THE EFFECT OF SCHOOL CHOICE ON STUDENT OUTCOMES:
EVIDENCE FROM RANDOMIZED LOTTERIES

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NBER Working Paper No. 10113
November 2003
JEL No. I28, H72

ABSTRACT

School choice has become an increasingly prominent strategy for urban school districts seeking to

enhance academic achievement. Evaluating the impact of such programs is complicated by the fact

that a highly select sample of students takes advantage of these programs. To overcome this

difficulty, we exploit randomized lotteries that determine high school admission in the Chicago

Public Schools. Surprisingly, we find little evidence that attending sought after programs provides

any benefit on a wide variety of traditional academic measures, including standardized test scores,

attendance rates, course-taking, and credit accumulation. This is true despite the fact that those

students who win the lotteries attend better high schools along a number of dimensions, including

higher peer achievement levels, higher peer graduation rates, and lower levels of poverty. We do,

however, uncover evidence that attendance at such schools may improve a subset of non-traditional

outcome measures, such as self-reported disciplinary incidences and arrest rates.

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I. Introduction

Although the current school choice debate centers on private school vouchers and charter schools, the most common form of choice available to students in urban areas is open enrollment (NCES 1996). Under open enrollment, public school students can apply to gain access to magnet public schools and programs outside of their neighborhood school. This form of choice is likely to become even more prevalent under the recent federal education legislation *No Child Left Behind* (NCLB). 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. A necessary condition for this form of choice to improve student achievement is that those students who gain access to desirable schools experience better academic outcomes than they otherwise would.

Much of the evidence on the link between access to school choice and student outcomes comes from contexts other than open enrollment, and estimating a causal relationship has proven difficult. Observational studies of private schools (Coleman et. al. 1982, Bryk et. al. 1993) and magnet schools (Blank 1983; Metz 1986; Gamoran 1996) find that students who attend these schools experience better educational outcomes, but these studies potentially suffer from important selection bias since the students who take advantage of school choice are unlikely to be representative of students more generally. Studies that use instrumental variables approaches to account for endogenous school choice find mixed effects, with some showing benefits of attending Catholic schools (Evans and Schwab 1995) and others showing little or no effect (Sanders 1996, Neal 1997).[1] More recently, there have been a series of studies that exploit randomized voucher lotteries to estimate the effect of attending a private school. 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. Analyses of this program obtain sharply conflicting estimates of the impact on achievement depending upon the assumptions made to deal with selective attrition of lottery losers from the sample (Witte, Sterr et al. 1995; Green, Peterson et al. 1997; Witte 1997; Rouse 1998). Although in theory randomization provides an ideal context for the evaluation of school choice, 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).[2]

In this paper, we are able to overcome many of the empirical difficulties confronting earlier studies by using detailed administrative data from the Chicago Public Schools (CPS). Like the Milwaukee voucher experiment, we avoid the issue of non-random selection into school choice by using lottery data. Many CPS schools use lotteries to allocate spots when oversubscribed. In this paper, we use lottery data from 194 separate lotteries at 19 high schools. The CPS data we use offer a number of important advantages over the Milwaukee experiment. First, selective attrition is less of a concern in our sample since more than 90 percent of lottery participants remain in CPS, and losing a lottery has only a minor impact on a student's propensity to stay. Moreover, there is little evidence that those who remain in the sample differ on observable dimensions from those who leave. Second, we have access to a far broader range of student outcomes than is typically available. In addition to standard achievement and attainment measures, we also have student survey responses covering their degree of satisfaction

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[1] Altonji et al. (2002) suggests that the IV strategies used in prior studies may not be valid.
[2] An additional limitation to studies of the Milwaukee program is that direct information on the schools that students applied to is not available, forcing researchers to impute likely application schools (see Rouse 1998 for details). Evidence from other voucher experiments is similarly mixed. 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, OH 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. Studies of private school choice in other countries find improved educational and social outcomes (Angrist et. al. 2002). Prior studies that exploit lotteries to examine the benefit of attending magnet schools with a

with the school attended, how they are treated by teachers and peers, expectations about college attendance, and self-reported arrest data.[3] Understanding how reforms affect these non-traditional measures of student outcomes is an area about which little is known and of great importance, particularly in light of the frequent inability of school-based interventions to induce large changes in standard educational measures like test scores (Hanushek 1997). Third, CPS has been one of the most aggressive school districts in the country in implementing intra-district school choice. Over half of high school students in CPS take advantage of the program by attending a school other than the one they are assigned, allowing us to examine the benefits of a systemic program rather than one where a small percentage of children participate. Finally, 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.

Our use of lotteries as the source of identifying variation permits straightforward analysis based on comparisons of means. In principle and in practice, controlling for other characteristics will have little impact on any conclusions drawn, although we do so to increase the precision of our estimates. Sample selection in terms of which students choose to apply to a particular school will not bias our estimates, since among the applicants to a given school, those who win or lose the lottery will on average have the same characteristics.[4]

Comparing lottery winners and losers, we find little evidence that attending sought after

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vocational focus find mixed evidence of any long-term benefit (Crain et. al. 1992, 1999; Kemple and Snipes 2000).
[3] Few studies have examined the effects of specialized schools on non-traditional outcome measures for students. Two recent studies examine the impact of Catholic schools on non-market behaviors such as drug use, sexual behavior and criminal activity, finding opposite results (Figlio and Ludwig 2000, Mocan et. al. 2002). Randomized housing mobility experiments show that households offered vouchers to low-poverty neighborhoods experience increased safety, fewer behavior problems, and improved health outcomes (Katz et. al. 2001).
[4] Although one does still need to use care in properly interpreting the resulting parameter, which is an unbiased estimate of the impact of winning a lottery for the students applying to the lottery, but may not generalize to other students in the system.

programs provides any benefit on a wide variety of traditional achievement measures, including standardized test scores, attendance rates, course-taking patterns, and credit accumulation. These results are robust to a variety of sensitivity analyses, and do not vary substantively across student subgroups. This finding is surprising since students who win contested lotteries would be expected to fare better academically for a number of reasons. They might gain access to better resources, better peers, or be able to attend a program that better suits their learning needs for idiosyncratic reasons.

We explore a variety of possible explanations for the lack of academic benefits that would still be consistent with schooling choices being driven by the anticipation of such benefits. These include: students winning lotteries end up attending similar schools to those who lose (i.e., the "treatment" is limited); students who are selected have to travel much greater distances to school; attendance at a choice school is a substitute for parental involvement. None of these hypotheses has much explanatory power. Those students who win lotteries to the most select programs do end up attending what appear to be substantially better high schools – e.g., schools with higher achievement levels and graduation rates and lower levels of poverty. And, we find little evidence that excess travel or parental compensation mitigate potential achievement gains.

The coexistence of intense competition for entry and little academic benefit to students attending lottery schools could indicate that parents are not well-informed about the education production function, and mistake higher outputs for higher 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. Using a unique set of survey data on student attitudes and behaviors, we examine the impact of winning a lottery on measures such as enjoyment of school, behavior of peers, student-teacher trust, expectations for the future, and

self-reported disciplinary incidents. If parents and children choose schools for non-academic reasons, one would expect positive effects on these non-traditional outcomes. We find some, though limited, support for this hypothesis: winners report lower incidences of disciplinary action and fewer arrests, but are no more likely to report positive outcomes on other measures such as liking school, trusting their teachers, and having high expectations for the future.

Overall, our findings suggest that parents and children are not choosing schools that provide significant academic benefit to them, and may be choosing schools for other reasons. More importantly, this analysis suggests that open enrollment is unlikely to be an effective mechanism to improve academic achievement in urban school districts (as envisioned by NCLB). However, it may confer other benefits not captured by test scores or traditional outcome measures, such as matching idiosyncratic tastes of parents and students and improving social circumstances.

An important caveat to interpreting our results is that we are only able to evaluate the partial equilibrium effects of school choice. In other words, the lotteries allow us to estimate how winning access to a particular school affects educational outcomes for a student, holding constant the existence of a school choice program. We are unable to determine how the introduction of a school choice system affects outcomes, since the introduction may have altered the composition of students in the public sector, the overall level of public school quality (e.g., Hoxby 2000), and residential location patterns.

The remainder of the paper is structured as follows. Section II provides background on magnet schools in CPS, the lotteries, and the administrative data. Section III describes our estimation strategy, focusing on how we utilize the lottery randomizations. Section IV analyzes

the impact of lottery outcomes on a variety of traditional and non-traditional outcome measures. Section V concludes.

II. Institutional detail and data description

The Chicago Public Schools have one of the most extensive school choice programs available.[5] Each student is guaranteed admission to an assigned neighborhood school, but can also apply to any other CPS school. Indeed, more than half of all high-school students in CPS in 2000 and 2001 exercised their choice and elected not to attend their assigned school.

In order to attend a school other than the assigned school, a student must submit an application in the spring of the preceding year. A student must reside within the school district, but does not need to be currently enrolled in CPS in order to submit an application, and there is no restriction placed on the number of applications an individual student can submit. 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 programs, typically the most selective, admission is based on criteria such as test scores, and lotteries are not used.

