The Effect of School Choice on Student Outcomes:

Evidence from Randomized Lotteries*

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School choice has become an increasingly prominent strategy for enhancing 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. Compared to those losing lotteries, students who win attend better high schools along a number of dimensions, including higher peer achievement levels, higher peer graduation rates, and lower levels of poverty. Nonetheless, we find little evidence that winning a lottery provides any benefit on a wide variety of traditional academic measures such as graduation, standardized test scores, attendance rates, course-taking, and credit accumulation. Lottery winners do, however, experience improvements on a subset of non-traditional outcome measures, such as self-reported disciplinary incidences and arrest rates.

Key words: School choice; Randomized lottery; Student outcomes.

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I. Introduction

Leading economists have long been advocates of school choice (Freidman 1955, Becker 1995, Hoxby 2002), arguing that increased market pressure will lead to more efficient utilization of the resources devoted to public education. In recent years, countries around the globe have adopted a range of policies increasing the amount of school choice available to students. The United Kingdom (Gorad 2001), New Zealand (Fiske and Ladd 2000), Colombia (Angrist et al. 2002), Chile (Hsieh and Urquiola 2003), and even China (Tsang 2000) are among the many countries that have instituted policies enhancing school choice.

Estimating a causal relationship between access to sought-after schools and student outcomes has proven difficult. In the United States, observational studies of private schools (Coleman et al. 1982, Bryk et al. 1993) and magnet schools (Blank 1983; Gamoran 1996) find that students who attend these schools experience better educational outcomes, but these studies suffer from potentially 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 (Sander 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 et al. 1995; Green et al. 1997; Witte 1997; Rouse 1998). Although in theory randomization provides an ideal context for evaluating the benefits of expanding students' choice sets, in the Milwaukee case less than half of the unsuccessful applicants returned to the public schools and those who did return were from less educated, lower income families (Witte 1997).[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) to study one particular form of school choice known as "open enrollment," in which public school students can apply to gain access to public magnet schools and programs outside of their neighborhood school, but within the same school district.[3] We avoid the issue of non-random selection into school choice by using lottery data. Many CPS high schools use lotteries to allocate spots when oversubscribed, and we analyze 194 lotteries at 19 of these schools. The CPS data we use offer a number of additional advantages beyond randomization. First, selective attrition is not an important 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 a wide range of issues such as their degree of satisfaction with the school attended, how they are treated by teachers and

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[1] Altonji et al. (2002) suggests that the instruments used in prior studies may not be valid.

[2] 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 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. Angrist et al. (2002) use a unique telephone survey to study the impact of randomly assigned vouchers in Columbia, and, in that context, find improved educational and social outcomes from attending private school. Prior studies that exploit lotteries to examine the benefit of attending magnet schools with a vocational focus find mixed evidence of any long-term benefit (Crain et al. 1992, 1999; Kemple and Snipes 2000).

[3] This form of choice is the most common form of choice available to students in urban areas (NCES 1997), and it is likely to become even more prevalent under the recent federal education legislation *No Child Left Behind*. School

peers, expectations about college attendance, and self-reported arrest data.[4] Little is known about how reforms affect these non-traditional measures of student outcomes, although this issue may be of considerable importance given 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 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.[5]

Comparing lottery winners and losers, we find little evidence that winning a lottery provides any benefit on a wide variety of traditional achievement measures, including standardized test scores, graduation, attendance rates, course-taking patterns, and credit

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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.

[4] Few prior 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).

[5] 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

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 because of access to better resources, better peers, or a program that better suits their learning needs for idiosyncratic reasons.

We explore a variety of potential explanations underlying the lack of academic benefits. One possibility is that students winning lotteries end up attending similar schools to those who lose (i.e., the "treatment" is limited). This is not the case, however. Students who win lotteries to the most select programs do attend what appear to be substantially better high schools – e.g., schools with higher achievement levels and graduation rates and lower levels of poverty. A second explanation is that attending a choice school is a substitute for parental involvement. We find only weak support for this hypothesis. Finally, students winning lotteries may have to travel much greater distances to school, and these travel costs might interfere with academic success. On average, however, lottery winners attend schools approximately one-half mile further from home, suggesting that the marginal increase in travel costs is unlikely to be large.

The coexistence of intense competition for entry and little academic benefit to students winning the lotteries schools could indicate that parents are not well-informed about the education production function, and mistake higher school outputs for higher school value added. Alternatively, parents and children might apply to magnet schools for predominantly non-academic reasons, in which case systematic academic gains would not be expected. 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-

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students in the system.

traditional outcomes. We find some, though limited, support for this hypothesis: winners report lower incidences of disciplinary action, fewer arrests, and lower incarceration rates, 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.

There are at least two important caveats to interpreting our results. First, we look at one particular form of school choice, open enrollment within the public schools. Other forms of school choice, such as vouchers, might yield substantially greater benefits. Second, 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 school choice 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, Rothstein 2004), and residential location patterns.

The remainder of the paper is structured as follows. Section II provides background on open enrollment 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 offers a brief conclusion.

II. Institutional detail and data description

The Chicago Public Schools have one of the most extensive school choice programs available.[6] Each student is guaranteed admission to an assigned neighborhood school, but can

[6] 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

also apply to any other CPS school. Indeed, more than half of all high-school students in CPS in 2000 and 2001 elected to attend a school other than the school assigned.

In order to attend a school other than the assigned school, a student must submit an application in the Spring of the preceding year. 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 which conducts separate lotteries. As a consequence, one school can potentially have a large number of lotteries each year.[7]

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

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racial composition of the school system. Since that time, the size and scope of school choice has expanded dramatically.

[7] 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

transition period from primary to secondary school in CPS and thus is the juncture at which school choice is most frequently exercised.[8]

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. Students applying from outside the district are much less likely to enroll in CPS the following year, particularly when losing the lottery.[9]

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 not only information on demographics and prior school performance of the student, but also information on whether the student enrolled in the CPS the following year and, if so, all of the student's future outcomes. In addition, for a subset of students we have responses to an extensive survey administered in 8th or 9th grade (see Appendix A for a detailed description of the variables and data sources used in this study). Our data have 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

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nearby ("proximity lotteries"). Because such lotteries are rarely oversubscribed, they do not provide useful variation for our empirical work.

[8] The only other grade within CPS for which substantial numbers of school assignments are allocated by lottery is kindergarten. Test score data for the 2000 and 2001 cohort of kindergarten applicants will only become available once these students age into tested grades.

[9] 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

incomplete lottery outcome data, we are left with a baseline sample of 19 schools and 194 lotteries.[10] Our baseline sample contains 19,520 applications submitted by 14,434 students. The students in our sample constitute approximately one-fourth of all eighth graders in CPS during this two-year period. Overall, these lotteries are quite competitive, with only 15 percent of applicants winning in the average lottery. Because a student can apply to multiple lotteries, roughly 20 percent of the students in the sample win at least one lottery.

Table 1 presents information about the 19 schools represented in the data set. Schools are ranked according to the eighth grade test score performance of students enrolling in ninth grade, which is presented in column (1) of the table. These schools range from the top ten percent among the 70 regular high schools in CPS along this test score dimension (Von Steuben and Chicago Agricultural) all the way down to the very bottom (Orr is the second lowest scoring high school in CPS). Columns (2)-(4) report alternative indicators of school quality: a school's "value added" in reading test scores,[11] how competitive the lotteries are (a smaller percentage of lottery entrants selected indicates a greater imbalance between demand and supply), and the fraction of lottery winners who actually choose to enroll in the school when given the opportunity. There is substantial variation across schools along all of these dimensions.[12] Schools that attract high achieving students tend to be popular with students, as measured by either the competitiveness of the lotteries or the take-up rates of lottery winners (the correlation between columns 1 and 3 is .34 and between columns 1 and 4 is .71). Our measure of value

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availability of school choice serves to attract students to the public sector.

[10] Appendix A describes the construction of our sample in greater detail.

[11] Value added is computed as the mean residual by high school from a student-level regression of ninth-grade reading percentile score on flexible controls for 8th grade reading score, student demographic characteristics, and fixed-effects for the middle-school a student attended in 8th grade.

[12] Other natural dimensions of school quality include financial resources and teacher quality. In the CPS, funding is allocated largely by formula whereby schools with larger populations of poor, special education and language minority students receive compensatory funding, making it difficult to interpret higher levels of expenditures as a

added is less highly correlated with the other three indicators of school quality (correlation coefficients of .32, -.16, and .18 respectively with columns 1, 3, and 4). In terms of the number of lottery participants, the high-achieving schools (particularly Von Steuben) are heavily overrepresented.[13]

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 higher 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 are 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 the students in our sample.

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 will be identical on average, in terms of unobservable as well as observable characteristics. Consequently, a simple difference of observed student outcomes between students who win and lose the lottery provides a consistent estimate of the impact of

signal of quality. Some information on teacher characteristics by school is available, but we were unable to find a measure that both varied across schools and had an unambiguous association with the quality of instruction.

[13] Von Steuben has received national media attention and was included in a two recent lists of America's best public high schools (Toch 1999, Matthews 2003).

winning the lottery.

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

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

where Y is some outcome measure for student i, Win_{ij} is a binary variable indicating whether the student won lottery j, and $Apply_{ij}$ is a binary variable equal to one if the student applied to the lottery and zero otherwise. Then, d_j indicates whether winners are systematically higher or lower on the characteristic Y than losers in the same lottery.

While d_j is clearly an unbiased estimate of the impact of winning this lottery, it is important to consider its interpretation. The parameter measures the impact of winning *conditional* on deciding to apply. 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, d_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 on students in our sample, even if we are missing 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:

$$d_{jk} = E[Y_i \mid Win_{ij} = 1; Apply_{ij} = 1, z_i = k] - E[Y_i \mid Win_{ij} = 0; Apply_{ij} = 1, z_i = k]. \qquad (2)$$

In practice, the standard errors for particular lotteries and subgroups within lotteries in our data are too large to make such estimates informative. Therefore, we instead report results from ordinary least-squares regressions (or probits when the dependent variable is binary) of the form:

$$Y_i = d\,(Win\_Lottery_{ia}) + \Gamma(Lottery_a) + e_{ia}, \qquad (3)$$

where the subscripts i and a index students and applications, respectively. $Win\_Lottery_{ia}$ is a binary variable that indicates whether application a for student i was a lottery winner. $Lottery_a$ is a vector of fixed effects indicating the lottery to which the observation refers, and e is a stochastic error term. In this specification, the d coefficient is a weighted average of the d_j's for the various lotteries, with the weight for lottery j equal to $\frac{N_j P_j (1-P_j)}{\sum_j N_j P_j (1-P_j)}$, where N_j is the number of students entered in lottery j and P_j is the proportion of students entered in lottery j who win the lottery. The weights are proportional to the number of students in the lottery and the variance of the treatment. In all specifications, we include covariates such as student demographics, prior achievement and neighborhood characteristics. Our conclusions are not sensitive to the inclusion of these covariates.[14] Because 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

[14] In large samples, the estimates will be the same with and without this conditioning, as long as there is no selective attrition from the original sample.

account for clustering at the school level.[15]

Establishing the validity of the randomization and the potential confounding role of selective attrition

If the lotteries were conducted properly, then one would predict that the winners and losers of a given lottery will be, on average, perfectly balanced on all predetermined characteristics. Even if the lotteries are valid randomizations, however, selective attrition may bias our findings since we only observe subsequent student outcomes if the student enrolls in CPS. In Table 3, we test for the validity of the lotteries and the presence of selective attrition by estimating equation (3) for a series of demographic and achievement variables that are predetermined at the time of the lottery. The pre-determined variables include student, school, and neighborhood characteristics at the time an application is made and survey responses given in 8th grade prior to application. While the other measures are available for all students, the survey responses are available only for the subset of the 2001 cohort who attended an 8th grade school at which the survey was administered and who completed the survey. Column 1 presents the mean for each measure among the control group of lottery losers.[16] Columns 2 and 3 present the coefficient and standard error on an indicator variable reflecting whether the student won the lottery. The full sample of students is used in these columns, providing a test of the validity of the initial lottery. Columns 4 and 5 are identical to columns 2 and 3, except that the sample is limited to students who actually enroll in CPS in 9th grade. These latter columns thus reflect the degree to which, even after attrition occurs, the lottery winners and losers that we observe in our

[15] Because each student attends only one school, allowing for arbitrary correlation at the school level captures any within-student correlation as well.

sample are matched on observable characteristics.

