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School choice reforms have been proposed as ways to enhance efficiency, equity, and effectiveness in education. This study examines the consequences of participating in public high school choice in Chicago, a city with a wide variety of choice programs, including career academies, charter schools, magnet schools, and selective test-based college prep high schools. The analysis uses population-level administrative and survey data on all public school eighth graders enrolled in Chicago to estimate the effect of school choice participation on on-time graduation propensity (i.e., in 4 years). Techniques employed to estimate this effect include propensity score, catchment area fixed effects, and multilevel analysis. Results suggest that there is a modest positive graduation benefit from exercising school choice. There are no racial/ethnic differences in the choice benefit, but low-achieving students benefit less from high school choice than high-achieving students. In addition, students in high-poverty neighborhoods gain less from exercising choice than do students in low-poverty neighborhoods. These findings call into question the extent to which school choice enhances equity for low-achieving students and students in high-poverty neighborhoods.

Keywords: *school choice, high school reform, evaluation*

The recent growth of public and private school choice programs (Wirt et al., 2004) has injected additional urgency into the questions of whether such programs promote competition, improve the educational outcomes of participants, and enhance equality of educational opportunity. Some argue that expanding school choice through vouchers, charter schools, or public school choice will allow disadvantaged students in low-performing schools to access higher quality schools (Friedman, 1955; Neal, 1997). Others believe that expanding school choice, which privileges parental preferences over state prerogatives, will worsen inequality and segregation because more advantaged parents will be more likely to participate in such programs (Henig, 1995; Lee, Croninger, & Smith, 1996; Reay, 2003; Reay & Ball, 1997). A 2002 review found that a sizable majority of studies found beneficial effects of competition, although the sizes of the reported effects were generally small (Belfield & Levin, 2002). Studies of the effect of school voucher program participation on test score achievement have shown mixed results with some studies showing modest positive effects for some groups of students (Barnard, Frangakis, Hill, & Rubin, 2003; Greene, 2000; Greene, Peterson, & Du, 1997; Mayer, Peterson,

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Myers, Tuttle, & Howell, 2002; Peterson & Howell, 2004; Rouse, 1998) and others showing no effects (Krueger & Zhu, 2004; Witte, 2000; Wolf, Gutmann, Eissa, Puma, & Silverberg, 2006).

This article examines differences in graduation rates between participants and nonparticipants of Chicago's many public high school choice programs. To test whether school choice promotes equity in educational attainment, this study also examines whether the association between school choice and attainment varies for students in poor and nonpoor neighborhoods. This study investigates high school graduation as an outcome because compared to test score outcomes, it is relatively understudied in the school choice literature, and it is an outcome of critical importance for adult employment prospects, earning potential, and transition to postsecondary education. Although scholars have attempted to estimate the effects of school choice participation on test score achievement from attending particular types of choice programs, very few, if any, have investigated whether school choice participants are more likely to graduate than nonparticipants in such a wide range of school and program types represented in this study.

This study examines school choice in Chicago, a city with a large public school choice program in which more than half of secondary-aged students participate. School choice in Chicago emerged in the early 1980s as a solution to a problem of educational inequity and segregation (Lauen, 2006). Specifically, it was intended to address the concern that poor and minority students tended to enroll in high-poverty, predominantly minority schools without access to high-quality teachers, middle-class peers, and adequate access to enriched programs and activities. As the demographics of Chicago's public school population shifted from predominantly White to predominantly minority, school choice became a way to retain and attract high-achieving White and Asian American students, many of whom were from middle-class households. As a consequence, today, public school choice in Chicago encompasses a wide variety of schools for the wide range of student backgrounds and abilities. The range of schools includes public military academies, charter schools for at-risk youth, career academies, and highly selective college preparatory academies for the academic elite.

Due to the range of schools included in this study and the absence of experimental data to use to estimate the causal effect of attending a school of choice in Chicago, this study cannot distinguish between the consequences of attending a school of choice and the act of choosing, which could be driven by unobservables such as family commitment to education. If the design of this study included random assignment and there was little attrition, then the inferences would be on stronger ground. Even small-scale randomized studies of vouchers and charter schools, however, have stirred a great deal of debate, suggesting that no one study, even one with a randomized design, is decisive. This study relies on observational data and a number of data analysis techniques, including propensity score matching and comparing students from the same neighborhood, to examine the association between school choice participation and graduation propensity.