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, separate lotteries are conducted for each gender-race-grade combination. A particular school may also house multiple separate magnet programs, each of

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[5] 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 whose racial composition roughly matched the racial composition of the school system. Since that time, the size and scope of school choice has expanded dramatically.

which would conduct separate lotteries. As a consequence, one school can potentially have a large number of lotteries each year.[6]

Working with the CPS, we have obtained detailed administrative data on applications submitted in spring 2000 and spring 2001. The application data include the name, race, gender, date of birth, home address, current school, and grade of each applicant, as well as the program a student is applying to, whether that application was part of a lottery, and if so, the lottery outcome. We focus on eighth grade students applying for ninth grade admission. This is the transition period from grade school to high school in CPS and thus is the juncture at which school choice is most frequently exercised.[7]

We exclude the small fraction (7 percent) of eighth grade students applying from outside CPS. Note that excluding these students does not affect the validity of the randomization since enrollment status at the time of application is a predetermined variable. These students are no more or less likely to be represented among winners than among losers of any given lottery. However, excluding these students greatly reduces problems of selective attrition. They are much less likely to enroll in CPS the following year, particularly when losing the lottery.[8]

For our sample of eighth-grade applicants attending public schools in the district, the application data also provide their CPS identification number. Using this number, we link each application to a student's school records. This provides us with not only information on demographics and prior school performance of the student, but also information on whether the

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[6] 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.

[7] The only other grade within CPS for which there is substantial numbers of school assignments allocated by lottery is kindergarten. The 2000 and 2001 cohort of kindergarten applicants will complete third grade in 2004 and 2005 respectively, at which point test score data will become available.

[8] Applicants for 9th grade slots from eighth-graders not enrolled in CPS during 8th grade are 35% more likely to enroll in the CPS if they win a contested high school lottery than if they lose. This provides evidence that the

student enrolled in the CPS the following year and, if so, all of the student's outcomes (See

Appendix A for a detailed description of the variables and data sources used in this study). Our

data has the shortcoming that we do not observe outcomes for students who do not attend CPS in

subsequent years.

After eliminating applications to schools that do not use lotteries to assign slots (a

handful of very selective test-based schools), special education schools, and schools with

incomplete lottery outcome data, we are left with a baseline sample of 19 schools and 194

lotteries.[9] Our baseline sample contains 19,520 applications submitted by 14,438 students. The

students in our sample represent approximately one-fourth of all eighth graders in CPS during

this two-year period.

Table 1 presents information about the 19 schools represented in the data set. Schools

are listed according to the eighth grade test score performance of students enrolling in ninth

grade, with the highest achieving schools listed first. These schools range from the top ten

percent among the 70 regular high schools in CPS along the test score dimension (Chicago

Agricultural) all the way down to the very bottom (Orr is the second lowest scoring high school

in CPS). For purposes of analysis, we divide these schools into two groups: "high achieving"

and "other." Somewhat arbitrarily, we assign the first five schools to the high achieving

category and the remaining schools to the other category. Later in the paper, we explore the

sensitivity of our results to alternative classifications of schools. The schools in our sample

exhibit substantial racial heterogeneity (ranging from more than half White to all minority),

differences in selectivity of the lotteries (only 2.9 percent of lottery applicants are admitted in

Von Steuben compared to 53 percent at George Washington), and widely varying take-up rates

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availability of school choice serves to attract students to the public sector.
[9] Appendix A describes the construction of our sample in greater detail.

among lottery winners (as high as 64.7 percent at Curie and as low as 6 percent at Lake View). Overall, these lotteries are quite competitive. On average, only 15 percent of applicants actually win in a particular lottery. Because a student can apply to multiple lotteries, roughly 20 percent of the students in the sample win at least one lottery.

Analysis of the raw data at the student level in Table 2 clearly demonstrates the important differences between the pool of applicants entering our lotteries (column 1) and other eighth grade students in CPS (column 2) along a variety of dimensions. Students entering lotteries are less likely to be Black or male, have substantially lower test scores, and are less likely to be poor (as proxied by free lunch eligibility and census tract poverty rates). Given the substantial differences in observable characteristics, one might also be concerned that lottery applicants may also be systematically different on unobservable dimensions (for example, motivation level, parental involvement, etc.). It is precisely for this reason that lottery-induced randomization is likely to be important for drawing conclusions about the causal impact of school choice on student outcomes.

III. 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 have the same set of background characteristics on average, both for observables and unobservables. Consequently, a simple difference of observed student outcomes between students who win and lose the lottery provides a consistent estimate of the impact of winning the lottery.[10]

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[10] One can also condition on observable student characteristics in constructing the estimate. In large samples, the estimates will be the same with and without conditioning on observables, as long as there is no selective attrition

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 admitted to the school

represented by lottery *j*:

$$\delta_j = E[Y_i \mid Win_{ij} = 1] - E[Y_i \mid Win_{ij} = 0] \tag{1}$$

where *Y* is some outcome measure for student *i* and Win_{ij} is a binary variable indicating whether

the student won lottery *j*. Then, δ_j indicates whether winners are systematically higher or lower

on the characteristic *Y* than losers in the same lottery.

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 the characteristics of the applicants. Particularly relevant is the case where

applicants apply to more than one lottery. Because students may win other lotteries (10 percent

of losers in our sample win another lottery) and not all winners choose to attend the lottery

school, δ_j measures the impact of having the option to attend the lottery school. Importantly, as

long as the lottery is truly randomized and there is no selective attrition, the parameter estimate is

an unbiased estimate of this intention-to-treat (ITT) effect even if we are missing background

information about other schools to which a student may have applied or been accepted.

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. For example, the impact of winning for students in lottery *j* with a specific value for

a characteristic *z* would be:

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from the original sample. In small samples such as ours, controlling for observable characteristics will correct for
any imbalances that arise on these dimensions in a particular draw. The addition of covariates will also improve the
precision of the estimates.

$$\delta_{jk} = E[Y_i \mid Win_{ij} = 1; z_i = k] - E[Y_i \mid Win_{ij} = 0; z_i = k] \tag{2}$$

That is why excluding students who are not enrolled at the time of application does not introduce any bias, but rather, merely alters the interpretation of the estimates.

In practice, the standard errors for particular lotteries and subgroups within lotteries are too large to make such estimates informative. Therefore, we instead report results from ordinary least-squares regressions of the form:

$$Y_i = \delta_1(Win\_High_{ia}) + \delta_2(Win\_Other_{ia}) + \Gamma(Lottery_a) + e_{ia}, \tag{3}$$

where the subscripts i and a index students and applications, respectively. Win_High_{ia} is a binary variable that indicates whether application a for student i was a winner at a lottery to a high-achieving school (92 of our lotteries). Win_Other_{ia} reflects whether this application won a lottery to a non-high-achieving school (102 of our lotteries). $Lottery_a$ 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 coefficients are simply applications-weighted averages of the δ_j's for the lotteries belonging to each of the two school categories. In some specifications, we include covariates such as student demographics, prior achievement and neighborhood characteristics.[11] Since the same student is included in the regression more than once if he or she submits multiple applications (we have roughly 1.3 applications per student), we report robust standard errors that account for clustering at the student level. When we examine high school outcomes, we report robust standard errors that account for clustering at the school level.[12]

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[11] Note that this specification restricts the coefficients of other covariates to be the same for applications to high-achieving and other schools. Relaxing this assumption does not change any of our estimates.

[12] Technically, the standard errors should reflect the correlation structure at the student as well as the school level. The precision of the estimates from the equation that treats the student rather than the application as the unit of observation (presented as a sensitivity test) suggests that standard errors adjusted for clustering at both levels would be quite similar to the ones we report.

We prefer the above specification that treats the application as the unit of analysis since this is the point at which randomization occurs. However, it is also possible to specify alternative models with the student as the unit of observation. Consider the following equation:

$$Y_i = \delta_1'(WinOne\_High_i) + \delta_2'(WinOne\_Other_i) + \Pi(Apps_i) + u_i, \qquad (4)$$

where $WinOne\_High_i$ is a binary indicator of winning at least one lottery to a high-achieving school, $WinOne\_Other_i$ is a binary indicator of winning at least one lottery to a non-high-achieving school, and $Apps_i$ is a vector of indicators capturing the set of lotteries to which the student applied.[13] In the case where all students only apply to only one lottery, this equation is identical to equation (3). While this strategy may be somewhat easier to interpret than the previous specification, one shortcoming of this strategy is that it uses less information. For example, if only one student applied to a particular combination of schools, the observation for this student would not be used in estimating the impact of winning a lottery. As a sensitivity check, we re-estimate our main results using this specification and find very similar results. Note, however, that the treatment effects in (3) and (4) measure different things and are not directly comparable. The strategies will produce somewhat different results if many students apply to and win lotteries at multiple schools, and the impact of winning more than one lottery strongly affects the likelihood of attending that type of school.

Establishing the validity of the randomization

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

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[13] This is analogous to the specification used by Dale and Krueger (2002) in estimating the payoff to attending a more selective college and by Rouse (1998) in examining the Milwaukee choice program. Note that it is not sufficient to include a single dummy for each lottery. Instead, one must include a separate fixed effect for every

characteristics. We test this proposition by estimating equation (3) for a series of demographic

and prior achievement variables. The results are compiled in Table 3. Columns 1 and 2 present

coefficient estimates on Win_High_{ia} and Win_Other_{ia}, respectively, along with robust standard

errors that account for correlation across applications by the same student in parentheses. We

show the control group means (i.e., values for those who lost the lottery) in brackets. Each row

represents a separate regression corresponding to a different dependent variable. Student-

specific characteristics are shown in the top panel; school and neighborhood characteristics are

shown in the bottom panel. The results of Table 3 confirm that lottery winners and losers appear

similar on a wide range of observable characteristics at the time when the lottery applications are

submitted. There are actually fewer significant differences between lottery winners and losers

across the twenty characteristics considered than might be expected by chance.[14]

Selective attrition

Even if the lotteries were properly carried out, selective attrition may bias our findings

since we only observe subsequent student outcomes if the student enrolls in CPS. Table 4

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possible combination of lotteries to which students can apply.

