The Impact of Charter Schools on Student Achievement

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Abstract

Charter schools are public schools that are funded by a per-person fee for each student they attract. They are freed from some regulations on school management but obey safety regulations, follow non-discrimination rules, participate in state testing, and choose students by lottery when they are oversubscribed. We investigate the impact of a group of schools that enroll most of Chicago's and many of Illinois' charter school students. The schools have more applicants than places. We use the "lotteried-out" students as a control group for the "lotteried-in" students because randomization makes the groups similar on unobservable characteristics, such as motivation, as well as on observable traits, such as race and prior achievement. We estimate both the effect of attending charter schools (the treatment-on-the-treated effect) and the effect of being offered the chance to attend a charter school (the intent-to-treat effect). We show that, compared to their lotteried-out fellow applicants, students who apply to and attend charter schools starting in the elementary grades score about six national percentile rank points higher in both math and reading. These effects are for students who have spent an average of two years in charter school. In the Chicago schools we investigate, students who apply in later grades are too rare for the random lotteries to generate balanced groups of charter school and regular public school students. We therefore do not have meaningful estimates for them. This study includes discussions of methods for dealing with lotteries, pre-lottery information, re-application to charter schools, siblings, attrition, and grade-of-entry effects. We also compare the results from the lottery method with results from value-added analysis and demonstrate the latter results are non- representative and problematic.

1 Introduction

In this study, we use data from the largest charter school system in the United States, which is located in Chicago, to investigate how students' achievement is affected by their attending charter schools. Determining how charter schools affect achievement is an important step in evaluating the state policies that have permitted almost 3000 charter schools to open in the United States since 1992, when the first was founded. Although charter schools enroll only a very small 1.5% of American students, 38 states have laws that support charter schools and charter enrollment continues to grow.[1]

There are a few things that all or most charter schools have in common. Like regular public schools, they are supported by public funds and may not charge tuition. Unlike regular public schools, they receive fees on a per student basis. Charter schools cannot select their students based on admissions tests or similar criteria. They obey many public school regulations, including testing and other requirements of states' accountability systems. On the other hand, charter schools are often exempt or partially exempt from regulations about teacher certification. Although all charter schools must be chartered by a public authority, the range of authorities depends on the state and may include a state board, school districts, and public universities.[2] The result is that charter schools are diverse. Most charter holders are non-profit organizations, teacher groups, or parent groups, but not all charter

[1] General information on charter schools and charter school laws is collected by the Center for Education Reform (www.edreform.com).

[2] Charter granting authority varies by state and is set by state law. Local school districts, state education departments, and other educational institutions (e.g., universities) are typically given the authority to grant charters.

holders manage their schools on a day-to-day basis. Many contract with school management organizations, which are most often non-profit but sometimes for-profit. Although all charter schools have open enrollment (if oversubscribed, they typically must hold lotteries) and a conventional academic curriculum, some charter schools have a curriculum that targets a certain student population, such as likely drop-outs, students who do badly in formal school settings, students who reject traditional curricula, or students interested in the arts.

Supporters cite two broad reasons why charter schools may provide higher quality education than traditional public schools. First, charter schools may be more flexible or innovative than regular public schools, and this may allow them to better serve student needs. Second, no student goes to a charter school unless his parents want him to go there, and funding is attached to individual students. Therefore, charter schools must attract parents in order to survive. The ability of parents to "vote with their feet" may be a form of accountability that raises school quality.[3]

Currently, there is very little credibly identified evidence on how students' achievement is affected by their attending charter schools. This may seem strange, given the importance of charter schools as a policy issue. However, researchers who attempt to provide evidence face a number of difficulties. Foremost is that charter schools are heterogeneous, as described above. Thus, we expect charter schools' "treatment" effects on student achievement to be heterogeneous. An evaluation of one group of charter schools can only shed light on the conduct of other charter schools with similar management and student populations.

Even if researchers choose to evaluate charter schools that are representative of a rea-

[3]Increased accountability, and competition, may also create pressure on traditional public schools to raise school quality. This issue, while important, is not the focus of this study.

sonable share of charter schools in the United States (as we do), they still face some other evaluation problems. Researchers want to know whether charter school students' achievement would have been different if they had not attended charter school. The counterfactual achievement is never directly observable, so charter schools must be evaluated by comparing the achievement of charter school students and other students. Identifying a good comparison group is important because students who apply to charter schools may be a non-random sample of students in the area. Students may apply to charter schools because (1) they are already doing badly in regular public school; (2) their performance is typical of their regular public school but it is a particularly bad school; (3) they are exceptionally able students who need resources or flexibility not offered by their regular public school; (4) their parents are highly motivated in a constructive (positive) way; or (5) their parents are highly motivated to intervene in a dysfunctional (negative) way. In short, students who apply to charter schools are likely to be self-selected, and a researcher who simply compares charter school students to all other local public school students is likely to produce results that suffer from self-selection bias.

In this study, we overcome the self-selection problems listed above by using the fact that state laws typically require charter schools to select students by lottery when the number of applicants exceeds the number of available places.[4] That is, our treatment group is composed of lotteried-in students and our control group is composed of lotteried-out students. Our empirical design, in other words, uses randomized assignment to treatment. The treatment and control groups should be similar on average, in both their observable and unobservable

[4]Of course, such a lottery is only binding when the number of applicants is greater than the number of students the school can accommodate.

characteristics. For this reason, we believe that the results of this study are as credible or more credible than any existing evidence on how charter schools affect student achievement. We compare the results of the lottery method to results we obtain when we employ less reliable methods, such as value-added analysis and simple comparisons between charter and regular public schools. We demonstrate that value-added analysis in particular can be very misleading. This is important because it has been employed in some influential studies.

We use this study to explore several issues in interpreting evidence on charter schools and several methodological problems that arise in the evaluating lotteried-in and lotteried-out students. For instance, use of lotteries eliminates self-selection problems between charter school applicants and non-applicants, but they do not guarantee that applicants are like non-applicants. It is possible that charter schools affect their applicants differently than they would affect students who decide not to apply. Each charter school may be capable of generating heterogeneous treatment effects, and we will see only treatment effects for the sort of student who applies. This is not a problem for policy makers' using the results, so long as the charter school law always lets students choose whether to apply to a charter school. We, as researchers, cannot claim to show how charter schools would affect the sort of student who never or rarely applies. What we can do (and do, in this study) is make the reader aware of the characteristics of students who are in our applicant pool, in order that the reader knows to whom our results may reasonably extrapolate.

Heterogeneous treatment effects may take other forms. For example, the effect of a charter school may depend on the age or grade at which a student enrolls, or the number of years for which a student has been enrolled. Also, the same charter school may have one effect on achievement in the first year that it opens and another effect on achievement once

it has been operating for a few years. Policy makers that are interested in whether charter schools make sense as an "in-place" policy should focus mainly on how a charter school in operation for some years affects students who are typical of an entering student. (For instance, primary school students typically enter in kindergarten or 1st grade). However, a policy maker who was interested in the *transition* to charter schools might look at results that indicate how a brand-new charter school affects the achievement of students who transfer from regular public schools in the middle of the primary or secondary grades. In this study, we focus mainly on results that are fairly typical of an in-place charter school, but we clarify throughout how much our results depend on charter schools' start-up years.

Studies with a randomized design face a few complications. Not all students comply with the treatment that the lottery assigns to them. In our study, students who are lotteried-in may choose to stay in their regular public schools, may decide to attend a private school, or may even move outside the area altogether. Students who are lotteried-out cannot choose to attend the charter school, of course, but they may attend a school other than their regular public school. It will turn out that we observe some of the non-compliant students (those who attend a public school still in the local district, even if it is not their regularly assigned one), but students who attend a private school or move away are attritors whom we do not observe once they leave the district's schools altogether. A minor problem is created by students who are attending private schools when they apply to charter schools—if they are lotteried-out, we never observe their achievement. In addition, a student may comply with his initial lotteried-out treatment but then reapply to the charter school the next year and become a lotteried-in student. Such reapplicants are compliant, but they need to be compared to other reapplicants who are lotteried-out twice if we are to maintain the

randomized design.

In this study, we analyze the achievement of students in the charter school system with the largest enrollment in the United States. The charter is held by the Chicago Charter Schools Foundation, which is a charitable organization, and the system is called the Chicago International Charter School ("CICS"). CICS draws students from the Chicago Public School district (CPS), which is the third largest district in America (only New York City and Los Angeles Unified have larger enrollment). Almost all of the charter school students in Illinois attend school in Chicago, and more than half of charter school students in Chicago are enrolled at CICS schools. Currently, CICS includes seven schools with a total enrollment of 4,430 students in fall 2003. Five of the schools are primary schools (serving some or all of grades pre-kindergarten to eight), one of the schools serves all grades from kindergarten to twelve, and the final school serves grades nine through twelve.

CICS has its own management organization run its high school and one of its primary schools. Four CICS primary schools are managed by a non-profit organization, American Quality Schools. The final (kindergarten through grade twelve) school is managed by Edison Schools, which is for-profit. Thus, CICS represents a good part of the spectrum of school models and management models among charter schools. In addition, CICS students are fairly typical of students who attend charter schools located in highly urban areas: its students are mainly black or Hispanic and most participate in the federal Free and Reduced Lunch Program, which means that they come from households with incomes below 185 percent of the federal poverty line.[5]

[5]Detailed information about this charter school system and the students who attend is given in the next section.

In the summer of 2004, CICS schools had approximately 1700 more applicants than they could admit, given their space. Each school uses randomized lotteries to admit applicants when oversubscribed. CICS provides us with an excellent environment for investigating charter schools' effects, not only because it is representative of a key part of the charter school sector in the United States, but also because access to its data and CPS data allow us to examine both lotteried-in and lotteried-out applicants. The data also let us address issues like non-compliance, attrition, and reapplication. Finally, we are able to examine whether the charter school treatment effect depends on a student's grade level, initial achievement, and the number of years the school has been operating.

