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ABSTRACT

In this paper, we examine whether expanded access to sought-after schools can improve academic achievement. The setting we study is the "open enrollment" system in the Chicago Public Schools (CPS). We use lottery data to avoid the critical issue of non-random selection of students into schools. Our analysis sample includes nearly 450 lotteries for kindergarten and first grade slots at 32 popular schools in 2000 and 2001. We track students for up to five years and examine outcomes such as standardized test scores, grade retention and special education placement. Comparing lottery winners and losers, we find that lottery winners attend higher quality schools as measured by both the average achievement level of peers in the school as well as by value-added indicators of the school's contribution to student learning. Yet, we do not find that winning a lottery systematically confers any evident academic benefits. We explore several possible explanations for our findings, including the possibility that the typical student may be choosing schools for non-academic reasons (e.g., safety, proximity) and/or may experience benefits along dimensions we are unable to measure, but find little evidence in favor of such explanations. Moreover, we separately examine effects for a variety of demographic subgroups, and for students whose application behavior suggests a strong preference for academics, but again find no significant effects.

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I. Introduction

In 2004, roughly 13 million children in the United States were living below the poverty line. While social programs enacted since the Great Society have done a great deal to mitigate the immediate effects of poverty, education has become increasingly important in escaping poverty. The returns to education, and to skill, have increased dramatically over the past 40 years. Where it was once possible to earn a productive living with only the most rudimentary of academic skills, it is increasingly difficult to find a job that offers a living wage with anything less than a college degree (Murnane and Levy 1996).

At the same time, poor children attend schools that appear worse on a number of dimensions. In 2004, high-poverty districts received nearly $1,000 less per pupil in state and local revenues than low-poverty districts within the same state (Education Trust 2006). According to a recent analysis by *Education Week*, only 65 percent of teachers in high-poverty districts in California met the new federal guidelines for highly qualified teachers in 2004-05, compared to 81 percent in low-poverty districts in the state (Keller 2005). In New York, 81 percent of teachers in high-poverty districts were highly qualified, compared to almost 100 percent in low-poverty areas.[1]

These facts have spurred many initiatives to improve school quality for disadvantaged children. For example, over the past three decades, a number of states have passed school finance reforms to reduce disparities in revenues and to guarantee a minimum adequate level of spending for districts with difficult-to-educate student populations. The federal government also targets aid to schools with high poverty rates through the Title I program. Complementary policies have been introduced to ensure that available resources are used effectively, such as

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[1] See Clotfelter et al. (2007) for evidence on similar disparities across high and low poverty schools in North Carolina.

state charter school laws that allow alternative schools to compete with the traditional public schools and the federal *No Child Left Behind* school accountability legislation that requires states to adopt universal testing and minimum performance standards.

While it certainly seems plausible that attending a better school should improve student achievement, the existing evidence is far from clear. For example, many studies have shown that schools (and districts) with higher per pupil expenditures do not necessarily have higher achievement scores than schools (and districts) with less spending (Hanushek 1997). Similarly, recent evidence suggests that certified teachers are not substantially better at raising student performance than uncertified teachers (Boyd et al. 2005; Kane et al. 2006). More generally, evidence from a recent housing mobility experiment suggests that poor children whose families are given the opportunity to move to a lower poverty neighborhood do not show improvement on a variety of academic measures, even after living in their new neighborhood for up to seven years (Kling et al. 2007).

In addition, technical shortcomings of many of the studies in this literature make them difficult to interpret. The key difficulty is that families and students choose schools, often at the same time they choose where to live. This means that characteristics of the chosen school may signal something about the child, such as level of motivation or degree of family support, rather than serving solely as an independent measure of the quantity and quality of inputs applied to the student. Resources will appear effective if otherwise able students tend to attend high resource schools, while they will appear ineffective if more resources are applied to less able students, as is the case with many state and federal compensatory education programs.

Hence, the importance of school quality is an open question. This is not simply an academic issue. As a society, we are faced with a number of important tradeoffs between competing goods – a cleaner environment, better health care, international aid, etc. Of course, one can argue that we are a wealthy country and can afford to have higher quality education for poor children as well as these other important goods. However, there is then the question of what is the most effective way to achieve a better education for disadvantaged children. Given the multiple disadvantages faced by poor families and the multiplicity of support services, along with the uncertainty regarding the impact of school quality on student outcomes, simply attending a better school may not be the most effective intervention.

In this paper, we first review the existing evidence more completely and then provide new evidence on whether expanded access to sought-after schools can improve achievement. The setting we study is the "open enrollment" system in the Chicago Public Schools (CPS). Elementary students in Chicago can apply to gain access to public magnet schools and programs outside of their neighborhood school, but within the same school district. We use lottery data to avoid the critical issue of non-random selection of students into schools. All but a handful of academically advanced elementary schools use lotteries to allocate spots when oversubscribed, and we analyze nearly 450 lotteries for kindergarten and first grade slots at 32 popular schools in 2000 and 2001. Since those who randomly win and lose any given lottery will on average have the same characteristics, we can obtain unbiased estimates of the impact of gaining access to one of these schools through a straightforward comparison of subsequent mean outcomes across the two groups, as long as there is not selective attrition.

Comparing lottery winners and losers, we find that lottery winners attend higher quality schools as measured by both the average achievement level of peers in the school as well as by value-added indicators of the school's contribution to student learning. Yet, tracking students for up to five years following the application, we do not find that winning a lottery systematically confers any evident academic benefits. This suggests that the strong cross-sectional relationship that we observe between test score performance and school quality for the typical CPS elementary student is largely spurious, and highlights the importance of using a research design that compares likes to likes.

In the discussion section below, we explore several possible explanations for our findings, including the possibility that the typical student may be choosing schools for non-academic reasons (e.g., safety, proximity) and/or may experience benefits along dimensions we are unable to measure. Regardless of the explanation, the lack of a robust relationship between access to sought-after schools and achievement undermines the practical relevance of relying solely on enhanced school choice or higher inputs to remedy existing achievement gaps. Moreover, our cross-sectional results support this pessimistic view, demonstrating that much of the achievement gaps observed system-wide across race/ethnicity and income subgroups persist across students *within* schools.

The remainder of this chapter is structured as follows. Section II reviews the most relevant prior literature. Section III describes our data and empirical strategy. Section IV presents our results, and Section V discusses the implications of our findings for the construction of policies to benefit disadvantaged children.

II. Literature Review

This section begins by defining school quality and describing the potential channels through which school quality may influence student outcomes. We then provide a broad overview of the existing evidence on the strength and nature of the link between school quality and student outcomes. Rather than attempt to provide an exhaustive summary of findings, we emphasize the strengths and weaknesses of the variety of methods used.

Conceptual framework

School quality is a complex and multidimensional concept. There are many ways that one might define school quality and, thus, many ways in which school quality might influence student outcomes. One of the most straightforward definitions of school quality involves the financial and other "tangible" resources available to students and teachers, including things such as adequate textbooks, new computers, clean and spacious classrooms, small class sizes, and highly qualified teachers. The theoretical mechanisms through which these factors could influence student performance are straightforward, even if there continue to be fierce debates about the actual empirical relationship between such resources and outcomes.

Another common measure of school quality involves the students themselves. Schools with higher performing and/or more motivated peers are often considered higher quality due to the influence that one's peers and their families have on one's own outcomes. Peers are thought to influence individual outcomes in a number of ways, from providing good role models (e.g., friends that think studying is "cool") to changing the expectations of the teacher and thus the pace and content of classroom curriculum (e.g., to the extent that the teacher focuses attention on the median or modal student in the class, higher-achieving peers may translate into a more rigorous curriculum).

There is a third aspect of school quality – the quality of the match between the school and an individual student – that is not as frequently discussed in the traditional literature. The focus on "match quality" recognizes that students have different learning styles and/or needs, and that what might be beneficial for one student might be benign or detrimental to another student.

Indeed, this is one of the premises underlying many current school-choice programs, including charters and public school choice programs like the one analyzed in this chapter.

Given the multitude of channels through which various aspects of school quality could influence student outcomes, is it plausible to imagine that there might *not* be a relationship between school quality and student performance? While there is no way to be certain, several factors suggest that this relationship might not be particularly strong. First, there is considerable evidence on the primitive importance of the family, both in terms of genes and environment. Second, there are undoubtedly important interactions between home and school, some of which might serve to mitigate the importance of school quality. If, for example, parents view their financial or other support as, at least in part, a substitute for formal schooling, then we might expect parents to become more involved when their child is faced with an incompetent teacher or under-resourced school (e.g., they may seek out an after-school program or help the child more with their schoolwork at home). This type of behavior, while completely natural, will serve to undermine the relationship between school quality and student achievement. Third, unlike previous generations in this country and current generations in many developing countries, the vast majority of children in the U.S. today have what one might consider the bare essentials of an education. If school quality is most important at the very low and very high levels of quality, it may be that we simply cannot detect any important relationship in current U.S. data.

<u>Existing Evidence</u>

Researchers have long sought to examine how school quality influences child outcomes. This research falls into two broad categories. Perhaps the most common approach to this problem has been to measure the impact of observable school inputs such as spending per pupil, student-teacher ratios, and teacher credentials on student outcomes. Studies that analyze the impact of policies that dramatically reallocated resources, such as desegregation and school finance equalization, find a modest convergence in educational outcomes across previously advantaged and disadvantaged students (e.g., Card and Krueger 1996; Card and Payne 2002). But, the literature to date has yielded mixed results regarding the ability of policymakers to influence educational outcomes through less radical adjustments to the set of inputs to the educational process. See Hanushek (1997) for an overview of this literature, and Hanushek et al. (2005), Rockoff (2004) and Aaronson et al. (2007) for recent evidence on the impact of observable teacher characteristics on value-added. There is a related and vast literature that seeks to estimate the impact of peer characteristics on individual educational outcomes, also with mixed results (Hoxby 2000; Zimmerman 2003; Graham 2004; Lefgren 2004).

The analysis in this chapter is most closely related to the second strand of school quality research that has focused on private schools and other "choice" schools. Studies in this strand have sought to compare the performance of students in public vs. private schools, or traditional public schools vs. magnet or charter schools, as a way to say something about the benefits of attending a "choice" school which, by its nature, is presumably "better" along some important dimension. Unfortunately, estimating a causal relationship between access to sought-after schools and student outcomes has proven difficult. In the United States, observational studies of private schools (Coleman et al. 1982; Bryk et al. 1993) and magnet schools (Blank 1983; Gamoran 1996) find that students who attend these schools experience better educational outcomes. But these studies suffer from a potentially important source of bias driven by the fact that children who attend private or "non-traditional" public schools may differ from their peers in ways that are difficult to capture in a statistical analysis, but may nonetheless be quite important in determining life outcomes.