[14] This test indicates whether winners are systematically better or worse than losers across the lotteries. Even if there are no differences on average, however, it is theoretically possible for winners to be better in some lotteries and worse in other lotteries, calling into question the validity of the randomization. One approach to testing this hypothesis is to estimate a model similar to equation (3) that includes separate indicators for winning each particular lottery and then conduct an F-test on the joint significance of the set of variables that indicate winning a lottery. The problem with that strategy is that because we have many very small lotteries we must rely on the assumption that the dependent variables are distributed normally in the population, which is clearly not true for the binary outcomes. For this reason, we take a different approach. For our binary background characteristics, we calculate Fisher's exact p-value for the difference in proportions across winners and losers for each lottery. For the continuous background characteristics, we calculate the p-value from unpaired t-tests for the difference in means across winners and losers (which in small samples requires the maintained assumption of normality of the underlying distribution in the population). Because the Fisher exact p-values are not continuous, these statistics may not be less than 0.05 in five-percent of the cases. Hence, we use a permutation test (whereby participants in a given lottery are repeatedly randomly assigned to winning or losing at the same rates as in the actual lottery) to determine the expected distribution of p-values across the background characteristics and lotteries. Comparing expected and actual distributions of p-values using chi-squared tests, we cannot reject random assignment. This is not surprising given that the lottery outcomes were computer-generated and the output was write-protected to prevent tampering.

demonstrates that the issue of selective attrition in the Chicago data is much less severe than in previous studies, both in terms of overall attrition rates and whether attrition is correlated with characteristics that may influence subsequent achievement. The first column of the table reports ninth grade enrollment rates for lottery losers in square brackets, along with the regression estimates of the impact that winning a lottery has on enrollment. The regression estimates are based on equation (3), using an indicator variable for whether the student enrolls in CPS in ninth grade as the dependent variable.

Winning a lottery to a high-achieving school increases the probability of enrollment by four percentage points (4.6 percent) and winning other lotteries raises enrollment rates by one percentage point, although the latter estimate is not statistically different from zero. While these results demonstrate that there is some differential attrition between winners and losers of lotteries to high-achieving schools, the remaining columns suggest that the differential attrition is not selective – that is, the attrition does not appear to be correlated with observable characteristics that may influence subsequent outcomes. Columns 2-5 show the effects of winning a lottery on prior student achievement and demographics among the sample of students who enrolled in the CPS in 9th grade. If the losers who left the CPS were systematically different than the winners who left, then we would expect the coefficients on winning a lottery to be different than zero. However, the estimates shown in columns 2-5 are statistically insignificant and very small in magnitude.[15] Given the low levels of attrition, it is not particularly surprising that the randomization continues to perform well on the subset of students who enter ninth grade.

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[15] Because of space constraints, we present results for a representative subset of the characteristics included in Table 3. For two of the 20 dependent variables (mean achievement in the 8th grade school and fraction homeowners in 8th grade census tract) , the coefficient on the indicator for winning a lottery to a high-achieving school are significant at the five percent level; none of the 20 coefficients on winning a lottery to other schools are significant at either the five or ten percent levels.

Even if the differential enrollment attrition is minor and not correlated with any observable student characteristics, it is still possible that, conditional on enrollment, winners and losers may have different propensities to have valid outcome data. This might be the case if, for example, lottery losers become discouraged and while still attending school, do not show up to take the standardized achievement exams at a greater rate than lottery winners. To examine this issue of outcome attrition, we estimated models similar to those shown in Table 4 and find no evidence of systematic selection in the presence of missing data. As a final check, we conduct a series of sensitivity analyses and selection corrections, which further confirm that our results are unlikely to be heavily influenced by attrition (see Appendix B for more detail and the relevant regression results).

Section IV: The impact of winning a lottery on student outcomes

To estimate the effect of winning a lottery on student outcomes, we estimate equation (3) using outcome variables on the left-hand-side of the regression. Because the unit of analysis is an application, the coefficient reflects the impact of winning a particular lottery. It is likely that there is substantial heterogeneity in the impact that winning a lottery will have on student outcomes, depending for instance on the student's next best schooling option. We begin by focusing on the average impact of winning across all participants in the lotteries at the two types of schools, and then attempt to identify heterogeneous treatment effects as a function of a variety of observable student characteristics

Table 5 presents results where the outcome measures are characteristics of the high school the student attends. Knowing how the schooling environment differs between winners and losers helps to establish how the intention-to-treat translates into an average treatment for

winners. Each row of the table presents a separate regression corresponding to the dependent variable named in the first column. The values reported in the second and third columns are the δ coefficients, the standard errors, and the control group means. Note that the specifications reported here include a series of student demographic and prior achievement variables (detailed in the notes) as covariates.

The results demonstrate that lottery outcomes have a substantial impact on the high schools students attend within CPS. In the top panel of Table 5, the dependent variables are a series of indicator variables for the type of school attended. The top row, for instance, shows that winning a lottery to a high-achieving school increases the probability a student attends the school for which the lottery is held by 36.3 percentage points. For lotteries to other schools, winning the lottery increases the likelihood that the student enrolls at that school by 23.5 percent. Note that some students who lose the lottery nonetheless are sometimes able to enroll in the school, although the rates are low (8.2 percent for high-achieving schools, 5.5 percent for other schools). That is because there are sometimes multiple programs offered within a given school, some of which may not be oversubscribed.[16]

Winning a lottery has a smaller effect on whether a student attends any lottery school or any school other than the assigned school. Some of the students who win this particular lottery will also win other lotteries or will apply to schools that do not use lotteries. Winning a lottery to a high-achieving school is positively related to attending high-achieving schools. Winning other lotteries is weakly negatively related to attending high-achieving schools, presumably because the student sometimes accepts the lottery-generated spot at the low-achieving school even though there was the option of attending a different, high-achieving school. Winning either type of

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[16] Unfortunately, we only know the school in which a student is enrolled, and not the particular program within the school. Another channel through which lottery losers could gain access to the school is through the discretion that

lottery reduces the likelihood a student will attend either of the sought after types of schools that do not use lotteries: selective admissions schools and career academies.

The bottom panel of Table 5 explores the impact that winning a lottery has on the characteristics of the school that a student attends (regardless of whether the student actually chooses to go to the school at which he or she wins the lottery). On average, students winning lotteries to high-achieving schools attend schools with peers who test substantially higher on standardized tests, have lower rates of poverty as measured by free lunch eligibility, and have higher graduation rates.[17] The schools attended are in higher income and lower crime neighborhoods. Thus, on a wide range of dimensions that might be expected to reflect school quality, lottery winners go to better schools than students entering but losing the same lottery to high-achieving schools. Winning a lottery to other schools also increases the average characteristic of the school attended, but generally by a much smaller margin.

One would expect that lottery winners would experience improved academic outcomes, both because they are attending higher quality schools on average and because their choice set is increased. To the extent that there are idiosyncratic features of a student-school match that the student observes, but we do not, winning a lottery may improve student outcomes even without a change in our proxies for school quality.

Table 6 presents surprising results to the contrary. Each row of Table 6 corresponds to a different regression with a traditional measure of school performance on the left-hand side and standard errors clustered to account for the correlation among students within the same 9th or 10th grade high school. The specifications estimated are otherwise identical to those in Table 5. Overall, we find no evidence that students winning lotteries to select schools perform better on

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principals have to admit a small number of students outside of the lottery process.
[17] Table 7 also reveals that winners in high-achieving school lotteries are much more likely to have ready access to

any of several academic achievement measures. Indeed, students winning lotteries to high-achieving schools perform slightly worse on four of the six standardized tests administered in ninth and tenth grade, although the difference is only statistically significant for 10th grade reading scores.[18] There are also no significant differences between lottery winners and losers at high-achieving schools on dropout rates through tenth grade, absences, or course credits. Students winning lotteries to other schools also appear to perform no differently than lottery losers, with only one statistically significant exception (9th grade reading) in which the lottery winners perform worse.

Despite the limited size of many of the lotteries, the estimates are reasonably precise. The test of 9th grade reading scores, for example, is able to detect effects of 1.4 percentage points in comparison with a control group mean of 46.8. Given that the point estimates for many of the test score outcomes are negative, the 95% confidence intervals around the estimate suggest it is highly unlikely that winning a lottery to a magnet school has a positive effect of any substantive magnitude. The minimum detectable effects for 9th grade absences and credits are 0.82 and 0.32 respectively. Given the standard deviation of these outcomes among the control group is 11.5 and 2.5 respectively, the power of the estimates for the transcript outcomes is also high.

Table 7 analyzes the sensitivity of our findings to alternative specifications, choices of sample, and school classifications. Our baseline results are reprinted in the first column. The second column re-estimates the results using a student as the unit of observation instead of an application. In this column, instead of including lottery fixed-effects as controls, we include

computers. This suggests that winners are exposed to higher levels of school resources.
[18] We have also explored possible non-linearities in the impact of winning a lottery by looking at whether winning a lottery affects the likelihood that a student will perform in the top or bottom quartile of the standardized test distribution. We find no systematic impact of winning a lottery on whether a student tests in the bottom quartile. We do, however, find that high-achieving school lottery winners are systematically *less* likely to test in the top quartile on standardized tests, with the results statistically significant at the .05 level on two tests and at the .10 level on a third test.

dummies for each possible combination of lotteries to which a student could apply. None of the results are materially affected, which is not surprising given that the modal student in our data applies to only one lottery in our sample. Excluding small lotteries has no impact on the estimates. When we make our definition of high achieving schools stricter, the impact on a student's peers is greater, and reductions in arrest rates are particularly large, but other outcomes measures are unaffected. Correspondingly, when we relax the requirements for being classified as high achieving, the opposite pattern emerges.