As the final two rows of Table 3 demonstrate, enrollment rates among lottery losers are high (89.5 percent), and winning a lottery increases the likelihood of enrolling by only 1.9 percentage points. Thus, the degree of initial differential attrition is quite low. In both the full sample *and* in the sub-sample of students who actually enroll in CPS in 9th grade, lottery winners and losers are similar on a wide range of observable characteristics. The magnitudes of the implied differences are universally substantively small, and we observe only one statistically significant difference in each sample—the degree of parental supervision is higher among lottery winners in both cases.[17] The validity of the initial lottery is not surprising given that the outcomes were computer-generated and the output was write-protected to prevent tampering. More impressive is the fact that there is no evidence of selective attrition.[18]

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)

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[16] For some survey measures that are scaled in arbitrary units, the standard deviation across students is more informative than the mean of the variable. In such cases, which are noted in the table, we report the standard deviation rather than the mean in column 1.

[17] To determine how many statistically significant differences would be expected due purely to chance, we need to account for correlation across the background characteristics within students. We employ a simulation-based test, whereby we repeatedly randomly assign students (within lotteries) to winning or losing at the same rates as in the actual lotteries and re-estimate equation (3). In 1,000 trials, the marginal effect of winning was found to be statistically significant for exactly one background variable 30.8% of the time at the 5% level and 26.1% of the time at the 10% level—so that our finding is far from an extreme one. Since the results in Table 3 could mask systematic differences between winners and losers in opposing directions across lotteries, we have conducted more careful Monte Carlo tests based on lottery-specific comparisons of the absolute value of the difference between winners and losers on each background measure relative to the simulated distribution of this difference under random assignment (based on 1,000 trials). We can never reject that the across-lottery distribution of the number of background measures for which the difference is in the tail (the top 5% or 10% of the simulated distribution) is consistent with random assignment.

[18] It is nonetheless possible that, conditional on enrollment, winners and losers may have different propensities to have valid outcome data in subsequent years. For example, lottery losers might become discouraged and either drop out of school or fail to show up to take the standardized achievement exams at greater rates than lottery winners. To examine outcome attrition, we estimate models similar to those shown in Table 3 and find no evidence of systematic selection in the presence of missing data. We also conduct a series of sensitivity analyses and selection corrections,

(either using OLS, or probit when the dependent variable is binary) for a wide range of outcome variables on the left-hand side of the regression, including an extensive set of student and neighborhood covariates (all pre-determined, and listed in the table notes) in order to increase the precision of our estimates. We present results for the average impact of winning a lottery across all participants and schools (as we did in testing the validity of the lotteries and the relationship between lottery outcomes and attrition). We also explore possible heterogeneous treatment effects across a variety of school and student characteristics.

The first set of outcomes we examine are the characteristics of the high school the student attends. These results tell us the extent to which winning a lottery affects the student's school environment, and also provides a means for translating the intention-to-treat estimate we present later into estimates of treatment-on-the-treated. The results are presented in Table 4. Each cell of the table corresponds to a separate regression. The dependent variable of the regression differs by row. Columns reflect different subsets of lotteries. The first column uses all lotteries. The second through fourth columns include only the subset of lotteries from the five schools in our sample that are highest on each of our three proxies for school quality (high achieving peers, high value added, and high popularity).[19] In each cell of the table, we report the marginal effect of winning a lottery (from OLS regression for continuous outcomes and from Probit models for binary outcomes), a robust standard error in parentheses, and the control group mean in square brackets.

The results of Table 4 demonstrate that lottery outcomes have a substantial impact on the type and characteristics of high schools students attend within CPS. In the top panel, the

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).

[19] The top five schools on each proxy are highlighted in bold print in the relevant column in Table 1. In terms of high-achieving peers and value added, our top five schools fall into the top quartile of schools in CPS overall on

dependent variables are a series of indicator variables for the type of school attended. The first column of the top row, for instance, shows that winning any lottery increases the probability a student attends the school for which the lottery is held by 28.3 percentage points. Note that some students who lose the lottery nonetheless are sometimes able to enroll in the school, although the rates are low (between 6.9 and 8.2 percent depending on the type of school). That is because there are sometimes multiple programs offered within a given school, some of which may not be oversubscribed.[20] Winning a lottery to a high popularity school or one with high-achieving peers has an even larger impact on enrollment in that school (39.6 and 35.0 percentage points respectively), but winning a lottery to a high-valued added school has a slightly lower impact on enrollment (24.2 percentage points). The fact that take-up rates, while substantial, are far from complete is central to understanding the appropriate interpretation of the results presented later in the paper. Winning a lottery has a smaller effect on whether a student attends any school other than the assigned school (row 3) because students may win multiple lotteries or apply to schools that do not use lotteries. Nonetheless, winning a lottery is associated with a greater likelihood of attending any top quartile school as measured either by peer achievement or value added.[21]

The bottom panel of Table 4 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 attend schools with peers who score higher on standardized tests, have lower rates of

these measures. The top five schools are the same for (low) percent of students who win lotteries and (high) take-up rates among lottery winners, so we report the results only once under the title "high popularity school."

[20] 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 principals have to admit a small number of students outside of the lottery process.

[21] Winning a lottery does, however, slightly reduce 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.

poverty as measured by free lunch eligibility, and have higher graduation rates.[22] This is particularly true of students winning lotteries to schools that are high-quality on the dimensions we measure. For instance, winning a lottery to a high-popularity school raises the share of peers testing at or above national norms 5.8 percentage points (off of a baseline of 40.7 percent), reduces the fraction of free-lunch eligible peers by 5.6 percentage points (from a baseline of 74.4 percent), and raises the graduation rate of peers 4.3 percentage points (relative to a baseline of 70.9 percent). Since there are multiple programs within many high schools, a more accurate peer measure may be those students with whom a child attends class. Since we do not have information on program enrollment, we examine the peers in each student's 9th grade English class. We find that lottery winners attend class with peers who have significantly higher test scores than lottery losers, although the magnitude of the peer effect is smaller than for the school-based peer measure, indicating that some lottery winners may be in lower tracked classes within their school. The schools attended by lottery winners are also 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. One potential negative factor for students winning lotteries is that there is less continuity in the peers they go to school with moving from 8th to 9th grade, although all students experience a disruption of peers with the transition to high school.

Theory would predict that lottery winners would experience improved academic outcomes, not only because they are attending higher quality schools on average, but also because their choice set is increased. Moreover, to the extent that there are idiosyncratic features of a student-school match that we cannot observe, winning a lottery may improve student

[22] Table 8 also reveals that lottery winners are much more likely to have ready access to computers. This suggests that winners are exposed to higher levels of school resources.

outcomes even without a change in our proxies for school quality. Table 5, however, presents surprising results to the contrary. Each row of Table 5 corresponds to a different regression with a traditional measure of school performance on the left-hand side. The specifications estimated are otherwise identical to those in Table 4. Overall, we find no compelling evidence that students winning lotteries perform better on the range of academic measures we examine. The most straightforward outcomes to analyze are those related to attainment, since we observe these outcomes regardless of whether a student remains enrolled in CPS over time.[23] Four-year graduation rates from CPS are actually significantly lower for the lottery winners as a whole in column 1 (a 4.8 percentage point reduction of a baseline rate of 58.3 percent), although the estimates are statistically insignificant for lotteries within our high quality choice schools. There are multiple reasons why a student does not graduate from CPS in four years, including dropping out, failing a grade, moving out of the city of Chicago, or transferring to a private school in Chicago. For the overall sample, the non-graduates are about evenly split between those who drop out of CPS and those that transfer out of the CPS system, but not to local private schools. The magnitude of the differences in drop out rates for lottery winners and losers is substantial (2.1 percent off of a baseline of 19.2 percent), but not statistically different from zero at standard levels of confidence.[24] It is unclear why winning a lottery would be associated with an increased rate of transfer outside of the Chicago area, but one possibility is that match quality was worse than expected.

Analysis of other measures of academic success (e.g., test scores, absences, school credits, being retained, and class rank) is potentially more problematic because we only observe

[23] All of the results we report are conditional on a student enrolling in CPS in Fall of 9th grade. As we documented in Table 3, there is little evidence of selective attrition for that enrollment decision.
[24] Dropout rates by 11th grade (shown in the last panel of the table), if anything, also show a tendency to be higher among lottery winners, though these results are not statistically significant. The coefficient on 11th grade dropout is

the outcome if the student remains enrolled in the CPS, and in the case of test scores, is present on the day the exam is administered. However, we have extensively explored whether selective attrition occurs *after* enrollment in 9th grade (see appendix Table B1), finding no evidence that biases are introduced. The bottom panels of Table 5 reports results for these outcomes. Lottery winners do not perform systematically better on the various test measures that are available to us. For the full set of lotteries, the point estimate on winning a lottery is negative on four of the six test outcomes we observe in 9th and 10th grade. The only one of these that is statistically significant (percentile rank on the 9th grade reading exam) carries a negative sign, implying that lottery winners perform worse. The results on test scores are no more encouraging when we limit the sample to high-quality schools.[25] Nor do school credits and absences appear to be systematically affected by lottery status. The most consistent difference we observe is that students winning lotteries have statistically significantly worse class ranks by 2-7 percentile points, perhaps reflecting the fact that their own academic performance is not greatly affected, but the pool of peers against whom they compete is stronger.