The difficulty in drawing conclusions from comparing outcomes for students served in different schooling settings is clearly evident in the public debate over charter schools. The American Federation of Teachers (Nelson et al. 2004) produced a study comparing the achievement of students in traditional and charter schools using national NEAP test score data, finding results unfavorable to charter schools. The study has been strongly criticized for controlling for so few of the differences in characteristics in the student populations, particularly given that many charter schools are explicitly designed to serve at-risk students. A concurrent Hoxby (2004) study compares charter and public school student performance in neighboring schools with similar racial compositions, and comes to a starkly different conclusion. However, the findings of this study have also been challenged because of the relative crudeness of the school matching procedure (Carnoy et al. 2005).

Researchers have attempted to address these selection concerns in several ways. One method is to use longitudinal student level data, so that the same student can be tracked in different settings. If a student's prior test score serves as a summary statistic for that student's potential, then any changes from the baseline as compared to similarly able students can be attributed to the schooling choice. More recently, researchers have recognized that students differ not only in their current level of achievement, but also in their learning trajectory. Even more problematic for school choice studies, students may choose to switch schools in response to unexpectedly good or bad outcomes. Although some studies rely exclusively on past outcome histories to control for student heterogeneity, most incorporate these data along with other strategies.

One alternative method, instrumental variables, attempts to identify differences in access to and take-up of school choice options that are arguably as good as randomly distributed across students with differing propensities to achieve and learn. A number of researchers have attempted to use this strategy to ascertain the causal impact of attending a Catholic school. In an early influential paper, for example, Evans and Schwab (1995) use affiliation with the Catholic church as an instrument for attending a Catholic school. The idea here is that students who are affiliated with the Catholic church are more likely than other students to attend Catholic schools, so that one can infer the effectiveness of Catholic schooling by comparing the educational outcomes of Catholic children with those of other children (regardless of whether the student in particular attends Catholic school). Of course, the key assumption underlying this strategy is that Catholic children do not differ from other children in any way that (a) researchers cannot control for in their model and (b) will influence educational outcomes through channels other than attending a Catholic school. To support this assumption, Evans and Schwab (1995) document that Catholics are very close to the national average on a variety of socio-economic indicators. However, as others have noted, Catholics may well differ from others in less easily measurable ways that could still have an important impact on schooling outcomes (Neal 1997; Altonji et al. 2005).

One straightforward example pointed out by Neal (1997) is that students who attend Catholic schools might be more likely to self-report that they are affiliated with the Catholic church, regardless of their families' religious affiliations, which would introduce a mechanical correlation that could bias the results. Instead, Neal (1997) uses a student's proximity to Catholic schools as an instrument for attending this type of school. Insofar as students who live near Catholic schools are more likely to attend them, this is a plausible instrument. The assumption of this approach, however, is that a family's residential location – specifically whether it is close to a Catholic school or not – is not associated with any unmeasured family characteristics that might influence a student's outcomes independent of the type of school the student attends. This assumption would be violated not only if neighborhoods with Catholic schools tend to be somewhat wealthier, for example, than other neighborhoods, but also if such neighborhoods are different in less tangible ways such as having a greater sense of community (or what is often referred to in the sociology literature as social capital). Given the difficulty of finding a valid instrument, it is perhaps not surprising that these studies have found mixed effects, with some showing benefits (Evans and Schwab 1995) and others showing little or no effect (Sander 1996; Neal 1997).

More recently, there have been a series of studies that exploit randomized lotteries. The Milwaukee voucher program, offering vouchers to a limited number of low-income students to attend one of three private nonsectarian schools in the district, is the most prominent of these. Although in theory randomization provides an ideal context for evaluating the benefits of expanding students' choice sets, in the Milwaukee case less than half of the unsuccessful applicants returned to the public schools and those who did return were from less educated, lower income families (Witte 1997). As described in greater detail below, this type of selective attrition can seriously bias any statistical analysis of student outcomes. It is therefore not surprising that analyses of the Milwaukee program obtain sharply conflicting estimates of the impact on achievement depending upon the assumptions made to deal with the attrition of lottery losers from the sample (Witte et al. 1995; Greene et al. 1997; Witte 1997; Rouse 1998).

Evidence from other small-scale school choice experiments in the U.S. is similarly mixed. For example, Peterson et al. (1998) and Howell and Peterson (2002) find that the opportunity to attend a private school modestly increases student achievement for low-achieving

African-American students in New York City, Dayton and Washington, DC. A reanalysis of the New York City experiment by Krueger and Zhu (2003), however, suggests that even claims of modest benefits may be overstated.

Our own prior work examining the impact of attending magnet high schools in Chicago (Cullen et al. 2006) is part of the growing set of studies relying on explicit randomization. A comparison between lottery winners and losers reveals that students who win attend better high schools along a number of dimensions, including higher peer achievement and attainment levels. Nonetheless, we find little evidence that winning a lottery provides any systematic benefit across a wide variety of traditional academic measures. Lottery winners do, however, experience improvements on a subset of non-traditional outcome measures, such as self-reported disciplinary incidents and arrest rates.

Recent work examining public school choice in the Charlotte-Mecklenburg School District (CMSD) highlights the importance of accounting for heterogeneity in treatment effects. Hastings et al. (2006) measure the impact of attending one's "first choice" school by comparing outcomes of lottery winners and losers. And, like Cullen et al. (2006), they find that winning the lottery (and, thus, attending a desired school) has, on average, no impact on a student's academic performance, but does seem to moderate at least some non-academic outcomes.

The primary innovation of the analysis is that the authors use information from parental rankings of up to three most-preferred schools on school choice application forms to infer the weight that each family places on academics. For example, parents that passed up nearby schools and chose a high-achieving school farther from their house were assumed to place a high value on academics. When the authors incorporate this information into their analysis, they find that those students whose parents place a high weight on academics experience significant test score gains from attending their first-choice school, while those whose parents place little value on academics actually experience test score declines from attending their desired school. Moreover, the gains do not seem to be driven by differences in the likelihood that winners end up attending a school with higher test scores, but rather appear to be attributable to improvements in idiosyncratic match quality. If in fact true, the policy implications are unclear, since the schools that deliver achievement gains for the subset of highly motivated parents and students do not seem to confer gains more generally.[2]

In this paper, we further explore the impact of attending a choice school, considering elementary school students in Chicago Public Schools. An important limitation of our high school study is that the results may not generalize to younger students. It may be that high school is too late for students to benefit from improvements in their schooling environment, so that the option to attend a magnet school may have a stronger impact on students at younger ages (Heckman 2007; Heckman and Masterov 2007). We also attempt – within the constraints of the available data – to test for the presence of heterogeneous effects by preference for academics following the lead of Hastings et al. (2006).

III. Data and Empirical Strategy

This section describes the data and methodology for our analysis of elementary school students in Chicago. We start by describing the school system and its choice program, and then explain how we use the lottery data to estimate the academic return to attending a better school.

[2] While the study is carefully done, there are still reasons to question the validity of the findings. It seems likely that whether high-preference students ultimately enroll in a CMSD school will be more sensitive to whether they win or not, so that the degree of attrition could be directly related to measured preferences and potentially generate the observed pattern of findings. Below, we show evidence that differential attrition does vary systematically across similar sample splits in the Chicago setting.

Background on the Chicago Public Schools

Over 400,000 students are enrolled in the Chicago Public Schools (CPS) in grades K-12. As in most urban districts, students in the system are disproportionately minority (more than two-thirds) and poverty rates are well above those for the nation as a whole. Given the high rates of disadvantage and poor overall performance relative to national norms, our analysis provides evidence on the net benefits of providing choice to students with otherwise limited opportunities.

CPS has one of the most extensive school choice programs available.[3] At the elementary school level, each student is guaranteed admission to an assigned neighborhood school, but can also apply to any of at least 200 CPS magnet schools or regular schools with magnet programs. Indeed, more than a third of all elementary students in CPS in 2000 and 2001 elected to attend a school other than the school assigned.

In order to attend a school other than the assigned school, a student must submit an application in the Spring of the preceding year.[4] A student must reside within the school district, but does not need to be currently enrolled in CPS in order to submit an application. Moreover, the application process is extremely easy. Parents simply fill out a one-page form listing basic information such as their name and address, and the grade the student will be entering. They can either mail the form into the district office, or drop it off at their home school. There is no limit on the number of schools to which a student can apply. In most cases, if the number of applicants exceeds the number of available positions, randomized lotteries are used to determine the allocation of spots. For a limited number of selective programs admission is based on criteria such as test scores, and lotteries are not used.

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[3] School choice was first instituted in Chicago in response to a 1980 desegregation consent decree with the federal government. The goal of the consent decree was to create schools that roughly matched the racial composition of the school system. Since that time, the size and scope of school choice has expanded dramatically.

[4] Transportation is provided to students gaining access to elementary magnet schools (but not to magnet school programs housed in neighborhood schools) if they live more than 1.5 miles but less than 6 miles from the school.

For programs using lotteries, there are explicit rules governing the way in which the lotteries are conducted. Because of desegregation goals and variation in the number of available slots at different grade levels, lotteries are typically conducted separately for each gender-race-grade combination. Also, a particular school may house multiple magnet programs, each of which conducts separate lotteries. As a consequence, one school can potentially have a large number of lotteries each year.[5]

The Data

Working with CPS, we obtained access to detailed administrative data that provide us information on student enrollment and achievement for all students over a number of years. Moreover, unique CPS student identification numbers allow us to track students over time as they change schools or if they leave and then re-enter the school system.

For the purpose of this study, we obtained data on school choice applications submitted in Spring 2000 and 2001 for enrollment in the following Fall. The application data include the name, race, gender, date of birth, home address and grade of each applicant, as well as the program the student is applying to, whether that application was part of a lottery and, if so, whether the application was selected or not. In our prior work, we examined students applying to high school (Cullen et al. 2006). Here, we focus on students applying to kindergarten and first grade, which are the principal entry grades for elementary school. The subset of the applicants attending public school at the time of the application (32 percent) report their unique CPS identification number directly on the application, and this can be directly used to link students to

[5] There is a further layer of complexity with regard to lotteries, namely that schools also reserve a share of available seats and conduct special lotteries for siblings of current students ("sibling lotteries") and for students who live nearby ("proximity lotteries"). Because such lotteries are rarely oversubscribed, they do not provide useful variation for our empirical work.

the administrative records.[6] For the other applicants, we utilize a probabilistic matching technique to link applicants to subsequent administrative records using names, date of birth, gender and race/ethnicity.[7]

The full sample of applications for kindergarten and first grade openings includes 51,775 applications to 207 choice elementary schools. Only 10 of these schools are academically advanced schools that have selective test-based admissions policies. While nearly one in every five applications is to these schools, less than one percent of elementary school students and six percent of applicants are served by these schools. All other schools assign slots by randomized lotteries if oversubscribed.