A review of recent within-study comparisons of experimental and nonexperimental estimates found that propensity score methods offer substantial reduction in bias when the following practices are employed: matching on intact groups and/or on participants from the same geographic area, pretests, and measures associated with the selection process (Cook, Shadish, & Wong, 2008). This study matches treated students to untreated students from the same geographic location and takes into account a number of important predictors of choice propensity including mother's education, distance to choice and assigned schools, disciplinary incidences, parental support, student self-efficacy, and student achievement. These covariates model the selection criteria for many of the schools of choice in Chicago and the parental background factors that could bias the result if not taken into account. The results of this study suggest that there is a pressing need for both high-quality observational and experimental evidence about the relationship between school choice participation and high school graduation.

Background and Related Literature

For students to gain from school choice, we must believe that schools have effects on students

over and above what families and the students themselves bring to the educational process. Although it is reasonable to assume that schools have important effects on students, there is great debate about how large these effects are in relation to other important factors and whether or not school reform is likely to reduce inequality. Beginning with the Coleman report (Coleman et al., 1966) and Jencks (1972), scholars and policy makers began to examine the relative contribution of family and school factors in educational outcomes and the question of whether educational reform could reduce inequality (due to the large role that family background plays in explaining variance in test score achievement). Research has shown, however, that where one goes to school can affect the likelihood of graduating, an important precondition of attending college and increasing one's earning potential. Toles, Schulz, and Rice (1986) noted that there is wide between-school variation in dropout rates even once student differences are controlled. Bryk and Thum (1989) found that high levels of internal differentiation and weak school normative environments were associated with increasing the probability of dropping out. Rumberger and Thomas (2000) found that although much of the between-school variation in dropout rates can be attributed to student characteristics, about half was due to factors under the control of policy makers such as student–teacher ratios, teacher quality, school control, school size, and average daily attendance. Lee and Burkam (2003) found that curriculum, school size, and positive student–teacher relationships affected school dropout rates.

Research on Catholic schools helped sharpen the theoretical and methodological issues at stake in estimating school effects. Coleman and colleagues (Coleman & Hoffer, 1987; Coleman, Hoffer, & Kilgore, 1982) found evidence of positive effects of Catholic schools relative to public school students and especially strong test score growth and lower dropout rates for poor and minority students. Bryk, Lee, and Holland (1993) noted that prior Catholic school research had been limited by underspecified mechanisms, and they sought to remedy this by conducting case studies of 10 highly effective Catholic schools and quantitative analysis of the same national database used by Coleman and Hoffer. They found that student achievement in Catholic schools depended less on social and racial background because a common academic core was required of nearly all students. In a study from the late 1990s that employed an instrumental variables approach to control for selection bias, Neal (1997) found that Catholic high school education leads to higher probability of graduation and college graduation among minority youth. He argued that educational attainment is higher for urban minorities because public schools available to urban minorities are generally quite poor in comparison. He asked, "What are the benefits of Catholic schooling? The answer depends critically on the quality of available public school alternatives" (p. 121). Similarly, in a study using propensity score matching to reanalyze the Coleman and Hoffer Catholic school effect, Morgan (2001) found that the Catholic school effect is strongest among those students least likely to attend Catholic schools, which suggests that disadvantaged students may be the most likely to benefit from Catholic school education. Levin (1998) concludes in a review of empirical evidence on public and private school differences that whereas most studies report no or slight achievement advantages for private schools, Catholic schools tend to produce substantially higher rates of graduation, college attendance, and college graduation.