The lack of statistical significance for many of the measures is in part due to the limited power of our research design. One way to gauge the power for the various academic outcomes is to consider the implied magnitude of the treatment-on-the-treated estimates. As reported in Table 3, winning a lottery is associated with an increased likelihood of attending that school ranging from 24.2 to 39.6 percentage points, depending on the set of lotteries examined.[26] Thus, the treatment-on-the-treated estimate is 2.5 to four times larger than the intent-to-treat estimate.

estimated more precisely than the corresponding coefficient on 12th grade dropout because we have data for both cohorts on the former measure, but only for one cohort for the latter.

[25] Moreover, when we limit the sample to lotteries to schools with a math focus, lottery winners perform no better on math tests than lottery losers. Similarly, winning a lottery to a school with a reading focus does not improve reading test scores.

Assuming (for simplicity) a first-stage effect of 33 percentage points, the effect of attending the lottery school is between -7 and -1 percentage points off a baseline of 42 for 9th grade reading percentile scores, between -5 and 4 percentage points off a baseline of 45 percent for 9th grade algebra scores and between -0.5 and 0.6 off a baseline of 28 for the number of credits earned in Spring semester of 9th grade. The estimates are even less precise when looking at particular subgroups. Nonetheless, our estimates are predominantly negative and the 95 percent confidence intervals generally rule out even moderately positive effects on the longer-run outcomes, suggesting that winning a lottery does not confer substantial academic gains. In summary, there is little compelling evidence that winning a lottery improves academic outcomes along the range of dimensions we measure.

Even if the aggregate effects are zero, it is possible that winning a lottery has a positive effect for some schools or for some subset of students.[27] Figure 1 reports the distribution of treatment effects at each of the schools in our sample for a subset of the most relevant outcomes.[28] For six of the individual schools, winning the lottery is associated with statistically significant increases in dropping out by 12th grade. The two lottery schools with the lowest achieving peers fare the worst on this measure. For only two of the schools is the point estimate clearly negative, implying a reduction in drop out rates for lottery winners and in neither case can one reject the null of no impact. On 9th grade reading, lottery winners to seven of the schools experience statistically significant reductions in test scores; there is only one school in which reading scores show a statistically significant increase for lottery winners. The pattern is roughly

[26] If one thinks of the relevant treatment not as attending the particular school running the lottery, but rather, any high quality school, the impact of winning a lottery to a high-achieving school is almost unchanged, but the effect of winning any lottery is much smaller.

[27] 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.

[28] For the test score measures, we have data on all 19 schools. Only nine of the schools have survey data. Graduation data are only available for lotteries involving the 2000 cohort, which limits the sample to 14 schools.

reversed for 9th grade algebra: five schools see statistically significant improvements in test scores and only two have significant declines among lottery winners. For neither of the test score measures is there a strong relationship between peer quality and the impact on test scores. Overall, it does not appear that the aggregate estimates are masking substantially positive effects in specific schools, but rather reflect negative effects in a collection of schools along with no effects in the other schools.

We also explore the possibility of heterogeneous treatment effects along observable dimensions of the student population: race, gender, whether the student was below average in 8th grade test scores, and by the extent of the difference in peer quality at the lottery school relative to the student's next-best option.[29] Table 6, which follows the format of Table 4 (but reports only a representative subset of the school characteristics), presents results on how lotteries impact the characteristics of the school attended.[30] Our baseline results for the full sample are reprinted in the first column. There are no notable patterns across race, gender, and prior academic achievement (columns 2-5). There are, however, sizable differences across students in the top and bottom quartile of the gap in peer quality at the lottery school versus the next-best option. Students who stand to gain the least in terms of peer quality (column 6) are much less likely than those who would gain the most (column 7) to actually attend the lottery school when victorious (marginal effects of .187 and .504 respectively). Indeed, the bottom panel of Table 6 shows that for the students with the least to gain, peer quality is actually lower on average at the lottery school. In stark contrast, among lottery winners with the most to gain, average combined 8th grade scores among students at the high school attended jump almost nine percentile points

[29] Peer quality is measured as the average combined 8th grade math and reading scores of 9th graders at the same high school. We proxy for peer quality at the student's next-best option using the average high school peer quality experienced by students who attended the same 8th grade campus, are in the same ability quintile (as measured by

and value added is substantially higher.[31]

Table 7 reports the impact of winning a lottery on student outcomes for the various sub-samples of the student population. Winning a lottery appears to have a sizeable adverse impact on the graduation rate of Blacks (-10.9 percentage points with a standard error of 2.2), males (-6.6 percentage points with a standard error of 2.6), and below average students (-7.3 percentage points with a standard error of 2.6), with about half of each of these gaps due to the combination of dropping out or failing a grade, and half due to transfers. Remarkably, the group of lottery winners that fare the very worst in terms of graduation is the subset of students who gain the most in terms of peer quality. Graduation rates for these students are over 15 percentage points lower than for comparable lottery losers; less than forty percent of these students graduates from CPS in four years. Drop out rates among this group are a stunning 11.3 percent higher among lottery winners, off of a baseline rate of 22.7 percent. Thus, the group that *a priori* would be expected to benefit the most from access to high quality schools actually shows the worst response to winning lotteries.[32] Ironically, the students who stand the least to gain from winning a lottery (column 6) also experience particularly bad outcomes. Lottery winners in the middle two quartiles along this dimension (results not shown), actually graduate at slightly higher, but not statistically significantly different rates than lottery losers.

The remainder of Table 7 examines other academic outcomes such as test scores and class rank for the various subsets of students. While the results are quite mixed, the evidence is

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combined 8th grade reading and math scores), and who lost (or did not apply for) our lotteries. This strategy accounts for the fact that students may apply to a wide set of schools other than those we observe in our sample.

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

[31] The potential peer improvement from winning a lottery is a function of both the school to which the student applied as well as the school where the student would have most likely attended if he or she did not win the lottery. In practice, students in column 7 are relatively low-achieving students (based on 8th grade test scores) who apply to high-achieving lottery schools. Conversely, students in column 6 tend to be moderate or high-achieving students who apply to low-to-moderate achieving lottery schools.

more consistent with lottery winners being hurt rather than helped by winning.

Given the absence of systematic academic benefits to students attending lottery schools, why is it that competition for entry is so intense? One possible explanation for why lottery winners, despite attending better quality schools, perform no better on average, is that other factors may mitigate any achievement benefits they receive from the school. For instance, winners travel a greater distance to attend school than losers, although only about an extra one-half mile, which is unlikely to impose substantial costs.[33] Alternatively, school quality and parental involvement may be substitutes in the education production function. For example, parents whose children win lotteries to select magnet schools or programs 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 8 provides mixed evidence as to whether parental inputs substitute for school quality. The structure of the table is identical to the preceding tables, 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.[34] We report only results for the full set of lottery schools and the schools with high-achieving peers; for other breakdowns of the data the standard errors are too large to be informative. The top three rows in the table show no evidence that for the full set of lotteries

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[32] These findings cannot be explained by higher rates of initial attrition. Winners and losers in this sample enroll in CPS in 9th grade at rates of 92.7 and 90.5 percent, respectively, and the difference is not statistically significant.
[33] Of course, parents and students should factor this time cost into their application and attendance decisions.
[34] Three schools (Von Steuben, Lake View, and Roosevelt) did not administer the survey.

parental inputs diminish for lottery winners. For those winning access to high-achieving schools, parents are less likely to help with homework, but are more likely to discuss school-related issues. 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 8 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.[35] Students winning lotteries to high-achieving schools are less likely to report that they were subject to disciplinary action at school, and self-reported arrest rates for students winning lotteries to high-achieving schools are reduced by 40 percent relative to students losing such lotteries (4.8 percent versus 8.9 percent). The pattern of self-reported arrest rates is corroborated by administrative data on incarceration rates for students in our

[35] The more positive estimates found for the non-traditional outcomes are not simply a function of the different sample. We re-estimated the results shown in Tables 5 and 6 for this restricted "survey sample" and obtained comparable results.

sample. We observe statistically significant reductions in the percentage of students behind bars when comparing lottery winners to losers, with the greatest reductions observed among the students whose peer quality stands to improve the most if they win.

VI. Conclusions

This paper uses lotteries to estimate the causal impact on student outcomes of gaining access to sought-after public schools. 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 systematic evidence of benefits to lottery winners (and even in some cases significant declines) on traditional outcome measures such as graduation rates, test scores, and school attendance. This is true for a variety of subgroups of students, including those that one would *a priori* expect to benefit most from winning the lottery. Our results do 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.

Our finding concerning the absence of a positive impact of public school choice and high-quality peers on traditional student outcomes stands in contrast to theoretical expectations, but is aligned with findings from other recent studies on the topic. In an earlier study of the CPS open enrollment program, for example, Cullen, Jacob, and Levitt (forthcoming) use distance from a student's residence to schools other than the assigned school as an instrument for attending a choice school, and find no evidence that exercising choice is associated with increased educational attainment, with the exception of those choosing career academies. Also using administrative data from Chicago, Lefgren (2002) finds no evidence that higher achieving peers raise own achievement.

Because our research design can only measure improvements of lottery winners relative to those of lottery losers, our results do not provide any evidence as to whether increased competition induced by school choice confers benefits to all students. Nor are our findings easily extrapolated to an evaluation of other forms of school choice such as vouchers. However, these results do provide some insights that may be relevant for school choice and school reform more generally. First, the results here suggest that when deciding which schools to attend, students and parents may be concerned not only with traditional academic benefits, but also other factors related to safety or non-academic amenities. This is consistent with several earlier studies that examine actual school choices made by families (Henig 1990, Glazerman 1997) as well as recent housing mobility studies in which public housing residents cited fear of crime (as opposed to better employment or educational opportunities) as the most important factor in seeking a housing voucher (Kling et al. forthcoming).

Second, the findings presented here suggest that a student's relative position among peers may be an important factor determining academic success. Lottery winners attended schools with higher-achieving peers than lottery losers and, as a result, had substantially lower class ranks than their peers from the beginning of high school. There is a considerable literature within sociology that emphasizes the importance of one's relative position (e.g., Kaufman and Rosenbaum, 1992) and the ambiguous welfare consequences of gaining access to a more-advantaged environment.

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Appendix A: Sample and Data Sources

CPS provided information on applications submitted in Spring 2000 and 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, and dropped out by reason (e.g. pregnancy, jailed). 9^{th} and 10^{th} 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. This provides us with data from the Spring of 8^{th} grade for our 2001 cohort and from the Spring of 9^{th} grade for our 2000 cohort. Several of the survey outcomes we use are composite measures created by CCSR from student responses to a collection of individual items. 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 8^{th} grade. Special education status covers a variety of disabilities ranging from mild learning disabilities to severe physical handicaps. 8^{th} 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 & CPS Board & CCSR	Basic information on the student’s census tract, such as median household income and percent below the poverty line, 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 3), there are other points at which students can be absent from our sample. Outcome data in the Spring 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. We show the results for the subset of the background variables that a priori seemed the most compelling set to focus on, but find no systematic evidence of selective attrition across the other background variables either.