The final three columns are particularly noteworthy. Given that students may be applying to specific magnet programs within high schools, it is possible that students are selecting programs that match their academic goals in ways that would not be expected to show up in higher academic achievement. To test this hypothesis, we divide the lottery high schools according to the types of programs that are offered. Consistent with our earlier findings, students winning lotteries at schools with a math focus do not obtain higher math scores (and obtain lower reading scores). And, those winning lotteries at schools with a reading focus actually score lower than losers on reading exams, while performing no worse on math exams.

Even if the aggregate effects are zero, it is possible that winning a lottery has a positive effect for some students. For example, in a series of voucher experiments, Peterson et. al. (1998) find that the opportunity to attend a private school increases student achievement for low-achieving African-American students, but not others (see also Howell and Peterson 2002). To explore this possibility here, Table 8 examines how the reduced-form results differ across sub-groups, with each column representing a different subset of the student population.[19] The first column presents results for the full sample as a baseline. The other columns show results for Blacks, Hispanics, Males, those with below average eighth grade test scores, relatively long trips

to school, and students with low quality schools as their next best academic option within CPS. Not surprisingly, those students with low eighth grade test scores and bad outside options show the greatest responsiveness in terms of attendance decisions to winning a lottery and also experience the greatest increases in average peer test scores. Yet, there is no systematic pattern of positive achievement effects for any of the groups. None of the subsets exhibit any test score benefits in 9th grade. Hispanic lottery winners have fewer absences than their peers in 9th grade, but actually score lower on the reading exam.[20] Hispanics and males do score significantly higher than other students on geometry in 10th grade, but show no difference on the reading and English exams.

Given the absence of systematic academic benefits to students attending lottery schools, why is it that competition for entry is so intense? One simple explanation is that, absent good information about the education production function, families base their enrollment choices on school outputs. The most sought after schools in our sample do have higher outputs, but apparently due to better inputs, rather than extra value added. School quality is likely to be more of a blunt instrument for improving a given student's outcome than parents perceive. For example, if we assume that the benefits of winning a lottery operate entirely through increasing the quality of one's peers, and assume a peer effect of 0.1-0.2, this suggests that that the 4.2 percentage point increase in peer 8th grade test scores associated with winning a lottery to a high-achieving school would translate into a roughly 0.4 – 0.8 percentage point effect on a 9th grade achievement score.[21] This is a small effect that we might not be able to detect. Note, however,

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[19] Full results for all outcome measures in all sub-groups are available on request from the authors.

[20] None of the results presented in Table 8 are the result of selective attrition of lottery losers for a particular subgroup. Attrition estimates by subgroup are available from the authors upon request.

[21] The range of peer effects reported in prior research, even among the set of reasonably well-constructed studies, is quite large (Lefgren 2002). A broad reading of the literature suggests that 0.1-0.2 is a mid-range estimate. A recent study by Hanushek, Kain, Markman and Rivikin (2001) that examines public school students in Texas that utilizes student fixed effects and detailed administrative data finds estimates on the order of 0.1.

that for some subgroups (i.e. those with poor schooling alternatives) the change is much larger and that, for all students, lottery success changes a variety of dimensions of the student's high school experience at the same time, not just peer quality.

Even if the lottery winners are, on average, attending higher quality schools, other factors may mitigate any achievement benefits they receive. One possibility is that winners might have to travel greater distance to attend school than losers, and this reduces the time they can spend studying.[22] Using the student home addresses provided in school records, we can measure the distance each student travels to school. The estimates on the bottom row of Table 5 indicate that students winning lotteries only travel an extra half-mile school on average compared with their peers who lose the lotteries. Given the number of choice options throughout the district, this is not necessarily surprising, but does suggest that extra travel time cannot explain the lack of apparent achievement effects.

Another possibility is that school quality and parental involvement are substitutes in the education production function. For example, parents whose children win lotteries to select magnet schools may feel less need to carefully monitor their children's academic progress or assist their children with their schoolwork. While such behaviors are generally difficult to measure, a survey administered to one cohort in our sample when these students were in the 9th grade affords some insight. Students were asked a series of questions that capture both parental support of student learning and the level of parental supervision of their school and non-school activities.

The top panel of Table 9 provides evidence that some parental behaviors are indeed substitutes for school quality. The structure of the table is identical to the preceding tables,

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[22] Of course, parents and students should factor this additional time cost into their decision to apply for and attend a choice school. It is possible, however, that ex-ante, parents and children underestimate the extent of this cost.

except that the dependent variables are taken from student survey responses to a wide range of questions. The sample is restricted to students in our 2000 cohort that applied to schools that administered the survey.[23] The top rows in the table reveal that parents of lottery winners to high-achieving schools are less likely to help with homework or check whether the student has completed homework. However, these same parents are actually more likely to discuss school-related activities with the student, including homework. Finally, the parents of lottery winners do not appear to exert a greater or lesser degree of supervision over their children. While a benefit of school choice may be that parents of lottery winners trade less enjoyable for more enjoyable interactions with their children, it is not clear that home inputs are lowered in an absolute sense, so that it is unlikely home efforts are completely undoing school efforts.

Policymakers generally view open enrollment or other forms of school choice as a lever to raise academic performance. However, it is possible that parents and children seek alternative schooling environments for other reasons. Parents might be interested in ensuring a safer or more nurturing environment for their children; students may be interested in attending particular schools for extracurricular activities or for a different peer group. While many of these reasons may lead to improved life outcomes in the long-run, they are less likely to influence traditional academic achievement measures in the short-run. In this case, however, we would expect school choice to affect measures of school satisfaction, safety or expectations for the future.

The remaining rows of Table 9 examine the effect of winning a lottery on a variety of non-traditional student outcome measures. The results present a somewhat more optimistic picture for open enrollment.[24] Students winning lotteries to high-achieving schools are

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[23] Three schools (Von Steuben, Lake View, and Roosevelt) did not administer the survey.
[24] As a sensitivity test, we re-estimated the results shown in Tables 5 and 6 for this restricted "survey sample" and obtained comparable results (which are available from authors upon request), indicating that the more positive estimates for non-traditional outcomes are not simply a function of the different sample used in these estimations.

significantly less likely to report that they were subject to disciplinary action at school, and much less likely to say they have been arrested. Self-reported arrest rates for students winning lotteries to high-achieving schools are less than half as high (3.9 percent) as for students losing such lotteries (8.9 percent). However, these students are not significantly more likely to expect to graduate college, enjoy school, have positive interactions with peers or teachers, or feel safe at school. And, winning a lottery to a non-high achieving school does not appear to provide any measurable benefits, even on these non-traditional outcome measures. Overall, these data present only a limited case for public school choice conferring non-academic benefits to the typical participant.[25]

VI. Conclusions

This paper uses lottery outcomes to estimate the causal impact on student outcomes of access to public school magnet schools and programs. Although students often take advantage of winning a lottery by attending that school, and on average the schools lottery winners attend are better on observable dimensions than the schools attended by lottery losers, we observe no difference between winners and losers on traditional outcome measures such as test scores or school attendance. This is true for a variety of subgroups of students and does not depend on the program offered in the school. This does not appear to be due to winners traveling greater distances to school or because of compensating behavior on the part of parents. We do, however, find some evidence that winning a lottery is associated with positive outcomes on certain non-academic measures, namely self-reported disciplinary problems and arrests.

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[25] Appendix C presents estimates of several representative non-traditional outcome measures separately by student sub-group. These outcomes follow a pattern similar to those of the traditional achievement outcomes shown in Table 8. Estimates for all of the non-traditional outcomes separately by sub-group are available from the authors upon request.

Our finding concerning the absence of a positive impact of school choice and high-quality peers on traditional student outcomes stands in contrast with most, but not all, studies on the topic. In particular, two previous studies using CPS data obtain similar results. Cullen, Jacob, and Levitt (forthcoming), using distance from a student's residence to schools other than the assigned school as an instrument for attending a choice school, find no evidence that choice is associated with increased educational attainment, with the exception of career academies. Lefgren (2002), exploiting differences in classroom peer quality that arise based on the extent of tracking, finds little or no benefit from attending a school with high-achieving peers.

Our findings present a mixed picture for the potential gains from school choice in urban districts. If the primary goal is to improve measures of academic achievement and attainment, then it does not appear that this mechanism is effective. The findings are consistent with an even stronger conclusion that attending "better" schools as measured by a variety of level measures of student performance does not systematically improve short-term academic outcomes. However, open enrollment in the CPS may confer other benefits that are equally worthy, including matching idiosyncratic tastes of parents and students and improving social circumstances.

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Appendix A: Sample and Data Sources

CPS provided information on applications submitted in the spring 2000 and spring 2001. The sample of applications provided to us for eighth-grade applicants enrolled in CPS at the time of application includes 59,962 applications to 45 choice high schools. Approximately one-third as many applications are submitted to the selective enrollment high schools that do not use lotteries in admissions, none of which are included in our analysis. Although CPS has information on the applications submitted to 45 schools, it only tracks lottery outcomes for the 27 high schools for which the district manages the lottery process. There are a variety of idiosyncratic reasons why CPS manages the lottery for some high schools, and these schools are not systematically different from the schools that manage their own.

There are a total of 26,805 applications from 17,582 students for which we have lottery outcomes. Of the 375 lotteries represented by these lotteries, 10 lotteries have no winners and 171 have no losers. Lotteries that are not oversubscribed will not have any losers. A lottery will not have any winners if parents mistakenly submitted an application to a school-program-grade that was not accepting any students in a given year (because of space constraints) or if changing enrollment numbers led schools to not accept new students, even though application brochures had indicated that the school would have open slots. Since we cannot estimate any treatment effects from these degenerate lotteries, we exclude these from our analysis. Restricting our attention to lotteries that have winners and losers, our analysis sample includes 19,520 applications from 14,434 students participating in 194 lotteries at 19 different high schools.