2 The Schools of the Chicago Charter School Foundation

CPS has granted 15 charters to organizations to operate schools inside its district. This is the maximum number of charters allowed by Illinois state law.[6] One of the charters has been held by the Chicago Charter School Foundation since 1997 (renewed in 2002).[7] Charter schools in Illinois are free to establish their own missions and curricula, but they participate in the state's accountability system and have personnel restrictions that are similar to those of regular public schools. To pay for operational expenses, charter schools in Chicago receive a per-student fee equal to 75 percent of the average per-pupil spending in CPS.[8] The per pupil fee for the 2003-2004 school year was $5,279. Charter schools receive state and federal aid associated with disabled and other high-need students they enroll. Charter schools

[6] In Illinois, local school districts (in this case Chicago Public Schools) have the authority to grant charters, but these charters are subject to approval by the state board of education. Complete information on Illinois charter school regulations can be found in Illinois Compiled Statutes, Chapter 105, Act 5, Article 27a. Illinois Compiled Statutes can be found online at http://www.legis.state.il.us/legislation/ilcs/ilcs.asp.

[7] Charters may be granted for between five and ten years, but are typically granted for five years.

[8] 75% is the minimum allowable by Illinois state law and 125% is the maximum.

cannot charge tuition, but may collect reasonable fees for books and other materials. They can also accept donations. Illinois does not require local districts to provide charter schools with funding for startup costs, for the purchase of facilities, or for renovating buildings to make them appropriate for schools.[9] Most donations are from philanthropies and are channeled into purchasing, leasing, and renovating buildings.

The Chicago Charter School Foundation places schools in neighborhoods where the population is disproportionately minority and low income. Within that loose constraint, they locate vacant buildings—often former private or parochial schools—suitable for schools, purchase or lease these facilities, and perform necessary renovations. The first two CICS schools began operating in the 1997-1998 school year, and a third began in the 1999-2000 school year. The remainder opened in 2000-01 or after. Our study focuses on the three oldest CICS schools: Longwood, Bucktown, and Prairie.[10] Longwood serves approximately 1200 students in Kindergarten through 12th grade. Bucktown and Prairie serve, respectively, 600 and 350 students in kindergarten through eighth grade. For these schools, we collected information on all students who participated in the lotteries held in the spring of 2000, 2001, and 2002. These students, whether lottereried-in or lotteried-out, were matched to CPS administrative data by the Consortium for Chicago School Research. The CPS data provide us with pre-application achievement data and, even more crucially, with post-application achievement data for lotteried-out students.

In Table 1, we show demographic and program participation information for students

[9] They do establish a fund for loans to charter schools to pay startup costs. However, these loans are restricted to $250 per student, and only one loan can be made to each charter school organization.

[10] There were not enough lotteried-out students at other CICS schools in our current data to include them in this study. However, we hope include these schools in future analysis using students involved in the most recent lotteries.

in the three CICS campuses we study, from the 2001-02 school year. For purposes of comparison, the table also shows information for all CPS students, other Chicago charter schools, and students in Chicago private schools.[11] For CICS students, it is important to base program participation numbers on students' prior classification when they were at CPS. This is because schools exercise discretion over the degree to which they participate in the federal lunch program and in how they classify students for special education and bilingual education services. Indeed, Cullen and Rivkin (2003) show that some parents switch schools in a deliberate attempt to change their child's classification. We want to compare all students against the same thresholds for entry into the lunch program, special education, and bilingual education. Therefore, we base the comparisons on students' prior classification at CPS.[12] One drawback with using prior information is that approximately 60% of CICS students were not enrolled in CPS prior to entering charter school, in part because many CICS students apply to kindergarten.

Overall, students at the three CICS schools were 74 percent black, 22 percent Hispanic, 81 percent in the federal lunch program, 10 percent in special education, and 16 percent in bilingual education. However, the three schools were not alike. Students at Longwood were 99 percent black; Bucktown's students were 33 percent black and 54 percent Hispanic; and Prairie's students were 55 percent black and 44 percent Hispanic. Based on their

[11] Information on other Chicago charter schools excludes the Youth Connections Charter, which is an umbrella organization for programs to help high school dropouts. Information of private schools comes from the Private School Universe Survey. Unfortunately, this survey only has information on students' ethnicity.

[12] Note that very few Black students receive bilingual education, but roughly 40% of Hispanic students do. Lunch program participation is an imperfect indicator of poverty because a student can be eligible for lunch yet attend a school that does not offer lunch or offers only a partial program. The newness and small size of some charter schools prevents them from participating in the federal lunch program, so participation understates poverty at some charter schools (though not necessarily CICS schools). In any case, we avoid the problem by using prior CPS participation in the lunch program.

prior classification at CPS, 82 percent of Longwood's, 76 percent of Bucktown's, and 84 percent of Prairie's students participated in the federal lunch program. Based on their prior classification at CPS, 0.5 percent of Longwood students, 36 percent of Bucktown students and 38 percent of Prairie students received bilingual education; and 12 percent of Longwood students, 11 percent of Bucktown students, and 3 percent of Prairie students received special education services.

The CICS students we study were more likely to be black than the average CPS student (74 versus 51 percent), less likely to be Hispanic (22 versus 36 percent), similarly likely to be in the federal lunch program (81 versus 78 percent), similarly likely to be in special education (10 versus 12 percent), and similarly likely to be in bilingual education services (16 versus 14 percent). A characterization that is perhaps more accurate is that Longwood is most like very black Chicago schools, while Bucktown and Prairie are most like Chicago schools that are about evenly split between blacks and Hispanics, with a small share of non-Hispanic white students.

Although a fair number of students who apply to CICS schools after kindergarten are attending private schools or other non-CPS schools (such as other districts' schools) when they apply, CICS students do not look similar to the private school population of Chicago, largely because they are so much more likely to be black. Only 31 percent of private school students in Chicago are black, while 74 percent of CICS students are. 22 percent of Chicago's private school students and 22 percent of CICS students are Hispanic. Unfortunately, there is no information on the share of private school students who participate in special needs programs (or would participate in programs were they attending CPS). On the whole, the comparison between CICS and private school students suggests that, when CICS attracts a

private school student, he or she is not the typical Chicago private school student.

Table 2 shows Census of Population information on the neighborhoods (i.e., census tract) in which the three CICS schools are located. For purposes of comparison, we also display the Census information for the city of Chicago as a whole. Information from both the 1990 and 2000 Censuses is presented.[13] The neighborhood around Longwood is much more black than Chicago as a whole: while Chicago was only about 37 percent black in 2000, the Longwood neighborhood was 98 percent black. Thus, it is not surprising that Longwood enrollment is 99 percent black. A relatively small share of Longwood families are reported as being in poverty (6 percent in 2000), but 82 percent of Longwood's students participated in the federal lunch program, which requires a family to be within 185 percent of the poverty line. It is difficult to reconcile the poverty numbers unless we conclude that families inaccurately report income to the Census or that Longwood draws very disproportionately from poor families in the neighborhood. The neighborhood around Bucktown was 2 percent black, 23 Hispanic, and 7 percent poor in 2000. In contrast, Bucktown's students were 33 percent black, 54 percent Hispanic, and 67 percent in the federal lunch program. It is fairly clear, therefore, that Bucktown is drawing disproportionate numbers of minority and poor students from its neighborhood. Finally, in 2000, the neighborhood around Prairie was 56 percent black, 41 percent Hispanic, and 31 percent poor. Prairie students had rather similar minority status: they were 55 percent black and 44 percent Hispanic. However, 89 percent of Prairie

[13] One should keep in mind that the 2000 Census collected income information for 1999, which was a multiple decade low point for poverty, unemployment, and similar indicators in the United States. Thus, we expect improvements from 1990 to 2000 in Chicago neighborhoods that unless they deviated from the typical American experience. Indeed, we can see that throughout Chicago, the share of adults who were high school dropouts fell from 34 to 28 percent over the decade and the poverty rate fell from 18 to 17 percent over the decade.

students participated in the federal lunch program.

Students who apply to charter schools may live anywhere in the city of Chicago, and those who attend may travel considerable distances. Figure 1 shows the cumulative distribution of the distance from each CICS school to the school its students would attend based on their attendance areas.[14] Prairie has by far the most local student population; more than half of its students' local schools are less than one mile away. This evidence reinforces what we have seen above: Prairie students are typical of their neighborhood. In contrast, the median distance for Longwood and Bucktown students is two and five miles, respectively. Figure 2 shows that, in our sample, more than 75 percent of students who attend traditional CPS schools live in the same attendance area as their school. However, non-CICS students who attend non-traditional CPS schools (i.e., magnet schools, CPS career academies, and non-CICS charter schools) travel about the same distance as do CICS students.[15] That is, CICS students travel about the same amount as other students in our sample who do not attend their neighborhood school.

Above, we observed that estimated effects of charter schools can only safely be extrapolated to students like those in the sample schools. Our results are thus most applicable to urban students who are black or Hispanic and who come from low-income households. However, they (or their parents) must have been motivated enough to apply to charter schools

[14] We do not have data on students' actual addresses. These distances are calculated using the latitude and longitude of school addresses. Note that, according to this measure, students are considered as living zero miles away from their local school. The average distance from a school to its attendance boundary is about one half mile.

[15] Non-traditional schools in CPS are: magnet schools, selective enrollment high schools, military academies, "career academies," charter schools (non-CICS), "academic prep centers," alternative schools for pregnant girls and at-risk students (i.e., those who have been expelled from other schools). Chicago has a school choice program that allows students to attend a traditional public school outside of their attendance area (see Cullen et al. 2003).

and (in some cases) travel a considerable distance to attend one.