Scholarly attention to school vouchers is an example of a more recent debate about the complexity of estimating school effects. Initial evaluations of Milwaukee's voucher program found no consistent difference in the test scores of voucher participants and random samples of Milwaukee public school students (Witte, 2000). Two subsequent reanalyses estimated an "average effect on the treated" approach, which attempted to purge estimates of selection bias by comparing the outcomes of "lotteried-in" students with a control group who were "lotteried out." One study found consistent gains for voucher recipients with this technique (Greene et al., 1997). A second study found gains in some academic subjects and not others (Rouse, 1998). Estimates from private voucher programs suggest small positive, but nonrobust, effects for low-income Black males at only a few grade levels (Gill, Timpane, Ross, Brewer, & Booker, 2007). In a reanalysis of a private voucher

experiment in New York City, Krueger and Zhu (2004) found that claims of test score gains to Black students were not statistically significant when students with missing baseline scores were included in the analysis and when both mother's and father's race were taken into account when coding student's race.

Although there are relatively few studies of the effect of exercising public school choice on student outcomes, at least four studies have been conducted on public school choice programs in Chicago. These studies present a mixed picture of the effect of choice on student outcomes. A study of test score outcomes of students in three charter elementary schools found that among public school applicants, the treatment effect on the treated is higher than a control group of public school applicants who were lotteried out, but only for students who applied in early grades (Grades 1-5; Hoxby & Rockoff, 2004). A more recent study found that charter school attendance was associated with higher graduation and college attendance rates (Booker, Sass, Gill, & Zimmer, 2008). A much larger student lottery experiment found that students who attended a Chicago magnet high school/program were no more likely than nonattendees to benefit academically (i.e., they were no more likely to graduate, have higher test score gains, or have higher credit accumulation rates) but were less likely to be arrested and were less likely to report disciplinary incidents (Cullen, Jacob, & Levitt, 2006). An observational study covering an earlier time period and with a larger sample size found that whereas students who attend a nonneighborhood high school through Chicago's extensive open enrollment program were more likely than those who remained in their assigned school to graduate, this observed cross-sectional effect was most likely the result of selection effects (i.e., unobserved differences between choosers, who likely had higher levels of motivation, for example, and nonchoosers; Cullen, Jacob, & Levitt, 2005). The one exception to this finding was that students who attended career academies were more likely to graduate than observationally similar students who remained in their neighborhood schools.

Each of the four Chicago-based studies addresses a different population of choosers: elementary school choosers of three purposely chosen charter schools (Hoxby & Rockoff, 2004), attendees of between six and seven charter high schools (Booker et al., 2008), secondary magnet school/program choosers (Cullen et al., 2006), and all public school choosers (Cullen et al., 2005). The first study (Hoxby & Rockoff, 2004) is severely limited in its generalizability (although the students were chosen randomly, the schools in the study were not). The second study (Booker et al., 2008) includes all charter high schools in Chicago and provides confirmatory evidence from Florida but ignores other types of school choice programs that serve substantially more students in Chicago (in spring 2001, for example, charter school secondary enrollment made up less than 2% of total ninth-grade enrollment). The third study on Chicago's public magnet school/program lotteries (Cullen et al., 2006) also covers only a fraction of the secondary chooser population. The fourth study (Cullen et al., 2005) is the most comprehensive, but it covers the time period 1993–1995, before the mayor of Chicago and his designees created a number of selective enrollment programs to attract high-achieving and middle-class students away from the private sector and suburbs.

This study includes students in all types of public secondary choice programs, including the highly sought-after selective enrollment high schools, in Chicago during the 2000–2001 school year. About 11% of the 2,000 public school choice participants enrolled in choice programs that did not exist in 1995. These new programs included four additional selective enrollment high schools and seven charter schools. In 2000–2001, the city's six selective enrollment high schools enrolled students from throughout the city who met rigorous academic standards. To apply, students were required to have test scores at least at the national average (i.e., stanine five or higher) in both reading and mathematics.[1] Eligible students then sat for an entrance exam. On their applications, students rank ordered up to four schools. Entrance to schools was based on the students' priority ranking, their score on the entrance exam (30%), their seventh-grade reading and math test results (30%), seventh-grade reading, math, science, and social studies grades (30%), and seventh-grade attendance (10%). Only one in four students who applied survived the selection process

and enrolled. In addition to the selective enrollment college prep high schools, there were 11 International Baccalaureate programs within neighborhood high schools, which were also highly selective. Some career and technical schools also only accepted students who performed at grade level on standardized tests. In summary, in 2000–2001, only one quarter of high schools had no application or selection processes in place to admit students. About one in five was either selective or highly selective (students had to be at or above grade level to be considered eligible for admission). About one in three had a mix of selective and nonselective programs within the same school, and about one in six had no selection criteria but required an application for admission.[2]

