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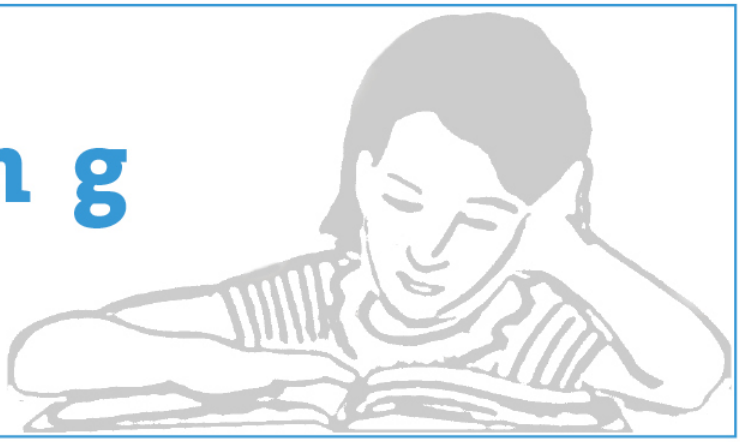

2010

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ncsrp working paper # 2010-2

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Abstract

Charter schools continue to be controversial as many debate the merit of whether we should promote these schools through formal policies. The debate has intensified over the last few years with release of studies with conflicting outcomes as well as the promotion of charter schools by the Obama administration. However, some have moved beyond the overarching debate of whether we should have charter schools to a discussion of how best to design charter policies. Crucial to this discussion is the charter authorizing process, which varies across the nation. For instance, some states only allow districts to authorize charter schools while other states allow a range of authorizers, including the state, districts, nonprofit organizations, counties, and higher educational institutions. In this paper we use individual student-level data from Ohio (which has a range of authorizers) to examine the effectiveness of various types of authorizers. The findings suggest that charter schools authorized by nonprofit organizations in Ohio are less effective in promoting student achievement than are other charter schools (though there are likely to be more differences among individual authorizers than across authorizer types). Given that nearly all of the recent growth in the state's charter sector involved nonprofit authorizers, there may be a tension between maximizing charter quality and rapidly increasing the number of charter schools.

Introduction

Nearly two decades after the establishment of the first charter schools, debate continues over whether, on average, they are more or less effective than conventional public schools in raising the achievement of the students they serve (see, e.g., Abdulkadiroglu et al., 2009; Zimmer et al., 2009; CREDO, 2009; Hoxby, Murarka, and Kang, 2009; Betts and Tang, 2008; Gleason, Clark, Tuttle, and Dwoyer, 2010). Differences in the results of different studies could be attributable to varying methods for estimating impacts (see the debate among CREDO, 2009; Hoxby, 2009); to differences in outcomes measured; to differences in charter laws and policies across states producing real differences in average effectiveness of charter schools (see Zimmer, Guarino, and Buddin, 2010; CREDO, 2009), or to a combination of these factors. Despite this ongoing debate, however, one area of clear consensus has emerged: Everyone agrees that the variation in the performance of individual charter schools is wide, with some performing admirably and others struggling.

The debate over the average effectiveness of charter schools is not likely to be settled anytime soon, but in the meantime, the variation in the performance of individual charter schools can be used by researchers to inform the improvement of policies governing charter schools. Policymakers need not wait for a definitive bottom-line evaluation of average effectiveness before modifying charter laws and regulations in an effort to increase the number of effective charter schools and reduce the number of ineffective charter schools. This is, of course, the explicit goal of the Obama administration's charter-school initiatives.

Our aim in this paper is to use the variation in charter-school performance in one state to assess one of the key levers of charter-school policy: the decision about who has the authority to authorize charter schools. Prospective charter-school operators must petition an authorizing

authority for the right to open a school, and must re-apply for renewal at the conclusion of the charter term. "Through the granting and monitoring of charter schools, authorizers are administrative and accountability gatekeepers in states that have enacted charter school legislation" (Anderson and Finnigan, 2001, p. 6). Charter-school authorizing authority varies in different states. Authorizing authority can reside in local school boards, postsecondary educational institutions, the state department of education, an independent state charter board, county educational agencies, or nonprofit groups (Palmer, 2007).

Theoretically, authorizers fulfill three key roles: they decide which charter schools will be permitted to open, they monitor each school's performance and determine when to offer help, and they decide which schools deserve reauthorization (Vergari, 2001). Many authorizers consider the first role most important. "There is a belief, at least among staff at some of the larger authorizers, that if they make the process rigorous enough at the beginning, then they won't need to 'worry' as much about the school in practice" (Bulkley, 2001). The initial authorizing process usually includes performance expectations in the form of a contract. When contract expectations are not met, schools can face sanctions and, in the most serious cases, closure (Bulkley, 2001).

There is a debate about which types of authorizing agencies have the proper incentives to fulfill these roles effectively. Local school districts, for example, typically have little incentive to authorize charter schools, which compete for students and resources. Other authorizers, such as nonprofits and universities, may have less of a conflict of interest, but they too can possibly have distorted incentives created by the fees they can charge for authorizing charter schools. At the other end of the process, authorizers of all types may be reluctant to take on the challenge of

shutting down a low-performing charter school that has its own political constituency (Bulkley, 2001).