Given our research design – which involves comparing students who won a lottery with their peers who lost the same lottery – our analysis is necessarily limited to the set of lotteries where there were at least some winners and losers. Among applications to lottery schools, 50.2 percent were to lotteries with both winners and losers, 42.0 percent were to lotteries with no winners and 7.8 percent were to lotteries with no losers. A lottery will not have any winners if the campus is unable to accept applications to a specific grade due to overcrowding. Since we cannot estimate any treatment effects, we exclude applications to both types of degenerate lotteries from our analysis.

Two factors drive differences in the availability of slots for applicants across lottery schools and, hence, determine whether a campus is included in our analysis or not. First, much of the variation in capacity is geographic, with space constraints pervasive in the booming neighborhoods in the Northwest, Southwest, and South regions of the city (NCBG 1999).

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[6] The fraction applying from inside CPS is, not surprisingly, lower for pre-kindergarten applicants to kindergarten slots (24.7 percent). For applicants to first grade slots, the fraction is 42.9 percent.
[7] The matching process works extremely well. We verify that we correctly identify nearly 95 percent of students with an existing CPS ID at the time of application.

Second, the availability of *any* slots for applicants at the entry grade levels is only an issue for neighborhood schools that house magnet programs, since these schools have to first accommodate students living in the attendance zone. The overcrowded choice schools are neighborhood schools concentrated in the congested regions that otherwise appear similar on observable dimensions (e.g., average achievement level) to the campuses included in our analysis, while the schools that hold uncontested lotteries are substantially lower-performing.

After excluding another 10.8 percent of applications to non-degenerate lotteries at schools with fewer than 100 lottery participants across the two cohorts, we are left with 15,403 applications from 7,469 students to 32 schools. The great majority of the applications (79.1 percent) are for kindergarten slots. Most of the schools (22) are magnet schools that accept students from throughout the district and organize the curriculum around a specific theme (e.g., math/science, humanities, fine arts, or world language). No students are assigned to these schools by default. The remaining schools do also serve neighborhood students, but enrollment is dominated by students from outside the neighborhood drawn to magnet programs housed at the schools. Overall, these lotteries are quite competitive, with the typical application having a 13.3 percent chance of being selected. Because a student can apply to multiple lotteries, roughly one out of every four students in the sample wins at least one lottery.

We examine student outcomes through the Spring of 2005, when those applying to kindergarten from our 2000 cohort will have progressed to fourth grade, and those applying to first grade will have progressed to fifth grade. Applicants from the 2001 cohort can be tracked for only four, rather than five, years subsequent to the application. We are able to track students as long as, and only if, they are enrolled in CPS. Among our outcome measures for applicants who attend CPS schools are indicators for whether the student is currently receiving special education services and whether the student has been retained (i.e., is repeating a grade).

The academic outcomes that we focus most on are achievement test scores. Students in CPS take the Iowa Test of Basic Skills (ITBS), which is a nationally-normed multiple-choice exam that measures student proficiency in reading comprehension and a variety of basic math skills. The tests are mandatory and universally administered to CPS elementary students starting in third grade. In our sample, more than 99 percent of students enrolled in these grades have valid test score data, including students who receive special education or bilingual education services. Schools have discretion over whether to administer the tests to first and second grade students (none do to kindergarteners), so that some elementary schools assess students using the ITBS exams while others use alternative assessments for which results are not automatically reported to the district. We observe ITBS scores for 79.8 percent of enrolled first grade students, and for 87.1 percent of second grade students. The choice to administer ITBS at these grade levels appears to be idiosyncratic, since the schools that choose to do not differ in systematic ways from the schools that do not.[8]

The reading and math tests are designed so that a student's scores across grades can be mapped to a rate of learning. The scale is set so that a score of 185 represents achievement of the typical student in the nation in third grade, and a score of 200 is the same for fourth grade. Typical national achievement gains steadily decline from 15 points for fourth graders to 11 points for eighth graders, reflecting the idea that younger students learn more than older ones. The major advantage of using these standard scores is that a one unit change represents the same amount of learning regardless of the location on the scale, which facilitates comparisons across students in different grades and at different points in time.

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[8] This is also supported by the findings in Table 6 that enrolled lottery winners are no more or less likely to have valid ITBS scores than enrolled lottery losers.

Empirical strategy

In theory, lottery-induced randomization provides a simple solution to the problem of endogenous sorting of students. Because lottery outcomes are randomly assigned, winners and losers of a particular lottery will be identical on average, in terms of unobservable as well as observable characteristics. Consequently, a simple difference of observed mean outcomes between students who win and lose the lottery provides a consistent estimate of the impact of winning the lottery.

In the presence of *J* independently conducted lotteries, we could in principle generate *J* different estimates δ_j that capture the marginal impact of being allowed admission to the school represented by lottery *j*:

$$\delta_j = E[Y_i \mid Win_{ij} = 1; Apply_{ij} = 1] - E[Y_i \mid Win_{ij} = 0; Apply_{ij} = 1] \quad (1)$$

where *Y* is some outcome measure for student *i*, Win_{ij} is a binary variable indicating whether the student won lottery *j*, and $Apply_{ij}$ is a binary variable equal to one if the student applied to the lottery and zero otherwise. Then, δ_j indicates whether winners are systematically higher or lower on the outcome *Y* than losers in the same lottery. Note that it is also legitimate to estimate separate treatment effects for subgroups of students, as long as the sample is split according to characteristics that are predetermined at the time of application.

While δ_j is clearly an unbiased estimate of the impact of winning this lottery, it is important to consider its interpretation. The parameter measures the impact of winning *conditional* on deciding to apply, which means that any findings may not generalize to non-applicants. Also, because not all winners choose to attend the lottery school, δ_j measures the impact of having the *option* to attend the lottery school, or the intention-to-treat (ITT) effect. One can also infer the treatment effect for actually attending the lottery school, called the treatment-on-the-treated (TOT) effect, by scaling the ITT effect by the increased likelihood of attending the school for winners. For example, if winners are 50 percentage points more likely to attend, then the impact of actually attending would be twice as large as the observed mean difference in outcomes between winners and losers.

In practice, the standard errors for particular lotteries and subgroups within lotteries in our data are too large to make such school-specific estimates informative. Therefore, we instead pool information across the lotteries:

$$Y_i = \delta(Win\_Lottery_{ia}) + \mathbf{\Gamma}(Lottery_a) + e_{ia}\,, \qquad (2)$$

where the subscripts *i* and *a* index students and applications, respectively. $Win\_Lottery_{ia}$ is a binary variable that indicates whether application *a* for student *i* was a lottery winner. $Lottery_a$ is a vector of fixed effects indicating the lottery to which the observation refers, and *e* is a stochastic error term. In this specification, the δ coefficient is simply a weighted average of the δ_j's for the various lotteries.[9]

The unit of analysis in this model is a student-application. Students will appear more than once in the data if they applied for multiple lotteries. Moreover, a student who won one lottery but lost another lottery will serve as a member of the "treatment group" in the first case,

[9] The weight for lottery *j* is equal to $\frac{N_j P_j (1-P_j)}{\sum_j N_j P_j (1-P_j)}$, where N_j is the number of students entered in lottery *j* and P_j is the proportion of students entered in lottery *j* who win the lottery. Holding the likelihood of winning constant, weights are proportional to the number of students in the lottery. The closer a lottery is to having half the applicants win, the more weight it receives.

and a member of the "control group" in the second case.[10] While this setup may seem odd, it builds on the logic of estimating separate lottery effects and does indeed produce consistent parameter estimates. For the intuition, recall that winners and losers in each lottery will be balanced along all observed *and* unobserved dimensions due to the randomization. While a certain fraction of winners in any given lottery may have applied to and won other lotteries, the same is true for losers in that lottery. Our estimates capture the impact of winning a lottery *conditional* on the set of other lotteries to which an individual applied to and may have won. What multiple applications do influence is the magnitude of the treatment effect (e.g., the change in the quality of the school attended) associated with winning any given lottery.

In addition to pooling applications across lotteries, we pool student outcome information across the years 2001 to 2005 to further increase precision. Rather than estimating equation (2) separately by year, we form a panel where the unit of observation becomes the application by year since the application. We then estimate ordinary least-squares regressions of the form:[11]

$$Y_{it} = \delta(Win\_Lottery_{ia}) + \mathbf{\Gamma}(Lottery_a) + \mathbf{X}_i\boldsymbol{\beta} + \mathbf{\Pi}(g_{it}) + e_{ita} \qquad (3)$$

The specifications include separate indicators for each cohort, initial application grade and current year combination (g_{it}), to absorb mean differences across students who applied in different years and to different grades and are observed in a different number of years since application. We also include a set of student demographic and home census tract characteristics, as well as variables measuring the number and types of applications submitted by the student, that are all predetermined at the time of application ($\mathbf{X}_i$). These covariates increase precision by absorbing residual variation. Since the lottery balances students along these dimensions, the

[10] On average, students in our analysis sample participate in two lotteries, and the typical winner has about a one in five chance of winning another lottery (as does the typical loser).

[11] For the binary dependent variables, we confirmed that the reported coefficients estimated from linear probability models are always quite similar to the mean marginal effects estimated from comparable Probit specifications.

results will not be sensitive to this conditioning unless there is selective attrition from the sample, under which circumstances these controls will then help to mitigate any bias. In order to account for correlation in outcomes for the same student across applications and years, we report robust standard errors that allow for clustering at the student level. This clustering ensures that we do not overstate the precision of our inferences by recognizing that observations from the same student do not provide as much independent information as observations from different students.[12]

IV. Results

In this section, we present the main results of our analysis. We begin by providing some basic statistics on the differences in student performance and school quality between more and less advantaged students in the Chicago Public Schools. We then explore the relationship between school quality and student outcomes in CPS using a common but naïve approach – namely, simple regression analysis that does not account for student self-selection into magnet schools. Third, we turn to our lottery sample in order better isolate the causal impact of attending a sought-after school. Before presenting our main findings, we show a set of results aimed at exploring the scope for differential attrition. In presenting the main results of our analysis, we pay careful attention to understanding the magnitude of the effects, the statistical power of the estimates and the distinction between ITT and TOT estimates.