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 I scores in 9th grade and 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 the 9th grade reading score results to a variety of possible sample selection correction methodologies. We use three bounding techniques:

1. Generic bounding: All missing test scores are set to the value at a specific percentile in the overall 9th grade reading test score distribution (or set to the student's own 8th grade reading score plus the gain at a specific percentile in the overall distribution of gains).
2. Worst-case bounding: This method lottery winners and losers with missing data asymmetrically. For example, an upper bound on the effect of winning is attained by replacing missing values for losers to the bottom of the test score distribution and missing values for winners to the top, while the lower bound is based on the opposite assignment.
3. Optimistic bounds: This technique is based on the trimming method developed in Lee (2002). Under a maintained assumption that winning a lottery only makes an individual less likely to have non-missing outcome data, upper and lower bounds on the effect of winning can be attained by selectively trimming the sample of winners with non-missing data to eliminate excess attrition among losers within the same lottery. If the fraction of winners within a given lottery with non-missing test scores is x and the fraction of losers with non-missing scores is y, then excess attrition among losers is equal to $p = (x-y)/x$. To provide an upper bound on the effect of winning, we trim winners from the sample whose 9th grade test scores fall below the pth quantile in the test score distribution observed for the lottery's winners. The lower bound is found by trimming winners whose test scores fall above the $(1-p)$th quantile. We condition the trimming on observable characteristics by running an initial regression of 9th grade reading scores on

our set of student and neighborhood covariates and then applying the trimming procedure within lottery-specific quintiles of this predicted score.

We also implement a control-function approach. For each lottery, we calculate the difference between the rate at which winners and losers enroll in the 9th grade in the CPS in the following Fall. Then we create interactions between a cubic of this measure of initial differential attrition and an indicator for being selected in the lottery, and add these three variables to the control set. 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. This is not surprising since we have high overall attrition but relatively low relative attrition of losers compared to winners.

Table B3 calculates optimistic bounds (Lee, 2002) for all of our outcome measures. We condition the trimming on covariates using the method described above (e.g. by assigning observations within each lottery to quintiles according to the predicted value of the outcome measure from an initial regression using the full sample, and then conducting the trimming within those quintiles).

Table B1. Additional evidence on sample attrition

Dependent variable	Mean among lottery losers	The effect of winning a lottery to … Any school	High-achieving school	High value-added school	High popularity school
	(1)	(2)	(3)	(4)	(5)
Panel A: Enrollment attrition between 8th and 9th grade					
Enrolled in CPS in the 9th grade in the Fall	0.895	0.019**	0.038**	0.022*	0.045**
		(0.007)	(0.010)	(0.013)	(0.011)
Leaves for private high school in the Fall	0.031	-0.007**	-0.016**	-0.011	-0.021**
		(0.003)	(0.006)	(0.008)	(0.006)
Number of observations	16,576	19,520	9,473	7,454	9,178
Panel A′: Is enrollment attrition between 8th and 9th grade selective?					
8th grade math percentile score	0.522	-0.002	0.009	-0.004	0.016
		(0.006)	(0.009)	(0.010)	(0.011)
8th grade reading percentile score	0.481	-0.004	0.000	-0.002	0.003
		(0.005)	(0.009)	(0.010)	(0.011)
Free lunch eligible	0.743	0.003	0.031	0.025	0.015
		(0.011)	(0.020)	(0.022)	(0.024)
Receiving special ed. in 8th grade	0.112	0.008	0.013	0.006	0.017
		(0.008)	(0.015)	(0.015)	(0.016)
Tract poverty rate	0.224	0.000	-0.003	0.002	-0.004
		(0.003)	(0.005)	(0.005)	(0.006)
Parents' support for learning [a]	{1.550}	0.011	-0.042	0.077	-0.089
		(0.060)	(0.103)	(0.096)	(0.122)
Degree of parental supervision [a]	{2.204}	0.157*	0.161	0.312**	-0.045
		(0.090)	(0.153)	(0.146)	(0.182)
Attends religious services weekly [a]	0.416	0.011	-0.019	-0.024	0.010
		(0.021)	(0.037)	(0.036)	(0.046)
Reports getting into trouble at sch [a]	0.680	0.004	0.021	-0.020	0.003
		(0.020)	(0.035)	(0.034)	(0.043)
Mother completed some college [a]	0.578	-0.006	0.026	-0.045	-0.026
		(0.027)	(0.046)	(0.045)	(0.054)
Number of observations	14,830	17,492	8,459	6,613	8,191
Sample limited to students enrolled in CPS in Fall of 9th grade	Yes	Yes	Yes	Yes	Yes
Panel B: Outcome attrition in 9th grade					
Enrolled in CPS in the 9th grade in the Spring	0.959	0.003	-0.002	0.002	0.002
		(0.005)	(0.008)	(0.008)	(0.009)
Has complete outcome data	0.657	0.024**	0.048**	-0.001	0.045**
		(0.012)	(0.019)	(0.021)	(0.022)

Has reading exam score	0.864	0.003	0.004	-0.003	0.011
		(0.008)	(0.013)	(0.014)	(0.015)
Has algebra score	0.743	0.016	0.028	-0.016	0.037*
		(0.011)	(0.018)	(0.020)	(0.020)
Has English I score	0.762	0.019*	0.033**	0.012	0.035*
		(0.010)	(0.017)	(0.018)	(0.019)
Has transcript information	0.933	0.012**	0.007	0.011	0.013
		(0.006)	(0.010)	(0.010)	(0.011)
Number of observations	14,830	17,492	8,459	6,613	8,191
Sample limited to students enrolled in CPS in Fall of 9th grade	Yes	Yes	Yes	Yes	Yes
Panel B′: Is outcome attrition in 9th grade selective?					
8th grade math percentile score	0.555	-0.002	0.007	-0.005	0.013
		(0.007)	(0.011)	(0.012)	(0.013)
8th grade reading percentile score	0.506	-0.004	-0.004	-0.001	0.001
		(0.007)	(0.011)	(0.011)	(0.013)
Free lunch eligible	0.723	0.010	0.035	0.024	0.019
		(0.015)	(0.025)	(0.028)	(0.029)
Receiving special ed. in 8th grade	0.080	0.008	0.014	0.005	0.015
		(0.009)	(0.016)	(0.017)	(0.017)
Tract poverty rate	0.220	0.002	-0.003	0.002	-0.006
		(0.003)	(0.006)	(0.006)	(0.007)
Parents' support for learning [a]	{1.496}	-0.034	-0.196*	-0.014	-0.222
		(0.072)	(0.118)	(0.117)	(0.140)
Degree of parental supervision [a]	{2.191}	0.159	0.124	0.381**	-0.134
		(0.109)	(0.181)	(0.178)	(0.218)
Attends religious services weekly [a]	0.426	-0.001	-0.27	-0.040	0.016
		(0.026)	(0.043)	(0.044)	(0.055)
Reports getting into trouble at sch [a]	0.659	0.022	0.029	-0.008	0.001
		(0.025)	(0.041)	(0.042)	(0.052)
Mother completed some college [a]	0.567	-0.027	0.024	-0.040	-0.043
		(0.033)	(0.055)	(0.054)	(0.066)
Number of observations	9,745	11,462	5,914	4,616	5,671
Sample limited to students with complete 9th grade outcome data	Yes	Yes	Yes	Yes	Yes
Panel C: 9th grade survey attrition(2000 cohort only) [b]					
Responded to the survey	0.534	0.065**	-0.003	0.005	-0.009
		(0.020)	(0.039)	(0.045)	(0.045)
Number of observations	4,367	5,492	1,413	345	1,524
Sample limited to students enrolled in a surveyed high school	Yes	Yes	Yes	Yes	Yes
Panel C′: Is 9th grade survey attrition selective (2000 cohort only) [b]					

8th grade math percentile score	0.536	-0.006	-0.014	-0.029	0.007
		(0.012)	(0.019)	(0.022)	(0.029)
8th grade reading percentile score	0.473	-0.008	-0.015	-0.002	-0.015
		(0.011)	(0.018)	(0.025)	(0.027)
Free lunch eligible	0.804	0.020	0.045	0.015	0.053
		(0.021)	(0.041)	(0.045)	(0.055)
Receiving special ed. in 8th grade	0.091	0.006	0.030	---	0.077
		(0.015)	(0.026)		(0.049)
Tract poverty rate	0.231	-0.002	-0.012	-0.003	-0.010
		(0.005)	(0.009)	(0.011)	(0.013)
Number of observations	2,333	3,014	863	280	828
Sample limited to students responding to the survey	Yes	Yes	Yes	Yes	Yes
Panel D: Outcome attrition in 10th grade (2000 cohort only)					
Enrolled in CPS in the 10th grade in the Spring	0.890	-0.017	-0.010	-0.013	-0.000
		(0.013)	(0.021)	(0.026)	(0.022)
Has complete outcome data	0.574	0.002	-0.015	-0.052	0.014
		(0.019)	(0.031)	(0.039)	(0.035)
Has reading exam score	0.744	-0.007	0.006	0.033	0.008
		(0.017)	(0.026)	(0.033)	(0.030)
Has geometry score	0.685	-0.012	-0.044	-0.091**	-0.018
		(0.018)	(0.030)	(0.038)	(0.033)
Has English II score	0.720	0.002	0.057**	0.031	0.048*
		(0.017)	(0.025)	(0.033)	(0.029)
Has transcript information	0.863	-0.007	0.001	0.008	0.007
		(0.013)	(0.022)	(0.026)	(0.024)
Number of observations	7,144	8,356	4,071	3,079	4,177
Sample limited to students enrolled in CPS in Spring of 9th grade	Yes	Yes	Yes	Yes	Yes
Panel D′: Is outcome attrition in 10th grade selective? (2000 cohort only)					
8th grade math percentile score	0.578	0.003	0.014	-0.013	0.024
		(0.010)	(0.015)	(0.020)	(0.019)
8th grade reading percentile score	0.512	0.007	0.013	0.012	0.011
		(0.010)	(0.016)	(0.021)	(0.019)
Free lunch eligible	0.740	-0.007	0.002	-0.041	0.009
		(0.024)	(0.041)	(0.054)	(0.044)
Receiving special ed. in 8th grade	0.074	-0.007	-0.005	-0.005	-0.013
		(0.012)	(0.019)	(0.023)	(0.020)
Tract poverty rate	0.213	-0.002	-0.012	-0.010	-0.013
		(0.005)	(0.009)	(0.010)	(0.011)
Number of observations	4,103	4,783	2,469	1,895	2,516
Sample limited to students with complete 10th grade outcome data	Yes	Yes	Yes	Yes	Yes

Notes: Column 1 reports the mean (or standard deviation {} for index measures) among losers for lotteries at all schools for the dependent variable indicated in the row heading and for the sample of students indicated. The remaining columns report results from separate regressions of the dependent variables on an indicator for being selected in a lottery and a full set of lottery fixed effects. Except for the binary variables, the models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. The models with binary dependent variables are estimated using a Probit specification, and we report the marginal effect of being selected evaluated at the mean of the lottery indicators. Eicker-White robust standard errors adjusted to account for the correlation of errors across applications within a single student are shown in parentheses. The results shown in column 2 are based on applications to all of our analysis schools, while columns 3-5 restrict the sample to the subset of applications to the type of lottery schools indicated. The number of observations shown is the total number of applications in the relevant sample. The number of observations in any given regression varies due to differences in data availability. ** significant at the 5% level * significant at the 10% level

[a] Sample limited to the 2001 cohort since the 8th grade survey was not administered to the 2000 cohort.