3 Charter School Lotteries: Our Data and Common Methodological Issues

In the late spring of each year, the Chicago Charter School Foundation holds lotteries among the applicants to each CICS school. While charter school lotteries differ somewhat in the United States, the CICS lotteries are fairly typical. Thus, our discussion of them and the methodological issues they raise should be instructive for evaluation of most charter schools.

Each CICS lottery is specific to a school and grade. For instance, suppose that Bucktown has 60 kindergarten places available for 120 applicants and has five 2nd grade places available for 25 applicants. There would be a separate lottery for each grade at Bucktown. After a charter school's first year of operating a particular grade, it is normal for the most places to be available in kindergarten. Places in higher grades usually are only made available when a student leaves. In each lottery, applications are assigned a random number and ordered according to it. Using this ordering, the available places in each grade in each school are filled. In CICS, 90 percent of the places offered in the lottery are taken up by the students to whom they are offered. About 10 percent of the places offered are not taken up, usually because the student's family moves or otherwise experiences changed circumstances over the spring and summer. Given typical rates of family and residential changes among the poor, central city population served by CICS schools, it is not surprising that 10 percent of places are not taken up. These places are reoffered in the late spring and summer as they become available, with the schools working their way down the list of lottery numbers. A few places

are even offered at the start of the school year when some students who were offered places and initially accepted them fail to show up.[16]

3.1 Defining the Lotteried-In and Lotteried-Out

Throughout this paper, it is useful to remember that it is the lottery number, as opposed to charter school enrollment, that is randomly assigned to a student. There are two reasonable ways to draw the line between the lotteried-in and lotteried-out. One can describe as lotteried-in all those students whose lottery numbers are good enough to ensure that they are offered places by the start of school year. All of the remaining applicants are then lotteried-out. Alternatively, one can view a late offer of a place (one that takes place in late spring or summer, for instance) as inferior to an offer made at the time the lottery is held. For instance, parents whose child initially loses the lottery may make alternative plans and refuse late place offers when and if they are made. If so, then a late offer is not equivalent to an on-time offer, and it is reasonable to call those with on-time offers the "lotteried-in," call those with no offer the "lotteried-out," and keep those with late offers in a separate category. On the other hand, if parents whose child gets a lottery number that is just short of admission conclude that their child is likely to get a late offer and are as ready to accept a late offer as they would have been to accept an on-time offer, then the bipartite division into lotteried-in and lotteried-out is reasonable. On the whole, we feel that one makes the cleanest use of the lotteries by showing results that classify students with on-time offers as lotteried-in and students no offers as lotteried-out. Observe that such

[16] See Hanushek et al. (2001) for information on "turnover" among students from minority, low income families.

results exclude students who get late offers, whether or not they accept them. However, our results are robust to classifying students who have *either* on-time or late offers as lotteried-in and students with no offers as lotteried-out.

3.2 Siblings

In CICS as in most charter schools, a sibling of a student who is currently at the charter school has priority over other applicants. That is, lotteries are effectively held at the sibling group level. If a student is lotteried-in, then his or her siblings are also lotteried-in if they apply in a subsequent year for which there is space available. For instance, if a family has two children and the older one applies for kindergarten in 2001 and is lotteried-in, his younger sibling will be automatically lotteried-in if he applies for kindergarten in 2002. The fact that randomization occurs at the sibling group level is not a problem for analysis: the lotteried-in variable must simply be extended to siblings. The main consequence is that more observations are needed for a randomization to balance students' characteristics across the lotteried-in and lotteried-out groups: siblings' characteristics are obviously not independent.[17]

3.3 Applicants from Local Public, Private, and Other Schools

We collected CICS applications for students in the lotteries held in spring 2000, 2001, and 2002 for the Longwood and Prairie campuses; and collected applications for the lotteries held in spring 2001 and 2002 for the Bucktown campus. On their applications, students reported

[17] If there are more siblings of lotteried-in students than there are spaces in the grade to which the siblings are applying, the school holds a lottery among the siblings. This situation is, however, rare. One might worry that children in bigger families are more likely to be admitted to charter schools because only one sibling has to win a lottery for the sibling group to win. We are looking into this possibility in the data but do not expect it to be a factor of significance.

the school from which they were applying (that is, the school that they were currently attending). Regardless of whether they reported applying from a CPS school or not, the Consortium for Chicago School Research generously agreed to attempt to match students to the CPS database using their names, dates of birth, and the school and grade they were attending when they applied. 65 percent of the students claimed to be applying from a CPS school and, of these, 81 percent of the students were matched to their CPS records. 26 percent of students claimed to be applying from a private school. Interestingly, we were able to match 56 percent of such students to CPS records because they had been enrolled at CPS prior to their private school enrollment. 8 percent of applicants (especially applicants for kindergarten) applied from a daycare center. We were able to match 66 percent of these to the CPS database because they were enrolled in a CPS pre-kindergarten or similar program. The final 1 percent of students claimed to be applying from a public school outside the Chicago district. Of these, we were able to match 60 percent to the CPS database owing to their prior enrollment in a CPS school. Overall, we matched 73 percent of CICS applicants to a CPS record.

Nevertheless, it is important to recognize that there are some charter school students for whom we do not have an effective control group of lotteried-out students. For instance, consider a charter school student who applies from a private school and who would have continued at the private school if lotteried-out. The correct control student is a lotteried-out student for whom we have no or only pre-lottery information (which we have if the control student attended a CPS school prior to attending the private school). It is not appropriate for our treatment group to include students for whom our control group has no parallel. For this reason, we generally limit our analysis to students who applied from a CPS

school for and who could be matched with the CPS database at the time of their lottery. Using this "focus" sample is important because, within it, the lotteries credibly balance the unobserved characteristics of the lotteried-in and lotteried-out students. In order to show how much we can extrapolate from the focus sample, it is important to show how the focus sample compares to CICS applicants overall. We do this below.

One should interpret the results based on our focus sample as follows. We estimate the effect of CICS charter schools on students who would be in a CPS school at the time of their application and at least one year thereafter. In other words, our results show how charter schools affect public school students, not student who would otherwise be attending private schools.

3.4 Compliance and Attrition

Not all students comply with their intended treatment. There are two types of non-compliance. First, even among students who apply from a CPS school, some lotteried-in students do not enroll. Instead, they enroll in a private school, a public school in another district, or–most often–continue in a CPS school. Second, some lotteried-out students who apply from a CPS school do not continue to attend a CPS school. They switch to a private school, a public school in another district, or even a non-CICS charter school. Notice that we continue to observe some non-compliers: the lotteried-in who stay in a CPS school, students who attend a non-CICS charter school. Thus, non-compliers can be usefully divided in "observed non-compliers" and attritors.

Suppose that all non-compliers were observed non-compliers. Then, simple treatment-on-the-treated effects (estimated by instrumenting for enrollment in a charter school with

an indicator for being lotteried-in) would be fully purged of bias associated with selection into non-compliance. Simple intent-to-treat effects (the effect of the indicator for being lotteried-in) would also be fully unbiased.

If attrition from the lotteried-in and lotteried-out groups is the same, then it does not affect the *difference* in achievement between the groups, and it is this difference that we rely upon when we estimate intent-to-treat and treatment-on-the-treated effects. More precisely, if attrition were random, then we could obtain unbiased estimates of the achievement of charter school students, the achievement of CPS students, and the difference between the achievement of charter and CPS students. If attrition is non-random but has the same correlation with achievement in the lotteried-in and lotteried-out groups, then we can obtain unbiased estimates of the difference in achievement between charter and CPS students.

We find ourselves in the latter case: attrition is non-random but appears to be the same for lotteried-in and lotteried-out students. This is shown in Table 3. In column (1) of the table, we show that, both among the lotteried-in and lotteried-out students, approximately 6.5 percent of students atrit each year and just under 14% percent of students ever atrit.[18] In column (2) we show that students who had higher test scores in their lottery year are less likely to attrit.[19] This is evidence of non-random attrition and is what we expect. It is common to find that there is a correlation between test scores and family stability. The crucial column is (3), in which we show that the relation of initial test scores with attrition is same for the lotteried-in students (a coefficient of 0.994 with a standard error of 0.004) and lotteried-out students (a coefficient of .995 with a standard error of 0.006). That is,

[18] Note that, since our focus group only includes students observed the year after their lottery, and we cannot observe attrition in the final year, the median number of years students are "at risk" of attrition is 2.

[19] The p-value on the test score coefficient is .07.

the lotteried-out attritors are no more likely to be high scoring or low scoring than the lotteried-in attritors.[20]

3.5 Randomness, the Law of Large Numbers, and Grade-of-Entry

One might like to know whether the CICS lotteries were really random. Table 4 shows that they apparently were. The table shows the results of linear regressions of various student characteristics on a dummy variable for the student's having been lotteried-in or lotteried-out.[21] The top part of the table shows the results for our focus sample: students who were observed in CPS both before and after their lottery.[22] The bottom part of the table shows results for all students in our sample. Looking at test scores, race, special education, bilingual education, and free and reduced-price lunch, we see that the differences between lotteried-in and lotteried-out students are statistically insignificantly different from zero. This is true whether we look at our focus sample of students who applied from CPS or whether we look at all of the students in our sample.

Randomization combined with the law of large numbers, not randomization by itself, generates lotteried-in and lotteried-out groups that are credibly balanced on unobservable as well as observable characteristics. Charter school lotteries can be small, especially for

[20] We considered retention-in-grade, which occurs when a student is forced to repeat a grade. A student who is forced to repeat the fifth grade, say, lacks a measure of what his achievement would be on the sixth grade exam in the year in which he would be a sixth grader. Retention-in-grade will obviously not be random among students, but what matters for our estimates is whether regular public schools and charter schools are *differentially* likely to retain a student with a given level of achievement. We find no evidence that such differential effects exist. Indeed, there are so few students retained that we do not show results on this matter, but simply note it for the reference of other researchers, who may have a sample in which retention is a more common phenomenon.

