zero.

31. For further discussion of estimating panel data models that include individual and group fixed effects see Andrews, Schank, and Upwad (2004) and Abowd, Kramarz, and Margolis (1999). Although this estimation strategy provides unbiased estimates of the parameters of interest, the estimates are less efficient than the alternative of including explicit school dummies.

Table 10A Estimated Impacts of Charter Schools on Reading Scores of Traditional Public School Students

	Levels	Gains	Student Fixed Effects	Student & School Fixed Effects
Within 2.5 miles of a charter	0.023 (0.013)	−0.002 (0.004)	−0.013**(0.006)	0.010 (0.017)
2.5–5 miles from a charter	0.035**(0.012)	0.004 (0.004)	0.000 (0.006)	0.021 (0.018)
5–10 miles from a charter	0.026**(0.009)	0.006* (0.003)	0.002 (0.004)	0.016 (0.016)
Gender (male = 0, female = 1)	0.174**(0.002)	0.001 (0.001)		
Ethnicity[a]				
Black	−0.351**(0.023)	−0.028**(0.004)		
Hispanic	−0.004 (0.025)	0.042**(0.005)		
White	0.235**(0.023)	−0.010**(0.004)		
Parent education[b]				
High school grad	0.443**(0.005)	0.005* (0.002)		
Some college	0.678**(0.006)	0.016**(0.003)		
Two-year college degree	0.784**(0.006)	0.016**(0.002)		
Four-year college degree	1.124**(0.007)	0.022**(0.002)		
Graduate school degree	1.411**(0.011)	0.027**(0.003)		
Change schools in last year	−0.139**(0.005)	−0.016**(0.003)	−0.011**(0.004)	−0.022**(0.004)
Made structural change in last year	−0.049**(0.007)	−0.064**(0.006)	−0.055**(0.008)	−0.067**(0.003)
Observations (students)				
Total	1,512,892	1,498,460	1,475,833	
	(443,514)	(439,841)	(420,036)[d]	
Within 2.5 miles of a charter[c]	166,077	163,929	161,408	
	(87,379)	(86,179)	(81,641)[d]	
2.5–5 miles from a charter[c]	274,977	272,087	265,705	
	(163,518)	(161,748)	(153,337)[d]	
5–10 miles from a charter[c]	324,432	321,460	317,621	
	(160,470)	(158,912)	(155,079)[d]	

Notes: All models include grade/year fixed effects. Dependent variable is EOG developmental scale score expressed as a standard score. Figures in parentheses are standard errors computed using generalization of Huber/White/Sandwich estimator and are robust to clustering within schools.

[a] Reference category is Asian and Native American. [b] Reference category is high school dropouts. [c] Observations count number of times students are observed in a school during a year when school was in specified category, which is less than the number of times the students are observed overall. [d] Sample count includes only those observations and students with at least three valid test score measures, which is the minimum required to identify fixed effects and effect estimates for nonconstant variables.

* statistical significance at .05 level, ** statistical significance at the .01 level.

Table 10B Estimated Impacts of Charter Schools on Math Scores of Traditional Public School Students