A Preliminary Look at Differences in Student Outcomes and School Quality in Chicago

Table 1 presents some descriptive statistics to highlight the differences in school quality

[12] Where appropriate, we tested sensitivity to allowing for clustering at the level of the school attended instead and found quite similar levels of precision.

and academic performance between more and less advantaged children in CPS. It is worth noting that the differences revealed in these figures are likely an understatement of the actual differences since the most advantaged families in the Chicago metropolitan area do not attend CPS, with one in every five elementary students opting instead for private schools. The table compares students along three dimensions of socioeconomic status: race/ethnicity, individual poverty status and neighborhood poverty status. The statistics are based on the sample of all third grade students enrolled in CPS in 2004 and 2005. These are the same years that the majority of our applicants, those applying for kindergarten slots, are enrolled in third grade. Each column presents means for the sub-sample indicated by the column heading.

The top panel documents dramatic differences in performance between advantaged and disadvantaged students. The average standard reading and math scores of White third grade students in CPS are both 194, which are substantially above the overall national averages of 185. However, Black and Hispanic children in Chicago score roughly 20 points lower than White children, implying a deficit of more than one year of learning. Similar differences are apparent when low-income students who are eligible for free or reduced-price lunch[13] and/or live in high poverty neighborhoods are compared with their more advantaged peers.

The bottom panel reveals equally striking differences in school quality.[14] White children are nearly six times more likely to attend academically advanced schools and nearly twice as likely to attend magnet schools relative to Black and Hispanic children. Similarly, students who

[13] Eligibility for the federally assisted meal programs operated in schools is a useful indicator of household income. Students are eligible for free (reduced-price) lunch if income is below 130 (185) percent of the relevant federal poverty threshold given the household size. The fraction eligible for meal assistance is a measure of student disadvantage commonly factored into federal and state funding allocation rules.

[14] Natural dimensions of school quality that we do not consider include financial resources and teacher quality. In CPS, funding is allocated largely by formula whereby schools with larger populations of poor, special education and language minority students receive compensatory funding, making it difficult to interpret higher levels of expenditures as a signal of quality. Some information on teacher characteristics by school is available, but we were unable to find a measure that both varied across schools and had an unambiguous association with the quality of instruction.

are not eligible for free or reduced-price lunch attend schools where mean third grade test scores are roughly 10 points – or two-thirds of a year's worth of learning – higher than eligible students.[15] Of course, this may simply reflect the fact that more advantaged students have higher initial ability levels than disadvantaged students. In order to explore the contribution of the school itself, we calculated a crude value-added measure for each school equal to the average deviation of students' fourth grade reading and math standard scores from expected, given students' prior scores and demographic characteristics.[16] While still not perfect, this measure should come a great deal closer to capturing school quality than simply the level of achievement. Yet the difference in school value-added across demographic groups is, if anything, even more striking than the differences in achievement levels. Black and Hispanic children in CPS, for example, attend schools with value-added scores close to zero whereas White children attend schools with value-added scores of roughly 0.7, indicating that students at these schools improve nearly one point each year relative to similar CPS students at less effective schools.

The statistics presented in Table 1 paint a portrait of a school system with highly unequal outcomes across demographic groups, and provide some evidence that children from more advantaged backgrounds attend higher quality schools. This does not prove that the differences in school quality are responsible for the differences in student outcomes, however. Indeed, this type of correlation is exactly what one would expect if children from more advantaged families have a greater inclination and/or ability to find a good school for their children and also provide the type of home support that fosters high academic achievement.

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[15] This score is the average composite reading and math standard score for third grade students in the 2000 and 2001 cohorts at the school. This is the group of students attending when our sample was in the process of applying.

[16] Specifically, using all fourth graders in 2000 and 2001, we separately regressed reading and math standard scores on demographic variables (race/ethnicity, gender, age and free/reduced-price lunch eligibility) and 20 indicators each for location of third grade reading and of third grade math scores by 5 percentile point ranges of the third grade test score distribution. The control variables explain approximately 60 and 70 percent of the variation in fourth grade reading and math scores, respectively. We then predict the residuals, and average these residuals across students by campus.

Table 2 explores the correlation between school quality and student achievement more closely. Using the same sample (i.e., all third grade students enrolled in CPS in 2004 and 2005), we estimate a series of regressions where the dependent variable is the average of the student's third grade reading and math standard scores. Each column presents the results from a separate regression, with the difference being the specific set of controls included. In all cases, the control set includes student demographic and neighborhood characteristics as detailed in the notes to the table. Our goal is to see to what extent it appears that different levels of school quality can help to explain outcome inequities across student groups, under the assumption that students who choose to attend better schools would otherwise have similar outcomes to other students. These results provide a benchmark for comparison to our later results.

The specification in column 1 shows the relationship between student achievement and student race and poverty status with no school-level controls. We see that Black and Hispanic children score 7-8 points lower than otherwise similar White children, and students eligible for free or reduced-price lunch score roughly 9 points lower than otherwise similar ineligible students. These differences are large relative to the scale of the scores, as every 15 points represents the amount of learning a typical student can expect in a year.

In column 2, we control for both the school mean achievement level as well as the school value-added measure, both of which are calculated for earlier cohorts of students in the school so that they are not "mechanically" related to the performance of students in our analysis sample. We see a significant positive relationship between both school quality measures and student outcomes. For example, the coefficient of 0.58 on mean achievement level indicates that students who attend schools where prior students scored 10 points higher score, on average, 5.8 points higher themselves. Perhaps more interestingly, the coefficients on student race and poverty status drop noticeably when these school quality measures are included. Even conditional on these measures of the quality of the school attended, however, poor and minority children substantially underperform their peers, scoring 5-6 points lower.

These two variables may well miss many important aspects of school quality, however. For this reason, the specification in column 3 adds separate indicators for each school to the control set, so that the estimates shown here come from a comparison of students within the same schools. While there may still be some within-school differences in school quality across race or poverty status (e.g., special enrichment programs for higher-achieving students in the school, or ability tracking that places more advantaged students with better teachers), this approach will account for any difficult-to-measure school-level quality factors, such as the ability of the principal or the level of parent/community involvement. We continue to find that poor and minority children score 5-7 points lower than their peers in the *same* school. This comparison provides a useful bound on the potential impact of school quality. These results tell us that even if we attribute as great a role as possible to schools by ignoring that able students are likely to choose better schools, completely equalizing school quality would reduce the achievement gap by less than one-fifth to one-third.

Why are the estimates of the impact of school quality likely to be overstated in these regressions? The primary concern is that it does not account for unobservable factors such as student motivation or family support that might be correlated with school quality and student performance. While it appears that better schools lead to better outcomes, it may simply be that better students attend these schools, and would perform well regardless—and vice versa for worse students. The specifications in columns 4 and 5 introduce proxies for motivation and ability to test how these moderate the results. Our proxies come from our application data. We know which of these students expressed an interest in attending a choice school for kindergarten. The results show that students who applied to any lottery-based or academically advanced (i.e., test-based) magnet school do in fact score between 3 and 7 points higher than other students. Notably, adding these two indicators reduces the point estimate for the effect of mean school peer achievement by more than 10 percent. Column 5, which includes school fixed effects, shows that applicants are outperforming students attending the same schools. Clearly, students who seek out better schools are not like other students.

Table 3 directly compares our sample of lottery participants and the general CPS population. Column 1 begins by displaying the summary statistics for the full sample of participants in our non-degenerate lottery sample. Columns 2 and 3 focus on students enrolled in pre-kindergarten at the time of the applications, comparing lottery participants (column 2) with students who were not observed submitting any applications to choice schools (column 3). The first point to note is that lottery participants tend to be relatively more advantaged than other CPS students on a variety of dimensions. Participants are disproportionately White and Asian relative to the broader CPS population, and they live in neighborhoods with lower poverty rates. As a way to quantify the implications of these differences for achievement, we predicted third grade scores for these students as a function of student and neighborhood background characteristics.[17] Given the differences in these, lottery participants would be predicted to score more than 11 points higher on average on future reading and math exams than non-applicants.

The bottom panel of Table 3 demonstrates that lottery participants and non-applicants have access to neighborhood schools of somewhat unequal quality. The neighborhood elementary schools for lottery participants have lower proportions of students who receive free

[17] Specifically, we regressed third grade reading and math standard scores (separately) on the background characteristics that are available for all applicants and enrolled students: race/ethnicity, gender, and the set of home tract variables detailed in the notes to Table 2. We then averaged the two predicted values for each student.

or reduced-price lunch, and higher achievement levels and value-added. Since lottery participants are generally applying to higher-ranking schools than their neighborhood schools, differential take-up of school choice will tend to lead to larger differences in the characteristics of the schools actually attended than those that are observed for the default school.

Given these differences, it is reasonable to ask whether the relationship between student demographics, school quality and student performance operate differently within our sample of lottery participants relative to the general CPS population. It is possible, for example, that lottery participants come from supportive family environments that mitigate the importance of school quality for these students. To explore this possibility, columns 6-8 of Table 2 reproduce our cross-sectional regressions of student performance on student characteristics and school quality for our analysis sample. The results in column 6 indicate that race and poverty gaps are even larger among our lottery sample. The estimated effect of school quality in column 7 suggests that peer ability continues to be correlated with individual performance in the analysis sample.[18]

In summary, it appears that lottery participants differ from other CPS students in many important and readily observable ways, such as race and poverty status. Moreover, the results in columns 4-5 of Table 2 indicate that lottery participants differ along other unobservable dimensions that exert an additional influence on their academic performance. It is precisely for this reason that lottery-induced randomization is likely to be important for drawing conclusions about the causal impact of school quality on the students in our sample. We now turn to this task.

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[18] Mean value-added is no longer statistically significantly related to own achievement, though the point estimate is still sizeable. The loss of precision is attributable to a correlation between the two school quality measures that is twice as strong in the analysis sample.

Using Lotteries to Estimate the Causal Impact of School Quality on Student Outcomes

The Lottery Schools

Students in our analysis sample applied to at least one of 32 schools that admitted students through a competitive lottery. Table 4 illustrates that these schools vary widely in terms of quality. In column 1, for example, we see that the average third grade test scores in these schools ranged from 206 in LaSalle Language Academy to 176 in Ericson Scholastic Academy. This 30-point difference is equivalent to two entire years worth of learning. In other words, the average third grade student in LaSalle is roughly two years ahead of the average student at Ericson. In the majority of the lottery schools, however, students scored above the national average (i.e., 185 points), a considerable feat considering the high levels of poverty in CPS and that none of these schools accept students on the basis of academic ability. Column 2 shows that students at most of these schools are learning at a faster rate than otherwise similar students in CPS, since the value-added measures are generally positive. Finally, Columns 3 and 4 report measures reflecting the popularity of the schools. Column 3 shows the fraction of applicants who were rejected, which captures the competitiveness of the lottery. Column 4 shows the fraction of lottery winners who actually choose to enroll in the school when given the opportunity.