[21] These regressions also include a fixed effect for each CICS-school year-grade combination, since this is the level at which being admitted is randomized.

[22] Our focus group consists of students observed in CPS before and after their lottery who apply while in 1st to 7th grade, and students who apply

grades to which students do not ordinarily apply. Thus, CICS' conducting every lottery in a completely random fashion does not guarantee balanced treatment and control groups. Put another way, seeing whether lotteried-in/lotteried-out differences are *statistically* insignificant is useful for determining whether lotteries are random, but we also need to see that lotteried-in/lotteried-out differences are insignificant in magnitude. Looking back at Table 4, we see that the differences are indeed very small in magnitude for our focus sample and our overall sample. It turns out, however, that this balance is not a feature of all grades-of-entry.

In Table 4A, we divide the lotteries into the grade for which students were admitted. Notice first that most students are admitted to charter schools in the early grades. Kindergarten and first grade alone account for 47.2 percent of the charter schools' admittees. In contrast, sixth and seventh grade account for only 8.6 percent of the admittees. The decline in the percent admitted with each subsequent grade-of-entry is almost certainly caused by parents' being averse to switching schools when their child is in the midst of a school's normal grade progression ("midstream" grades). Unless a family is moving or otherwise changing in composition, only students who are struggling socially or academically are likely to be shifted between schools midstream. Thus, few places open up in grades like six or seven and, except in its first years of operation, a charter school enrolls only a small share of its students in such grades.

Table 4A shows that the consequence of the tendency to enter in early grades is that students' observable characteristics are more likely to be balanced for grades in which entry is common. This is perhaps most obviously seen in the probability of a student's being female. For instance, among students who enter as kindergarteners and first graders, the lotteried-in/lotteried-out differences in the probability of being female is one percent, a difference that

is both statistically insignificant and insignificant in magnitude. On the other hand, among students who enter as sixth and seventh graders, the lotteried-in/lotteried-out differences in the probability of being female are, respectively, plus five percent and minus nine percent. These differences are both statistically significantly different from zero and substantial in magnitude. Keep in mind that these differences do not imply that the lotteries for sixth and seventh grade places were non-random. Rather, there are so few places available in these lotteries that randomization was insufficient to generate balanced lotteried-in and lotteried-out groups.

The validity of lottery-based methods of evaluating charter school students' achievement rests on achieving actual balance between the lotteried-in and lotteried-out groups. Because we do not attain this standard equally well for all grades-of-entry, we focus on the results for grades of entry that are common and in which balance is credibly attained. In particular, we will focus on results for students who enter in kindergarten through grade five, where balance is fairly plausible. Notice that although the percent of students admitted falls with each subsequent grade, there is a sharp drop-off between the fifth and sixth grades. It is this drop-off, as well as the increasing signs of lack of balance, that made us choose to focus on students who enter in kindergarten through fifth grade. While this focus slightly limits the range of the estimates we can show, it is largely a good thing. We want to focus on how charter schools affect the achievement of a student who is a fairly typical entrant, and early grade entrants *are* typical. Indeed, if anything, our data underrepresents how typical early grade entrants are because the charter schools we examine are relatively new. If we want to focus on results that are typical of charter schools' future, we should focus heavily on grades that will be typical grades-of-entry for a mature charter schools. New charter schools have

more late-grade entrants for two reasons. First, a brand new school usually accepts an equal number of students in all of its grades, producing a pattern of late grade entry that will not be repeated. Second, new charter schools are relatively unknown quantities so parents are more likely to find that they have unintentionally enrolled their child in a school that is a bad fit. They are more likely shift their child to another school (perhaps his former school. perhaps another charter school), thereby creating space for late grade-of-entry applicants.

In additon, we control for observable covariates to improve the balance between the lotteried-in and lotteried-out groups. See the next section for more on this.

3.6 Grade-of-Entry and Value-Added Analysis

We have seen that, for lottery-based studies like ours, the rareness of late grade entry is slightly limiting but largely generates a focus that is good. However, the rareness of late grade entry poses very serious problems for value-added analysis of charter schools, such as Sass (2004), Bifulco and Ladd (2004), and Hanushek et al. (2002). Why does the rareness of late grade entry seriously compromise the validity of value-added studies. The typical value-added study compares the annual rate of achievement gain of a student *after* he enters a charter school to his annual rate of achievement gain *before* he enters charter school. Essentially, this is a difference-in-differences analysis. The problem with such estimates is that they necessarily rely entirely on students who are tested for at least two years in the regular public schools prior to attending a charter school for at least two grades. (Two grades are needed in each type of school in order to estimate the rate of gain.) The result is that the value-added studies' results are based entirely on a small group of students who are likely to be non-representative because they are engaging in behavior that is rare. For

instance, all of the studies use state tests that are administered for the first time in the third grade. Moreover, the studies listed above focus mainly on students in elementary schools, simply because most charter schools are elementary schools Because two years of test data are needed in each type of school, the studies' results are based almost exclusively on students who attended third and fourth grade in the regular public schools, applied to a charter school's fifth grade, and then attended charter school for the fifth and sixth grade. But, we have seen that such students are rare. Moreover, the fact that they are rare is not accidental; it is the result of parents' unwillingness to move children who are in midsteam grades. Students who are moved midstream are disproportionately likely to be struggling socially or academically, but any such disproportionateness means that estimates based on them will be problematic. In the language of selection and causal inference, value-added estimates are local to a group a students who are, by their nature, not representative of the typical students. It is dangerous to extrapolate from such estimates, and it is wrong to portray them as representative in the absence of independent evidence that they are.

Toward the end of this paper, we show a comparison between our lottery-based estimates and estimates that we obtain using value-added analysis on the same data. However, comparing estimates only helps us to learn about our sample. The problems with value-added analysis of charter schools are problems that are inherent in the structure of the method. There can be no remedy for them unless and until the typical parent interested in charter schools wants his child to enter in a midstream grade.

4 How Charter Schools Affect the Student Population of the Regular Public Schools

One concern about charter schools is that they may disproportionately remove non-minority, non-poor, high achieving students from the regular public school system. There is little solid evidence about the consequences of changing peer composition, but it is possible that changes in peer composition would affect students who remained in regular public schools. Therefore, an interesting question is how the existence of CICS schools affects the student population of the regular public schools.

There are two ways to answer this question. Each has a slightly different counterfactual. First, we can take a simple approach and view each CICS school as a "helicopter drop" into a Chicago neighborhood. We can then compare CICS students to the students in CPS schools within some radius of the CICS school (we use three miles). This exercise assumes that all students would attend local schools in the absence of CICS and that the CICS school simply adds another local school to the mix. This simple exercise answers the question, "If the CICS school were to admit one more student and he or she were to be typical of existing students at that CICS school, how would the student who has left CPS differ from the average local CPS student?"

Carrying out this exercise, we find that students at the Longwood school are very similar to those at surrounding CPS schools on ethnicity (more than 90 percent black), participation in the free lunch program (roughly 80 percent), special education (14 percent in surrounding schools versus 12 percent in Longwood), bilingual education (less than 1 percent). Their prior math test scores are somewhat lower (37 versus 42 percentile, 39 versus 38 percentile

in reading). These measures are shown in table 5.[23]

Compared to their surrounding schools, Bucktown students are a bit more likely to be Hispanic (53 versus 47 percent), less likely to be Black (33 versus 39 percent), somewhat less likely to have received special education (10 versus 15 percent), and much more likely to have received bilingual education (36 versus 16 percent). They also have lower math scores (46 versus 54 percentile, 46 versus 47 percentile in reading). Students at the Prairie school differ the most from students in surrounding schools. They are much more likely to be Hispanic (44 versus 6 percent) and less likely to be black (55 versus 91 percent). They are also much less likely to have been in special education (3 versus 14 percent), much more likely to have received bilingual education (38 versus 1 percent), and have higher prior reading test scores (40 versus 36 percentile, 43 versus 41 percentile in math). However, we saw above that Prairie students are much more local than the 3 mile radius and they are representative of the school's immediate neighborhood.[24] In short, it appears that CICS schools may draw students who are more likely to be Hispanic (and receiving bilingual education) than the average local CPS student, but otherwise CICS schools draw students typical of the local population.[25]

[23] Summary statistics on CICS students, from table 1, are included for ease of comparison.

[24] Almost one half of the Hispanic students enrolled at CICS Prairie live in the attendance areas of Pullman Elementary and Curtis Elementary. Both schools had significant declines in their Hispanic population coincident with the opening of the Prairie campus.

[25] A second approach is to compare lotteried-out CICS applicants to the students in the schools that they attend. This approach recognizes that CICS applicants do not always stay in their local public school. Put another way, the thought experiment closest to removing the CICS presence from Chicago is to ask, "What if CICS lotteried-out all of its applicants because its number of places was zero?" To answer this question, we compare CICS students to the average characteristics of students in schools the lotteried-out students subsequently attend. This is because the lotteried-out students' behavior is the best indicator of what the lotteried-in students would be doing if CICS did not exist. These comparisons provide a very similar story to that shown in table 5. This suggests that more recent CICS applicants and enrollees would attend schools more closely resembling those in the surrounding area.

5 Charter Schools and Student Achievement

Because our evaluation of charter schools' effect is based on a design with randomized assignment to treatment, we start with a simple estimation strategy. We then add features to deal with issues like compliance and attrition. In our focus sample, students apply to charter schools from a regular public school (unless they are kindergarten or grade one applicants, in which case no prior school is expected) and are offered a charter school place at random. Randomization is at the sibling group level but, the sake of notational convenience, we will write the variable as though randomization occurred at the individual student level.