	Levels	**Gains**	**Student Fixed Effects**	**Student & School Fixed Effects**
Within 2.5 miles of a charter	0.020 (0.016)	0.012 (0.006)	−0.007 (0.009)	−0.006 (0.024)
2.5–5 miles from a charter	0.026 (0.014)	0.015* (0.006)	0.003 (0.009)	0.018 (0.020)
5–10 miles from a charter	0.020 (0.011)	0.021** (0.005)	0.010 (0.007)	0.013 (0.016)
Gender (male = 0, female = 1)	0.036** (0.002)	0.009** (0.001)		
Ethnicity[a]				
Black	−0.463** (0.023)	−0.019** (0.005)		
Hispanic	−0.048 (0.024)	0.019** (0.006)		
White	0.155** (0.022)	−0.019** (0.005)		
Parent education[b]				
High school grad	0.386** (0.005)	−0.007** (0.002)		
Some college	0.603** (0.006)	0.004 (0.003)		
Two-year college degree	0.705** (0.006)	0.004 (0.003)		
Four-year college degree	1.071** (0.008)	0.026** (0.003)		
Graduate school degree	1.398** (0.013)	0.054** (0.004)		
Change schools in last year	−0.164** (0.005)	−0.028** (0.004)	−0.024** (0.005)	−0.034** (0.003)
Made structural change in last year	−0.044** (0.008)	−0.066** (0.008)	−0.059** (0.011)	−0.063** (0.003)
Observations (students)				
Total	1,519.078	1,498,460	1,483,186	
	(444,806)	(439,841)	(421,904)[d]	
Within 2.5 miles of a charter[c]	166,839	164,823	162,322	
	(87,724)	(86,658)	(84,105)[d]	
2.5–5 miles from a charter[c]	275,951	273,223	266,820	
	(164,021)	(162,328)	(155,894)[d]	
5–10 miles from a charter[c]	325,650	322,984	319,120	
	(160,979)	(159,608)	(151,730)[d]	

Notes: All models include grade/year fixed effects. Dependent variable is EOG developmental scale score expressed as a standard score. Figures in parentheses are standard errors computed using generalization of Huber/White/Sandwich estimator and are robust to clustering within schools.

[a] Reference category is Asian and Native American. [b] Reference category is high school dropouts. [c] Observations count number of times students are observed in a school during a year when school was in specified category, which is less than the number of times the students are observed overall. [d] Sample count includes only those observations and students with at least three valid test score measures, which is the minimum required to identify fixed effects and effect estimates for nonconstant variables.

* statistical significance at .05 level, ** statistical significance at the .01 level.

Table 11 Estimated Impacts of Charter Schools on Traditional Public School Students by Number of Charter Schools within Five Miles

	READING		MATH	
	Student Fixed Effects	**Student & School Fixed Effects**	**Student Fixed Effects**	**Student & School Fixed Effects**
1 charter within 5 miles	−0.001 (0.005)	0.014 (0.012)	−0.006 (0.007)	0.001 (0.016)
2 charters within 5 miles	−0.014 (0.010)	−0.004 (0.027)	0.004 (0.014)	0.031 (0.031)
>2 charters within 5 miles	−0.038** (0.009)	−0.020 (0.036)	−0.020 (0.014)	0.052 (0.044)
Change schools in last year	−0.010** (0.004)	−0.022** (0.004)	−0.023** (0.005)	−0.034** (0.003)
Made structural change in last year	−0.055** (0.008)	−0.067 (0.003)	−0.059** (0.011)	−0.064** (0.003)
Observations (students)				
Total	1,475,833 (420,036)		1,483,186 (421,904)	
1 charter within 5 miles[a]	281,144 (151,425)		282,418 (152,579)	
2 charters within 5 miles[a]	81,603 (51,760)		82,047 (51,311)	
>2 charters within 5 miles[a]	64,366 (35,793)		64,677 (36,109)	

Notes: All models include grade/year fixed effects and individual fixed effects. Dependent variable is EOG developmental scale score expressed as a standard score. Figures in parentheses are standard errors computed using generalization of Huber/White/Sandwich estimator and are robust to clustering within schools.

[a] Observations count number of times students are observed in a school during a year when school is in specified category, which is less than the number of times the students are observed overall. Sample count includes only those observations and students with at least three valid test score measures, the minimum required to identify fixed effects and effect estimates for nonconstant variables.

* statistical significance at .05 level, ** statistical significance at the .01 level.

Once school fixed effects are controlled for, the coefficients of some of the charter school competition variables become positive. However, in no case are the estimates statistically significant. Further, it is unclear why students in schools located between 2.5 and 10 miles of a charter school would benefit from charter school competition, but not students in schools located within 2.5 miles. The anomalous pattern of point estimates reinforces the conclusion that any apparent positive effects should be attributed to chance rather than to the beneficial effects of competition from charter schools.