In addition to challenges with incentives, authorizing agencies can have varying capacity to fulfill their roles effectively. For instance, some districts or nonprofit agencies may be too small to have the staff to sufficiently vet charter proposals or to provide appropriate support or oversight to charter schools. In contrast, Anderson and Finnigan (2001) argue that larger authorizers can and typically do require a more rigorous selection process suggesting "[they] may have learned from experience to be clear about their expectations at the beginning of their relationship with charter schools" (p. 9). These larger authorizers also had more "developed accountability systems" (2001, p. 6). Smaller authorizers, meanwhile, vary significantly in selection and requirements (Bulkley, 2001). Even apart from size, the capacity of authorizers may be limited by the fact that the authorizing function is often quite different from their core activities: universities and nonprofit agencies, for example, may have no prior experience relevant to the tasks of authorizing, overseeing, and supporting charter schools; school districts, by contrast, may be more likely to have relevant expertise. Finally, the funding available to support authorizers may be very limited.

In principle, authorizers could be a critical factor in the overall performance of charter schools. Differences in the interest and capacity of authorizers to approve, monitor, support, and regulate charter schools might be expected to lead to considerable variation in the effectiveness of charter schools. The importance of effective authorizing is now commonly recognized—there even exists a national association of authorizers that seeks to improve authorizer practices. This paper takes on the question by examining the performance of charter

schools authorized by various types of authorizers in Ohio.[1] Ohio represents one of the most flexible states in terms of authorizers: the state, local districts, county educational service centers (ESCs), and nonprofit organizations have all authorized charter schools. In the next section, we describe types of authorizers and discuss the evolution of charter authorizers in Ohio over time.

Previously, Carlson, Lavery, and Witte (2009) examined charter authorizers in Minnesota finding no variation in performance across authorizers. However, their analysis utilized school-level data, which makes it more difficult to tease out differential effects across authorizers. Our analysis uses longitudinal student-level data, which allows us to control for more nuanced characteristics. Subsequent sections describe in greater detail our data, research approach, results, and conclusions.

Chartering Authorities in Ohio and Across States

Currently, 40 states plus the District of Columbia allow charter schools. Charter laws vary across a number of dimensions, including the type of agencies that can authorize charter schools. Table 1 highlights the breakdown of chartering authorities across locations, highlighted in research by Hassel, Ziebarth, and Steiner (2005). The most common chartering authority is local school districts, which are granted authorizing authority in 39 locations. State education agencies have authorizing authority in 35 states. Nine states allow higher education institutions and seven states allow independent charter boards to authorize charter schools. Only two states allow nonprofit institutions to authorize charter schools. Additionally, one state each allows mayors or city councils to authorize charter schools.

[1] In Ohio, charter schools are called community schools. To be consistent with the general literature, we refer to these schools as charter schools.

Table 1: Types of Active Chartering Authorities Across States

Authorizer	*Number of States*
Local School Board	39
Independent State Charter Board	7
Postsecondary Education Institution	9
State Board of Education/Commissioner of Education	35
Mayor	1
City Council	1
Nonprofit Organizations	2
Regional School Districts	6

Source: Hassel, Ziebarth, and Steiner (2005) (Also shown in Carlson, Lavery, and Witte, 2009)

Authorizing authority in Ohio has evolved over time. Ohio established charter schools in June 1997, and authority to sponsor start-up charters was initially given to the University of Toledo and Lucas County Education Service Center (ESC) through a pilot program. In August of that same year, Ohio's eight urban districts (the "Ohio eight") and the State Board of Education were given the authority to sponsor schools as well. In 1999, new legislation expanded charter school authorization authority to the 21 largest districts (Fordham, 2006). The following year, legislation expanded charter schools even further to any school district determined to be in "academic emergency" and, with these expansions, traditional public school districts could sponsor charter schools in their own or in any other district in their county.[2] Finally, because of a state auditor's report in 2002 that raised questions about a possible conflict of interest for the state being both a promoter and regulator of these schools, the State Board ended its sponsorship role (Office of Community Schools, Ohio State Department of Education, 2007). Instead, the State Board and Ohio Department of Education (ODE) became an "authorizer of

[2] Ohio Department of Education, "Community School Legislative History," http://education.ohio.gov/GD/Templates/Pages/ODE/ODEDetail.aspx?page=3&TopicRelationID=662&ContentID=41601&Content=41601, Retrieved November 10, 2009.

sponsors" (Fordham, 2006). As a result, dozens of charter schools previously authorized by the state had to transition to other authorizers by June 30, 2005. Nonetheless, the legislation allowed for four types of authorizers: (1) public school districts, (2) educational service centers, (3) 13 state universities, and (4) qualified tax-exempt entities under Section 501(c)(3) of the Internal Revenue Code.[3] Prospective new authorizers were required to gain approval from the state.[4]

In sum, Ohio remains unusual in the wide range of different types of organizations it permits to authorize charter schools—but every one of the 40 charter laws across the country permits at least one of the authorizer types permitted in Ohio. In consequence, Ohio is a uniquely valuable site in which authorizer types utilized in states across the country can be examined in relation to student achievement outcomes.