The past 40 years of school effects research provide several lessons for this study. First, estimating such effects is methodologically complex, with the issue of selection looming large. Although lottery experiments usually have the benefit of randomization, which is a strong basis for estimating causal effects, voucher studies typically suffer from at least two weaknesses: limits to the generalizability due to the targeting to low-income and/or low-achieving students and attrition from the analytic sample (especially for those who do not get a voucher), which makes the treatment and control groups nonequivalent over time. A second lesson from school effects research is that treatment effects can vary across subpopulations. From the Coleman report, through the Catholic school research, and up to private school voucher studies, there is a consistent strand of evidence that suggests that Black and disadvantaged students tend to benefit more from school choice and private schooling. Finally, students whose administratively assigned schools are of particularly low quality may benefit more from school choice than students assigned to high-quality schools. This suggests that if school choice programs are designed to improve access to high-quality schools, then positive treatment effects for disadvantaged students would be a likely outcome. If, however, school choice programs are designed to provide access to schooling options for students who already have adequate access to high-quality options, we might expect minimal or null treatment effects for disadvantaged students.

Because experimental data on the wide variety of choice schools and programs were not available for this study, this study uses a variety of techniques to examine the association between choice participation and graduation propensity. The four techniques are linear probability and logistic regression (with and without interactions), propensity score matching, catchment area fixed-effects regression, and multilevel random-effects regression.

Empirical Strategy

Negotiating the school choice process in Chicago involves a great deal of time and effort on the part of students and families, with investigating alternatives, mapping transportation routes, and long nights of filling out applications. Designing school choice programs and admission criteria, operating lotteries, and conducting admissions take a great deal of institutional effort by school and district staff. Therefore, one might ask whether these substantial investments in school choice on the part of students, parents, and school professionals are worth the effort. On one hand, we might believe that facilitating a good match between emerging student interests and school mission might lead to better student graduation outcomes. On the other hand, if schools and programs are quite similar in content and quality, we might expect very little difference in student graduation outcomes. It could be, for example, that although students may gain higher quality schools by exercising school choice, they may be doing so simply by choosing schools along with similarly highly motivated peers. In other words, whether because of peer effects or the fact that highly motivated students were more likely to exercise school choice, school quality could be more a function of "selection" than "value added."

High school choice in Chicago involves a wide variety of programs, which vary in their selectivity and curricular approach. Table 1 outlines the destinations of the cohort of 2,000 eighth graders. About 40% of this cohort attended their assigned public high school. Black, White, Asian, female, higher income, and higher achieving students were less likely to remain in their neighborhood schools. Hispanic students were

TABLE 1

Percentage Distribution of 2000 Enrollment, Racial, Gender, and Poverty Status Composition, and Average Test Scores, by High School Type, Chicago, 2000

	% of Enrollment	% Black	% Hispanic	% White	% Asian	% Male	% Free Lunch	Mean Math Score
Total	100.0	51.1	34.2	11.1	3.6	49.0	83.5	8.35
Assigned neighborhood	41.2	42.8	45.4	9.3	2.5	52.1	88.8	7.89
Selective enrollment magnet	10.1	29.3	35.3	24.3	11.1	45.9	62.9	9.96
Career academy	11.0	87.6	9.4	2.6	0.0	42.8	89.9	8.33
Charter	1.4	55.3	39.8	4.5	0.0	48.9	89.8	8.00
Magnet	3.9	55.0	18.0	17.2	9.8	51.4	71.3	8.70
Other public	26.9	61.4	29.0	6.5	3.1	46.7	88.1	8.30
Private	5.5	25.6	33.6	37.0	3.8	52.6	53.0	8.90