If students comply fully by attending the charter school if an offer is received and attending a regular public school if an offer is not received, then the average impact of charter schools on achievement across all applicants (β) is identified through estimation of equation 1. That is, achievement of student i (A_i) is a function of admission to the charter school ($Admitted_i$), a lottery fixed effect (α_j), an indicator variable for time t (θ_t), and other (orthogonal) factors (ε_i).[26] A lottery fixed effect is necessary because randomization occurs *within* a lottery; selection into a lottery need not be random. (Indeed, we have shown that students who apply for late grade lotteries are not a random subset of applicants.). The indicator for time t picks up any changes over time in the environment that affect all students, whether in or out of charter schools. For instance, the time indicator would pick up a change in the test.

$$A_{it} = \sum_j \alpha_j D_i^{(j)} + \beta Admitted_i + \theta_t + \varepsilon_i \tag{1}$$

[26] $D_i^{(j)}$ is an indicator for student i having applied to lottery j. To be clear, a lottery consists of the CICS campus, school year, and grade to which the student first applied.

5.0.1 Treatment-on-the-Treated and Intent-to-Treat Effects

If all non-compliant students are observed non-compliers (no students attrit), then estimation of equation **??** via linear regression yields an "intent to treat" (ITT) estimate: the effect of being offered a place at (but not necessarily enrolling in) charter school. We can estimate the impact of actually enrolling in a charter school by instrumenting for an enrollment indicator variable with the offer of admission indicator variable.[27] The instrumental variables estimate is the average effect of charter schools on the achievement of students who comply with their initial assignment, or the "treatment on treated" (TOT) effect. The first and second stages of the instrumental variables estimation are:

$$Enrolled_i = \sum_j \delta_j D_i^{(j)} + \lambda Admitted_i + \kappa_t + \upsilon_i \qquad A_i = \sum_j \alpha_j D_i^{(j)} + \beta \widehat{Enrolled}_i + \theta_t + \varepsilon_i \quad (2)$$

We show both TOT and ITT effects of charter schools, but our tables and discussion focus on the TOT estimates because policy makers are far more interested in them. Policy makers interest is not arbitrary; it is a logical given that the fundamental idea of charter schools is to make choices available, not to force students to attend a charter school when they are offered a place but would prefer not to accept it. We show the ITT estimates in appendix tables.

[27] The assumption that being lotteried-in has no direct effect on achievement is needed in addition to our baseline assumption that lottery assignment is random.

5.0.2 Controlling for Covariates to Improve the Balance of the Lotteried-In and Lotteried-Out

If each lottery had a large number of participants, the law of large numbers would ensure that the lotteried-in and lotteried-out students in each lottery would be fully balanced on observable and unobservable characteristics that affect achievement. As noted above, although the total number of observations in our sample is large, individual lotteries can be small because they are held at the school-by-grade level. In the above equation, we effectively restrict the difference between lotteried-in and lotteried-out groups to be zero, for a given lottery. Because this restriction may not hold in small lotteries, we try controlling for pre-existing, observable differences between lotteried-in and lotteried-out students. The control variables (X) for each student are those variables, including prior achievement, that are observed before the lottery and that do not change over the sample period. If the unobservable determinants of achievement are correlated with the observable determinants of achievement, controlling for covariates relaxes the restriction of interest. That is, we effectively restrict the difference between the lotteried-in and lotteried-out students to be zero, for a given lottery within students whose observable characteristics are similar.

It is worth noting that students in the CICS applicant pool are not terribly heterogeneous to begin with (for instance, none of them are rich or very high achievers), so we are controlling for *local* differences in variables like prior achievement. Thus, we believe that we have not imposed strong linearity restrictions even though continuous covariates, like prior achievement, enter the equation linearly. Also note that, because students who apply to kindergarten and first grade do not take pre-lottery tests, we cannot control for their

pre-lottery achievement in our vector of control variables. Instead, students who apply to kindergarten or first grade have an indicator for the legitimate absence of a pre-lottery test score.

Our modified regression estimates the ITT effect as the coefficient on the interaction term, γ, in equation 3.

$$A_{it} = \sum_j \alpha_j D_i^{(j)} + \gamma Admitted_i + \theta_t + \beta X_i + \varepsilon_{it} \tag{3}$$

Naturally, there is an instrumental variables estimation that corresponds to the above equation and that gives us the TOT estimate of the effect of charter schools.

$$A_{it} = \sum_j \alpha_j D_i^{(j)} + \gamma \widehat{Enrolled}_i + \theta_t + \beta X_i + \varepsilon_{it} \tag{4}$$

5.0.3 Grade-of-Entry Specific Effects

Students who enter charter schools at different grade levels may be affected differently by attending charter schools. For example, because of their longer exposure to regular public schools, older entering students may have greater difficulty adjusting to institutional differences at charter schools. Also, if parents of older students only apply to charter schools when their child is struggling, the treatment effect may differ for late entrants not because the lateness *per se* but because charter schools have a different effect on students who are already struggling than they have on students who have not yet developed a problematic social or academic patterns. On the other hand, late grade entrants may have more direct influence on their decision to apply to charter schools (as opposed to parental influence) and

may therefore be better matched to the schools if lotteried-in.

We estimate an equation that allows the achievement effect to differ by the grade for which students applied to the charter schools:

$$A_{it} = \sum_j \alpha_j D_i^{(j)} + \sum_g \left(\gamma_g Admitted_i + \theta_{gt} + \beta X_i\right) * D_i^{(g)} + \varepsilon_{it} \quad (5)$$

where $D_i^{(g)}$ is an indicator variable for having applied to either: (1) kindergarten through 5th grade, and (2) 6th to 8th grade. Above, we explained that we selected these grade grouping primarily because it is only credible that randomization produces balanced lotteried-in and lotteried-out groups for students who enter in kindergarten through fifth grade. The instrumental variables estimation that corresponds to the above equation and generates the TOT effect of charter school on achievement follows the same pattern as equations 2 and 4.

5.0.4 Year-of-Charter-School-Operation Effects and Time-in-Charter-School Effects

Because CICS charter schools are recent start-ups, we would like to allow the effect of charter schools on achievement to vary with the length of time that the school has been in operation. However, in our data, this is about equivalent to allowing the charter school effect on achievement to vary with the student's lottery cohort. That is, we would like to allow the achievement effect to be different for students who entered a CICS school after the 2000, 2001, and 2002 lotteries. Because students' achievement sometimes displays odd patterns after they have switched schools (an initial dip followed by a recovery is expected), we would also like to allow the effect of charter schools on achievement to vary with the length of time that a student has been enrolled at the charter school. That is, we would

like to allow the achievement effect to be different for one year of charter school experience, two years of charter school experience, and so on.

If we had a long panel of data with charter schools beginning their operations in a wide variety of years, we could identify both year-of-charter-school-operation effects and time-in-charter-school effects. At this time, we cannot identify both sets of effects because we do not yet have a sufficiently long panel of data or sufficient variation in school start-up dates. We therefore leave this for future analysis, when we will have a greater number of lotteries, test scores, and schools to examine.

5.1 "Treatment on Treated" Estimates of Charter School Effects

Our estimation strategy is shown by equation 5. That is, we measure the "treatment on treated" (TOT) effects of being enrolled in a charter school and allow the effects to vary by grade of entry into the charter school.[28] We show eight specifications, four for math and four for reading, in tables 6a and 6b, respectively. Full results of these TOT regressions and full results of ITT estimates are given in the appendix. In column (1) of these tables, we show a baseline specification that includes only lottery and time fixed effects.[29] In column (2) we include additional controls for student characteristics as measured before the admissions lotteries took place. These include gender, ethnicity, participation in the free lunch program, special education, or bilingual education, and whether or not the student had previously repeated a grade.

In columns (3) and (4) we add controls for students' prior achievement in two different

[28] Modifying our groupings does not affect the tenor of our results. For instance, we repeated our analysis changing groups to students applying for (1) kindergarten and 1st grade, (2) 2nd, 3rd, and 4th grade, (3) 5th and 6th grade, and (4) 7th and 8th grade.

[29] Time effects are allowed to vary by grade grouping.

ways. In column (3) we include students' most recent test score.[30] Because many students (e.g., those applying to kindergarten) do not have previous scores, we include a dummy variable for whether or not a student was tested prior to the lottery. In column (4) we make use of the fact that many students (e.g., those applying to upper elementary grades) have been tested more than once prior to their lottery. This additional information may help to better identify prior differences in achievement between lotteried-in and lotteried-out students. We use up to three previous test scores to predict a counterfactual post-lottery achievement level for each student, and we include this predicted score as a control variable, as well as a dummy variable for whether the predicted score is available.[31]

TOT estimates of charter school effects on math achievement are shown in table 6a.[32] For students who apply to kindergarten through grade five, we find positive and statistically significant improvements in math test scores of 6 to 7 percentile points. These estimates are quite stable across the regressions and they are the key results of this paper. Because the vast majority of students apply to the charter schools in kindergarten through the fifth grade,

[30] That is, we include the test score matching the subject of the dependent variable, math or reading.

[31] We generate predicted counterfactual achievement scores in the following manner. We first take all achievement test scores in the four years prior to students' lotteries. Then we regress students' test scores in the year prior to their lottery on the (up to three) test scores that preceed it. We allow the estimated effect of previous tests to vary with the combination of previous tests available. For example, the effect of a test from two years prior is allowed to differ between students who have only two years of prior tests and students who have three years of prior tests. We assume that the pre-lottery relationship between current and prior test scores is constant across lotteries, and therefore include all students for which we have data, not just those in our focus group. Using only focus group students does not alter our results to a large degree, but decreases considerably the predictive power of this regression. Using the coefficients from these regressions, we predict students' counterfactual post-lottery scores using test scores from the three years prior to their lottery.