[3] Ohio Department of Education, "Community School Legislative History," http://education.ohio.gov/GD/Templates/Pages/ODE/ODEDetail.aspx?page=3&TopicRelationID=662&ContentID=41601&Content=41601, Retrieved November 10, 2009.

[4] However, entities authorizing charter schools prior to 2003 were excluded from this new legislation and grandfathered as sponsors of charter schools.

Data

To address our research question, we collected statewide longitudinally linked student-level data from Ohio's Department of Education from school years 2004-05 through 2007-08 in elementary and middle grades.[5] The state provided both raw and scaled test scores for many students. However, we only had either the raw or scaled scores (not both) for a subset of students. To have the scores on a consistent scale, we normalized the raw and scaled scores by year and grade. More specifically, we converted all raw and scaled test-score results into rank-based z-scores, by year and grade and by subject, with a mean of zero and a standard deviation of one.[6] The conversion of raw and scaled scores to rank-based z-scores means that we cannot make claims about the absolute amount of learning in one school or another (lacking a psychometrically valid developmental scale), but it permits an examination of changes in rank with fewer assumptions than would be needed under other kinds of scaling.[7] For students missing scaled normalized scores, we used normalized values of the raw scores. Raw and scaled scores had a high correlation of 0.87 across students that had both scores.

Using these test data, individual student demographic data, and unique student identifiers, we created a statewide longitudinal student-level data set that follows students as they move from traditional public schools (TPSs) to charter schools and vice versa. This is important because it allows us to examine the performance of students before, during, and after attending a charter school.

[5] Most students were tested in the spring, but a small portion were tested in the fall, and because this would have meant that we would have inconsistent intervals of testing periods for all students, we eliminated the fall test from our data set.
[6] The correlation between the raw math scores and scaled scores as well as the raw reading scores and scaled scores is 0.87.
[7] For further discussion of the use of rank-based z-scores, see Gill et al., 2005.

Table 2 highlights the descriptive characteristics of non-charter students as well as charter students by the various types of chartering authorities across the 2004-05 through 2007-08 school years. The table includes only students who have test scores in either math or reading because these are the students ultimately included in the achievement analysis. We categorize the charter schools by the authorizer we first observe in the data (i.e., in 2004-05 or in the year the charter school was established, whichever came first). For schools established before 2004-05, we cannot be certain that this was the original authorizer, because our data do not extend farther back.

On average, all the charter schools sponsored regardless of authorizer type have a larger share of black students and smaller share of white students than traditional public schools in Ohio. These differences are most likely related to the disproportionate representation of charter schools in urban districts, which have a disproportionate share of the state's black students. District-authorized charters serve a population that is most similar to that of TPSs in racial terms; charter schools initially authorized by the state department of education, by contrast, serve a population that is overwhelmingly black. Across the various authorizer types, charter schools have substantially lower average test score levels than TPSs. ESC-authorized schools have the lowest average test scores of all the authorizer groups. These descriptive averages, however, do not account for student differences and should not be viewed as evidence of effectiveness. Finally, among the authorities, nonprofit-authorized charter schools have been in operation the shortest amount of time while state-authorized charter schools have been in operation the longest length of time.

Table 2: Descriptive Statistics of Ohio's Tested Students, 2004-05 through 2007-08 School Years

	Traditional Public Schools	*District Authorized Charter Schools*	*ESC Authorized Charter Schools*	*Nonprofit Authorized Charter Schools*	*State Authorized Charter Schools*
Proportion Black	.15	.25	.54	.41	.85
Proportion White	.78	.68	.41	.52	.10
Proportion Hispanic	.02	.02	.03	.03	.02
Proportion Other	.03	.04	.03	.03	.03
Proportion Male	.51	.50	.53	.50	.48
Math Z Score	.02	-.61	-.77	-.70	-.64
Reading Z Score	.01	-.41	-.66	-.50	-.59
Average Years of Operation	NA	4.19	4.74	3.97	6.63

Note: Table only contains students that have gain scores in either math or reading.

Table 3 shows the total number of elementary and middle charter schools authorized by type of authorizer by year. The information of type of authorizer for each school was collected by the Fordham Foundation using the Ohio Department of Education's Annual Reports and Ohio Education Directory System Redesign.[8] We could not identify the authorizers of a small number of charter schools. Most of these are schools that are no longer open or have changed authorizer status over time. In addition, the table excludes all schools for which we cannot observe test score data—notably charter high schools.

As Table 3 indicates, the number of schools authorized by nonprofit organizations increased rapidly over time. In 2007-08, there were 130 schools authorized by nonprofit organizations compared to 30 in 2004-05. In contrast, the number of charter schools authorized

[8] Ohio Department of Education's Annual Reports on Ohio Community Schools, available at: http://education.ohio.gov/GD/Templates/Pages/ODE/ODEDetail.aspx?page=3&TopicRelationID=662&ContentID=42095&Content=66605 and the Ohio Education Directory System Redesign, accessed here: http://webapp2.ode.state.oh.us/oeds-r/query/ on June 17 and 18, 2009.

by districts has increased only slightly over time while the number of charter schools authorized by ESCs has slightly declined. Finally, while the state department of education authorized 45 schools in 2004-05, the change in legislation took the department out of the authorizing business after that.