There is substantial variation across schools along all of these dimensions. Schools with high mean achievement tend to be popular with students, as measured by either the competitiveness of the lotteries or the take-up rates of lottery winners (the correlation between columns 1 and 3 is .56 and between columns 1 and 4 is .64). Notably, the schools that we identify as high value-added are somewhat less popular schools (the correlations between our value-added measure and the acceptance and take-up rates are both .45). In terms of the number of lottery participants, the high-achieving schools are overrepresented.

Empirical Concern #1: Valid Randomization

The key to our research design rests on the assumption that admission to our sample of schools was determined randomly. CPS officials indicate that the lotteries used to determine admission were conducted using a computer algorithm that generated random numbers. However, given the importance of this issue, we confirm that the randomization indeed occurred. If the lotteries were conducted properly, then one would predict that the winners and losers of a given lottery will be, on average, perfectly balanced on all predetermined characteristics. We test this by estimating the cross-sectional equation (2) for a series of student demographic and neighborhood variables that are predetermined at the time of the lottery, as well as variables capturing the number and types of applications submitted by the student. In results not presented here (but available from the authors upon request), we find that the number of statistically significant differences between winners and losers are no more than would be expected by chance if the lotteries were, in fact, truly random. Hence, we conclude that the lotteries were conducted properly.

Empirical Concern #2: Selective Attrition

Even if the lotteries are valid randomizations, however, the fact that not all applicants end up enrolling in CPS may bias our findings since we only observe subsequent student outcomes for CPS students. Of course, if the students who choose to remain in CPS are identical to their peers who choose to leave CPS, then the attrition of some students from our sample will not influence the results of our analysis. On the other hand, if this attrition is non-random, then it could bias our findings. For example, imagine that high-achieving students' decisions about whether to remain in a city public school are more sensitive to whether they win the lottery at a choice school or not. That is, they are more likely to stay if they win, and to leave if they lose. In this case, the sample of winners we observe in subsequent years will contain a disproportionate share of these "good" students relative to the sample of lottery losers – even if the full sample of winners and losers were identical at the time of the lottery. This type of attrition would lead us to overstate the benefits of winning a lottery and attending a higher quality school. Conversely, if students who leave CPS when they lose the lottery tend to be the lower-achieving students (which might be the case if families are more concerned about prospects for an "at-risk" child in a traditional public school), then our results would tend to understate any benefits of attending a higher quality school.

We provide evidence on the degree of overall attrition and test for whether it is selective in Table 5. The first row shows results from estimating equation (3) with an indicator for enrollment in CPS as the dependent variable, for the overall sample and for various student subgroups. We do not condition on observable characteristics for these diagnostic analyses – the only variables included as controls are the set of lottery indicators. Recall that we pool observations across multiple years, so that this outcome indicates whether a student is enrolled in CPS in each year following the lottery. As a baseline, it is useful to consider the fraction of lottery losers subsequently enrolled in CPS. The fraction .527 in square brackets in the first column indicates that the typical lottery loser is enrolled in 52.7 percent of post-lottery years. The fact that relatively few students end up enrolling reflects the selective nature of the sample. The students who applied for choice schools indicated a willingness to look beyond their neighborhood school.

The estimated coefficients on the indicator for being selected in the lottery for that application reveals that, while many students in our sample choose to attend school outside of CPS, the difference in enrollment between winners and losers is modest. For example, the coefficient of .050 in column 1 indicates that students who win a lottery are five percentage points more likely to be enrolled in CPS in subsequent years than their counterparts in the same lottery who are not selected. Given the baseline enrollment rate of 52.7 percent, we infer that selection in the lottery increases the likelihood of enrolling by roughly nine percent.

While the extent of differential attrition is modest, if the students who left CPS because they did not win a lottery were substantially different from their counterparts who won a lottery and therefore remained, we would be concerned about the validity of our estimates. Row 2 explores this concern by examining the sub-sample of winners and losers enrolled in CPS. Rather than examine a series of background characteristics separately, we use a summary measure – namely, the student's predicted third grade achievement score (reading and math combined). We predict this on the basis of a linear regression that includes a host of student and neighborhood characteristics at the time of application, as well as indicators for the number of applications and acceptances to test-based schools observed for the student.

If the lottery losers who left were systematically different than their winning counterparts who remained, we would expect there to be a significant relationship between selection in the lottery and student characteristics among enrolled students. However, as we see in row 2, there is no such relationship across the sub-samples we examine, other than for low-income students, where winners appear to be slightly negatively selected. Moreover, the estimates are small in magnitude and precisely estimated across the board. For example, the estimate of -0.15 in column 1 is tiny relative to the standard deviation of the predicted third grade score measure (10.75) shown in brackets. This provides some evidence that the differential attrition we observe overall and across demographic groups will not skew our baseline estimates.[19] However, we reconsider the issue of selective attrition below when we examine whether the effect of winning a lottery varies according to the induced change in school quality or the family's revealed preference for academics, since any differences between winners and losers could be exaggerated in these types of sample splits.

Baseline Estimates of the Effect of Winning a Lottery on Student Outcomes

Having established that lottery applicants differ in important observable and unobservable ways from non-applicants, the choice-school lotteries were indeed random and selective attrition is unlikely to be a substantial concern in our analysis, we now proceed to our main results, shown in Table 6. Each column refers to a different student subgroup and each row reports the results for a different outcome variable. Though we present results for various student subgroups, we focus our discussion on the overall results where the precision of our estimates is greatest. In each case, our estimates are based on ordinary least squares regressions of the type shown in equation (3), pooling across years since the application. Robust standard errors clustered by student are shown in parentheses, and the mean of the outcome variable for lottery losers is shown in square brackets.

We first present results that characterize the nature of the treatment for winners. How does winning affect their schooling? The top row of Table 6 shows the likelihood of attending the lottery school, conditional on enrolling in CPS. If all winners chose to attend the lottery

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[19] Of course, it is still possible that winners who were induced to remain in CPS are different in unobservable ways – e.g., they have more or less supportive families – which could lead to a bias in our findings.

school to which they applied and the losers were precluded from attending, we would expect to find a point estimate of 1, indicating that winning the lottery shifts the likelihood of attending the lottery school from 0 to 100 percent. That is not what we see. In our case, 8.2 percent of lottery losers end up enrolling in the school, likely moving off the waitlist. The estimate of 0.312 indicates that students who won the lottery were 31.2 percentage points more likely to attend that school, implying that winners take-up the option to attend approximately 39 percent of the time. So, while the attendance rate of winners is nearly five times higher among winners than losers, less than half of the students who win a lottery end up attending the school. This is not surprising since about one in five of the winners in any given lottery included in our analysis had successful applications to at least one other lottery-based school, and many also applied to the academically advanced programs with selective admissions. However, it is due to this slippage that any differences in mean outcomes between winners and losers have to be scaled up in order to be interpreted as impacts of attending the lottery school.

Rows 2 and 3 characterize the change in exposure to school quality that is induced by winning a lottery. For example, the estimates in row 2 for the overall sample indicate that students who won a lottery attend schools where students from prior cohorts scored roughly 2.3 points higher than at schools attended by lottery losers. Given a standard deviation of 12.1 points among the control group, this translates into a 20 percent increase in this measure of school quality. The results in row 3 indicate that winning a lottery also increases the quality of the school a child attends as measured in terms of value-added, and to a similar degree.

However, it is again important to realize that these estimates only reflect the effect of winning the lottery, not the effect of actually attending the lottery school. For example, in order to obtain an estimate of the effect of attending the lottery school for which the student gained admission on mean peer achievement, it is necessary to divide 2.3 by the estimate of the fraction of years spent at the lottery school shown in row 1. Doing so, one finds that students who actually attend the lottery school after winning a lottery experience peers who score roughly 7 points higher in reading and math (2.3/.312 ≈ 7.4 points, or more than 60 percent of the control group standard deviation) than they would have had they lost the lottery. This suggests that attending a sought-after school can substantially change a student's educational experience.

Now that we have established that winners are attending what appear to be better schools, the bottom panel of Table 6 evaluates whether this leads to better academic outcomes.[20] The outcome measures include an indicator for whether the student is repeating a grade (defined starting after the first year following the application), an indicator for whether the student currently receives special education services, an indicator for whether ITBS test scores from the Spring administration are available for the student, and the student's composite (reading and math) test score. Recall that we are estimating these results on a panel that includes either four or five years of outcome data for each student.

Focusing on the results for the full sample of students shown in column 1, we find little evidence that winning the lottery had any impact on student achievement. Despite the fact that winners, on average, attend schools with higher achievement levels and value-added measures, test scores of these students are virtually identical to their peers who lost the lottery. There is no indication that these students were any more or less likely to receive special education services, to be retained in grade or to take the standardized exams.

While the average student does not appear to benefit from winning a lottery, it is possible that the opportunity to attend a more desirable school has a greater impact for disadvantaged

[20] However, even without changes in observable school quality, winners should presumably benefit from better match quality. They have gained a schooling option they expressed an interest in having.

students. To explore this possibility, columns 2–8 of Table 6 present results separately for various subgroups. Looking at the subgroup analyses in columns 2-8, we see no evidence that winning a lottery had a discernible positive impact on test scores for any subgroup.

While these results suggest that attending a higher quality school does not improve academic achievement for young children, it is worth exploring the magnitude and precision of our estimates more carefully. To begin, recall that the estimates presented in Table 6 reflect the average difference between lottery winners and losers regardless of the school they end up attending. This "intention-to-treat" (ITT) estimate captures the effect of the *opportunity* to attend the school, where the treatment refers to attendance at the school. As above, in order to understand the effect of attending a lottery school, one needs to divide the outcome estimates by the figures shown in row 1. Doing so for the test score result, one gets $0.17/0.31 \approx 0.55$. We can calculate an approximate 95 percent confidence interval (ignoring the variability in the estimated attendance response) of attending the lottery school as $0.55 +/- (0.53/0.31)\times 1.96. = 0.55 +/- 3.35$. This means that attending a sought-after lottery school leads to a change in test performance of −2.8 to +3.9 points. Given the standard deviation of test scores in our control group is 19.6, we can rule out average increases in achievement from attending a lottery school (for all sample years following the application) of more than 20 percent of a standard deviation. Hence, we can rule out modest or large impacts, but cannot discount the possibility that attending a choice school has a small positive (or negative) impact on achievement.