Our school-level and student-level data come from a variety of sources described in Table A1 below.

Table A1: Data sources and variable construction

Data	Source	Construction
Academic Outcomes	CPS Board	Standardized test scores, grades, absences, credits and course taking outcomes are taken directly from student test and transcript files provided by the Board. Information on enrollment and exit status/reason is from administrative records provided by the Board. Various fields in these data allow us to determine the reason why a student has exited (is not enrolled in) the public school system, including moved out of the district, transferred to private school, graduated, dropped out, etc. 9th and 10th grade reading scores come from the Test of Academic Proficiency (TAP), a nationally-normed standardized achievement exam published by Riverside, and are measured in terms of national percentile rank. The end-of-course exams (algebra, geometry, English I, English II) were developed specifically to coincide with the Chicago high school curriculum, and consisted of multiple-choice as well as open-response items. We use only the multiple-choice items, which were graded electronically by a scanning machine. The test score is measured as a fraction of the items answered correctly. Absences are defined as the average number of days absent across courses for the Spring semester of a given year. Total credits are defined as the sum of all credits earned in the Spring semester of a given year (students receive credits if they do not fail the course – i.e., earn any grade above F).
Non-Traditional Outcomes	CCSR	The Consortium on Chicago School Research (CCSR) administered a survey to CPS students in grades 6-10 in Spring of 2001, which asked students a variety of questions about their teachers, schools and peers as well as about their own attitudes and behaviors relating to school. Several of the survey outcomes we use are composite measures created by CCSR from student responses to a collection of individual items (see the notes associated with Table 7). Greater detail on the construction of these items is available from CCSR.
Student Demographics	CPS Board	Student demographic variables (race, gender, age) come directly from student records provided by the Board. All of the demographics are based on status as of 8th grade. Special education status covers a variety of disabilities ranging from mild learning disabilities to severe physical handicaps. 7th and 8th grade achievement scores come from the Iowa Test of Basic Skills, a nationally-normed standardized achievement exam published by Riverside, and are measured in terms of the student's national percentile rank.
Neighborhood Characteristics	2000 Census &	Basic information on the student's census tract, such as median household income and percent below the poverty line,

	CPS Board & CCSR	comes from the 2000 Census. Student census tract was determined on the basis of student address, which is contained in the CPS school records. The crime composite is an index created by factor analysis using official crime statistics for 1994 provided by the Consortium on Chicago School Research (CCSR). The index was created at the block group level. The variable used in this analysis is a tract-level average (for the student's tract in the Spring of 8th grade), weighted by the total population in each block group.
Distance from home to school	CPS Board	Student and school census tracts were determined based on address information provided in CPS records. Distance from home to school was calculated as the distance from the centroid of home tract to the centroid of school tract.

Appendix B: Testing for Attrition Bias

Although we have demonstrated that there is little evidence of selective attrition at the point of enrollment in 9th grade (see the discussion in the text and Table 4), there are other points at which students can be absent from our sample. Outcome data in the springs of the first and second years may be missing if a student has dropped out or does not attend on the day of the exam. Thus, if winning a lottery has positive (negative) attainment and attendance effects, winners may be more (less) likely to continue in our sample. Table B1 provides information on the rates at which outcome data are missing in 9th and 10th grade, and whether winners and losers with outcome information are systematically different according to predetermined characteristics. Even when data are missing at statistically significantly different rates for winners and losers, the differences in these rates are not large in practical terms. Further, winners and losers with outcome information continue to appear comparable along important observable dimensions.

The fact that differential attrition over the first two years of high school is not great is further evidence that winning does not confer attainment gains. An interesting exception is the finding that winners of lotteries to high-achieving schools are significantly less likely to be missing English II scores in 10th grade (but not TAP reading scores), which implies that they are more likely to take this course. We have found that this apparent course-taking effect is greatest for Hispanic students, implying that lottery losers with limited English proficiency might be taking less rigorous courses.

To further support the proposition that selective attrition is unlikely to introduce much bias to our estimates, Table B2 examines the sensitivity of our results to a variety of possible sample selection correction methodologies. We present results only for ninth grade reading

scores; other outcome variables yield similar results. In all cases, other than when we construct

worst-case bounds, we can rule out effects of winning the lottery that are of any meaningful

magnitude.

Table B1: Additional evidence on sample attrition

Panel A – Outcome Attrition in 9th Grade

	Missing Any Outcome Data	Missing Trans. Data	Missing Read Score	Missing Algebra Score	Missing English I Score
Win lottery to high-achieving school	-0.044 (0.019) [0.305]	-0.005 (0.010) [0.060]	-0.001 (0.013) [0.106]	-0.025 (0.018) [0.225]	-0.029 (0.017) [0.204]
Win lottery to other choice school	-0.010 (0.015) [0.383]	-0.014 (0.008) [0.072]	-0.001 (0.011) [0.166]	-0.008 (0.014) [0.289]	-0.010 (0.014) [0.272]
Number of observations	17,470	17,470	17,470	17,470	17,470
Sample limited to those who are enrolled in 9th grade in the CPS in the Fall	Yes	Yes	Yes	Yes	Yes

Panel B – Is Outcome Attrition in 9th Grade Selective?

	8th Grade Reading	8th Grade Math	Free or reduced priced lunch	Special Educ.	Tract Pov. Rate
Win lottery to high-achieving school	-0.004 (0.011) [0.554]	0.007 (0.011) [0.602]	0.012 (0.018) [0.792]	0.011 (0.012) [0.067]	-0.003 (0.006) [0.206]
Win lottery to other choice school	-0.004 (0.008) [0.448]	-0.008 (0.009) [0.496]	0.002 (0.013) [0.899]	0.005 (0.012) [0.095]	0.005 (0.004) [0.237]
Number of observations	11,389	11,396	11,462	11,462	11,452
Sample limited to those who have outcome data (all samples conditional on enrollment in the 9th grade in the CPS in the Fall)	Yes	Yes	Yes	Yes	Yes

Panel C – Survey Attrition [a]

	Took Survey	8th Grade Reading	8th Grade Math	Free or reduced price lunch	Special Educ.	Tract Pov. Rate
Win lottery to high-achieving school	-0.010 (0.032) [0.612]	-0.015 (0.018) [0.560]	-0.014 (0.019) [0.629]	-0.020 (0.026) [0.835]	0.027 (0.022) [0.053]	-0.012 (0.009) [0.228]

Win lottery to other choice school	0.091 (0.023) [0.542]	-0.004 (0.013) [0.438]	-0.002 (0.014) [0.499]	0.020 (0.019) [0.899]	-0.002 (0.019) [0.107]	0.003 (0.006) [0.232]
Number of observations	5,234	2,926	2,926	3,015	3,015	3,013
Sample limited to those who took survey (all samples conditional on enrollment in the 9th grade in the CPS in the Fall)	No	Yes	Yes	Yes	Yes	Yes

Panel D – Outcome Attrition in 10th Grade (2000 Cohort Only)

	Missing Any Outcome Data	Missing Trans. Data	Missing Read Score	Missing Geometry Score	Missing English II Score
Win lottery to high-achieving school	0.009 (0.031) [0.389]	-0.008 (0.009) [0.028]	-0.016 (0.020) [0.135]	0.039 (0.029) [0.218]	-0.068 (0.022) [0.174]
Win lottery to other choice school	-0.026 (0.024) [0.334]	-0.012 (0.009) [0.035]	-0.002 (0.019) [0.206]	-0.022 (0.021) [0.251]	0.014 (0.021) [0.218]
Number of observations	7,471	7,471	7,471	7,471	7,471
Sample limited to those who are enrolled in the CPS in the Spring of 10th grade	Yes	Yes	Yes	Yes	Yes

Panel E – Is Outcome Attrition in 10th Grade Selective? (2000 Cohort only)

	8th Grade Reading	8th Grade Math	Free or reduced priced lunch	Special Educ.	Tract Pov. Rate
Win lottery to high-achieving school	0.013 (0.016) [0.5563]	0.014 (0.015) [0.631]	0.002 (0.034) [0.682]	-0.003 (0.017) [0.058]	-0.012 (0.009) [0.198]
Win lottery to other choice school	0.003 (0.013) [0.449]	-0.003 (0.014) [0.514]	-0.011 (0.026) [0.811]	-0.009 (0.017) [0.095]	0.004 (0.006) [0.231]
Number of observations	4,747	4,744	4,783	4,783	4,780
Sample limited to those who have outcome data (all samples conditional on enrollment in the CPS in the Spring of 10th grade)	Yes	Yes	Yes	Yes	Yes

Notes: All regression estimates include a set of lottery fixed effects, but no other covariates. Eiker-White robust standard errors adjusted to account for the correlation of errors across applications within a single student are shown in parentheses. Control group means (i.e., the means for students who lost these lotteries) are shown in square brackets. [a] The sample in Panel C includes only students in the 2000 cohort and excludes students who applied to three schools (Von Steuben, Roosevelt and Lake View) that did not administer the survey.