Note. Math score is measured in grade equivalent (GE) units.

more likely to remain in their neighborhood schools. Ten percent of students attended a highly selective enrollment public high school, which admitted students based on test scores and grades. Students in these schools were disproportionately White and Asian, female, higher income, and higher achieving. Eleven percent of students attended a career academy, which enrolled disproportionate shares of Black, female, and low-income students. These schools generally admitted students who performed at or above grade level and offered a variety of career-themed curricula. A very small proportion (about 1%) of students attended charter high schools, which at this time served Black, Hispanic, low-income, and low-achieving students in disproportionate percentages.[3] About 4% of students attended stand-alone magnet schools, which admitted students by lottery and served disproportionate shares of Black, Asian, White, low-income, and higher achieving students. About one in five students attended another public alternative, which includes participation in the district's open enrollment program and magnet programs within high schools (schools within schools). These programs served Black and low-income student populations in disproportionate percentages. About 5% of eighth graders opted to attend a private high school for ninth grade. The students who did so were disproportionately White and high achieving, although not as high achieving as the students who enrolled in the selective enrollment magnets.

As indicated by Table 1, public school choice in Chicago, as in many large cities, is a heterogeneous collection of programs serving different types of students. Such programs emerged over time to serve a variety of public policy and educational objectives, including racial desegregation, curricular variety, and programs for at-risk youth and the gifted and talented. Some serve relatively advantaged students whereas others serve relatively disadvantaged students; therefore, it is difficult to ascertain for certain whether positive or negative selection bias of the choice effect would predominate. On balance, however, given the low level of achievement of students who remain in neighborhood schools, it is likely that estimates of the unadjusted effect of public school choice on graduation in Chicago would likely be upwardly biased. Given this, it is critical to employ a research design that attempts to adjust for the possibility of selection bias in estimates.

The goal of this analysis is to estimate the effect of exercising school choice on a student's propensity to graduate on time (i.e., in 4 years). Let Y represent the outcome, high school graduation. This variable takes on two values: $Y = 1$ if the student graduated from high school in 4 years (or less), and $Y = 0$ if the student dropped out of school or was still in school after 4 years. Let Z, our treatment variable, take on two values: $Z = 1$ if the student exercised school choice by attending any public or private high school other than her or his assigned neighborhood

school, and $Z = 0$ if the student attended her or his assigned neighborhood school. The empirical strategy employed in this article will be variations on the theme of controlling for observables and will seek to maximize coverage of a population to estimate an average treatment effect for all students who exercised high school choice in the 2000–2001 school year. Because students participated in a wide variety of school choice programs present in Chicago during the 2000–2001 school year, there is no experimental approach that would cover the entire cohort of interest. Therefore, the study uses several techniques and compares the results obtained from four different kinds of models. First, the study estimates a variety of linear probability models of the general form:

$$Y_i = \beta_0 + \beta_1 Z_i + \beta_2 \mathbf{X}_i + \varepsilon_i \qquad (1)$$

where **X** is a vector of student observables such as race/ethnicity, gender, socioeconomic status (SES), prior test score achievement, and measures of self-efficacy and self-reported disciplinary incidents. Linear probability models, which produce the same results as a probit evaluated at the mean, provide immediately interpretable results for the reader and allow for comparable results across analyses.

Although regression adjustment is a good first step, it is not without shortcomings (Hong & Raudenbush, 2005). First, it assumes that all students are equally likely to exercise school choice. In theory, we could divide our analytical sample into two halves: Group 1, which is strongly likely to exercise school choice, and Group 2, which is strongly unlikely to exercise school choice. It is plausible that the effect of school choice on graduation could be quite different between these two groups. Traditional regression analysis does not reduce bias when treatment and control groups are noncomparable (i.e., when the results suffer from selection bias, for example). In addition, regression adjustment can severely constrain the number of covariates that can be controlled.