To examine whether charter school effects are larger when there are multiple charter schools located near a traditional public school, we replaced the three variables indicating a school's distance from the nearest charter school with three new variables indicating, respectively, whether the school had one, two, or more than two charter schools located within 5 miles. The results of this analysis are presented in Table 11.

Again, it appears that models omitting school fixed effects provide downward-biased estimates of the impact of charter schools. The preferred results for reading suggest no benefits from charter school competition. Not only are none of the estimated impacts statistically different from zero, but, contrary to the expectation of a larger impact when nearby charter schools are more numerous, the estimated effects are more negative for schools exposed to competition from larger numbers of charter schools. The results for math are more consistent with expectations. The estimated effects of charter school competition on math gains are all positive, and they grow larger as the number of charter schools within 5 miles increases. However, none of the estimates is statistically different from zero.

We conclude that charter schools appear to have no statistically significant effects on the achievement of the traditional public school students in North Carolina. We emphasize, however, that the intensity of competition is not very great. Even schools located close to a number of charter schools are unlikely to lose a substantial percentage of students to charter schools. Thus, our finding that charter schools have no effects on traditional public schools in North Carolina should not be interpreted as a general statement about the potential of charter school competition to influence traditional public schools. Nonetheless, the finding that the effects of charter schools on students in traditional public schools are small and statistically insignificant implies that competitive effects generate essentially no bias in our estimates of charter school impacts on charter school students.

7. WHY DO STUDENTS MAKE SMALLER GAINS IN CHARTER SCHOOLS?

Our estimates indicate that North Carolina students who transfer into charter schools make smaller gains than they would have had they remained in traditional public schools, even when the charter schools they attend have been operating for five years. Several factors could account for these smaller gains. The mix of peers that students encounter in charter schools might negatively affect test scores, resources might be less adequate in the average charter school than in traditional public schools, and/or charter schools might be less efficient than traditional public schools.

Another reason that charter schools might have difficulty providing effective educational programs is student turnover. Table 12 shows that, on average, the percentage of students in a school between grades 4 and 8 that have made a nonstructural transfer in the last year is higher in charter schools than in traditional public schools. As expected, the average rate of student turnover is lower in charter schools that have been open longer. However, average turnover rates in charter schools remain twice as high as those in traditional public schools, even for charter schools that have been open for five years. Changing student populations makes student grouping and scheduling more

Table 12 Average Percent of Students in Grades 4–8 That Have Made a Nonstructural Transfer in the Last Year, by Charter School Status

Traditional public schools	13.7%
First-year charters	100.0%
Second-year charters	46.1%
Third-year charters	37.2%
Fourth-year charters	39.5%
Fifth-year charters	25.4%

Figures represent unweighted school-level averages.

challenging, intake of new students can distract administrators from other tasks, and assessing and helping new students can place extra demands on teachers' time. Hanushek, Kain, and Rivkin (2001) find that higher student turnover harms all students in school regardless of whether they themselves are movers.

To determine the role that high student turnover rates play in explaining the poor performance of charter schools, we add two school-level variables to our fixed effects model of student achievement (equation (4)): the percent of students in the school who have made a nonstructural school change and the percent who have made a structural change during the last year. As we saw in Table 6, students in charter schools in their first year of operation show especially small test score gains. In order to focus on the role student turnover plays in explaining quality differences between charter and traditional public schools that remain after start-up challenges have been met, we exclude observations of students in first-year charter schools from these estimations. As in our earlier analyses, we use the Huber/White/Sandwich estimator to compute standard errors that are robust to clustering within schools.