Table B2: Sensitivity of reduced form reading score estimates to alternative selection corrections

	The effect of winning a lottery to a …	
	High-Achieving School	Other Choice School
Baseline Estimates	-0.010	-0.013
	(0.007)	(0.005)
Generic Bounding		
Assign students with missing outcome data a score equivalent to the 10th percentile of the sample, regardless of whether the student won or lost the lottery.	0.002	-0.006
	(0.008)	(0.005)
Assume a score comparable to the 25th percentile	-0.003	-0.007
	(0.007)	(0.004)
Assume a score comparable to 75th percentile	-0.016	-0.011
	(0.007)	(0.005)
Assume a score comparable to 90th percentile	-0.022	-0.013
	(0.008)	(0.007)
Assign students with missing outcome data a score equivalent to what they would have scored if they had made <u>gains</u> comparable to the 10th percentile of the sample, regardless of whether the student won or lost the lottery.	0.000	-0.009
	(0.007)	(0.005)
Assume gains comparable to the 25th percentile	-0.004	-0.010
	(0.006)	(0.004)
Assume gains comparable to the 75th percentile	-0.013	-0.013
	(0.006)	(0.004)
Assume gains comparable to the 90th percentile	-0.016	-0.014
	(0.007)	(0.005)
Lee's Trimming Method (Lee 2002)		
Trim the worst X% of winners in lotteries where X% more losers are missing outcome data (if X<0, do nothing). This assumes that, among those who lose the lottery, the lowest-achieving students will leave the system.	-0.005	-0.007
	(0.007)	(0.005)
Trim the best X% of winners. This assumes that, among those who lose the lottery, the highest-achieving students will leave the system.	-0.022	-0.022
	(0.007)	(0.005)
Control Function Method		
Includes a cubic in the percentage point difference in the enrollment rate between winners and losers in a particular lottery along with interactions between these measures and an indicator for whether the student won the lottery. D=mean enrollment rate among losers – mean enrollment rate among winners. S=indicates whether the student won the lottery. Control function variables include D, D^2, D^3, S*D, S*D^2, S*D^3. Because D is the difference and we want to evaluate the function at zero, we can simply read the coefficient on the main effect for S.	-0.011	-0.013
	(0.008)	(0.005)
Same as above except that we allow the D and S*D variables to vary across good and other lotteries (basically the same as running these applications differently).	-0.016	-0.011
	(0.009)	(0.006)
Heckman Selection Model		
Instrument: Distance from home to application school (F-stat = 9.06 for predicting 9th grade enrollment but only 3.61 for predicting missing reading score in 9th grade)	-0.000	-0.009
	(0.009)	(0.006)
Instrument: % of 9th –12th grade children in the student's census	-0.001	-0.009

tract who are enrolled in private schools (F-stat = 5.76 for predicting 9th grade enrollment but only 1.44 for predicting missing reading score in 9th grade)	(0.009)	(0.006)

Worst Case Bounding

For students with missing outcome data who lost the lottery, assume that the student would have made gains comparable to the bottom 10th percentile of the sample. For students with missing outcome data who won the lottery, assume that the student would have made gains comparable to the top 10th percentile of the sample.	0.076 (0.007)	0.096 (0.098)
Vice versa	-0.093 (0.007)	-0.120 (0.005)
Missing losers set to bottom 10% of scores and missing winners set to top 10% of scores	0.119 (0.009)	0.158 (0.007)
Vice versa	-0.140 (0.009)	-0.177 (0.006)

Notes: Lee's method requires an assumption of monotonicity, which may not hold in our context. Our implementation assumes that winning a lottery can only make an individual more likely to have non-missing outcome data. For more detail on the methodology, see Lee (2002).

Appendix C: The impact of winning a lottery on non-traditional measures of student outcomes by student characteristic

Dependent Variable	The effect of winning a lottery to a high-achieving school for the following subgroups of students…						
	All students (i.e., those applying to high-achieving school) (n=8,448)	Black (n=3,568)	Hispanic (n=3,146)	Male (n=3,512)	Below average 8th grade test scores (n=3,284)	Home-School Distance > 2 miles (n=4,143)	Alternative school has scores below national norms (n=4,735)
Likes school	0.148	0.365	0.143	-0.150	-0.348	0.284*	-0.084
	(0.125)	(0.333)	(0.135)	(0.254)	(0.299)	(0.150)	(0.175)
	[4.14]	[3.96]	[4.36]	[3.90]	[4.35]	[4.09]	[4.28]
Arrested by police in past year	-0.050**	-0.038	-0.054**	-0.070*	0.038	-0.037	-0.022
	(0.024)	(0.054)	(0.025)	(0.042)	(0.072)	(0.026)	(0.031)
	[0.089]	[0.086]	[0.094]	[0.129]	[0.115]	[0.073]	[0.087]
School has enough computers	0.119**	0.183**	0.089**	0.256**	0.058	0.137**	0.116**
	(0.030)	(0.076)	(0.028)	(0.056)	(0.068)	(0.057)	(0.039)
	[0.643]	[0.669]	[0.611]	[0.610]	[0.629]	[0.664]	[0.633]

Notes: The specifications here mirror those shown for the traditional outcome measures in Table 8.

Table 1. Chicago public high schools represented in the analysis

School Name	Achievement Rank (percentile) Among CPS High Schools	Number of Analysis Lotteries	Number of Lottery Participants	Percentage of Participants Selected	Percentage of Winners Enrolling in the School	Percentage of Students Black or Hispanic	Percentage of Students Receiving Special Education
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Chicago Agricultural	90.0	9	627	17.7	53.6	73.7	12.4
Von Steuben	89.8	15	5,888	2.9	49.4	44.5	10.2
Curie Metro	78.7	56	898	18.7	64.7	78.1	14.2
Hyde Park Academy	75.3	5	1,243	9.1	41.1	99.4	18.7
Kennedy	74.6	7	817	29.2	28.2	55.9	20.0
George Washington	72.4	5	355	53.0	18.6	85.9	21.2
Taft	72.3	16	1,881	27.7	22.8	53.4	17.7
Lake View	70.9	9	144	35.1	6.0	77.8	15.9
Bogan Tech	66.1	12	3,289	15.3	38.7	86.3	20.3
Amundsen	59.3	6	522	5.4	42.9	60.8	18.7
Senn	46.2	11	831	18.4	14.7	75.0	20.4
Juarez	45.0	5	241	19.5	12.8	97.2	18.4
Roosevelt	39.4	16	860	13.8	13.8	69.9	21.6
Wells	35.4	7	654	39.8	24.7	92.4	23.3
Hirsch Metro	31.4	2	240	52.1	32.0	100.0	20.4
Corliss	24.8	2	365	43.3	25.9	99.9	30.7
Robeson	10.0	2	131	24.4	9.4	99.8	29.4
Harper	6.2	7	366	10.5	8.6	100.0	28.0
Orr	2.5	2	168	18.5	22.6	99.6	27.7

Notes: These summary statistics are based on the combined 2000 and 2001 applications. Student achievement is measured as composite 8th grade math and reading scores (expressed as national percentile ranks) among 9th graders in the school. The school's achievement rank is calculated as its percentile in the distribution of regular (i.e. non-alternative) high schools, where the distribution is weighted by 9th grade enrollment at each high school. The values in columns 3 and 4 are totals across the two years. The values in columns 2 and 5-8 are weighted averages of the 2000 and 2001 values, where the weights are proportional to the number of applications received by the school in those years. The schools that are shaded are classified as high-achieving schools.

Table 2. Comparison of lottery participants to other CPS 8th graders

Student Characteristic	Lottery Participants (1)	Other Students (2)	Difference (1) – (2)	Standard Error of Difference
White	0.119	0.109	0.010	0.003
Black	0.460	0.535	-0.075	0.005
Hispanic	0.370	0.326	0.043	0.004
Male	0.423	0.531	-0.107	0.005
8th gr. math national percentile score	0.526	0.425	0.101	0.003
8th gr. reading national percentile score	0.485	0.401	0.083	0.002
Free lunch eligible	0.725	0.752	-0.027	0.004
Receiving special education	0.116	0.220	-0.104	0.004
Ever received bilingual education	0.432	0.352	0.080	0.005
Living with a biological parent	0.800	0.794	0.006	0.004
Tract poverty rate	0.218	0.248	-0.029	0.001
Tract fraction HS graduates	0.646	0.639	0.007	0.001

Notes: The unit of observation is the student. There are 14,434 students participating in at least one of the lotteries included in our analysis. Mean characteristics for lottery participants are shown in column 1. There are 46,748 other 8th graders enrolled in CPS in Spring 2000 and Spring 2001. Mean characteristics for these students are shown in column 2. Alternative schools include special education schools, juvenile detention centers, and alternative education programs for pregnant girls.

Table 3. The validity of the lotteries

Dependent Variable	The effect of winning a lottery to …	
	High-Achieving School	Other Choice School
Student's characteristics at time of application		
Math percentile score	0.011	-0.006
	(0.009)	(0.007)
	[0.578]	[0.456]
Reading percentile score	0.000	-0.005
	(0.009)	(0.006)
	[0.535]	[0.417]
Age	0.02	-0.00
	(0.02)	(0.01)
	[13.92]	[13.99]
Free lunch eligible	0.025	-0.005
	(0.017)	(0.012)
	[0.667]	[0.808]
Reduced-price lunch eligible	-0.011	0.006
	(0.013)	(0.008)
	[0.121]	[0.091]
Receiving special education	0.006	0.011
	(0.013)	(0.010)
	[0.100]	[0.133]
Ever received bilingual education	0.007	-0.011
	(0.015)	(0.009)
	[0.497]	[0.404]
Living with a biological parent	0.016	-0.010
	(0.016)	(0.011)
	[0.785]	[0.814]
Attends assigned 8th grade school	0.012	0.001
	(0.019)	(0.014)
	[0.589]	[0.644]
Number of applications submitted	0.01	-0.00
	(0.07)	(0.06)
	[3.01]	[3.82]
School and neighborhood characteristics at time of application		
Mean achievement level in school	0.010*	-0.001
	(0.006)	(0.004)
	[0.467]	[0.384]
Fraction transferring into school	-0.010	-0.002
	(0.013)	(0.008)
	[0.355]	[0.305]
Tract fraction Black	-0.004	0.006
	(0.008)	(0.006)
	[0.357]	[0.497]
Tract fraction Hispanic	-0.002	-0.005
	(0.008)	(0.006)
	[0.319]	[0.319]
Tract poverty rate	-0.002	0.003
	(0.005)	(0.003)
	[0.207]	[0.238]
Tract fraction HS graduates	0.002	0.001
	(0.005)	(0.003)
	[0.662]	[0.630]

Tract fraction homeowners	0.018**	-0.001
	(0.008)	(0.005)
	[0.422]	[0.416]
Tract fraction not-in-the-labor force	0.000	0.003
	(0.003)	(0.002)
	[0.403]	[0.422]
Tract crime index	0.008	0.015
	(0.022)	(0.014)
	[-0.115]	[0.074]
Tract fraction in private high schools	0.009	-0.002
	(0.006)	(0.003)
	[0.163]	[0.124]

Notes: The sample consists of 19,520 applications involved in the 194 lotteries with both winners and losers. The coefficients on indicators for the application being selected in a lottery at a high-achieving and at a non-high-achieving school are reported. The coefficients are estimated from ordinary least squares specifications that also include the full set of individual lottery fixed effects, with the dependent variable as indicated in each row and panel. Robust standard errors that account for correlation across applications by the same student are reported in parentheses. * = significance at the 10% level; ** = significance at the 5% level.