To address these potential shortcomings, this study conducts a propensity score analysis using the principal stratification approach (Frangakis & Rubin, 2002; Hong & Raudenbush, 2005; Rosenbaum & Rubin, 1983). This technique first estimates a propensity model in which choice is regressed on a number of covariates predicting treatment group assignment:

$$p(X) \equiv \Pr[Z = 1 | X] = E[Z | X] \qquad (2)$$

The propensity score, $p(X)$, is then used to break participants into strata in which those who got the "treatment" (choosers in my case) and those who did not (nonchoosers) are balanced on the propensity score itself and measurable covariates. Thus, this method creates subgroups of participants who are good "matches" for each other and compares the treatment effect among just these participants. Whereas this approach is another way to control for observables, it has been shown that if the propensity score and observable covariates are balanced, and treatment group assignment is strongly ignorable, propensity score modeling produces unbiased causal effect estimations (Rosenbaum & Rubin, 1983). Furthermore, as mentioned above, a review of recent within-study comparisons found that propensity score methods offer substantial reduction in bias as long as certain best practices are followed such as matching on intact groups and/or on participants from the same geographic area, pretests, and measures associated with the selection process (Cook et al., 2008).

The estimand of interest in a principal stratification propensity score analysis is the average effect on the treated (ATT), weighted by the proportion of treated units across strata. Within each stratum, denoted here with q, mean differences are computed:

$$\tau_q^s = \frac{\sum_{i \in I(q)} Y_i^1}{N_q^1} - \frac{\sum_{i \in I(q)} Y_i^0}{N_q^0} \qquad (3)$$

where $I(q)$ denotes the set of units in block q and N_q^1 and N_q^0 are numbers of treated and control participants in block q, respectively. The ATT estimator in principal stratification is

$$\tau^s = \sum_{q=1}^{Q} \tau_q^s \frac{\sum_{i \in I(q)} D_i}{\sum_{\forall_i} D_i} \qquad (4)$$

This model has the potential to reduce bias in the effect of exercising school choice,

but omitted unobservable neighborhood- and school-level characteristics could bias the results of the analysis. For example, if students in neighborhoods with low-quality public schools are more likely to exercise school choice and such students are less likely to graduate from high school, then the choice effect is likely to be downwardly biased. To examine the plausibility of this hypothesis, this study estimates a linear probability regression model with neighborhood high school attendance area fixed effects:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_2 X_{ij} + \beta_3 \mathbf{S}_j + \varepsilon_{ij} \quad (5)$$

where $\mathbf{S}_j$ is a vector of $n-1$ fixed effects and ε_{ij} is a student-specific residual. This model removes the effects of any observable and unobservable neighborhood high school confounds that could bias the estimation of the school choice effects. This specification provides a pooled within-neighborhood estimate of the choice effect. It compares the effect of school choice on graduation among students in the same neighborhood and pools the results across the 48 high school attendance areas in the city of Chicago. It could be, however, that this effect varies across neighborhoods. Therefore, the final analysis estimates a random-effects model.

To determine whether graduation rates and the choice effect on graduation vary randomly across neighborhoods, the study estimates a multilevel model with varying intercept and choice slope. Estimating such a model allows for the intercept and slope to be modeled with neighborhood high school predictors such as school quality and poverty rates. This permits testing hypotheses about whether the student graduation rates, or the choice effect on graduation, vary by the quality or poverty level of the student's default alternative. The base model is

$$\begin{aligned} Y_{ij} &= \beta_0 + \beta_1 (Z_{ij} + \bar{Z}.j) + \beta_2 (X_{ij} + \bar{X}.j) + \gamma_{ij} \\ \beta_{0j} &= \gamma_{00} + u_{0j} \\ \beta_{1j} &= \gamma_{10} + u_{1j} \end{aligned} \quad (6)$$

where γ_{00} is the average graduation rate across the population of schools, γ_{10} is the average school choice–graduation slope across those schools, u_{0j} is the unique increment to the intercept associated with school j, and u_{1j} is the unique increment to the slope associated with school j.