Table 3: Number of Charter Schools Authorized by Type of Authorizer by Year (Cumulative Totals)

Type of Authorizer	*2004-05*	*2005-06*	*2006-07*	*2007-08*
District	45	55	47	46
Educational Service Centers	80	86	74	70
Nonprofit	30	92	112	130
State	45	0	0	0

The legislation precluding state sponsorship of charter schools largely explains the dramatic increase in the number of charter schools sponsored by nonprofits in the 2005-06 school year: 41 of the 45 state-authorized schools transferred to nonprofit authorizers. The remaining four transferred to ESC sponsorship. Thirteen ESC-authorized schools transferred to nonprofit sponsorship between 2006 and 2008.

Research Design

The biggest challenge in estimating the effects of charter schools is selection bias. Because students choose to attend charter schools, they may be different than students who do not (Hoxby and Murarka, 2006; Ballou, Teasley, and Zeidner, 2007). Students choosing charters might be more motivated with more engaged parents, or they might be students who are looking for alternatives because they have struggled in TPSs. Either way, these students may be

different from a random set of TPS students in unobserved ways that could bias the results of a comparative analysis.

To address the possibility of selection bias, much of the charter school student achievement research has used one of two designs. First, researchers have compared the achievement of students accepted into charter schools with that of students denied admission via randomized admission lotteries. Because the random draw of the lottery is the only factor that distinguishes students admitted from those denied admission, comparisons of lottery winners and losers control for unobservable differences between the populations and produce unbiased estimates of impacts (Gleason et al., 2010; Hoxby and Rockoff, 2005; Hoxby and Murarka, 2006 Abdulkadiroglu et al., 2009). Second, when lottery data is not available, researchers have often used a student fixed-effect approach, which examines whether each student's achievement is higher while attending a charter school relative to the same student's achievement while in a traditional public school (TPS) (in other grades) (Zimmer et al., 2003; Hanushek, Kain, Rivkin, and Branch, 2007; Betts et al., 2006; and Zimmer and Buddin, 2006; Sass, 2006; Bifulco and Ladd, 2006; Witte, Weimer, Shober, and Schlomer, 2007; Booker, Gilpatric, Gronberg, and Jansen, 2007; and Imberman, forthcoming). This approach compares students to themselves over time, which to the extent that students' unobservable characteristics remain constant over time, should minimize selection bias in the analysis.

Neither of these approaches is suitable as our primary approach. We do not have access to admissions lottery data for Ohio charter schools. Moreover, even if lottery data did exist, not all charter schools are oversubscribed, so the method would not allow a comprehensive evaluation of all schools across all chartering authorities. The student fixed-effect approach, meanwhile, has recently been challenged because it includes only a subset of charter students

(which raises external validity concerns) and because prior achievement trajectories may not provide good evidence on future achievement trajectories for the subset of students who can be included (which raises internal validity concerns) (Hoxby and Murarka, 2006). Zimmer et al. (2009) developed a modified version of the student fixed-effect approach that addresses these criticisms by focusing on schools in which the lowest grade starts above the lowest grade tested. By doing so, the analysis includes nearly all students attending these schools because nearly all students "switch" into these schools from elementary (for charter middle schools) or from middle schools (for charter high schools). In Ohio, however, this approach would require a focus exclusively on middle schools, since students are not tested in consecutive grades at the high school grades. This would require restricting our attention to only 16 district-authorized, 21 ESC-authorized, 18 nonprofit-authorized, and 6 state-authorized charter schools—collectively a small minority of all Ohio charter schools, not sufficient to allow us to detect moderately sized effects of authorizer type.

We use two approaches in combination to address selection issues while including charter schools serving elementary as well as middle-school grades. First, we bypass the problem of comparing choosers to nonchoosers by constraining the comparison groups to consist entirely of other students in charter schools. We conduct separate analyses for each authorizer type, in which the comparison students are drawn from charter schools authorized by any of the other authorizer types. In consequence, we need not make the strong assumption that choosers are similar to non-choosers; our treatment groups and comparison groups consist entirely of choosers. Indeed, we directly address the internal validity concern of Hoxby and Murarka (2006) by ensuring that both the charter students in the treatment group and the charter students in the comparison group were in TPSs prior to entering charters. The fact that

the treatment students have changed schools will therefore not bias expectations about their future performance, because the comparison students have likewise transferred.[9]

Second, among the population of charter students potentially available as comparison students, we use a propensity match to identify the specific students to be used for comparison purposes. A similar approach (without a prior restriction to a charter population) was used in a recent report on charter middle schools affiliated with the Knowledge is Power Program (KIPP) (Tuttle et al., 2010). Creation of a carefully matched comparison group has been shown in some circumstances to produce impact estimates that replicate the findings of randomized experiments (Cook, Shadish, and Wong, 2008). More specifically, recent research has suggested that matching strategy can replicate randomized design results when examining school choice programs (Bifulco, 2010).