It is also useful to compare these estimates to our earlier cross-sectional estimates of the impact of mean peer achievement on own achievement. The point estimate of 0.52 in column 4 of Table 2 indicates that a 1 point increase in mean peer achievement is associated with a half a point increase in the student's own third grade test score. The estimates in row 2 of Table 6 show that the typical winner attends a school with mean peer achievement 2.34 points higher than the typical lottery loser. Together, these imply that lottery winners should score 1.2 points (2.34×0.52, with an approximate 95 percent confidence interval of +/− 0.11) higher themselves. This is the upper bound of the ITT effect implied by our test score estimate of 0.17 (0.53) in Table 6. Hence, if one believes that there are no other channels through which attending a choice school would improve one's achievement, we cannot quite rule out the OLS estimates. However, to the extent that there are any other significant pathways through which choice impacts student outcomes (including the demonstrated independent effect of measured value-added), then we can infer the naïve OLS estimates are inflated.

Interaction Effects of Winning a Lottery on Student Outcomes

Though we can rule out sizeable academic benefits from winning the lottery on average, it is possible that the impact varies depending on the nature of the treatment and the reasons for choosing the application schools. We present results from specifications that allow for differential impacts along these lines in Table 7.

The first row of Table 7 explores whether our estimates vary by the number of years since the application. Recall that our sample includes information on student outcomes for up to five years following application. To the extent that the effects of attending a high quality school are cumulative, we would expect the benefit of attending a choice school to increase with time. To explore this possibility, we estimate the specification shown in equation (3), but include an interaction between being selected in the lottery and years since application (with the first year normalized to zero). Note that the main effect (years since application) is subsumed by other variables in our control set (i.e., the indicators for each cohort, initial application grade and current year combination).

The table shows results for four different outcomes. In order to examine whether attrition changed across years since the application, columns 1a and 1b show the main effect for being selected and the interaction with years since application for a model where the outcome measure is an indicator for whether the student was enrolled in CPS. The coefficient of -0.011 on the interaction between winning a lottery and the number of years since the application indicates that the *difference* in enrollment rates between lottery winners and losers *shrinks* over time. In the initial year after the application, lottery winners are estimated to be 6.9 percentage points more likely to be enrolled. Five years following the application, winners are only 2.5 percentage points ($6.9 - 4 \times 1.1$) more likely to be enrolled in CPS. This convergence between winners and losers is driven both by further exit from CPS of some lottery winners as well as later entry by lottery losers.

In order to examine whether selective attrition changed across years since the application, columns 2a and 2b show the main effect and interaction term for a model where the outcome measure is the student's predicted third grade test score. There is no evidence that selective attrition differed as time elapsed.

The results in columns 3a and 3b focus on the mean peer achievement in the school(s) that the student actually attends, which provides an indication of the "treatment" effect of winning a lottery. The coefficient estimate of -0.33 on the interaction term indicates that the "benefit" of winning a lottery, in terms of the quality of school attended, diminishes somewhat over time. This may be due to the fact that students who lost the lottery continue to seek opportunities to attend better schools, and so slowly "catch-up" with their peers who were initially lucky enough to win the lottery.

Columns 4a and 4b show the results for student achievement. Here we see no indication that the benefit from winning the lottery changed over time. The fact that achievement effects do not increase with longer "exposure" to the "treatment" (i.e., a longer period of time in the choice school) could be taken as further evidence that the link between schools and test scores is weak. On the other hand, the fact that the quality differential diminishes over time might serve to mitigate the greater length of exposure. In either case, these results speak to the potential importance of compensating behavior on the part of families.

To get more directly at any heterogeneity according to intensity of the treatment, row 2 presents a similar specification where the interaction term is the potential gain in mean peer achievement (and we add the main effect to the specification as well). This measure varies by student and application, and is defined as the difference between the peer achievement in the application school and the level of peer achievement the student is likely to experience if he or she does not win the lottery to the application school.[21] Not surprisingly, winners are increasingly more likely than losers to enroll as the potential gain increases, though it continues to appear that remaining winners and losers are not systematically different from one another. The change in school quality experienced by winners, by definition, increases with the potential gain, as documented in columns 3a and 3b. Yet, we see no interaction between potential gains and achievement effects in columns 4a and 4b. In other words, the effect of winning a lottery is no different for those students who sought out high-achieving schools compared with otherwise comparable students those who applied to schools with lower test scores. So, the apparent lack of benefits for the overall sample cannot simply be explained by gains for some and losses for others due to differences in what winning confers in terms of changes in the schooling

[21] The expected quality of a student's alternative options is inferred from the mean experienced by lottery losers from the same neighborhood (there are 77 community areas) who are predicted to be in the same quintile of third grade achievement.

environment.

As a final test, we consider the possibility that the overall null effect masks test score gains from the choices made by students expressly interested in academics, and test score losses for those more willing to trade-off distance or other school features against academic quality. This test is motivated by the finding of Hastings et al. (2006) that, among non-White students, those students whose parents exhibit the strongest preferences for academic quality benefit from the opportunity to attend a more-preferred choice school, while those seeking same-race settings are harmed (in terms of test score outcomes). Moreover, the apparent test score gains and losses appear to come from idiosyncratic match quality, rather than aspects of quality that are enjoyed by all students at a school.

Note that the potential gains interaction described will, to a large extent, capture parent preferences for academic quality since the measure incorporates the family's choice of application schools. However, this measure varies by application as well as student, and also incorporates information on the student's likely default option. To the extent that families who care strongly about academics make sure their children attend a high-achieving school regardless of the lottery outcome (e.g., through application to test-based school), the potential gain measure may understate the family's preference for academics. For these reasons, we create a more direct measure of parent preference for academics.

While the structure of the school choice program in Chicago does not allow us to calculate the same type of preference measure, we are able to create a similar indicator to explore this possibility in our sample. We estimate a regression of the following form:

$$Q_{ia} = \mathbf{X}_i \boldsymbol{\beta} + \boldsymbol{\Gamma}(Z_i) + e_{ia}, \qquad (4)$$

where Q_{ia} is the average combined (reading and math) test score in the school that student i

applied to in application a, $\mathbf{X}_i$ is a vector of student demographics and home tract characteristics, Z_i is a vector of community area fixed effects, and e is a stochastic error term. We then average the residuals across applications for each student and use this as a measure of the family's preference for academics. The intuition behind our measure is that the parents who apply to schools with the highest achievement levels – conditional on their observable demographics and the neighborhood in which they live, which captures the distance to various schooling options – place the greatest weight on academics. Note that it is likely that all of the families in our analysis sample place more weight on academics than the average CPS family since they have taken the step of applying for a choice school rather than simply attending their default neighborhood school, and we observe average improvements in observable school quality for winners (as do Hastings et al. 2006). However, there remains significant variation in revealed preferences among students in our lottery sample, and our measure has a standard deviation of 6 points.

The results in row 3 of Table 7 are based on a specification that adds an interaction between this academic preference measure and winning the lottery (as well as the main effect for the academic preference measure). In columns 1a and 1b, we see that there is a systematic relationship between preference for school quality and attrition. Specifically, the coefficient of 0.004 on the interaction term in column 1b indicates that students whose parents express a greater preference for academic quality are *more* likely to leave CPS if they lose the lottery. While the difference in enrollment rates between winners and losers increases with the parents' preference for academic quality, the results in columns 2a and 2b provide no evidence that the attrition is more selective (along observable dimensions) among high-preference parents. Of course, it is still possible that student attrition was selective along unobservable dimensions such as parental motivation or family support. The results in columns 3a and 3b show that the improvement in measured school quality induced by winning does not vary by preferences. This is somewhat surprising given that one would expect families with a stronger preference for academics to end up in higher-achieving schools if they win the lottery. On the other hand, this result is consistent with the findings in Hastings et al. (2006), and likely reflects the fact that a family's preferences as measured by the application behavior is strongly correlated with the student's next-best alternative. However, unlike the Hastings et al. (2006) analysis of Charlotte-Mecklenburg, the achievement results in columns 4a and 4b reveal no test score gains from winning a lottery in the Chicago setting, even among high-preference families.

V. Conclusions and Implications

The original analysis conducted in this chapter suggests that schools are a blunt instrument for improving the achievement of disadvantaged students. First, for elementary school students in CPS, we demonstrate that the gap in achievement across advantaged and disadvantaged students is two-thirds as great within schools as across schools. The great inequities that we observed in school quality across these groups, then, cannot explain the bulk of the differences in outcomes. Further, this surely overstates the role for schools. Part of the convergence in outcomes within schools as compared to across schools is due to the fact that minority and poor students who attend the same schools are similar in family background and other characteristics that are difficult to measure. These students would have more similar outcomes regardless of their shared schooling experience.

We then use lotteries to examine whether elementary school students who gain access to desirable schools do better. The great advantage is that randomly selected winners and losers are by definition exchangeable. Although students in our sample often take advantage of winning a lottery by attending that school, and on average lottery winners attend schools that are better on observable dimensions than the schools attended by lottery losers, we observe no systematic improvement in student performance among winners relative to losers. This finding is surprising since students who win contested lotteries would be expected to fare better because of access to better resources, better peers, or a program that better suits their learning needs for idiosyncratic reasons. The fact that these students do not appear to benefit further undermines the likelihood that changes in broad aspects of school quality will radically change students' fates.

There are several explanations for the lack of average positive effects. One possibility is that attending a choice school is a substitute for parental involvement. In prior work, however, we find only weak support for this hypothesis (Cullen et al. 2006). Another explanation is that students winning lotteries may have to travel much greater distances to school, which might interfere with academic success. However, in results not shown here, we find that the travel costs experienced by lottery winners appear to be quite small, and thus unlikely to explain our results.

The coexistence of intense competition for entry and little academic benefit to students winning the lotteries could indicate that parents are not well-informed about the education production function, and mistake higher school outputs for higher school value-added. Alternatively, parents and children might apply to magnet schools for predominantly non-academic reasons, in which case systematic academic gains would not be expected. Indeed, studies of school choice programs that attempt to ascertain parent preferences generally conclude that parents value factors such as convenience (i.e., distance from home to school) and the racial composition of the school at least as much as measures of academic quality (Glazerman1998;

Hastings et al. 2005). In the Chicago setting, however, we are unable to find evidence that either winning a lottery that induces a large change in mean peer achievement or when choosing with academic motivations in mind confers any greater benefits.

The type of school choice we analyze in this paper is particularly relevant to the current federal accountability mandate insofar as our analysis focuses on public schools in a large, disadvantaged urban district. This form of choice is the most common form of choice available to students in urban areas (NCES 1997), and it is likely to become even more prevalent under the recent federal education legislation *No Child Left Behind*. School districts that accept Title I funds must allow students at lagging schools to attend other schools in the district, giving preference to low achieving and low income students. We cautiously conclude that access to ‘better” schools is likely to be less effective than more targeted interventions.