[b] The samples in Panel C and C′ include only students in the 2000 cohort and exclude students who applied to three schools (Von Steuben Metro, Roosevelt and Lake View) that did not administer the 9th grade survey.

Table B2: Sensitivity of 9th grade reading score estimates to alternative selection correction methods

Dependent variable	The effect of winning a lottery to …			
	Any school	High-achieving school	High value-added school	High popularity school
	(1)	(2)	(3)	(4)
Baseline Estimates (Table 5)	-0.013**	-0.009	-0.010	-0.010
	(0.005)	(0.007)	(0.008)	(0.009)
Generic Bounding				
Assign students with missing scores the score at the 10th percentile of the full sample	-0.003	0.004	0.007	0.003
	(0.004)	(0.008)	(0.010)	(0.009)
Assign the score at the 25th percentile	-0.005	-0.001	0.004	-0.003
	(0.004)	(0.007)	(0.008)	(0.008)
Assign the score at the 75th percentile	-0.011**	-0.013**	-0.002	-0.017**
	(0.005)	(0.007)	(0.007)	(0.009)
Assign the score at the 90th percentile	-0.014**	-0.019**	-0.005	-0.025**
	(0.007)	(0.009)	(0.010)	(0.011)
Assign a score equal to own prior 8th grade score plus the gain at the 10th percentile	-0.006	0.001	-0.004	0.002
	(0.005)	(0.008)	(0.010)	(0.010)
Assign the gain at the 25th percentile	-0.008**	-0.003	-0.007	-0.003
	(0.004)	(0.006)	(0.008)	(0.008)
Assume the gain at the 75th percentile	-0.012**	-0.011*	-0.011*	-0.012
	(0.004)	(0.006)	(0.006)	(0.008)
Assume the gain at the 90th percentile	-0.014**	-0.014**	-0.012*	-0.017**
	(0.005)	(0.007)	(0.007)	(0.009)
Worst Case Bounding				
Losers with missing scores set to the 10th percentile; Winners with missing scores set to the 90th percentile	0.143**	0.119**	0.134**	0.117**
	(0.014)	(0.018)	(0.015)	(0.018)
Vice versa	-0.160**	-0.135**	-0.133**	-0.139**
	(0.011)	(0.015)	(0.014)	(0.014)
Losers with missing scores assigned gains at the 10th percentile; Winners with missing scores assigned gains at the 90th percentile	0.091**	0.078**	0.080**	0.078**
	(0.009)	(0.012)	(0.010)	(0.013)
Vice versa	-0.111**	-0.091**	-0.097**	-0.093**
	(0.008)	(0.011)	(0.012)	(0.012)
Lee's Trimming Method (Lee 2002)				
Trim the lowest-scoring winners to eliminate any excess attrition among losers	-0.004	-0.003	-0.002	-0.005
	(0.005)	(0.007)	(0.007)	(0.009)
Trim the highest-scoring winners	-0.022**	-0.015**	-0.017**	-0.015
	(0.005)	(0.008)	(0.008)	(0.009)

Control Function Method				
Includes a cubic in the difference in the initial enrollment rate between winners and losers in a particular lottery, and the cubic interacted with an indicator for winning the lottery	-0.014** (0.006)	-0.013 (0.009)	-0.015* (0.009)	-0.012 (0.012)

Notes: Each cell reports results from a separate regression. All regressions include a set of lottery fixed effects as well as the student and neighborhood characteristics detailed in the notes to Table 4. The models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. Eicker-White robust standard errors adjusted to account for the correlation of errors within high school are shown in parentheses. ** significant at the 5% level * significant at the 10% level

Table B3. The impact of winning a lottery on student outcomes by school type, Lee (2002) bounding

	The effect of winning a lottery to …							
	Any school				High-achieving school			
	Bottom		Top		Bottom		Top	
	coeff	s.e.	coeff	s.e.	coeff	s.e.	coeff	se
Outcomes at the end of 4 years								
Graduated [a]	-0.029	0.021	-0.063	0.019	0.003	0.040	-0.031	0.035
Enrolled in the CPS [a]	0.003	0.009	-0.012	0.009	0.001	0.012	-0.011	0.012
Dropped out [a]	0.027	0.016	0.001	0.015	0.018	0.024	-0.005	0.025
Transferred to a private school in the Chicago MSA [a]	0.004	0.004	0.001	0.004	-0.003	0.005	-0.003	0.005
Moved out of the district [a]	0.030	0.010	0.007	0.009	0.016	0.021	-0.006	0.020
9th Grade Outcomes								
Reading percentile score	-0.004	0.005	-0.022	0.005	-0.002	0.007	-0.015	0.007
Algebra end-of-course exam score	0.012	0.007	-0.020	0.006	0.007	0.011	-0.032	0.010
English I end-of-course exam score	0.009	0.004	-0.010	0.004	0.009	0.005	-0.012	0.005
Spring semester fraction of days absent	0.006	0.003	-0.004	0.004	0.002	0.005	-0.004	0.005
Spring semester credits earned	0.103	0.091	-0.124	0.097	0.119	0.160	-0.050	0.166
Class percentile rank (1=best)	-0.004	0.010	-0.037	0.010	-0.034	0.025	-0.061	0.024
10th Grade Outcomes								
Reading percentile score [a]	0.007	0.006	-0.027	0.007	-0.007	0.009	-0.036	0.011
Geometry end-of-course exam score [a]	0.034	0.009	-0.006	0.009	0.035	0.018	-0.004	0.019
English II end-of-course exam score [a]	0.017	0.006	-0.012	0.006	0.004	0.011	-0.027	0.010
Spring semester fraction of days absent	0.012	0.005	-0.007	0.006	0.003	0.006	-0.011	0.006
Cumulative Spring semester credits earned	0.014	0.253	-0.495	0.226	0.353	0.303	-0.162	0.285

Class percentile rank (1=best)	-0.001	0.011	-0.046	0.009	-0.022	0.022	-0.065	0.020
11th Grade Outcomes								
Dropped out by Spring	0.017	0.009	0.001	0.008	0.009	0.010	-0.003	0.010
Retained (enrolled in grade below 11th grade)	0.011	0.013	-0.023	0.010	0.012	0.030	-0.011	0.024
Parental Support and Supervision								
Parents regularly help with schoolwork	0.080	0.030	-0.128	0.024	-0.081	0.047	-0.156	0.047
Parents regularly discuss class- and school-related issues with student	0.109	0.017	-0.047	0.027	0.131	0.025	0.081	0.031
Degree of parental supervision (composite)	0.533	0.130	-0.498	0.118	0.284	0.203	-0.121	0.232
Other Outcome Measures								
Student's liking for school (composite)	0.394	0.093	-0.391	0.106	0.446	0.126	-0.084	0.105
Degree of student-teacher trust (composite)	0.325	0.071	-0.323	0.054	0.166	0.097	-0.158	0.098
Positive classroom behavior of peers (composite)	0.134	0.072	-0.169	0.062	0.175	0.113	-0.005	0.098
Reports getting into trouble at school	0.055	0.045	-0.093	0.039	-0.064	0.051	-0.147	0.061
Arrested by police in past year	0.005	0.018	-0.053	0.017	-0.034	0.020	-0.044	0.016
Expects to graduate college	0.072	0.017	-0.030	0.020	0.044	0.034	0.009	0.028
Reports the classrooms/hallways are safe	0.128	0.022	-0.034	0.033	0.063	0.043	0.006	0.046
Reports school has enough computers for students to use	0.118	0.037	-0.035	0.027	0.185	0.035	0.098	0.035

Notes: Each cell reports the results from a separate regression. Outcomes for the sample of lottery winners have been replaced to missing either at the bottom of the distribution (in the columns labeled "Bottom") or at the top of the distribution (in the columns labeled "Top") at the rate that eliminates excess attrition among losers participating in the same lottery. All regressions include a set of lottery fixed effects as well as the student and neighborhood characteristics detailed in the notes to Table 4. Except for the binary variables, the models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. The models with binary dependent variables are estimated using a Probit specification, and we report the marginal effect of being selected evaluated at the mean of the other control variables. Eicker-White robust standard errors adjusted to account for the correlation of errors within high school are shown in parentheses. [a] Sample limited to the 2000 cohort due to data availability

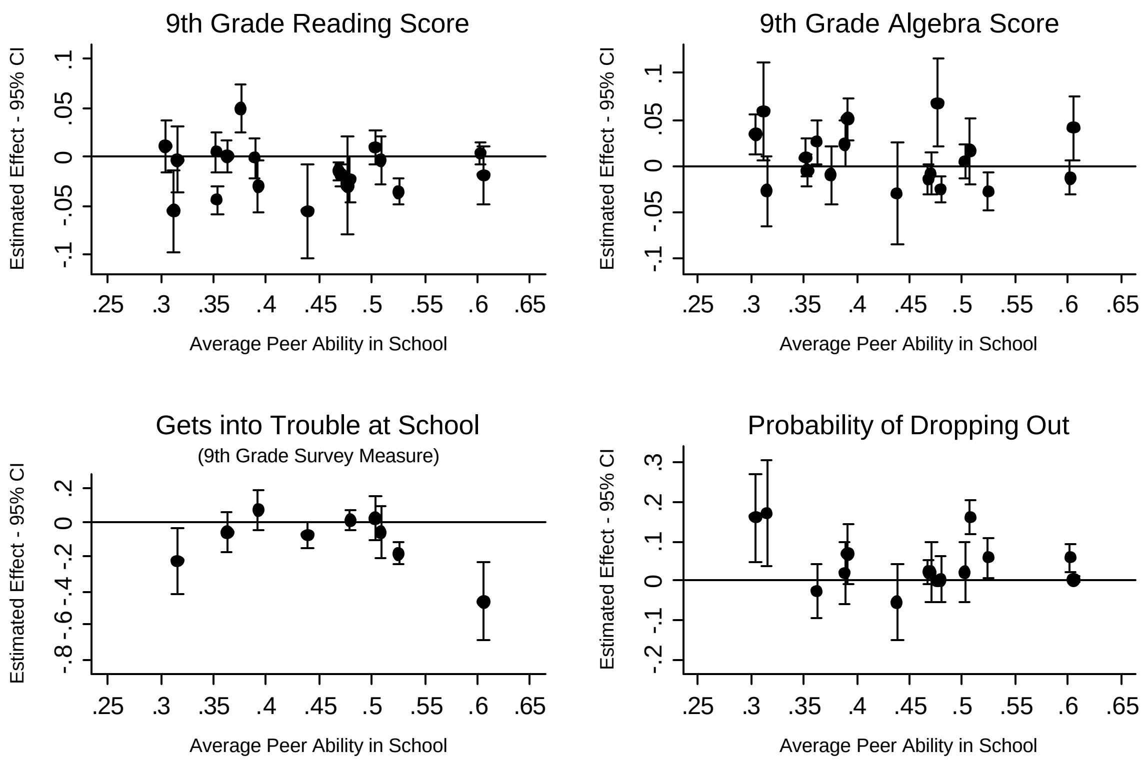
Table B3 (continued). The impact of winning a lottery on student outcomes by school type, Lee (2002) bounding