[32] As noted above, full results are shown in the appendix. In general, the relations between student characteristics and student achievement are as we might expect. For example, students who have repeated grades, received special education or participated in the free lunch program have lower test scores than other students, though students who participated in bilingual education do not. White students have higher test scores than either Black or Hispanic students, and female students have higher reading test scores than males. However, these coefficients move towards zero when controls for student achievement are added to the regressions.

the 6 to 7 percentile point effect is what most students experience. Moreover, with each passing year, a greater share of the schools' students apply in grades kindergarten through five. This is simply because the schools are no longer new schools, seeking to fill empty places in upper grades.

In contrast to students who applied for a lower or middle elementary grade, estimated effects on math scores for students applying for sixth, seventh, or eighth grade vary substantial in sign and statistical significance as we add information about a student's pre-lottery characteristics and achievement. This is a bad sign: adding controls should not affect results much if randomization has produced groups that are similar on observable and unobservable traits. The result shown in the right-hand column, which uses the most pre-lottery information in our greatest attempt to balance the lotteried-in and lotteried-out groups, is statistically insignificant. Overall, the results for students who apply in grades six through eight suggest that there are too few applicants to these grades-of-entry to produce meaningful estimates. The only thing that we can conclude about these grades-of-entry is that the lotteries were too small to balance the lotteried-in and lotteried-out students.

TOT estimates of charter school effects on reading achievement are shown in table 6b. For students who apply to kindergarten through grade five, we find positive and statistically significant improvements in reading test scores of 5 to 6 percentile points. These estimates and their statistical significance are quite stable across the four specifications. We consider these estimates to be the other key results of this study, along with the charter school effects on math for students who apply in kindergarten through grade five. In addition, we note the fact that the charter school effects are very similar in math and reading, suggesting that the schools either emphasize the two subjects equally or that both types of achievement are

affected by the same factors in the charter schools' model of teaching.

For students applying to the sixth, seventh, or eighth grade, we find that the charter schools' effects were statistically insignificant. Moreover, adding controls for pre-lottery student characteristics and achievement makes the point estimates vary by a factor of ten. These findings are consistent with our findings for math achievement among students who applied in grades six through eight. Again, they suggest that the lotteries among students who applied to these later grades were too small to balance the lotteried-in and lotteried-out groups.

In short, we find positive and statistically significant charter school effects for students who applied to charter schools in kindergarten through the fifth grade. For this group, the charter school effect was about 6 percentage points in both reading and math. Our results for students who applied to grade six through eight are not useful because this group was evidently too small for the random lotteries to guarantee that the treatment and control groups were similar.

We would like to explore the dynamics of charter school effects. For instance, we would like to know whether students' gains are about the same every year or whether a student makes small gains in his first year in a charter school, followed by larger gains therefore. Unfortunately, we do not yet have sufficient data to identify the dynamics of charter schools' effects. This is an important issue for further research.

5.2 A Comparison of Lottery-Based Results, Value-Added Results, and the Results of Simple Comparisons

[to be added]

6 Conclusion

Our analysis of the Chicago International Charter Schools suggests that students who apply to the first through fifth graders have higher subsequent achievement than students who are lotteried-out and cannot therefore attend CICS. The effects of the charter schools cannot be estimated for the relatively rare students who apply to later grade because there are so few of them that the lotteries do not produce balanced groups of charter school and regular public school students. Of course, after a charter school is established, the vast majority of its students enter in the elementary grades, so the positive effects we find for applicants to kindergarten through fifth grade are our key results. They are the results one needs to assess the long run impact of the charter schools on their students.

Research on charter schools, like the schools themselves, is fairly new. We are not aware of any other studies that use random lotteries, but there a few studies that employ value-added analysis: Sass (2004), Bifulco and Ladd (2004), and Hanushek et al. (2002) perform value-added analysis on data from (respectively) Florida, North Carolina, and Texas. We have demonstrated that value-added analysis produces results that rely entirely upon (are local to) an unusual group of students who switch from regular public schools to charter schools late in their elementary school careers. (Nearly all the students in question switch between the fourth and fifth grades.) We have shown that estimates that rely on this peculiar group of students are very different from estimates that are representative of students who apply to charter schools. These differences probably stem from the fundamental tendency of parents to move children in midstream grades only if they are already struggling in school. Thus, we are skeptical of the idea that value-added analysis can produce results that are

generally useful or that can be extrapolated beyond the peculiar, rare set of students on which they depend. For this reason, we have little to say about the conclusions of the three studies in question, except that we see no point in attempting further to reconcile their results with ours. Evaluation of charter schools ought to rely on students who are typical of charter school applicants, not rely exclusively on students who are atypical. Randomization provides us with estimates that are inherently greatly superior to those based on value-added analysis.

The CICS schools in our analysis were founded at least two years prior to the lotteries for which we have data. Therefore, we examine the performance of students who have been exposed to charter schools that are young (four years old on average), but not brand-new start-ups. Because charter school policies would eventually produce a group of schools with some experience (not a perpetual stream of brand-new start ups), it is important to learn about how charter schools' effects evolve with their years of operation. Using data on newly founded CICS schools and future CICS lotteries, we hope to explore this question. In addition, we would like to understand the dynamics of a charter school's effect on a student: what is the effect after one year of charter experience, after two years, and so on? We intend to the pursue the dynamics of charter school effects in subsequent work as we obtain more years of data. Finally, we would like to see whether charter schools' effects are heterogeneous with respect to students' pre-existing characteristics, such as their race, poverty, and incoming achievement. This investigation will also require additional data.

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Figure 1: Distance from attendance area school to school, CICS students

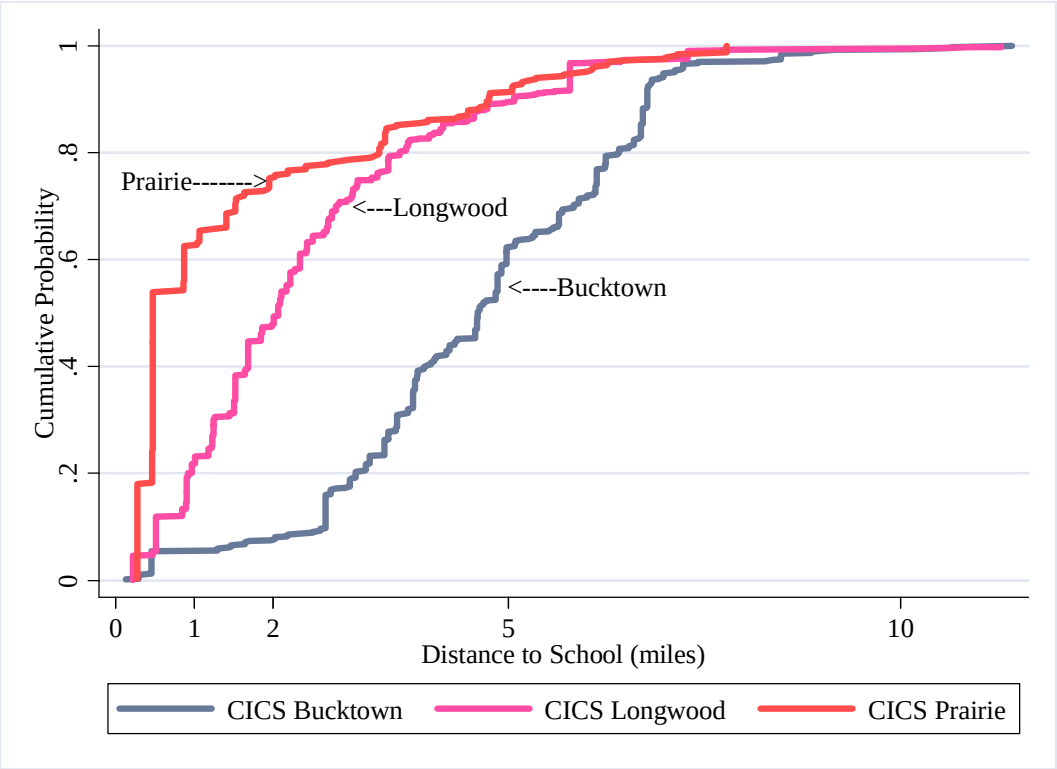

Figure 2: Distance from attendance area to school, CICS students and applicants in traditional and non-traditional CPS schools