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APPENDIX A

TABLE A1: DATA SOURCES

Data	Source	Construction
Academic Outcomes	CPS Board	Information on enrollment, special education placement and retention is from administrative records provided by the Board. Special education status covers a variety of disabilities ranging from mild learning disabilities to severe physical handicaps. Standardized test scores are from separate administrative test files provided by the Board. Students are tested in grades 1−8 on the Iowa Test of Basic Skills (ITBS), which is a nationally-normed standardized achievement exam. The scores that we use are developmental standard scores, which are designed so that a unit change represents the same amount of learning at all points on the scale.
Student Demographics	CPS Board	Student demographic variables (race, gender, age) come directly from information reported on the applications. Eligibility for free or reduced-price lunch is only available for students who enroll in CPS, and is recorded each Spring.
Neighborhood Characteristics	2000 Census & CPS Board & CCSR	We mapped the home addresses reported on the application files to census tracts. Basic information on the student's census tract, such as median household income and percent below the poverty line, comes from the 2000 Census. The crime composite was provided by the Consortium on Chicago School Research (CCSR) and is an index created by factor analysis using official block group level crime statistics for 1994. The variable used in this analysis is a tract-level average, weighted by the total population in each block group.

TABLE 1
SUMMARY STATISTICS FOR ALL CPS THIRD GRADE STUDENTS[a]

	Student race/ethnicity			Student poverty status		Tract poverty level	
	White	Black	Hispanic	Ineligible for free/reduced-price lunch	Eligible for free/reduced-price lunch	Poverty rate ≤ 25%	Poverty rate > 25%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of 3rd grade enrollment	0.074	0.510	0.390	0.116	0.884	0.562	0.438
Student achievement							
In special education in 3rd grade	0.176	0.120	0.109	0.136	0.116	0.124	0.112
Took the ITBS reading/math exams	0.975	0.980	0.968	0.974	0.975	0.974	0.976
3rd grade reading standard score	194.3	174.8	176.8	193.4	175.4	180.7	173.4
3rd grade math standard score	194.1	176.8	182.3	193.2	179.1	183.9	176.7
Elementary school characteristics							
Mean peer composite 3rd grade score	190.2	177.3	179.8	188.9	178.3	182.1	176.4
Mean value-added 3rd-4th grades	0.741	-0.004	0.098	0.960	0.009	0.330	-0.151
Academically advanced school	0.062	0.010	0.016	0.081	0.009	0.026	0.007
Magnet school	0.103	0.058	0.054	0.142	0.051	0.064	0.059
School included in our analysis	0.127	0.055	0.045	0.159	0.046	0.071	0.045
Number of observations	4,603	31,630	24,196	7,225	54,831	34,846	27,210

[a]The statistics are based on the sample of all 3rd grade students enrolled in CPS in 2004 and 2005, the same years that most kindergarten applicants from our two application cohorts attended 3rd grade. Each column presents means for the subsample indicated by the column heading. A value of 185 for the reading or math standard score indicates that the student is performing at the national average, and a difference of 15 points represents a year's worth of learning for the typical 3rd grade student. The mean peer composite 3rd grade score is the average combined reading and math standard score for 3rd grade students in the 2000 and 2001 cohorts at the school. Mean value-added is the average "gain" on reading and math exams between 3rd and 4th grades, controlling for prior scores and demographic characteristics. This is calculated by extracting the mean residual by campus from student-level regressions of fourth grade reading and math standard scores (separately) on flexible controls for third grade reading and math scores, and student demographic and neighborhood characteristics. A positive number indicates that the typical student at the school is progressing more quickly than similar students at other schools. Academically advanced schools include classical schools and regional gifted centers that have test-based admissions policies. Magnet schools organize their curricula around a specific theme (e.g., math/science, fine arts, world language, or humanities) and accept students from throughout the city via computerized lottery.

TABLE 2

CORRELATION BETWEEN ACADEMIC ACHIEVEMENT AND SCHOOL QUALITY[a]

Independent variable	Dependent variable: 3rd grade composite standard score							
	Sample: All 3rd graders in 2004 and 2005					Sample: Analysis 3rd graders		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Student characteristic								
Black	-8.3**	-6.3**	-7.0**	-6.6**	-7.1**	-12.6**	-12.4**	-13.2**
	(0.6)	(0.6)	(0.4)	(0.6)	(0.4)	(2.0)	(1.9)	(1.5)
Hispanic	-6.5**	-4.9**	-5.3**	-4.9**	-5.2**	-11.8**	-11.0**	-11.3**
	(0.5)	(0.5)	(0.3)	(0.5)	(0.3)	(1.7)	(1.7)	(1.4)
Eligible for free/reduced-price lunch	-9.4**	-6.3**	-5.8**	-5.8**	-5.5**	-9.9**	-5.0**	-4.5**
	(0.6)	(0.3)	(0.3)	(0.3)	(0.3)	(1.3)	(0.9)	(0.9)
Applied to at least one lottery school	–	–	–	2.9**	2.8**	–	–	–
				(0.4)	(0.4)			
Applied to at least one academically advanced school	–	–	–	6.7**	5.6**	–	–	–
				(0.7)	(0.5)			
School characteristic								
Mean peer composite 3rd grade score	–	0.583**	–	0.520**	–	–	0.557**	–
		(0.026)		(0.025)			(0.057)	
Mean value-added 3rd-4th grades	–	0.241**	–	0.209**	–	–	0.124	–
		(0.057)		(0.053)			(0.153)	
Includes school fixed effects	N	N	Y	N	Y	N	N	Y

[a]The sample in the left panel is all 3rd grade students enrolled in CPS in 2004 and 2005 with non-missing ITBS tests scores. The sample in the right panel is the subset of these students that participated in contested lotteries for kindergarten slots in 2001 and 2002, and so comprise part of our lottery analysis sample. The dependent variable is the mean of the student's 3rd grade reading and math standard scores. Coefficient estimates are shown for the control variables indicated in the rows, with standard errors that are robust to unspecified correlation across students within the same elementary schools in parentheses. What differs across the columns is whether and which controls for school quality and student applications are included. In addition to the variables shown, all specifications include an indicator for 2005, student demographic characteristics (Asian, Native American, gender, age) and home tract characteristics (population, fraction Black, fraction Hispanic, median income, poverty rate, fraction female-headed households, fraction of adults highest degree high school, fraction of adults completed at least some college, fraction homeowners, unemployment rate, share of students grade K-8 attending private school, 1994 crime index). The school characteristics are described in the notes to Table 1.

** significant at the 5 percent level * significant at the 10 percent level

TABLE 3
SUMMARY STATISTICS FOR LOTTERY APPLICANTS[a]

Background characteristic	All lottery participants (1)	Students enrolled in CPS in PK at time of application: Lottery participants (2)	Non-applicants (3)	Difference (2) – (3)
Student characteristics				
Applying to kindergarten	0.720	1	–	–
Applying to 1st grade	0.280	0	–	–
White	0.156	0.122	0.076	0.047**
Black	0.620	0.526	0.475	0.051**
Hispanic	0.161	0.239	0.416	-0.177**
Asian	0.058	0.107	0.032	0.075**
Male	0.492	0.508	0.520	-0.012
Age on Sept. 1 of school year following application	5.77	5.53	5.57	-0.05**
Eligible for free/reduced-price lunch[b]	–	0.412	0.799	-0.387**
Living with a biological parent[b]	–	0.876	0.813	0.063**
Enrolled in CPS at the time of application	0.303	1	1	–
Received special education in PK[b]	–	0.073	0.108	-0.035**
Received bilingual education in PK[b]	–	0.260	0.314	-0.054**
Tract poverty rate	0.208	0.212	0.261	-0.049**
Tract fraction completed at least some college	0.492	0.465	0.363	0.102**
Predicted 3rd grade composite score	192.0	192.1	180.7	11.4**
Default kindergarten school characteristics				
Fraction eligible for free/reduced-price lunch	0.860	0.869	0.902	-0.033**
Fraction Black	0.591	0.507	0.493	0.014
Fraction Hispanic	0.270	0.327	0.400	-0.073**
Mean peer composite 3rd grade score	179.2	179.5	178.6	0.9**
Mean value-added 3rd-4th grades	0.157	0.155	-0.237	0.392**

[a]The unit of observation is the student. There are 7,469 students participating in at least one of the lotteries included in our analysis. Mean characteristics for these lottery participants are shown in column 1. Column 2 restricts the sample to the 1,309 lottery participants enrolled in CPS in pre-kindergarten at the time of

the application. There are 31,050 students enrolled in pre-kindergarten in CPS in Spring 2000 and Spring 2001 that we do not observe submitting an application to a choice school. Mean characteristics for these students are shown in column 3.

[b]These variables are only available for students enrolled in CPS at the time of application.

** significant at the 5 percent level * significant at the 10 percent level

TABLE 4

CHICAGO PUBLIC ELEMENTARY SCHOOLS REPRESENTED IN THE ANALYSIS[a]

School name	Mean peer composite 3rd grade score (1)	Mean value-added 3rd-4th grades (2)	Fraction of applicants rejected (3)	Fraction of accepted applicants enrolling (4)	Number of participants in analysis lotteries (5)	Average size of 3rd grade class (6)
LaSalle Language Academy[m]	206.0[b]	3.09	0.93	0.57[b]	1372	65
Hawthorne Scholastic Academy[m]	202.7[b]	2.29	0.91	0.65[b]	674	63
Jackson Language Academy[m]	199.9[b]	4.77[b]	0.92	0.59[b]	714	62
Thorp Scholastic Academy[m]	198.5[b]	2.79	0.85	0.46	444	85
Stone Scholastic Academy[m]	197.9[b]	-1.30	0.89	0.47	718	61
Sheridan Math/Science Academy[m]	196.9[b]	-0.06	0.82	0.71[b]	423	58
Franklin Fine Arts Center[m]	194.7[b]	8.28[b]	0.96[b]	0.62[b]	511	42
Ray Elementary School	193.6[b]	5.46[b]	0.89	0.51[b]	223	91
Beasley Academic Magnet[m]	192.5	4.31[b]	0.83	0.38	696	156
Newberry Math/Science Academy[m]	192.5	4.36[b]	0.94[b]	0.41	1045	65
Gunsaulus Scholastic Academy[m]	192.2	2.02	0.77	0.66[b]	216	72
Blaine Elementary School	190.8	4.69[b]	0.56	0.13	337	45
Owen Scholastic Academy[m]	190.4	5.19[b]	0.91	0.54[b]	188	28
Galileo Scholastic Academy[m]	190.1	2.09	0.98[b]	0.17	120	68
Black Magnet School[m]	189.6	4.06[b]	0.95[b]	0.46	1369	55
Vanderpoel Magnet School[m]	189.2	3.33	0.95[b]	0.48	798	29
Pershing Magnet School[m]	188.1	1.63	0.95[b]	0.35	248	31
Burnside Scholastic Academy[m]	186.9	2.19	0.87	0.46	601	84
Disney Magnet School[m]	186.7	-0.07	0.90	0.35	1970	178
Hamilton Elementary School	186.0	-5.47	0.44	0.05	233	58
Turner-Drew Language Academy[m]	186.0	3.53	0.94[b]	0.38	271	29
Mayer Elementary School	184.7	1.84	0.73	0.10	372	83
Sayre Language Academy[m]	183.6	3.11	0.84	0.36	503	59
Budlong Elementary School	183.0	-1.08	0.93[b]	0.11	114	103
Agassiz Elementary School	182.9	2.88	0.53	0.11	115	64
Alcott Elementary School	181.8	0.98	0.55	0.17	133	47
Audubon Elementary School	181.6	-1.14	0.64	0.14	159	51
De Diego Community Academy	181.6	-0.71	0.39	0.38	146	137