	The effect of winning a lottery to …							
	High value-added school				High popularity school			
	Bottom		Top		Bottom		Top	
	coeff	s.e.	coeff	se	coeff	se	coeff	se
Outcomes at the end of 4 years								
Graduated [a]	0.031	0.061	0.002	0.052	-0.032	0.050	-0.062	0.046
Enrolled in the CPS [a]	-0.023	0.011	-0.027	0.010	0.010	0.015	-0.001	0.015
Dropped out [a]	-0.001	0.031	-0.029	0.038	0.036	0.030	0.022	0.031
Transferred to a private school in the Chicago MSA [a]	0.003	0.007	0.003	0.007	-0.003	0.005	-0.004	0.005
Moved out of the district [a]	0.029	0.028	0.003	0.026	0.018	0.028	0.008	0.026
9th Grade Outcomes								
Reading percentile score	-0.002	0.007	-0.016	0.008	-0.004	0.009	-0.016	0.009
Algebra end-of-course exam score	0.008	0.011	-0.021	0.012	0.008	0.013	-0.030	0.011
English I end-of-course exam score	0.021	0.006	0.003	0.006	0.007	0.006	-0.014	0.005
Spring semester fraction of days absent	-0.002	0.004	-0.010	0.004	0.006	0.006	0.002	0.006
Spring semester credits earned	0.119	0.109	-0.029	0.121	0.062	0.198	-0.127	0.202
Class percentile rank (1=best)	-0.014	0.028	-0.040	0.028	-0.055	0.026	-0.085	0.026
10th Grade Outcomes								
Reading percentile score [a]	0.000	0.013	-0.029	0.016	-0.014	0.012	-0.042	0.012
Geometry end-of-course exam score [a]	0.069	0.021	0.045	0.022	0.024	0.021	-0.025	0.023
English II end-of-course exam score [a]	0.003	0.012	-0.020	0.013	0.004	0.011	-0.028	0.009
Spring semester fraction of days absent	0.000	0.007	-0.011	0.006	0.009	0.008	-0.008	0.008
Cumulative Spring semester credits earned	0.278	0.193	-0.099	0.196	0.237	0.372	-0.271	0.355

Class percentile rank (1=best)	0.002	0.025	-0.029	0.024	-0.042	0.021	-0.094	0.017
11th Grade Outcomes								
Dropped out by Spring	-0.009	0.009	-0.021	0.009	0.017	0.013	0.007	0.012
Retained (enrolled in grade below 11th grade)	0.004	0.030	-0.027	0.023	0.028	0.032	0.000	0.026
Parental Support and Supervision								
Parents regularly help with schoolwork	-0.090	0.044	-0.105	0.049	-0.051	0.072	-0.186	0.073
Parents regularly discuss class- and school-related issues with student	0.086	0.042	0.032	0.042	0.138	0.041	0.071	0.061
Degree of parental supervision (composite)	0.064	0.317	-0.192	0.339	0.220	0.266	-0.331	0.292
Other Outcome Measures								
Student's liking for school (composite)	0.466	0.245	-0.059	0.196	0.358	0.184	-0.205	0.170
Degree of student-teacher trust (composite)	0.017	0.140	-0.166	0.133	0.393	0.136	-0.067	0.198
Positive classroom behavior of peers (composite)	0.097	0.108	-0.055	0.100	0.197	0.124	-0.030	0.133
Reports getting into trouble at school	-0.070	0.078	-0.113	0.080	-0.068	0.066	-0.195	0.064
Arrested by police in past year	-0.030	0.017	-0.030	0.017	-0.013	0.009	-0.026	0.019
Expects to graduate college	0.021	0.052	0.008	0.048	0.059	0.037	-0.019	0.044
Reports the classrooms/hallways are safe	0.014	0.054	-0.042	0.054	0.132	0.047	0.061	0.051
Reports school has enough computers for students to use	0.073	0.057	-0.009	0.060	0.258	0.071	0.168	0.070

Notes: Each cell reports the results from a separate regression. Outcomes for the sample of lottery winners have been replaced to missing either at the bottom of the distribution (in the columns labeled "Bottom") or at the top of the distribution (in the columns labeled "Top") at the rate that eliminates excess attrition among losers participating in the same lottery. All regressions include a set of lottery fixed effects as well as the student and neighborhood characteristics detailed in the notes to Table 4. Except for the binary variables, the models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. The models with binary dependent variables are estimated using a Probit specification, and we report the marginal effect of being selected evaluated at the mean of the other control variables. Eicker-White robust standard errors adjusted to account for the correlation of errors within high school are shown in parentheses. [a] Sample limited to the 2000 cohort due to data availability

Figure 1: The Effects of Winning a Lottery by School



Note: Each bar represents the effect for a separate school.

Table 1. Chicago public high schools represented in the analysis

High school name	Mean peer achievement	Mean value-added	Fraction of applicants accepted	Fraction of accepted applicants enrolling	Number of analysis lotteries	Number of participants in analysis lotteries
	(1)	(2)	(3)	(4)	(5)	(6)
Von Steuben Metro	**0.622**	**0.013**	**0.069**	**0.586**	15	5,888
Chicago Agricultural Science	**0.611**	-0.007	**0.127**	**0.663**	9	627
Curie Metro	**0.528**	0.000	**0.121**	**0.632**	56	898
Hyde Park Academy	**0.511**	-0.005	**0.104**	**0.399**	5	1,243
Kennedy	**0.500**	**0.005**	0.433	0.242	7	817
George Washington	0.492	-0.010	0.684	0.251	5	355
Lake View	0.488	**0.049**	0.540	0.185	9	144
Taft	0.486	-0.029	0.195	0.237	16	1,881
Bogan Technical	0.470	-0.004	0.174	0.364	12	3,289
Amundsen	0.439	-0.001	**0.052**	**0.593**	6	522
Senn Metro Academy	0.393	-0.008	0.327	0.175	11	831
Juarez Community Academy	0.376	-0.007	0.227	0.123	5	241
Roosevelt	0.371	-0.014	0.200	0.259	16	860
Hirsch Metro	0.353	**0.001**	0.569	0.342	2	240
Corliss	0.352	**0.024**	0.463	0.280	2	365
Wells	0.362	-0.020	0.619	0.261	7	654
Robeson	0.312	-0.012	0.303	0.116	2	131
Harper	0.310	-0.016	0.180	0.169	7	366
Orr Community Academy	0.305	-0.033	0.372	0.136	2	168

Notes: The summary statistics in columns 1 and 2 are based on all 9th-graders enrolled in these high schools in Fall 2000 and Fall 2001. Column 1 reports the mean composite 8th grade math and reading percentile scores for entering students, where a value of 0.5 indicates that the student is performing at national norms. Value-added in column 2 is calculated by extracting the mean residual by high school campus from a student-level regression of 9th grade reading percentile score on flexible controls for 8th grade reading score, student demographic characteristics, and 8th grade campus-fixed effects over a three-year period (Springs 1999-2001). The statistics reported in columns 3 and 4 are averages across all 2000 and 2001 applications, regardless of whether an individual application is involved in a non-degenerate lottery or not. A bold value indicates that the high school is in the top (or bottom for (3)) quartile of analysis schools on this measure. Columns 5 and 6 describe the non-degenerate lotteries and applications included in the empirical analysis.

Table 2. Comparison of lottery participants to 8th grade non-applicants

Student characteristic	Lottery participants (1)	Non-applicants (2)	Difference (1) – (2)	Standard error of the difference
White	0.119	0.113	0.006	0.003
Black	0.460	0.526	-0.066	0.005
Hispanic	0.370	0.338	0.032	0.005
Male	0.423	0.563	-0.139	0.005
8th grade math percentile score	0.526	0.389	0.137	0.003
8th grade reading percentile score	0.485	0.368	0.117	0.002
Free lunch eligible	0.725	0.757	-0.032	0.004
Receiving special education	0.116	0.256	-0.140	0.004
Ever received bilingual education	0.432	0.358	0.074	0.005
Living with a biological parent	0.800	0.786	0.013	0.004
Tract poverty rate	0.218	0.250	-0.031	0.001
Tract fraction high school graduates	0.646	0.638	0.009	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 34,570 8th graders enrolled in CPS in Spring 2000 and Spring 2001 that we do not observe submitting an application to a choice school. Mean characteristics for these students are shown in column 2.

Table 3. Testing the validity of the lotteries

	Lottery losers	All lottery participants		Participants enrolled in 9th grade the following Fall	
Dependent variable	Mean of dep. var.	Effect of winning	Std. Error	Effect of winning	Std. Error
	(1)	(2)	(3)	(4)	(5)
Student's characteristics at time of application					
8th grade math percentile score	0.520	-0.001	0.005	-0.002	0.006
8th grade reading percentile score	0.479	-0.003	0.005	-0.004	0.005
Age	13.951	0.006	0.011	0.007	0.012
Free lunch eligible	0.734	0.006	0.011	0.003	0.011
Reduced-price lunch eligible	0.106	0.000	0.007	0.000	0.008
Receiving special education	0.112	0.009	0.008	0.008	0.008
Ever received bilingual education	0.418	-0.012	0.018	-0.010	0.020
Living with a biological parent	0.800	-0.002	0.010	-0.004	0.010
Attends assigned 8th grade school	0.615	0.005	0.012	0.016	0.013
Number of applications submitted	3.397	0.001	0.048	0.003	0.051
School and neighborhood characteristics at time of application					
Mean achievement level in school	0.428	0.003	0.003	0.005	0.003
Fraction transferring into school	0.331	-0.005	0.007	-0.009	0.007
Tract fraction Black	0.423	0.003	0.005	0.002	0.005
Tract fraction Hispanic	0.319	-0.004	0.005	-0.005	0.005
Tract poverty rate	0.222	0.001	0.003	0.000	0.003
Tract fraction high school graduates	0.647	0.001	0.003	0.002	0.003
Tract fraction homeowners	0.419	0.005	0.005	0.007	0.005
Tract fraction not in the labor force	0.412	0.002	0.002	0.002	0.002
Tract crime index	{0.637}	0.012	0.012	0.010	0.013
Tract fraction in private high schools	0.144	0.002	0.003	0.002	0.003
8th grade survey measures (2001 cohort)					
Responded to survey	0.676	0.004	0.016	0.009	0.016
Social resources in community (composite)	{1.642}	0.020	0.065	-0.006	0.067
Student's liking for school (composite)	{2.046}	0.052	0.081	0.048	0.084
Parents' support for learning (composite)	{1.537}	0.024	0.058	0.011	0.060
Degree of parental supervision (composite)	{2.206}	0.190**	0.085	0.157*	0.090
Home educational resources (composite)	{1.815}	0.043	0.071	0.048	0.073
Regularly participates in school clubs or orgs.	0.479	0.022	0.021	0.024	0.021
Born in U.S.	0.858	0.005	0.014	0.004	0.015
Speaks a language other than English	0.549	-0.010	0.024	-0.000	0.025
Attends religious services weekly	0.417	0.015	0.020	0.011	0.021
Reports getting into trouble at school	0.682	0.009	0.019	0.004	0.020
Lives with both parents	0.465	-0.012	0.022	-0.027	0.022
Mother completed some college	0.573	0.005	0.025	-0.006	0.027
Status in the Fall following application					
Enrolled in CPS in the 9th grade in the Fall	0.895	0.019**	0.007	NA	NA
Leaves for private high school in the Fall	0.031	-0.007**	0.003	NA	NA