The results are presented in Table 13. The first and third columns show the estimates from our original student fixed-effects model, without controls for student turnover. These estimates differ from those reported in Tables 5A and 5B because observations of students in first-year charter schools are excluded. The second and fourth columns show estimates from models that include measures of student turnover. For both reading and math, both the percent of students in a school who have made a nonstructural transfer and the percent who have made a structural transfer have statistically significant, negative effects on student achievement. In addition, including these controls for student turnover reduces the coefficients on the charter school indicator, by 29 percent in the case of reading and by 30 percent in the case of math, suggesting that high student turnover rates account for almost one-third of

Table 13 Estimated Impacts of Attending a Charter School on Reading Test Scores Controlling for the School's Student Turnover Rate

	Reading		Math	
Charter school[a]	−0.062** (0.014)	−0.044** (0.015)	−0.106** (0.021)	−0.074** (0.022)
Individual-level controls				
Change schools in last year	−0.011** (0.004)	0.010** (0.003)	−0.025** (0.005)	0.002 (0.004)
Made structural change in last year	−0.055** (0.007)	−0.015** (0.006)	−0.060** (0.010)	−0.016** (0.007)
School-level controls				
Percent of students making nonstructural change		−0.106** (0.015)		−0.168** (0.027)
Percent of students making a structural change		−0.057** (0.011)		−0.060** (0.014)
Observations[b]	1,488,498		1,495,885	
Students[b]	420,521		422,060	

Notes: All models include grade/year fixed effects and individual fixed effects. Dependent variable is EOG developmental scale score expressed as a standard score. Standard errors are calculated using generalization of Huber/White/Sandwich estimator and are robust to clustering within schools.

[a] Observations of students attending charter schools in their first year of operation are excluded. [b] Sample count includes only those observations of students with at least three valid test score measures, which is the minimum required to identify fixed effects and effect estimates for nonconstant variables.

* statistical significance at .05 level, ** statistical significance at the .01 level.

the negative impact charter schools have on student performance.[32] However, the coefficients on the charter school variable remain statistically significant, suggesting that some combination of peers, resources, and efficiency also play a role in the poor performance of charter schools.

That high student turnover rates play a significant role in explaining the poor performance of charter schools has potentially important implications for debates about school choice. Because school choice plans lower the costs to families of switching schools, it is plausible that such plans will increase the movement of students across schools and thereby increase student turnover rates, to the detriment of students in those schools.

8. CONCLUSIONS AND FUTURE DIRECTIONS

Our estimates imply that students in North Carolina do less well in charter schools than they would have done in traditional public schools. Even in charter schools that have been open for more than one year, students gain on average

32. We also estimated the models presented in Table 13 with observations of students in first-year charters included. In these estimations, adding the school-level measures of student turnover also reduced the coefficient on the charter school indicator by about one-third for both reading (−0.095 to −0.062) and math (−0.160 to −0.103).

0.062 of a standard deviation less in reading and 0.106 of a standard deviation less in math than they would have had they remained in traditional public schools. Because they are based on a subsample of charter school students that overrepresents students who exit charter schools and underrepresents students who enter charter schools during the early grades, these estimates might overstate the magnitude of the true average impact of charter school across all charter school students. Nonetheless, all indications are that average charter school impacts across all charter school students are negative.

In contrast to the findings from comparable studies of charter school systems in Texas and in Florida, negative effects of charter schools hold even for charter schools that have been operating for several years. When we disaggregate the average charter school impact by the length of time the charter school student has been in the school and whether or not the student is observed exiting charter schools, we find that the large negative impacts of charter schools on average are driven largely, although not entirely, by students during their first year in a charter school regardless of the age of the school. Although negative impacts for charter school students who choose to remain in charter schools do not continue to accumulate after the first year, even this group of students shows lower achievement levels as a result of transferring into charter schools.

The effects of charter school competition on the achievement of students in traditional public schools appear to be negligible. That may well reflect the fact that North Carolina charter schools provide only a limited amount of competition for traditional public schools. As a result, the North Carolina charter school program does not yet provide a definitive test of the potential effects of charter school competition on traditional public school students.

Why students make smaller test score gains in charter schools than in traditional public schools is worth investigating. We provide evidence that high student turnover rates explain about 30 percent of the difference between test score gains made in charter schools and what we would expect the same students to make in traditional public schools. This finding suggests that student turnover can be an unintended negative side effect of school choice. However, charter schools in North Carolina still show negative impacts on student achievement even after controlling for student turnover rates. Further investigation to determine whether the remaining negative impacts are due to peer effects, resource inadequacies, or inefficiencies would be useful.

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