Some charter schools changed authorizers during the period included in our data. Nonetheless, our analysis permanently assigns to each school the authorizer type that was first observed for that school. We keep schools attached to their first observed authorizers on the assumption that schools that change authorizers are not likely to immediately change their performance to reflect the influence of the new authorizer. This approach is also consistent with the view that an authorizer's most important function is the initial screening of charter applicants.

Finally, the analysis excludes "virtual" charter schools—i.e., schools that deliver their educational services primarily via communication technology to students in their own homes, rather than in a conventional school building. Virtual charter schools tend to serve quite

[9] This does not directly address the concern about external validity—i.e., that students transferring into charter schools may differ from those who enroll in charters beginning in kindergarten. But we see no reason to believe that the differences between transfer students and kindergarten entrants systematically differ in charter schools authorized by different authorizers. So the fact that we are matching charter treatment students to charter comparison students is likely to minimize the external validity problem.

different populations than do other charter schools, and it is not clear that their performance can be evaluated using the same methods used for other charter schools (Zimmer et al., 2009). Virtual charter schools are unevenly distributed across the authorizers: of the observed 40 virtual schools in the analysis, districts have authorized 33, while ESCs and nonprofits have authorized 4 and 3, respectively.

Analytical Details

To create the matches, we first restricted the potential match for a "treatment" student to students within the same grade and same year. For example, in examining the effect of an ESC-authorized charter student, we identified a matched control student in the same grade and year in a school authorized by one of the three other types of authorizers. From this sample, we created a match for the treatment students based upon students' observable characteristics in the year prior to entering a charter school. The observable characteristics include math and reading test scores, gender, and race. Using these observable characteristics, logistic regressions were conducted separately for each treatment (i.e., district-authorized, ESC-authorized, nonprofit-authorized, state authorized) by each year by each grade, with the treatment variable serving as the dependent variable in each logistic regression. From the logistic models, we obtained propensity scores for the likelihood of participating in each treatment. Using the propensity score, we created a one-to-one match of a control student for each treatment student. After creating a match for a particular treatment for a particular grade and year, we pooled treatment and matched control students across grades and years into one data set for each treatment (in other words, we had four different pooled data sets—one for each type of charter authorizer).

Table 4 displays the observable characteristics of students for each authorizer type relative to the matched control students. Comparing the descriptive statistics suggests that the matching procedure created close matches on observable characteristics.[10] Moreover, because our pool of matches includes only charter students, our matched comparison groups are similar to the treatment groups not only in terms of demographic factors and baseline achievement levels, but also in the fact that they chose to enroll in charter schools.

Table 4: Quality of Matches by Authorizer Type

	District	*District Matches*	*ESC*	*ESC Matches*	*Nonprofit*	*Nonprofit Matches*	*State*	*State Matches*
Math Z-Scores	-0.38	-0.42	-0.71	-0.70	-0.77	-0.77	-0.88	-0.88
Reading Z-Scores	-0.33	-0.35	-0.63	-0.64	-0.71	-0.71	-0.80	-0.80
Proportion Black	0.43	0.45	0.68	0.69	0.71	0.71	0.84	0.84
Proportion White	0.50	0.50	0.25	0.25	0.23	0.23	0.11	0.11
Proportion Hispanic	0.03	0.02	0.04	0.04	0.05	0.04	0.03	0.03
Proportion Male	0.49	0.48	0.53	0.52	0.49	0.49	0.50	0.51

After identifying the matched comparison group, to increase precision and to control for any remaining observable differences between treatment and comparison students, we used a "value-added" model examining student math and reading test scores (separately) as the outcome measures and controlling for students' observable characteristics, including prior year test score (of the same subject as the outcome variable), whether the student transferred from one year to the next, and how long the school has been in operation.

The formal model is represented by:

[10] None of the differences in the average characteristics between the treatment and control groups are statistically significant.

$$Y_{ijt} = \beta_1 Y_{i,j,t-1} + X_{i,t}\beta_2 + \beta_3 \text{Mob}_{i,t} + OpYears\,\beta_4 + T_{i,t}\beta_5 + \text{GY}_{i,t} + e_{i,j,t} \quad \textit{Equation 1}$$

where $Y_{i,j,t}$ is the math or reading test score (run in separate models) for student *i* in subject *j*; $Y_{i,j,t-1}$ is the prior test score for student *i* in subject *j*; $X_{i,t}$ is a vector of controls for individual student characteristics (i.e., Black, White, Hispanic, "Other" race, gender, or Limited English Proficiency); Mob_{jt} is an indicator of whether student *j* transferred to a new school in the *t*th year; $OpYear_{jt}$ is a vector of dummy variables indicating whether the charter school has been in operation for one year, two years, or three or more years; T_{jt} is vector of binary variables indicating what type of charter school a student attends in the *t*th year (i.e., a binary variable for district-authorized, ESC-authorized, or nonprofit-authorized charter school); $\text{GY}_{i,t}$ is grade-year fixed effect; and $e_{i,j,t}$ is the error term.

If selection on unobservable characteristics is in fact comparable across authorizer types, then an analysis of student outcomes using a matched comparison group and additional controls for remaining observable characteristics should produce an unbiased estimate of the effect of authorizer types. However, because we cannot be assured that there are not unobserved differences between the population of students attending schools authorized by the various chartering authorities, we conducted a falsification test.