Notes: Column 1 reports the mean (or standard deviation {} for index measures) among lottery losers for the dependent variable indicated in the row heading. The remaining columns report results from separate regressions of the dependent variables on an indicator for being selected in a lottery and a full set of lottery fixed effects. Except for the binary variables, the models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. The models with binary dependent variables are estimated using a Probit specification, and we report the marginal effect of being selected evaluated at the mean of the lottery indicators. Eicker-White robust standard errors adjusted to account for the correlation of errors across applications within a single student are

reported in columns 3 and 5. The results shown in columns 2-3 are based on the full sample of 19,520 applications involved in the 194 non-degenerate lotteries. The results shown in columns 4-5 are based on the subset of applications from students who enroll in 9th grade in CPS in the following Fall. ** significant at the 5% level
* significant at the 10% level

Table 4. The impact of winning a lottery on the characteristics of the school attended by lottery school type

Dependent variable	The effect of winning a lottery to …			
	Any school	High-achieving school	High value-added school	High popularity school
	(1)	(2)	(3)	(4)
Type of high school attended				
School for which lottery applies	0.283**	0.350**	0.242**	0.396**
	(0.013)	(0.023)	(0.024)	(0.026)
	[0.069]	[0.082]	[0.072]	[0.082]
Any school other than the student's attendance area school	0.096**	0.128**	0.119**	0.136**
	(0.009)	(0.012)	(0.015)	(0.014)
	[0.759]	[0.763]	[0.739]	[0.759]
School in top quartile in terms of peer achievement	0.082**	0.372**	0.251**	0.395**
	(0.014)	(0.022)	(0.029)	(0.026)
	[0.350]	[0.431]	[0.422]	[0.407]
School in top quartile in terms of value-added	0.014	0.105**	0.274**	0.031
	(0.013)	(0.024)	(0.023)	(0.028)
	[0.391]	[0.452]	[0.501]	[0.455]
Non-lottery selective admissions school	-0.007**	-0.016	-0.025*	-0.027**
	(0.003)	(0.011)	(0.013)	(0.012)
	[0.125]	[0.187]	[0.222]	[0.187]
Non-lottery career academy	-0.044**	-0.057**	-0.005	-0.066**
	(0.007)	(0.007)	(0.010)	(0.007)
	[0.159]	[0.126]	[0.087]	[0.128]
High school characteristic				
Fraction of 9th graders at or above norms on high school exams	0.025**	0.057**	0.045**	0.058**
	(0.004)	(0.007)	(0.008)	(0.008)
	[0.349]	[0.408]	[0.434]	[0.407]
Mean combined 8th grade math and reading percentile scores of 9th graders	0.019**	0.043**	0.025**	0.049**
	(0.003)	(0.005)	(0.005)	(0.005)
	[0.482]	[0.517]	[0.528]	[0.514]
Mean combined 8th grade scores of 9th graders in the student's English class	0.009**	0.020**	0.014**	0.021**
	(0.003)	(0.005)	(0.005)	(0.006)
	[0.475]	[0.523]	[0.535]	[0.514]
Value-added measure	0.001*	0.003**	0.005**	0.002**
	(0.000)	(0.001)	(0.001)	(0.001)
	[0.003]	[0.006]	[0.008]	[0.006]
Fraction of 8th grade peers attending the student's 9th grade school	-0.039**	-0.058**	-0.054**	-0.060**
	(0.004)	(0.006)	(0.007)	(0.006)
	[0.167]	[0.168]	[0.174]	[0.169]
Fraction of 9th grade peers from the student's 8th grade school	-0.008**	-0.013**	-0.011**	-0.014**
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.042]	[0.038]	[0.039]	[0.039]

Fraction of students receiving free lunch	-0.021**	-0.044**	-0.027**	-0.056**
	(0.003)	(0.006)	(0.007)	(0.007)
	[0.786]	[0.747]	[0.736]	[0.744]
Graduation rate	0.018**	0.033**	0.023**	0.043**
	(0.003)	(0.004)	(0.004)	(0.005)
	[0.689]	[0.714]	[0.715]	[0.709]
Index of crime level in the neighborhood of the school	-0.194**	-0.268**	-0.151**	-0.287**
	(0.023)	(0.042)	(0.047)	(0.051)
	{1.052}	{1.189}	{1.272}	{1.201}
Ln(median household income) in the neighborhood of the school	0.053**	0.010	0.016	-0.010
	(0.010)	(0.017)	(0.018)	(0.019)
	[10.45]	[10.48]	[10.52]	[10.48]

Notes: Each cell reports results from a separate regression. All regressions include a set of lottery fixed effects as well as student characteristics (black, Hispanic, male, 8th grade math percentile score, 8th grade reading percentile score, age, free lunch eligible, receiving special education in 8th grade, ever received bilingual education up to and including 8th grade, living with a biological parent in 8th grade, attending assigned 8th grade school) and neighborhood (census tract) characteristics (fraction black, fraction Hispanic, poverty rate, fraction high school graduates, fraction home-owners, fraction not in the labor force, crime index, fraction of 9th –12th graders attending private high schools). Except for the binary variables, the models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. The models with binary dependent variables are estimated using a Probit specification, and we report the marginal effect of being selected evaluated at the mean of the other control variables. Eicker-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 (the means for applications from students not selected in the lottery) are shown in square brackets (standard deviations are shown in {} for index measures instead). ** significant at the 5% level * significant at the 10% level

Table 5. The impact of winning a lottery on traditional measures of student outcomes by lottery school type

Dependent variable	The effect of winning a lottery to …			
	Any school	High-achieving school	High value-added school	High popularity school
	(1)	(2)	(3)	(4)
Outcomes at the end of 4 years				
Graduated [a]	-0.048**	-0.019	0.012	-0.050
	(0.020)	(0.036)	(0.054)	(0.047)
	[0.583]	[0.636]	[0.635]	[0.621]
Enrolled in the CPS [a]	0.000	-0.001	-0.023**	0.008
	(0.009)	(0.012)	(0.011)	(0.014)
	[0.084]	[0.071]	[0.071]	[0.073]
Dropped out [a]	0.021	0.013	-0.005	0.033
	(0.016)	(0.024)	(0.031)	(0.030)
	[0.192]	[0.155]	[0.152]	[0.164]
Transferred to a private school in the Chicago MSA [a]	0.003	-0.003	0.003	-0.003
	(0.004)	(0.004)	(0.007)	(0.005)
	[0.016]	[0.017]	[0.016]	[0.017]
Moved out of the district [a]	0.024**	0.010	0.023	0.014
	(0.009)	(0.021)	(0.027)	(0.028)
	[0.107]	[0.104]	[0.108]	[0.108]
9th Grade Outcomes				
Reading percentile score	-0.013**	-0.009	-0.010	-0.010
	(0.005)	(0.007)	(0.008)	(0.009)
	[0.415]	[0.469]	[0.487]	[0.456]
Algebra end-of-course exam score	-0.002	-0.009	-0.005	-0.008
	(0.007)	(0.010)	(0.012)	(0.012)
	[0.446]	[0.492]	[0.518]	[0.488]
English I end-of-course exam score	-0.001	0.000	0.013*	-0.002
	(0.004)	(0.005)	(0.006)	(0.005)
	[0.596]	[0.620]	[0.628]	[0.619]
Spring semester fraction of days absent	0.003	-0.001	-0.005	0.004
	(0.003)	(0.005)	(0.004)	(0.006)
	[0.101]	[0.087]	[0.083]	[0.090]
Spring semester credits earned	-0.029	0.020	0.036	-0.052
	(0.093)	(0.165)	(0.114)	(0.201)
	[27.68]	[27.83]	[27.87]	[27.84]
Class percentile rank (1=best)	-0.020*	-0.047*	-0.029	-0.069**
	(0.011)	(0.025)	(0.029)	(0.027)
	[0.577]	[0.593]	[0.591]	[0.584]
10th Grade Outcomes				

Reading percentile score [a]	-0.010	-0.022**	-0.016	-0.027**
	(0.007)	(0.010)	(0.014)	(0.012)
	[0.467]	[0.523]	[0.543]	[0.509]
Geometry end-of-course exam score [a]	0.013	0.016	0.056**	0.001
	(0.009)	(0.018)	(0.021)	(0.021)
	[0.569]	[0.621]	[0.646]	[0.613]
English II end-of-course exam score [a]	0.002	-0.010	-0.009	-0.011
	(0.006)	(0.011)	(0.013)	(0.010)
	[0.517]	[0.548]	[0.556]	[0.543]
Spring semester fraction of days absent	0.006	-0.002	-0.003	0.003
	(0.005)	(0.006)	(0.007)	(0.007)
	[0.115]	[0.102]	[0.095]	[0.106]
Cumulative Spring semester credits earned	-0.279	0.073	0.077	-0.042
	(0.237)	(0.305)	(0.199)	(0.369)
	[55.61]	[55.76]	[55.80]	[55.81]
Class percentile rank (1=best)	-0.025**	-0.045**	-0.017	-0.068**
	(0.011)	(0.020)	(0.025)	(0.020)
	[0.548]	[0.562]	[0.563]	[0.552]
11th Grade Outcomes				
Dropped out by Spring	0.013	0.006	-0.012	0.015
	(0.009)	(0.010)	(0.008)	(0.012)
	[0.119]	[0.093]	[0.090]	[0.099]
Retained (enrolled in grade below 11th grade)	-0.001	0.001	-0.004	0.014
	(0.012)	(0.028)	(0.029)	(0.031)
	[0.133]	[0.113]	[0.111]	[0.120]

Notes: Each cell reports the results from a separate regression. All regressions include a set of lottery fixed effects as well as the student and neighborhood characteristics detailed in the notes to Table 4. Except for the binary variables, the models are estimated by ordinary least squares and the coefficient on the indicator for being selected is reported. The models with binary dependent variables are estimated using a Probit specification, and we report the marginal effect of being selected evaluated at the mean of the other control variables. Eicker-White robust standard errors adjusted to account for the correlation of errors within high school are shown in parentheses. Control group means (the means for applications from students not selected in the lottery) are shown in square brackets.
** significant at the 5% level * significant at the 10% level
[a] Sample limited to the 2000 cohort due to data availability.