School name	Mean peer composite 3rd grade score	Mean value-added 3rd-4th grades	Fraction of applicants rejected	Fraction of accepted applicants enrolling	Number of participants in analysis lotteries	Average size of 3rd grade class
	(1)	(2)	(3)	(4)	(5)	(6)
Sabin Magnet School[m]	181.4	-2.72	0.83	0.32	160	61
Nettelhorst Elementary School	179.6	-2.46	0.50	0.04	155	69
Jensen Scholastic Academy[m]	179.0	-2.27	0.76	0.42	131	73
Ericson Scholastic Academy[m]	176.2	2.29	0.44	0.39	244	98
CPS Elementary School Average	180.1	0.34	–	–	–	86.8

[a]The summary statistics in columns 1 and 2 are based on all students enrolled in these elementary schools in Spring 2000 and Spring 2001, at the time of application for our two cohorts. Column 1 reports the mean composite reading and math standard scores for 3rd graders at the school, where a value of 185 indicates that students are performing at the national average. Value-added in column 2 is calculated as described in Table 1. The statistics reported in columns 3 and 4 are averages across all 2000 and 2001 applications, regardless of whether an individual application is involved in a non-degenerate lottery or not. Column 5 refers to applications to the non-degenerate lotteries included in the empirical analysis. Column 6 shows average 3rd grade enrollment across 2000 and 2001 at the campus. The bottom row shows mean values across all 456 regular elementary schools in the CPS in fiscal years 2000 and 2001.
[b]The elementary school is in the top quartile of analysis schools on this measure.
[m]These schools are magnet schools, which accept students from throughout the district. No students are assigned to these schools by default. The other schools are neighborhood schools that also operate magnet cluster programs that enroll students from outside the attendance area.

TABLE 5
THE IMPACT OF WINNING A LOTTERY ON ENROLLMENT AND ATTRITION[a]

Dependent variable	All students	Student race/ethnicity: White	Student race/ethnicity: Black	Student race/ethnicity: Hispanic	Student poverty status[b]: Ineligible for free/reduced-price lunch	Student poverty status[b]: Eligible for free/reduced-price lunch	Tract poverty level: Poverty rate ≤ 25%	Tract poverty level: Poverty rate > 25%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Currently enrolled in CPS	0.050**	0.053*	0.045**	0.078**	–	–	0.066**	0.026
	(0.012)	(0.028)	(0.018)	(0.025)			(0.015)	(0.024)
	[0.527]	[0.420]	[0.534]	[0.609]			[0.516]	[0.559]
Conditional on current enrollment								
Predicted 3rd grade composite score	-0.15	0.29	-0.18	-0.51	-0.00	-0.50*	-0.28	0.21
	(0.23)	(0.51)	(0.32)	(0.44)	(0.32)	(0.30)	(0.27)	(0.35)
	{10.75}	{6.38}	{7.49}	{7.29}	{11.53}	{8.38}	{10.68}	{7.71}
Number of observations	70,114	13,063	41,115	11,625	–	–	51,218	18,896
Number of observations conditional on enrollment	37,569	5,620	22,241	7,283	18,541	18,795	26,836	10,733

[a]Each cell reports the coefficient on an indicator for being selected from separate ordinary least squares regressions of the dependent variables indicated by the row headings. The control set also includes a full set of lottery fixed effects. Eicker-White robust standard errors are shown in parentheses, clustered by student. Either the mean [ ] or the standard deviation { } of the dependent variable among lottery losers is shown beneath the standard error. The results shown in the first column are based on the full sample of applications involved in non-degenerate lotteries. The remaining columns are based on various subsamples.
[b]Eligibility for free/reduced-price lunch is only defined for students enrolled in CPS.
** significant at the 5 percent level * significant at the 10 percent level

TABLE 6
THE IMPACT OF WINNING A LOTTERY ON STUDENT OUTCOMES[a]

Dependent variable	All students (1)	Student race/ethnicity: White (2)	Black (3)	Hispanic (4)	Student poverty status: Ineligible for free/reduced-price lunch (5)	Eligible for free/reduced-price lunch (6)	Tract poverty level: Poverty rate ≤ 25% (7)	Poverty rate > 25% (8)
Attending school for which lottery applies	0.312**	0.210**	0.377**	0.275**	0.278**	0.367**	0.299**	0.381**
	(0.015)	(0.029)	(0.021)	(0.030)	(0.019)	(0.023)	(0.017)	(0.030)
	[0.082]	[0.105]	[0.073]	[0.083]	[0.084]	[0.080]	[0.082]	[0.081]
Characteristics of school attended								
Mean peer composite 3rd grade score (in 2000/2001)	2.34**	1.90**	2.67**	2.23**	1.50**	3.02**	2.04**	3.42**
	(0.28)	(0.65)	(0.41)	(0.53)	(0.39)	(0.40)	(0.32)	(0.54)
	{12.1}	{10.8}	{11.7}	{10.9}	{12.7}	{10.4}	{12.2}	{10.8}
Mean value-added 3rd-4th grades (in 2000/2001)	0.45**	0.41**	0.51**	0.53**	0.24**	0.64**	0.45**	0.57**
	(0.08)	(0.20)	(0.12)	(0.16)	(0.11)	(0.13)	(0.10)	(0.17)
	{3.19}	{3.05}	{3.23}	{3.12}	{3.18}	{3.12}	{3.22}	{3.09}
Student Outcomes								
Retained in grade	-0.002	0.005	-0.002	-0.006	0.001	-0.004	-0.004	0.005
	(0.002)	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.006)
	[0.019]	[0.007]	[0.023]	[0.018]	[0.012]	[0.025]	[0.016]	[0.025]
In special education	-0.003	0.000	0.010	-0.035*	-0.022*	0.019	0.000	0.008
	(0.009)	(0.020)	(0.012)	(0.019)	(0.012)	(0.013)	(0.010)	(0.019)
	[0.084]	[0.092]	[0.085]	[0.092]	[0.095]	[0.073]	[0.084]	[0.083]
Missing ITBS test scores	-0.007	-0.010	-0.000	-0.008	-0.006	-0.010	-0.002	-0.010
	(0.005)	(0.013)	(0.006)	(0.014)	(0.008)	(0.007)	(0.006)	(0.011)
	[0.265]	[0.306]	[0.220]	[0.357]	[0.279]	[0.254]	[0.270]	[0.253]
Composite (mean) reading and math standard score	0.17	0.02	-0.37	0.86	-0.18	-0.87	0.25	-0.99
	(0.53)	(1.23)	(0.75)	(1.07)	(0.74)	(0.78)	(0.64)	(1.05)
	{19.6}	{18.8}	{18.1}	{18.8}	{19.9}	{18.1}	{19.7}	{18.2}
Number of observations	37,569	5,620	22,241	7,283	18,541	18,795	26,836	10,733

[a]Each cell reports the coefficient on an indicator for being selected from separate ordinary least squares regressions of the dependent variables indicated by the row headings. The control set includes a full set of lottery fixed effects as well as student demographic characteristics, home census tract characteristics, and student application patterns. Eicker-White robust standard errors are shown in parentheses, clustered by student. Either the mean [ ] or the standard deviation { } of the dependent variable among lottery losers is shown beneath the standard error. The results shown in columns 1 are based on the full sample of applications involved in non-degenerate lotteries from students currently enrolled in CPS. The remaining columns are based on various subsamples.
** significant at the 5 percent level * significant at the 10 percent level

TABLE 7
IMPACT OF WINNING A LOTTERY– INTERACTIONS[a]

Interaction variable	Enrolled in CPS		Predicted 3rd grade combined score (conditional on enrolling)		Mean peer composite 3rd grade score at campus attended (in 2000/2001)		Student's composite reading and math standard score	
	Selected (1a)	Interaction (1b)	Selected (2a)	Interaction (2b)	Selected (3a)	Interaction (3b)	Selected (4a)	Interaction (4b)
Treatment exposure								
Years since application	0.069**	-0.011**	-0.22	-0.03	2.98**	-0.33**	0.18	-0.01
	(0.013)	(0.003)	(0.28)	(0.06)	(0.36)	(0.10)	(0.61)	(0.24)
Intensity of treatment								
Potential gain in mean peer	0.037**	0.003**	-0.32	0.01	1.73**	0.12**	0.06	0.00
composite 3rd grade score	(0.013)	(0.001)	(0.21)	(0.03)	(0.29)	(0.04)	(0.58)	(0.06)
[μ=4.4 σ=10.0]								
Preference for school quality								
Mean residual	0.057**	0.004**	-0.35	-0.05	2.32**	0.05	0.14	-0.02
[μ=-0.4, σ=6.0]	(0.013)	(0.002)	(0.24)	(0.04)	(0.33)	(0.05)	(0.56)	(0.09)

[a]The cells report results from separate ordinary least squares regressions of the dependent variables indicated by the column headings. The control set includes a full set of lottery fixed effects as well as student demographic characteristics, home census tract characteristics, and student application patterns (when the dependent variable is other than student's predicted 3rd grade score). The estimated coefficients on the indicator for being selected and the interaction between this indicator and the variable indicated by the row heading are reported in paired columns. Eicker-White robust standard errors are shown in parentheses, clustered by student. The results are based on the full sample of applications involved in non-degenerate lotteries.
** significant at the 5 percent level * significant at the 10 percent level