The falsification test examines whether charter authorizer type is associated with differential gains for students in the years *before* they enter the charter schools. If we detect spurious "effects" of charter type prior to entering the charter school, the success of our strategy in dealing with selection bias would be undermined. In fact, as Table 5 indicates, across reading and math outcomes for all four authorizer types, we find no cases in which the falsification test detects significant spurious "impacts" for students in the year prior to entering the charter

school, and no cases in which the point estimate exceeds 0.05 standard deviations. This provides some reason for confidence that our method is not substantially biased by unobserved selection.

Table 5: Falsification Test

Variable	*District Analysis*		*ESC Analysis*		*Nonprofit Analysis*		*State Analysis*	
	Math	**Reading**	**Math**	**Reading**	**Math**	**Reading**	**Math**	**Reading**
District	**-0.01 (0.07)**	**0.05 (0.07)**						
ESC			**0.00 (0.05)**	**-0.03 (0.05)**				
Nonprofit					**-0.01 (0.05)**	**-0.02 (0.05)**		
State							**-0.05 (0.07)**	**0.03 (0.08)**
Black	-0.19* (0.08)	-0.23* (0.07)	-0.23* (0.06)	-0.17* (0.06)	-0.35 (0.06)	-0.35* (0.05)	0.07 (0.17)	-0.09 (0.13)
Hispanic	0.20 (0.16)	0.28 (0.25)	-0.15 (0.27)	-0.28* (0.10)	-0.15 (0.10)	-0.21 (0.12)	0.58* (0.27)	0.56 (0.33)
Other	-0.10 (0.37)	-0.15 (0.28)	-0.12 (0.28)	0.03 (0.25)	-0.05 (0.21)	0.12 (0.19)	-0.37 (0.34)	0.57* (0.23)
Male	0.05 (0.07)	-0.09 (0.07)	-0.04 (0.05)	-0.05 (0.05)	0.01 (0.05)	-0.10* (0.05)	-0.02 (0.07)	-0.05 (0.08)
Mover	-0.08 (0.08)	-0.11 (0.07)	-0.10 (0.05)	-0.02 (0.06)	-0.03 (0.05)	0.02 (0.06)	-0.06 (0.07)	-0.07 (0.08)
Prior Math Z-Score	0.53* (0.05)	0.26* (0.05)	0.48* (0.04)	0.31* (0.04)	0.50* (0.04)	0.26* (0.04)	0.44* (0.06)	0.27* (0.07)
Prior Reading Z-Score	0.21* (0.05)	0.43* (0.05)	0.21* (0.04)	0.38* (0.04)	0.20* (0.04)	0.43* (0.04)	0.26* (0.05)	0.40* (0.06)
Constant	0.12 (0.12)	-0.06 (0.30)	0.04 (0.16)	0.28 (0.23)	0.19 (0.15)	0.42 (0.22)	-0.42 (0.29)	-0.23 (0.41)
Grade-Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES
OBS	357	357	676	676	771	771	296	296
R-Squared	0.51	0.51	0.48	0.43	0.55	0.52	0.53	0.47

* Indicates statically significant effects at the 5 percent level.
Note: Robust standard errors are in parentheses.

As an alternate approach to our primary analysis, we also employed a student fixed effect approach using the full sample of K-8 charter schools and TPSs across Ohio. Although we have noted the limitations of this approach above, we include it because it has become common practice in nonexperimental charter studies. We present the results of the student fixed-effect analysis in the appendix while presenting the results for our main analysis in the next section. The results for the fixed-effect approach are largely consistent with the matching results.

Results

Table 6 presents results of our primary analysis using the matched comparison groups with the value-added regression. Estimated achievement effects of ESC-authorized and state-authorized charter schools are statistically indistinguishable from those of other charter schools in both reading and math, based on the matched comparison analysis. District-authorized charters have the largest (positive) point estimates, but the number of district-authorized students is relatively small and the results do not achieve statistical significance. Finally, nonprofit-authorized charter schools are producing lower achievement effects than other charter schools by statistically significant margins in both reading and math.