Table 6: The impact of winning a lottery on the characteristics of the school attended by student type

Dependent variable	All students	Black	Hispanic	Male	Below average 8^{th} grade test scores	Potential increase in peer quality: Bottom Quartile	Top Quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Type of high school attended							
School for which lottery applies	0.283**	0.293**	0.276**	0.284**	0.310**	0.181**	0.504**
	(0.013)	(0.018)	(0.022)	(0.019)	(0.018)	(0.022)	(0.041)
	[0.069]	[0.062]	[0.060]	[0.079]	[0.052]	[0.069]	[0.034]
High school characteristic							
Mean combined 8^{th} grade math and reading percentile scores of 9^{th} graders	0.019**	0.016**	0.024**	0.024**	0.023**	-0.024**	0.089**
	(0.003)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)	(0.007)
	[0.482]	[0.458]	[0.477]	[0.485]	[0.415]	[0.579]	[0.417]
Mean combined 8^{th} grade scores of 9^{th} graders in the student's English class	0.009**	0.006	0.013**	0.010**	0.014**	-0.017**	0.043**
	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)	(0.008)
	[0.475]	[0.445]	[0.474]	[0.471]	[0.363]	[0.596]	[0.399]
Value-added measure	0.001*	0.000	0.001	0.001	0.002**	-0.003**	0.007**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.003]	[-0.000]	[0.002]	[0.003]	[-0.006]	[0.016]	[-0.006]
Fraction of 8th grade peers attending the student's 9th grade school	-0.039**	-0.030**	-0.057**	-0.039**	-0.044**	-0.010	-0.094**
	(0.004)	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)	(0.010)
	[0.167]	[0.140]	[0.197]	[0.173]	[0.188]	[0.134]	[0.204]

Notes: See notes to Table 4. For columns (6) and (7), the potential increase in peer quality (mean combined 8^{th} grade math and reading percentile scores of 9^{th} graders) is calculated as peer quality at the lottery school less peer quality at a student's inside option. We measure peer quality at a student's inside option using the average peer quality enjoyed by students who attended the same 8^{th} grade campus and placed within the same 8^{th}-grade test score quintile. ** significant at the 5% level * significant at the 10% level

Table 7. The impact of winning a lottery on traditional measures of student outcomes by student type

Dependent variable	All students	Black	Hispanic	Male	Below average 8th grade test scores	Potential increase in peer quality: Bottom Quartile	Potential increase in peer quality: Top Quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcomes at the end of 4 years							
Graduated [a]	-0.048**	-0.109**	0.003	-0.066**	-0.073**	-0.088**	-0.152**
	(0.020)	(0.022)	(0.028)	(0.026)	(0.026)	(0.040)	(0.061)
	[0.583]	[0.550]	[0.594]	[0.528]	[0.486]	[0.675]	[0.534]
Enrolled in the CPS [a]	0.000	0.005	-0.005	0.017	-0.018	0.004	0.011
	(0.009)	(0.017)	(0.012)	(0.018)	(0.016)	(0.013)	(0.026)
	[0.084]	[0.096]	[0.086]	[0.107]	[0.104]	[0.067]	[0.082]
Dropped out [a]	0.021	0.040*	0.027	0.016	0.040*	0.012	0.113**
	(0.016)	(0.025)	(0.026)	(0.024)	(0.025)	(0.022)	(0.055)
	[0.192]	[0.233]	[0.162]	[0.208]	[0.253]	[0.150]	[0.227]
Transferred to a private school in the Chicago MSA [a]	0.003	0.011	-0.003	0.001	0.013	0.000	0.060**
	(0.004)	(0.008)	(0.006)	(0.007)	(0.010)	(0.007)	(0.028)
	[0.016]	[0.019]	[0.014]	[0.020]	[0.021]	[0.008]	[0.018]
Moved out of the district [a]	0.024**	0.040**	-0.001	0.029**	0.043**	0.048**	0.023
	(0.009)	(0.014)	(0.017)	(0.014)	(0.019)	(0.018)	(0.041)
	[0.107]	[0.086]	[0.122]	[0.115]	[0.122]	[0.078]	[0.125]
9th Grade Outcomes							
Reading percentile score	-0.013**	-0.012*	-0.016**	-0.007	-0.003	-0.031**	-0.013
	(0.005)	(0.006)	(0.008)	(0.007)	(0.006)	(0.009)	(0.011)
	[0.415]	[0.370]	[0.422]	[0.420]	[0.267]	[0.563]	[0.327]
Algebra end-of-course exam score	-0.002	-0.006	0.006	-0.008	0.005	-0.014	0.012
	(0.007)	(0.008)	(0.011)	(0.009)	(0.008)	(0.011)	(0.013)
	[0.446]	[0.398]	[0.452]	[0.459]	[0.347]	[0.548]	[0.401]
English I end-of-course exam score	-0.001	-0.002	0.001	0.001	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.008)	(0.006)	(0.007)	(0.006)	(0.007)
	[0.596]	[0.569]	[0.605]	[0.580]	[0.522]	[0.665]	[0.542]

Spring semester fraction of days absent	0.003	0.008*	-0.008	0.004	0.009**	0.001	0.008
	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)
	[0.101]	[0.117]	[0.089]	[0.099]	[0.123]	[0.082]	[0.110]
Spring semester credits earned	-0.029	-0.089	0.144	0.069	-0.231*	-0.048	-0.317
	(0.093)	(0.141)	(0.129)	(0.108)	(0.142)	(0.130)	(0.282)
	[27.68]	[27.57]	[27.71]	[27.70]	[27.31]	[27.98]	[27.53]
Class percentile rank (1=best)	-0.020*	-0.032**	-0.000	-0.024**	-0.036**	0.007	-0.109**
	(0.011)	(0.012)	(0.012)	(0.012)	(0.014)	(0.011)	(0.036)
	[0.577]	[0.555]	[0.590]	[0.519]	[0.540]	[0.574]	[0.587]
10th Grade Outcomes							
Reading percentile score [a]	-0.010	-0.016	-0.005	0.002	0.008	-0.021**	-0.032
	(0.007)	(0.013)	(0.010)	(0.009)	(0.009)	(0.010)	(0.020)
	[0.467]	[0.418]	[0.471]	[0.457]	[0.312]	[0.638]	[0.368]
Geometry end-of-course exam score [a]	0.013	-0.002	0.026**	0.014	0.024**	-0.026*	0.051
	(0.009)	(0.011)	(0.013)	(0.009)	(0.012)	(0.013)	(0.034)
	[0.569]	[0.510]	[0.573]	[0.587]	[0.448]	[0.691]	[0.501]
English II end-of-course exam score [a]	0.002	0.004	0.003	0.001	0.010	-0.007	-0.013
	(0.006)	(0.005)	(0.010)	(0.008)	(0.008)	(0.011)	(0.010)
	[0.517]	[0.491]	[0.518]	[0.498]	[0.438]	[0.597]	[0.465]
Spring semester fraction of days absent	0.006	0.008	0.003	0.012	0.013*	0.009	0.026**
	(0.005)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.009)
	[0.115]	[0.140]	[0.096]	[0.110]	[0.141]	[0.093]	[0.124]
Cumulative Spring semester credits earned	-0.279	-0.359	-0.086	-0.196	-0.558**	-0.237	-0.472
	(0.237)	(0.292)	(0.334)	(0.289)	(0.265)	(0.191)	(0.389)
	[55.61]	[55.56]	[55.50]	[55.58]	[55.16]	[56.04]	[55.40]
Class percentile rank (1=best)	-0.025**	-0.034**	-0.014	-0.032**	-0.048**	0.006	-0.110**
	(0.011)	(0.014)	(0.013)	(0.015)	(0.013)	(0.015)	(0.022)
	[0.548]	[0.525]	[0.555]	[0.483]	[0.500]	[0.556]	[0.546]
11th Grade Outcomes							
Dropped out by Spring	0.013	0.029**	-0.003	0.022	0.028**	0.015	0.009
	(0.009)	(0.012)	(0.014)	(0.015)	(0.013)	(0.014)	(0.024)
	[0.119]	[0.131]	[0.112]	[0.129]	[0.160]	[0.092]	[0.137]

Retained (enrolled in grade below 11th grade)	-0.001	-0.003	0.006	0.017	-0.004	-0.013	0.017
	(0.012)	(0.017)	(0.019)	(0.017)	(0.019)	(0.013)	(0.028)
	[0.133]	[0.155]	[0.126]	[0.164]	[0.188]	[0.101]	[0.160]

Notes: See notes to Table 5. For columns (6) and (7), the potential increase in peer quality (mean combined 8th grade math and reading percentile scores of 9th graders) is calculated as peer quality at the lottery school less peer quality at a student's inside option. We measure peer quality at a student's inside option using the average peer quality enjoyed by students who attended the same 8th grade campus and placed within the same 8th-grade test score quintile. ** significant at the 5% level * significant at the 10% level

[a] Sample limited to the 2000 cohort due to data availability.

Table 8. The effect of winning a lottery on non-traditional outcomes, by school type

Dependent Variable	The effect of winning a lottery to …	
	Any school	High-achieving school
	(1)	(2)
Parental Support and Supervision		
Parents regularly help with schoolwork	-0.023	-0.113**
	(0.022)	(0.041)
	[0.443]	[0.436]
Parents regularly discuss class- and school-related issues with student	0.023	0.107**
	(0.021)	(0.028)
	[0.734]	[0.758]
Degree of parental supervision (composite)	0.025	0.110
	(0.107)	(0.202)
	{2.24}	{2.25}
Other Outcome Measures		
Student's liking for school (composite)	-0.008	0.215
	(0.073)	(0.133)
	{1.84}	{1.69}
Degree of student-teacher trust (composite)	-0.017	0.018
	(0.053)	(0.091)
	{1.46}	{1.43}
Positive classroom behavior of peers (composite)	-0.070	0.079
	(0.072)	(0.100)
	{1.12}	{1.10}
Reports getting into trouble at school	-0.007	-0.096*
	(0.037)	(0.050)
	[0.636]	[0.583]
Arrested by police in past year	-0.020	-0.040**
	(0.015)	(0.018)
	[0.116]	[0.089]
Expects to graduate college	0.011	0.019
	(0.017)	(0.027)
	[0.823]	[0.867]
Reports the classrooms/hallways are safe	0.043*	0.033
	(0.023)	(0.046)
	[0.643]	[0.671]
Reports school has enough computers for students to use	0.060**	0.142**
	(0.030)	(0.034)
	[0.621]	[0.644]

Notes: See notes to Table 5. The sample is limited to students in the 2000 cohort and excludes students who applied to three schools (Von Steuben Metro, Roosevelt and Lake View) that did not administer the survey.
** significant at the 5% level * significant at the 10% level