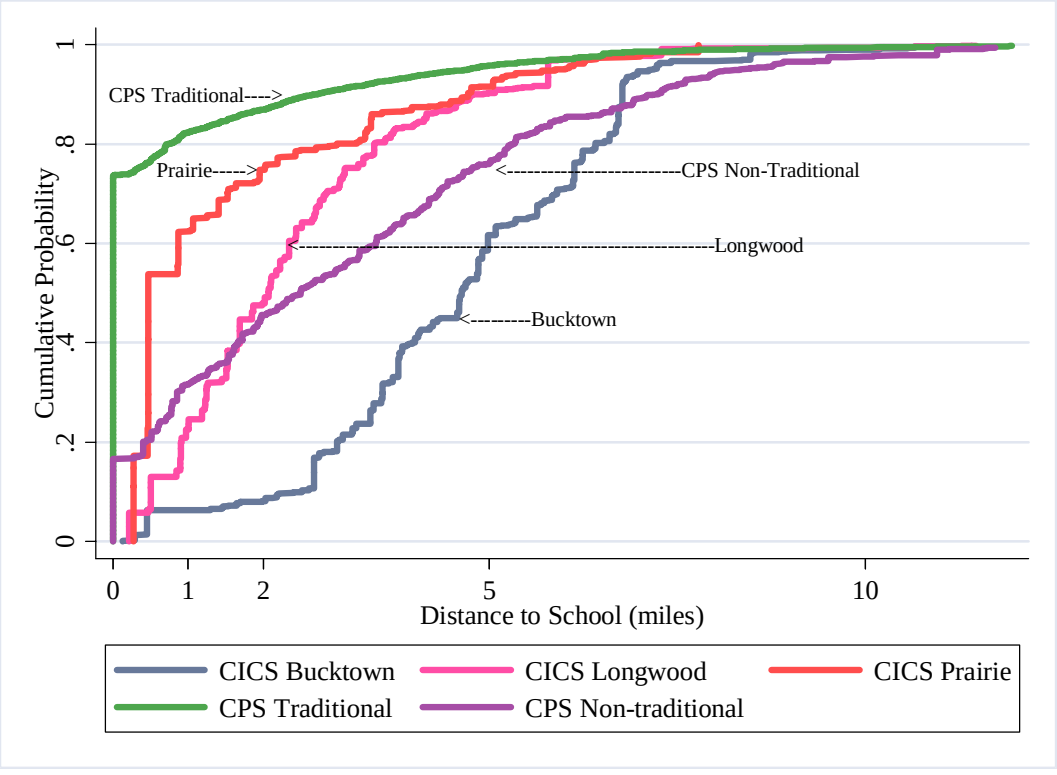

Table 1: Summary Statistics for CICS and other student populations, 2001-02

	CICS Bucktown	CICS Longwood	CICS Prairie	*All CPS*	*Other Charters*	*Private Schools*
Female	50.2%	53.0%	52.4%	*49.5%*	*55.4%*	*NA*
Black	33.3%	98.7%	54.4%	*51.3%*	*69.3%*	*29.2%*
Hispanic	53.3%	0.5%	44.4%	*35.8%*	*26.2%*	*21.6%*
Free/Reduced Lunch	76.4%	81.9%	84.0%	*78.0%*	*82.1%*	*NA*
Special Education	10.5%	12.2%	3.1%	*12.3%*	*9.5%*	*NA*
Bilingual Education	35.9%	0.5%	38.2%	*14.0%*	*2.3%*	*NA*
Number of Students	619	1,262	351	*437,418*	*3,715*	*87,135*

Note: CICS campus information comes from CPS data collected by the Consortium for Chicago School Research. Data on program participation is based on participation by students in CPS and before enrollment in CICS. All CPS and private school information come from the Common Core Data and the Private School Universe Survey, respectively. Both data sets are collected by the National Center for Educational Statistics. The Youth Connections Charter School is not included in the data on other charters; this is an umbrella organization for programs serving high school dropouts. Information on special education and bilingual education for charter schools comes from CPS Department of Research and Evaluation. In cells labeled "NA" the data is not available.

Table 2: CICS campus neighborhood characteristics

	Bucktown		Longwood		Prairie		*Chicago*	
	1990	2000	1990	2000	1990	2000	1990	2000
Black	2.2%	1.5%	96.8%	97.7%	55.6%	55.6%	*39.1%*	*36.8%*
Hispanic	51.5%	23.1%	0.3%	0.6%	40.0%	41.4%	*19.6%*	*26.0%*
% Not English at Home	53.6%	28.0%	2.9%	4.6%	42.6%	40.7%	*29.1%*	*35.5%*
% Spanish at Home	44.2%	17.1%	2.7%	3.1%	39.1%	39.6%	*17.4%*	*23.3%*
% HS Dropout (Age 25+)	41.6%	10.1%	27.7%	21.6%	54.5%	53.5%	*34.0%*	*28.2%*
% Families in Poverty	19.6%	7.0%	10.8%	5.9%	22.6%	31.4%	*18.3%*	*16.6%*

Note: Information taken from the 1990 and 2000 decennial census. CICS campus information is based on the census tract in which the school is located.

Table 3: Are Lotteried-in Students Differentially Likely to Atrit?

	(1)	(2)	(3)
Lotteried-in	0.919	0.918	0.900
	(0.142)	(0.142)	(0.165)
Student Has a Pre-Lottery Test Score		1.331	1.326
		(0.434)	(0.433)
Composite Score in Most Recent Pre-lottery Year		0.994	0.994
		(0.003)	(0.004)
Lotteried-in*Composite Score			1.001
			(0.006)
Observations	3942	3942	3942
Average Probability of Attrition	6.5%	6.5%	6.5%
Percentage of Students Who Atrit	13.8%	13.8%	13.8%

Coefficients are log-odds ratios for sample attrition, estimated with a cox proportional hazard model. Sample is limited to our focus group: students observed in CPS before and after their lottery who apply while in 1st to 7th grade, and students who apply below 1st grade who are observed after their lottery. Composite test score is the average of ITBS Math and Reading Scores. Standard errors in parentheses. All models are estimated with lottery fixed effects. * significant at 5%

Table 4: Are Lotteried-in and Lotteried-out Students Observably Similar?

	ITBS Math	ITBS Reading	Special Education	Free/Reduced Lunch	Bilingual Education
Focus Sample:					
Lotteried-in	0.577	0.899	-0.039	0.029	0.010
	(2.298)	(2.286)	(0.024)	(0.029)	(0.013)
Constant	39.980	40.185	0.129	0.783	0.044
	(0.890)*	(0.879)*	(0.010)*	(0.012)*	(0.005)*
Observations	1060	1058	1616	1616	1616
R-squared	0.05	0.05	0.05	0.13	0.37
Full Sample:					
Lotteried-in	-0.694	-1.190	0.025	-0.040	-0.002
	(1.965)	(1.950)	(0.020)	(0.026)	(0.012)
Constant	41.318	42.273	0.095	0.802	0.056
	(1.765)*	(1.754)*	(0.018)*	(0.023)*	(0.010)*
Observations	1415	1418	1988	1988	1988
R-squared	0.06	0.06	0.07	0.12	0.36

The Iowa Test of Basic Skills (ITBS) scores are available for grades 1-8. Our focus group consists of students observed in CPS before and after their lottery who apply while in 1st to 7th grade, and students who apply while below 1st grade and who are subsequently observed in CPS. The full sample includes students who applied while in 7th grade or below. Standard errors in parentheses. All regressions include lottery fixed effects. * significant at 5%

Table 4A: Charter School Applicants by Grade-of-Entry

		Difference in Probability of Being Female, Black, etc. for Lotteried-In and Lotteried-Out Students Who Applied to this Grade of Entry					
Grade	Percent Admitted to this Grade-of-Entry	Female	Black	Hispanic	Free or Reduced-Price Lunch	Special Education	Bilingual Education
K	33.10%	-0.03	-0.01	0.01	-0.01	0.01	-0.01
1	14.10%	-0.01	-0.01	0.02	-0.05	0.02	-0.01
2	10.30%	0.01	-0.01	0.01	0.02	-0.03	0.01
3	9.50%	0.04	0.00	-0.01	0.00	-0.01	-0.01
4	8.70%	0.05	0.00	0.01	0.01	-0.01	0.00
5	9.20%	0.07*	0.02	-0.03*	0.00	-0.03	-0.01
6	5.20%	0.05*	0.01	-0.02*	0.03	-0.02	0.00
7	3.40%	-0.09*	0.01	0.00	0.00	-0.06*	0.00
8	2.50%	0.15*	0.00	0.00	0.03	-0.07*	0.00
9	2.00%	-0.04	0.02*	-0.02*	0.04	-0.10*	-0.01
10	0.70%	-0.06*	insuff obs	insuff obs	0.06	-0.10*	insuff obs
11	0.60%	-0.17*	insuff obs	insuff obs	0.08	0.08*	insuff obs
12	0.60%	0.77*	-0.04	insuff obs	0.08	0.07	insuff obs

Note: An asterisk indicates that the difference between lotteried-in and lotteried-out students is statistically significantly different from zero. "Insuff. Obs." indicates that there were insufficient observations to compute the difference between lotteried-in and lotteried-out students in the relevant grade-of-entry, controlling for the school and year in which the lottery was held.

Table 5: Summary Statistics for Schools with 3 miles of CICS, 2001-02

	Schools Near Bucktown	*CICS Bucktown*	**Schools Near Longwood**	*CICS Longwood*	**Schools Near Prairie**	*CICS Prairie*
Black	**39.6%**	*33.3%*	**92.4%**	*98.7%*	**92.4%**	*54.4%*
Hispanic	**47.2%**	*53.3%*	**1.7%**	*0.5%*	**3.6%**	*44.4%*
Free/Reduced Lunch	**81.3%**	*76.4%*	**83.2%**	*81.9%*	**89.6%**	*84.0%*
Special Education	**14.3%**	*10.5%*	**14.6%**	*12.2%*	**13.5%**	*3.1%*
Bilingual Education	**16.3%**	*35.9%*	**0.3%**	*0.5%*	**1.1%**	*38.2%*
ITBS Math Percentile	**53.8%**	*46.4%*	**42.3%**	*37.0%*	**41.4%**	*42.5%*
ITBS Reading Percentile	**47.3%**	*46.0%*	**37.5%**	*39.1%*	**35.7%**	*39.8%*

Note: CICS campus information comes from CPS data collected by the Consortium for Chicago School Research. Information on gender, race, and free/reduced price lunch for students at nearby schools comes from the Common Core Data collected by the National Center for Educational Statistics. Information on special education and bilingual education for nearby schools comes from CPS Department of Research and Evaluation.

Table 6a: TOT Estimates of Math Effects by Grade Grouping

	Estimated Charter School Enrollment Effect			
Applied for KG to 5th Grade	6.38	6.18	6.68	6.40
	(2.37)	(2.79)	(3.02)	(3.18)
	[0.01]	[0.03]	[0.03]	[0.04]
Applied for 6th, 7th, or 8th Grade	-4.29	-4.35	-7.18	-4.14
	(1.90)	(2.80)	(3.68)	(3.80)
	[0.02]	[0.12]	[0.05]	[0.28]
Observations	3401	3401	3401	3401
R-squared	0.07	0.15	0.29	0.48
Pre-lottery Student Characteristics		√	√	√
Recent Pre-lottery Test Score			√	
Predicted Post-lottery Test Score				√
Lottery Fixed Effects	√	√	√	√
Grade Group*Year Fixed Effects	√	√	√	√

The dependent variable is percentile score on the Iowa Test of Basic Skills for tests taken subsequent to the students' admissions lotteries. Given are estimated coefficients on a dummy variable for being in a CICS charter, instrumented by a dummy variable for whether the student won admissions lottery. Standard errors (in parentheses) are clustered by student. P-values in brackets.