Table 6: Primary Results

Variable	*District Analysis*		*ESC Analysis*		*Nonprofit Analysis*		*State Analysis*	
	Math	**Reading**	**Math**	**Reading**	**Math**	**Reading**	**Math**	**Reading**
District	**0.09 (0.06)**	**0.08 (0.06)**						
ESC			**0.00 (0.03)**	**-0.01 (0.03)**				
Nonprofit					**-0.07* (0.04)**	**-0.06* (0.03)**		
State							**0.02 (0.04)**	**0.04 (0.03)**
Black	-0.08* (0.04)	-0.06 (0.04)	-0.11* (0.03)	-0.11* (0.02)	-0.13* (0.03)	-0.10* (0.03)	-0.06 (0.04)	-0.06 (0.03)
Hispanic	-0.28* (0.08)	-0.14* (0.06)	-0.16* (0.05)	-0.14* (0.03)	-0.20* (0.04)	-0.13* (0.04)	-0.06 (0.07)	-0.10* (0.04)
Other	-0.10 (0.06)	-0.01 (0.06)	-0.13* (0.04)	-0.03 (0.04)	-0.07 (0.05)	0.05 (0.06)	-0.04 (0.06)	0.01 (0.06)
Male	0.05* (0.02)	-0.16* (0.03)	0.01 (0.01)	-0.11* (0.01)	0.03* (0.01)	-0.09* (0.01)	0.02 (0.02)	-0.09* (0.02)
Mover	-0.13* (0.03)	-0.14 (0.04)	-0.08* (0.02)	-0.07* (0.02)	-0.06* (0.02)	-0.08* (0.02)	-0.02 (0.02)	-0.03 (0.02)
Prior Math Z-Score	0.51* (0.03)	0.25* (0.02)	0.46* (0.01)	0.22* (0.01)	0.45* (0.01)	0.22* (0.01)	0.41* (0.02)	0.21* (0.01)
Prior Reading Z-Score	0.23* (0.02)	0.53* (0.02)	0.22* (0.01)	0.51* (0.01)	0.20* (0.01)	0.50* (0.01)	0.23* (0.01)	0.50* (0.01)
Attend school in second year of operation	-0.11* (0.05)	-0.07 (0.07)	-0.11* (0.05)	-0.09* (0.05)	-0.10* (0.04)	-0.08 (0.05)	-0.07 (0.05)	-0.04 (0.06)
Attend school in third or more years of operation	-0.07 (0.05)	-0.12* (0.06)	-0.04 (0.03)	-0.05 (0.03)	-0.02 (0.03)	-0.04 (0.03)	0.04 (0.03)	0.01 (0.03)
Constant	0.81* (0.14)	0.48 (0.44)	0.32* (0.10)	0.55* (0.12)	-0.23* (0.10)	0.52* (0.14)	0.12 (0.12)	0.44* (0.14)
Grade-Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES
OBS	2860	2877	13255	13269	8872	8857	7164	7159
R-Squared	0.55	0.55	0.46	0.50	0.43	0.47	0.39	0.45

* Indicates statically significant effects at the 5 percent level.
Note: Robust standard errors are in parentheses.

One threat to the validity of these findings as reflecting the effectiveness of authorizers is the possibility that authorizer type is correlated with the length of time the charter schools have been operating. Many prior studies have found evidence that the performance of the typical charter school is weakest in its first year of operation. If nonprofit authorizers had a disproportionate share of newly opened charter schools in the years included in our data, this

could bias downward the estimated effectiveness of nonprofit authorizers. We hoped to be able to include a charter school's year of opening among the characteristics used in our matching procedure, but it was impossible to do this while maintaining close matches on other critical characteristics. Instead, we conducted a sensitivity analysis that is identical to our primary analysis except that it excludes all charter schools in their first or second year of operation. The results of the sensitivity analysis of charter schools with a minimum of three years of experience are presented in Table 7. The point estimates are generally comparable to those in our primary analysis. None of the effects for the subsample of experienced schools achieve statistical significance, but in the case of the nonprofit authorizers, this appears to be because of limited statistical power: the point estimates for nonprofit-authorized schools are nearly identical in the experienced subsample as in the full sample. In short, the sensitivity analysis provides results that are consistent with those of the primary analysis.

Table 7: Sensitivity Analysis
Students in charter schools that are in 3rd or more years of operation

Variable	*District Analysis*		*ESC Analysis*		*Nonprofit Analysis*		*State Analysis*	
	Math	**Reading**	**Math**	**Reading**	**Math**	**Reading**	**Math**	**Reading**
District	**0.06 (0.07)**	**0.02 (0.08)**						
ESC			**0.00 (0.04)**	**-0.02 (0.03)**				
Nonprofit					**-0.06 (0.04)**	**-0.05 (0.04)**		
State							**0.04 (0.04)**	**0.05 (0.03)**
Black	-0.05 (0.05)	-0.06 (0.06)	-0.08* (0.03)	-0.07* (0.03)	-0.09* (0.03)	-0.07* (0.03)	-0.08* (0.03)	-0.09* (0.03)
Hispanic	-0.21* (0.09)	-0.04 (0.06)	-0.16* (0.05)	-0.14* (0.04)	-0.14* (0.05)	-0.07 (0.06)	-0.17* (0.06)	-0.13* (0.05)
Other	-0.07 (0.11)	0.03 (0.10)	-0.09* (0.04)	-0.04 (0.05)	-0.10* (0.05)	0.03 (0.07)	-0.10 (0.06)	0.00 (0.06)
Male	0.05 (0.04)	-0.16* (0.05)	0.01 (0.01)	-0.11* (0.02)	0.01 (0.02)	-0.10* (0.02)	0.03 (0.02)	-0.09* (0.02)
Mover	-0.10* (0.04)	-0.15* (0.05)	-0.06* (0.02)	-0.07* (0.02)	-0.04 (0.03)	-0.04 (0.03)	-0.02 (0.02)	-0.03 (0.02)
Prior Math Z-Score	0.47* (0.03)	0.24* (0.03)	0.44* (0.01)	0.21* (0.01)	0.45* (0.02)	0.23* (0.02)	0.40* (0.02)	0.20* (0.01)
Prior Reading Z-Score	0.27* (0.03)	0.55* (0.03)	0.23* (0.01)	0.51* (0.01)	0.20* (0.02)	0.51* (0.02)	0.24* (0.01)	0.50* (0.01)
Constant	0.43* (0.17)	0.32 (0.38)	0.28* (0.11)	0.60* (0.13)	0.25 (0.14)	0.56* (0.16)	0.21 (0.11)	0.57* (0.13)
Grade-Year Fixed Effect	YES	YES	YES	YES	YES	YES	YES	YES
OBS	1249	1265	9433	9447	4107	4105	7046	7042
R-Squared	0.52	0.57	0.42	0.46	0.42	0.47	0.38	0.43

* Indicates statically significant effects at the 5 percent level.
Note: Robust standard errors are in parentheses.