Table 4. How much lottery-driven attrition exists and is it correlated with observable student characteristics?

	Dependent Variables					
	Enrolled in CPS in the 9th grade in the Fall	8th Grade Reading	8th Grade Math	Free or reduced priced lunch	Special Educ.	Tract Pov. Rate
Win lottery to high-achieving school	0.041 (0.011) [0.890]	0.001 (0.009) [0.537]	0.009 (0.009) [0.580]	0.009 (0.014) [0.799]	0.011 (0.012) [0.093]	-0.003 (0.005) [0.209]
Win lottery to other choice school	0.011 (0.009) [0.897]	-0.005 (0.007) [0.420]	-0.007 (0.007) [0.459]	-0.001 (0.009) [0.904]	0.007 (0.010) [0.132]	-0.001 (0.004) [0.240]
Number of observations	19,520	17,024	17,027	17,470	17,470	17,457
Sample limited to those students enrolled in 9th grade in the CPS in the Fall	No	Yes	Yes	Yes	Yes	Yes

Notes: All regression estimates include a set of lottery fixed effects, but no other covariates. Eiker-White robust standard errors adjusted to account for the correlation of errors across applications within a single student are shown in parentheses. Control group means (i.e., the means of students who lost the lotteries) are shown in square brackets. See Tables B1 and B2 for additional evidence on selective attrition.

Table 5. The impact of winning a lottery on the characteristics of the school attended

Dependent Variable	The effect of winning a lottery to …	
	High-Achieving School	Other Choice School
School Type		
School for which lottery applies	0.363	0.235
	(0.020)	(0.013)
	[0.082]	[0.055]
Any lottery school	0.259	0.186
	(0.021)	(0.015)
	[0.305]	[0.286]
Any school other than the student's attendance area school	0.144	0.099
	(0.015)	(0.013)
	[0.714]	[0.669]
Magnet school	0.221	-0.009
	(0.020)	(0.006)
	[0.094]	[0.061]
High-achieving lottery school	0.338	-0.021
	(0.021)	(0.008)
	[0.120]	[0.093]
Any high-achieving school	0.276	-0.045
	(0.018)	(0.012)
	[0.432]	[0.260]
Other lottery school	-0.079	0.207
	(0.011)	(0.014)
	[0.185]	[0.193]
Selective admissions school	-0.022	-0.007
	(0.013)	(0.006)
	[0.188]	[0.057]
Career academy	-0.074	-0.038
	(0.012)	(0.011)
	[0.127]	[0.195]
School Characteristic		
8th grade math scores of 9th grader students in the school (natl. percentile)	0.042	0.008
	(0.005)	(0.003)
	[0.510]	[0.431]
% students at or above norms on high school exams (all students in the school)	0.056	0.010
	(0.007)	(0.005)
	[0.408]	[0.286]
% students receiving free lunch	-0.044	-0.010
	(0.006)	(0.004)
	[0.747]	[0.830]
Graduation rate	0.032	0.010
	(0.004)	(0.003)
	[0.714]	[0.662]

Crime in neighborhood of the school	-0.261	-0.159
	(0.042)	(0.027)
	[0.309]	[0.184]
Ln Median HH Income in neighborhood of the school	0.008	0.076
	(0.017)	(0.012)
	[10.5]	[10.4]
Distance traveled to school (in miles)	0.52	0.47
	(0.10)	(0.07)
	[2.66]	[2.38]

Notes: All regressions include a set of lottery fixed effects as well as student characteristics (black, white, Hispanic, male, age, 8th grade math score, 8th grade reading score, 7th grade math score, 7th grade reading score, four indicators for missing 7th or 8th grade achievement scores, living with biological parent in 8th grade, attending attendance area school in 8th grade, free lunch eligible, reduced-price lunch eligible, participating in special education program, participating in bilingual program) and neighborhood (census tract) characteristics (crime rate, poverty rate, percent Hispanic, percent black, percent high school graduates, percent home-ownership, percent not in the labor force, percent of 9th –12th graders attending private school). Eiker-White robust standard errors adjusted to account for the correlation of errors across applications within a single student are shown in parentheses. Control group means (i.e., the means of students who lost the particular lottery) are shown in square brackets.

Table 6. The impact of winning a lottery on traditional measures of student outcomes

Dependent Variables	The effect of winning a lottery to …	
	High-Achieving School	Other Choice School
9th Grade Achievement		
Dropped out by Spring of 9th grade	-0.003	0.001
	(0.004)	(0.004)
	[0.012]	[0.022]
Reading Score percentile	-0.010	-0.013**
	(0.007)	(0.005)
	[0.468]	[0.352]
Algebra End-of-Course Exam	-0.008	0.002
	(0.010)	(0.008)
	[0.492]	[0.390]
English I End-of-Course Exam	0.000	-0.001
	(0.004)	(0.005)
	[0.620]	[0.567]
Absences	-0.04	0.40
	(0.42)	(0.41)
	[7.81]	[10.43]
Total Course Credits	0.03	-0.05
	(0.16)	(0.11)
	[27.8]	[27.5]
10th Grade Achievement [a]		
Dropped out by Spring of 10th Grade	-0.003	-0.007
	(0.007)	(0.008)
	[0.044]	[0.082]
Reading Score	-0.020**	0.000
	(0.009)	(0.009)
	[0.523]	[0.399]
Geometry End-of-Course Exam	0.014	0.012
	(0.017)	(0.007)
	[0.621]	[0.508]
English II End-of-Course Exam	-0.012	0.011
	(0.011)	(0.007)
	[0.548]	[0.481]
Absences	-0.13	0.89
	(0.53)	(0.68)
	[9.19]	[11.69]
Total Course Credits	0.01	-0.43
	(0.15)	(0.33)
	[27.9]	[27.8]

Notes: All regressions include a set of lottery fixed effects as well as student characteristics (black, white, Hispanic, male, age, 8th grade math score, 8th grade reading score, 7th grade math score, 7th grade reading score, four indicators for missing 7th or 8th grade achievement scores, living with biological parent in 8th grade, attending attendance area

school in 8th grade, free lunch eligible, reduced-price lunch eligible, participating in special education program, participating in bilingual program) and neighborhood (census tract) characteristics (crime rate, poverty rate, percent Hispanic, percent black, percent high school graduates, percent home-ownership, percent not in the labor force, percent of 9th –12th graders attending private school). Eiker-White robust standard errors adjusted to account for the correlation of errors within high school are shown in parentheses. Control group means (i.e., the means of students who lost the particular lottery) are shown in square brackets. [a] The following outcomes are limited to the 2000 cohort: reading score, geometry score, English 2 score.

Table 7. Sensitivity to alternative specifications and classifications of schools

	The effect of winning a lottery to a …							
	Baseline (n=8,448)	1 obs. per student	Excluding small lotteries with <50 students (n=7,675)	Very high-achieving school (n=6,581)	Moderately high-achieving school (n=10,566)	Schools with a math/technical focus (n=11,581)	Schools with a reading focus (n=5,288)	School with a vocational focus (n=10,708)
School Characteristic								
Attended high-achieving school	0.276**	0.295**	0.258**	0.335**	0.189	0.051**	-0.029*	0.060**
	(0.018)	(0.020)	(0.021)	(0.022)	(0.015)	(0.013)	(0.016)	(0.013)
	[0.432]	[0.425]	[0.428]	[0.435]	[0.398]	[0.359]	[0.316]	[0.357]
% students at or above norms on high school exams	0.056**	0.060**	0.057**	0.073**	0.040	0.033**	0.026**	0.029**
	(0.007)	(0.008)	(0.008)	(0.009)	(0.006)	(0.005)	(0.006)	(0.005)
	[0.408]	[0.409]	[0.415]	[0.434]	[0.395]	[0.367]	[0.276]	[0.373]
9th Grade Achievement								
Reading Exam	-0.010	-0.009	-0.005	-0.014	-0.011*	-0.011**	-0.018**	-0.017**
	(0.007)	(0.008)	(0.007)	(0.011)	(0.006)	(0.004)	(0.006)	(0.006)
	[0.468]	[0.469]	[0.471]	[0.478]	[0.454]	[0.429]	[0.355]	[0.417]
Algebra End-of-Course Exam	-0.008	-0.011	-0.008	-0.007	-0.007	0.001	-0.001	-0.002
	(0.010)	(0.011)	(0.011)	(0.014)	(0.008)	(0.009)	(0.010)	(0.007)
	[0.492]	[0.494]	[0.499]	[0.510]	[0.482]	[0.463]	[0.382]	[0.448]
Absences	-0.04	-0.04	0.12	-0.19	0.36	-0.10	0.52	0.34
	(0.42)	(0.44)	(0.46)	(0.45)	(0.33)	(0.39)	(0.61)	(0.35)
	[7.81]	[7.84]	[7.76]	[7.44]	[8.20]	[8.60]	[10.10]	[8.98]
10th Grade Achievement								
Reading Exam	-0.020**	-0.025**	-0.017	-0.026*	-0.019**	-0.005	-0.008	-0.008
	(0.009)	(0.011)	(0.010)	(0.014)	(0.009)	(0.008)	(0.012)	(0.006)
	[0.523]	[0.523]	[0.526]	[0.536]	[0.502]	[0.485]	[0.404]	[0.467]
Geometry End-of-Course Exam	0.014	0.018	0.018	0.025	0.010	0.027**	0.013*	0.013
	(0.017)	(0.020)	(0.022)	(0.025)	(0.016)	(0.010)	(0.008)	(0.008)
	[0.621]	[0.622]	[0.626]	[0.637]	[0.604]	[0.586]	[0.503]	[0.569]
Absences	-0.13	0.002	0.092	0.467	0.284	0.840	0.941	0.531
	(0.53)	(0.652)	(0.534)	(0.766)	(0.357)	(0.656)	(1.059)	(0.385)
	[9.19]	[9.21]	[9.14]	[8.58]	[9.51]	[9.73]	[11.50]	[10.21]