Table 6b: TOT Estimates of Reading Effects by Grade Grouping

	Estimated Charter School Enrollment Effect			
Applied for KG or 5th Grade	5.77	5.11	6.14	5.62
	(2.25)	(2.71)	(2.91)	(3.11)
	[0.01]	[0.06]	[0.04]	[0.07]
Applied for 6th, 7th, or 8th Grade	-2.69	-3.21	-3.03	-0.35
	(1.82)	(2.39)	(3.03)	(3.23)
	[0.14]	[0.18]	[0.32]	[0.91]
Observations	3407	3407	3407	3407
R-squared	0.09	0.18	0.29	0.45
Pre-lottery Student Characteristics		√	√	√
Recent Pre-lottery Test Score			√	
Predicted Post-lottery Test Score				√
Lottery Fixed Effects	√	√	√	√
Grade Group*Year Fixed Effects	√	√	√	√

The dependent variable is percentile score on the Iowa Test of Basic Skills for tests taken subsequent to the students' admissions lotteries. Given are estimated coefficients on a dummy variable for being in a CICS charter, instrumented by a dummy variable for whether the student won admissions lottery. Standard errors (in parentheses) are clustered by student. P-values in brackets.

Table A.1: TOT Charter School Effects on Math by Grade Grouping

Charter School Effect (Applied for KG or 1st Grade	4.62	5.68	5.44	5.27
	(4.58)	(4.36)	(4.39)	(4.45)
Additional Charter Effect for Students Who…				
Applied for 2nd or 3rd Grade	3.12	2.45	2.70	5.35
	(7.51)	(7.29)	(7.27)	(6.40)
Applied for 4th or 5th Grade	2.63	0.85	-0.18	-1.52
	(7.62)	(6.97)	(6.08)	(5.24)
Applied for 6th, 7th, or 8th Grade	-8.76	-12.86	-9.80	-9.57
	(5.95)	(5.73)*	(5.24)	(4.86)*
Free Lunch		-7.53	-5.49	-2.34
		(1.89)*	(1.72)*	(1.32)
Special Education		-20.88	-14.43	-8.12
		(2.04)*	(1.83)*	(1.54)*
Bilingual Education		6.36	4.66	4.37
		(4.50)	(4.35)	(3.69)
Repeated Grades		-18.85	-11.67	-8.55
		(3.15)*	(3.62)*	(2.89)*
White		11.56	14.67	9.18
		(7.49)	(7.11)*	(6.09)
Hispanic		-0.55	-0.27	-2.05
		(4.06)	(3.79)	(3.65)
Female		1.53	1.78	0.29
		(1.30)	(1.18)	(0.96)
Recent Test Score is Available			-22.66	
			(2.45)*	
Recent Test Score			0.54	
			(0.03)*	
Predicted Post-Lottery Score is Available				-23.93
				(3.05)*
Predicted Post-Lottery Score				0.76
				(0.02)*
Constant	49.35	48.53	48.04	49.44
	(5.01)*	(4.04)*	(4.86)*	(4.10)*
Observations	3401	3401	3401	3401
R-squared	0.07	0.15	0.29	0.48

The dependent variable is percentile score on the Iowa Test of Basic Skills. Standard errors (in parentheses) are clustered by student. All regressions include lottery fixed effects and grade group by year fixed effects. * significant at 5% or below;

Table A.2: TOT Charter School Effects on Reading by Grade

Charter School Effect (Applied for KG or 1st Grade	10.65	12.04	11.65	11.43
	(4.44)*	(4.13)*	(4.18)*	(4.26)*
Additional Charter Effect for Students Who…				
Applied for 2nd or 3rd Grade	-7.88	-8.54	-8.08	-8.35
	(7.39)	(6.98)	(7.01)	(5.90)
Applied for 4th or 5th Grade	-8.09	-10.12	-12.52	-9.57
	(7.42)	(6.72)	(5.89)*	(5.14)
Applied for 6th, 7th, or 8th Grade	-11.00	-15.12	-14.93	-14.17
	(5.50)*	(5.15)*	(4.84)*	(4.65)*
Free Lunch		-9.40	-7.45	-4.94
		(1.67)*	(1.57)*	(1.34)*
Special Education		-20.10	-13.46	-7.28
		(1.72)*	(1.66)*	(1.42)*
Bilingual Education		9.82	10.44	9.96
		(3.92)*	(3.67)*	(3.21)*
Repeated Grades		-14.53	-10.81	-3.37
		(2.56)*	(2.60)*	(1.91)
White		16.90	14.83	13.08
		(7.12)*	(7.54)*	(6.09)*
Hispanic		-4.74	-4.18	-4.68
		(3.58)	(3.37)	(3.10)
Female		3.80	2.78	1.30
		(1.22)*	(1.12)*	(0.93)
Recent Test Score is Available			-19.73	
			(2.19)*	
Recent Test Score			0.47	
			(0.03)*	
Predicted Post-Lottery Score is Available				-25.75
				(2.88)*
Predicted Post-Lottery Score				0.73
				(0.02)*
Constant	54.75	50.43	53.11	53.80
	(5.07)*	(3.96)*	(4.99)*	(5.02)*
Observations	3407	3407	3407	3407
R-squared	0.09	0.19	0.29	0.45

The dependent variable is percentile score on the Iowa Test of Basic Skills. Standard errors (in parentheses) are clustered by student. All regressions include lottery fixed effects and grade group by year fixed effects. * significant at 5% or below;

Table A.3: ITT Charter School Effects on Math by Grade Grouping

	Math		Reading	
Effect of Admission (Applied for KG or 1st Grade)	3.51	4.25	4.07	3.93
	(3.48)	(3.31)	(3.33)	(3.38)
Additional Effect of Admission...for Students Who...				
Applied for 2nd or 3rd Grade	2.54	2.04	2.23	4.30
	(5.84)	(5.67)	(5.65)	(4.92)
Applied for 4th or 5th Grade	1.74	0.46	-0.26	-1.23
	(5.69)	(5.21)	(4.59)	(3.94)
Applied for 6th, 7th, or 8th Grade	-6.93	-10.16	-7.65	-7.45
	(4.70)	(4.51)*	(4.07)	(3.73)*
Free Lunch		-7.31	-5.30	-2.09
		(1.89)*	(1.72)*	(1.33)
Special Education		-20.69	-14.23	-7.80
		(2.04)*	(1.82)*	(1.53)*
Bilingual Education		6.17	4.49	4.26
		(4.53)	(4.34)	(3.70)
Repeated Grades		-19.23	-12.04	-8.92
		(3.24)*	(3.68)*	(2.96)*
White		11.48	14.54	8.85
		(7.32)	(7.01)*	(6.10)
Hispanic		0.40	0.58	-1.16
		(4.04)	(3.74)	(3.60)
Female		1.47	1.72	0.21
		(1.30)	(1.18)	(0.96)
Recent Test Score is Available				-23.98
				(3.11)*
Recent Test Score				0.76
				(0.02)*
Predicted Post-Lottery Score is Available	41.48	48.50	46.07	38.99
	(2.30)*	(2.81)*	(2.93)*	(3.46)*
Predicted Post-Lottery Score			-22.55	
			(2.45)*	
Constant			0.54	
			(0.03)*	
Observations	3401	3401	3401	3401
R-squared	0.07	0.15	0.29	0.48

The dependent variable is percentile score on the Iowa Test of Basic Skills. Standard errors (in parentheses) are clustered by student. All regressions include lottery fixed effects and grade group by year fixed effects. * significant at 5% or below;

Table A.4: ITT Charter School Effects on Reading by Grade Grouping

	Math		Reading	
Effect of Admission (Applied for KG or 1st Grade)	8.09	9.08	8.79	8.62
	(3.38)*	(3.15)*	(3.18)*	(3.24)*
Additional Effect of Admission...for Students Who...				
Applied for 2nd or 3rd Grade	-5.92	-6.39	-6.04	-6.26
	(5.75)	(5.45)	(5.47)	(4.56)
Applied for 4th or 5th Grade	-6.23	-7.71	-9.45	-7.29
	(5.50)	(4.98)	(4.38)*	(3.86)
Applied for 6th, 7th, or 8th Grade	-8.38	-11.59	-11.45	-10.85
	(4.32)	(4.04)*	(3.75)*	(3.57)*
Free Lunch		-9.27	-7.31	-4.81
		(1.67)*	(1.57)*	(1.33)*
Special Education		-19.83	-13.10	-7.01
		(1.71)*	(1.64)*	(1.40)*
Bilingual Education		9.82	10.57	9.95
		(3.97)*	(3.70)*	(3.26)*
Repeated Grades		-14.70	-10.92	-3.51
		(2.59)*	(2.62)*	(1.93)
White		16.68	14.45	12.88
		(7.07)*	(7.53)	(6.00)*
Hispanic		-4.00	-3.56	-3.99
		(3.56)	(3.33)	(3.08)
Female		3.79	2.77	1.29
		(1.22)*	(1.12)*	(0.93)
Recent Test Score is Available				-25.82
				(2.91)*
Recent Test Score				0.73
				(0.02)*
Predicted Post-Lottery Score is Available	39.18	45.60	44.45	40.84
	(2.85)*	(3.11)*	(3.19)*	(3.68)*
Predicted Post-Lottery Score			-19.88	
			(2.19)*	
Constant			0.47	
			(0.03)*	
Observations	3407	3407	3407	3407
R-squared	0.09	0.18	0.29	0.45

The dependent variable is percentile score on the Iowa Test of Basic Skills. Standard errors (in parentheses) are clustered by student. All regressions include lottery fixed effects and grade group by year fixed effects. * significant at 5% or below;