Discussion and Conclusions

As other research (e.g., Buddin and Zimmer, 2005; CREDO, 2009; Hanushek et al., 2007) has shown, charter schools vary widely in academic performance. Authorizer type is only one factor among many contributing to the variation in performance among charter schools; it is surely not the most important factor. High performers and low performers exist among the schools authorized by each type of authorizers. And the variation in school performance is likely

to be greater within each authorizer type than it is between different authorizer types. Nonetheless, the analysis in this report suggests that authorizer type can make a difference. Even if this difference is not large, it is perhaps more readily susceptible to policy intervention than are many other factors determining school performance. Ohio charters that were originally authorized by nonprofit organizations are, on average, producing achievement gains (both in math and reading) that lag behind the gains of students in other charter schools. This result probably will not come as a surprise to many observers of the Ohio charter scene; lax oversight by some Ohio authorizers has been pointed out before (Fordham, 2006, 2008).

Ohioans have good reason to be concerned about the fact that the state's retirement from the authorizing business had the effect of turning over large numbers of charters to nonprofit authorizers, which have the poorest achievement record among those examined here. But it is also worth noting that there may be a tension between the goal of producing highly effective charter schools and the goal of producing a large number of charter options. In the absence of nonprofit authorizers, the number of charter schools in Ohio would almost certainly be a lot smaller. Virtually all of the growth in the state's charter sector between 2004 and 2008 was attributable to nonprofit authorizers. Policymakers who are seeking to promote the growth of the charter sector while simultaneously ensuring high quality therefore will need to find answers somewhere other than in the selection of authorizer type.

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Appendix

While there are both internal and external validity concerns of a fixed effect approach for estimating the achievement effects for various charter authorizers, it has been widely used by researchers to estimate charter effects (Imberman, forthcoming; Zimmer et al., 2009; Booker et al., 2007; Bifulco and Ladd, 2006; Sass, 2006; Zimmer and Buddin, 2006; Hanushek, Kain, and Rivkin, 2004). Therefore, it is prudent to include the fixed effect approach as sensitivity analysis. We should note that we restricted virtual school students from the analysis, but did not put any restrictions for the inclusion of other students.

In the model, we used annual gains in math and reading test scores as the dependent variable and also included a control for student mobility and grade-year fixed effects. As in our main analysis, all test scores have been normalized by grade and year with mean of zero and a standard deviation of one. Again, like our main analysis, we assigned the original authorizer for each school when creating the dummy variable for each authorizer. As a result, this can be viewed as an intent-to-treat analysis.

Table A-1 shows the results. We display the coefficient estimates for the four different authorized schools, which are identified by the changing performance of students in charter schools relative to traditional public schools. We also conducted pair-wise comparison by examining whether the differences in coefficient of the four different types of charter authorizers are statistically significant. The p-values of the differences are shown at the bottom of the table.

Table A-1: Fixed Effect Sensitivity Analysis

Variables	*Math*	*Reading*
District	0.05 (.08)	0.10 (0.07)
ESC	-0.05 (.05)	-0.04 (0.04)
Nonprofit	-0.18* (.04)	-0.10* (0.03)
State	0.06 (.04)	0.08* (0.04)
Mover	-0.05* (0.01)	-0.05* (0.01)
Grade-Year Fixed Effect	YES	YES
F Test of Significance Between Coefficients: P-Values		
District-ESC	.3055	.0718
District-Nonprofit	.0103*	.0059*
District-State	.9268	.8182
ESC-Nonprofit	.0246*	.2151
ESC-State Schools	.1015	.0175*
Nonprofit-State Schools	.0000*	.0001*
OBS	1,510,242	1,601,126
R-Squared	0.46	0.34

* Indicates statically significant effects at the 5 percent level.
Robust Standard Errors in Parentheses

The substantive results for the nonprofit-authorized schools remain true as the math and reading achievement estimates are negative and statistically significant. In addition, the analysis suggests a positive and statistically significant effect for state-authorized schools in reading. While the state-authorized effect was not statistically significant in our main analysis, the coefficient estimate is similar. All other estimates are statistically indistinguishable from zero. The pair-wise comparisons also suggest that the gains of students in nonprofit-authorized schools are lower than every other type of authorized school except for when compared to ESC-authorized schools in reading. The pair-wise comparison between ESC- and state-authorized charter schools is statistically significant as well. No other pair-wise comparisons are statistically significant. Overall, this suggests that our results for the nonprofit-authorized schools in our main analysis using a matching strategy are robust to the fixed effect specification.