Notes: The estimates in column 1 are taken from Tables 5-6 and reflect the coefficient on the variable for winning a lottery to a high-achieving school. (The estimates for winning a lottery to other schools are not shown here, but available upon request. The estimates in column 2 come from the coefficient on an indicator for whether the student won at least one lottery to a high-achieving school in equation (4) described in the text. The estimates in columns 3-4 resemble those from column 1 with the exception that they are estimated for a particular sub-sample of the data. For column 3, note that roughly 1% of applications and 20% of lotteries have fewer than 10 students and roughly 9% of applications and 53% of lotteries have fewer than 50 students. Columns 5-9 present estimates on

the effect of winning a lottery to various school and program types. Very high-achieving schools are defined as those in the three highest-achieving schools in our analysis sample (see Table 1). They are roughly in the top 20% of schools in the CPS and they are also the set of magnet high schools.   Moderately high-achieving schools are defined as those in the top 30% of schools in the CPS, which includes the eight highest-achieving schools from our analysis sample from Table 1. The effect of winning a lottery to other choice schools are not shown here, although the overall pattern of results does not differ considerably from the main effects shown on Tables 5-6.

Table 8. Reduced form estimates by student characteristic

	The effect of winning a lottery to a high-achieving school for the following subgroups of students…						
	All students (i.e., those applying to high-achieving school) (n=8,448)	Black (n=3,568)	Hispanic (n=3,146)	Male (n=3,512)	Below average 8th grade test scores (n=3,284)	Home-School Distance > 2 miles (n=4,143)	Alternative school has scores below national norms (n=4,735)
School Characteristic							
Attended high-achieving school	0.276**	0.276**	0.255**	0.267**	0.425**	0.239**	0.366**
	(0.018)	(0.030)	(0.030)	(0.028)	(0.034)	(0.020)	(0.026)
	[0.432]	[0.394]	[0.402]	[0.431]	[0.173]	[0.603]	[0.239]
% students at or above norms on high school exams	0.056**	0.053**	0.050**	0.055**	0.090**	0.032**	0.079**
	(0.007)	(0.010)	(0.012)	(0.010)	(0.011)	(0.009)	(0.008)
	[0.408]	[0.345]	[0.404]	[0.416]	[0.267]	[0.486]	[0.289]
9th Grade Achievement							
Reading Exam	-0.010	-0.013	-0.018**	0.000	-0.008	0.002	-0.008
	(0.007)	(0.013)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
	[0.468]	[0.410]	[0.473]	[0.473]	[0.279]	[0.514]	[0.335]
Algebra End-of-Course Exam	-0.008	-0.006	-0.009	-0.017	-0.013	-0.010	-0.005
	(0.010)	(0.012)	(0.018)	(0.013)	(0.015)	(0.010)	(0.013)
	[0.492]	[0.430]	[0.494]	[0.506]	[0.371]	[0.527]	[0.400]
Absences	-0.04	1.35*	-1.53**	-0.25	1.39*	0.15	0.88
	(0.42)	(0.72)	(0.44)	(0.64)	(0.76)	(0.42)	(0.68)
	[7.81]	[9.15]	[7.12]	[7.66]	[10.17]	[6.94]	[9.39]
10th Grade Achievement							
Reading Exam	-0.020**	-0.053**	-0.001	-0.008	-0.021	-0.011	-0.006
	(0.009)	(0.018)	(0.012)	(0.011)	(0.017)	(0.012)	(0.016)
	[0.523]	[0.464]	[0.522]	[0.516]	[0.329]	[0.567]	[0.372]
Geometry End-of-Course Exam	0.014	-0.023	0.036**	0.038**	0.056	0.008	0.021
	(0.017)	(0.031)	(0.017)	(0.016)	(0.040)	(0.018)	(0.022)
	[0.621]	[0.554]	[0.616]	[0.638]	[0.473]	[0.662]	[0.508]
Absences	-0.13	0.527	-1.175**	-0.262	1.286	-0.197	0.033
	(0.53)	(0.945)	(0.338)	(0.835)	(1.060)	(0.652)	(0.574)
	[9.19]	[11.45]	[8.05]	[8.91]	[11.98]	[8.57]	[11.15]

Note: The estimates shown above come from specifications identical to those which produced the main effects in Tables 5-6 with the only difference being that they are estimated on specific sub-samples of students. Robust standard errors that account for correlation in errors discussed in previous tables are shown in parentheses. Control group means are shown in square brackets. The home-school distance refers to the distance between the student's 9th grade residence and

9th grade school. The sample median is 2 miles. The measure of 8th grade test scores is a composite of reading and math, and represents students below the 50th percentile on the national achievement distribution. Since we cannot determine definitively which school was the next best alternative for any given student, we use a probabilistic approach to determine the likely achievement level at the school a student would have attended had the student not won the lottery. We calculate the expected peer quality at alternatives to the application school as the average achievement of 9th grade entrants at Fall schools for students of similar ability (in the same quintile according to 8th grade math and reading composite scores) and assigned to the same attendance area high school. The effect of winning a lottery to other choice schools are not shown here, although the overall pattern of results does not differ considerably from the main effects shown on Tables 5-6.

Table 9. The impact of winning a lottery on non-traditional measures of student outcomes

	The effect of winning a lottery to …	
Dependent Variables	High-Achieving School	Other Choice School
Parental Support and Supervision		
Parents help with homework most or all of the time	-0.053*	0.025
	(0.031)	(0.025)
	[0.204]	[0.248]
Parents check if homework is done most or all of the time	-0.075**	0.025
	(0.039)	(0.025)
	[0.385]	[0.392]
Parents regularly discuss courses with student	0.113**	0.012
	(0.037)	(0.030)
	[0.429]	[0.403]
Parents regularly discuss school activities with student	0.074*	-0.067**
	(0.043)	(0.028)
	[0.518]	[0.475]
Parents regularly discuss things child studied in school with student	0.047	0.007
	(0.038)	(0.029)
	[0.521]	[0.472]
Parents regularly discuss homework with student	0.082**	0.032
	(0.040)	(0.029)
	[0.525]	[0.504]
Parents closely supervise the student	0.094	-0.005
	(0.200)	(0.129)
	[6.51]	[6.50]
Other Outcome Measures		
Incidence of disciplinary action vs. student at school	-0.297**	0.058
	(0.146)	(0.152)
	[1.90]	[2.20]
Arrested by police in past year	-0.050**	-0.012
	(0.024)	(0.019)
	[0.089]	[0.127]
Likes school	0.148	-0.063
	(0.125)	(0.111)
	[4.14]	[4.23]
Expects to graduate college	0.011	0.015
	(0.027)	(0.026)
	[0.865]	[0.805]
School safety	0.111	0.115
	(0.196)	(0.104)
	[5.30]	[5.15]

Student-teacher trust	-0.006	-0.016
	(0.097)	(0.087)
	[3.49]	[3.65]
Positive classroom behavior of peers	0.051	-0.123
	(0.092)	(0.094)
	[5.39]	[5.32]
My school has enough computers for students to use (% reporting agree or strongly agree)	0.119**	0.029
	(0.030)	(0.044)
	[0.643]	[0.611]

Notes: The sample is limited to students in the 2000 cohort and excludes students who applied to three schools (Von Steueben, Roosevelt and Lake View) that did not administer the survey. All regressions include a set of lottery fixed effects as well as student characteristics (black, white, Hispanic, male, age, 8th grade math score, 8th grade reading score, 7th grade math score, 7th grade reading score, four indicators for missing 7th or 8th grade achievement scores, living with biological parent in 8th grade, attending attendance area school in 8th grade, free lunch eligible, reduced-price lunch eligible, participating in special education program, participating in bilingual program) and neighborhood (census tract) characteristics (crime rate, poverty rate, percent Hispanic, percent black, percent high school graduates, percent home-ownership, percent not in the labor force, percent of 9th –12th graders attending private school). Eiker-White robust standard errors adjusted to account for the correlation of errors within high school are shown in parentheses. Control group means (i.e., the means of students who lost the particular lottery) are shown in square brackets. The composite survey measures are all continuous variables with the following standard deviations: 2.25 for parent supervision; 1.87 for likes school, 1.98 for school safety, 1.47 for student-teacher trust, 1.10 for peer behavior and 2.02 for disciplinary action.