Data and Measures

The data for this analysis come from administrative records on all students enrolled in a Chicago public school eighth-grade classroom during spring 2000.[4] Included in this analysis are students who successfully made the transition from a public elementary/middle school[5] to a public or private high school. School choice is defined as attending a nonassigned public high school or exiting the Chicago Public Schools (CPS) for a private sector high school. On-time graduation is defined as graduating from a Chicago public high school by spring 2004. Students who graduated from high school in spring 2004 (or earlier) were coded as 1 on the dependent variable. Those who dropped out or were still enrolled as of fall 2004 were coded as 0. Students who left Chicago for another school district or the private sector and did not return to CPS were coded as missing on the dependent variable. Those who attended a private school and then returned to CPS sometime after spring 2001 were included in the analysis.[6]

Because these administrative data do not contain information about important pretreatment covariates, this study includes three survey measures to control for variables that could confound the relationship between choice and graduation propensity: student self-efficacy, self-reported disciplinary incidences, and family SES (a measure of learning resources in the home).[7] These measures come from the Consortium on Chicago School Research's spring 1999 survey of elementary school students. Although the survey is designed as a census of all students and schools, not all schools participated. As a result, about 40% of students have missing values on survey items. (Only a small portion of students are missing because they were not enrolled in CPS in 1999.) Black, poor, and low-achieving students were more likely than non-Black, nonpoor, and high-achieving students to be missing on survey data. Values for cases with missing data on these survey measures and the other covariates were imputed.[8]

The analysis includes 16,532 students enrolled in 410 elementary schools as eighth graders. These students were administratively assigned to 1 of 49 public high schools,[9] but

TABLE 2
Analytic Sample Descriptives, 2004

Variable	Description	*M*	*SD*	Min	Max
Grad	graduated in 4 years	0.62	0.486	0.00	1.00
Choice	exercised high school choice	0.58	0.494	0.00	1.00
White	White student	0.09	0.291	0.00	1.00
Hispanic	Hispanic student	0.34	0.474	0.00	1.00
Asian	Asian student	0.04	0.185	0.00	1.00
Male	Male student	0.48	0.500	0.00	1.00
Foster care	foster care student	0.05	0.210	0.00	1.00
Old for grade	old for eighth grade	0.17	0.379	0.00	1.00
Elem chooser	exercised elementary school choice	0.37	0.482	0.00	1.00
Num sch mvs	number of schools attended in past 3 years	0.37	0.628	0.00	5.00
SES	socioeconomic status (z-score)	–0.10	1.832	–6.38	6.75
Conc poverty	concentrated poverty of census tract (z-score)	0.31	0.803	–3.86	4.98
Math ach	eighth-grade ITBS math score (z-score)	0.00	1.002	–4.89	2.76
Self-efficacy	self-efficacy measure	4.88	1.618	0.00	10.00
Disc incidents	disciplinary incidents measure	2.54	1.895	0.00	9.84

Note. N = 16,532 for all variables.

TABLE 3
Nonimputed Analytic Sample Descriptives, 2004

Variable	Description	Obs	*M*	*SD*	Min	Max
Grad	graduated in 4 years	16532	0.62	0.486	0.00	1.00
Choice	exercised high school choice	16532	0.58	0.494	0.00	1.00
White	White student	16532	0.09	0.291	0.00	1.00
Hispanic	Hispanic student	16532	0.34	0.474	0.00	1.00
Asian	Asian student	16532	0.04	0.185	0.00	1.00
Male	Male student	16532	0.48	0.500	0.00	1.00
Foster care	foster care student	16532	0.05	0.210	0.00	1.00
Old for grade	old for eighth grade	16532	0.17	0.379	0.00	1.00
Elem chooser	exercised elementary school choice	16532	0.37	0.482	0.00	1.00
Num sch mvs	number of schools attended in past 3 years	16532	0.37	0.628	0.00	5.00
SES	socioeconomic status	9786	–0.02	1.845	–3.79	2.83
Conc poverty	concentrated poverty of census tract	16529	0.31	0.803	–3.86	4.98
Math ach	eighth-grade ITBS math score (standardized)	16504	0.00	1.002	–4.90	2.76
Self-efficacy	self-efficacy measure	9206	4.95	1.622	0.00	10.00
Disc incidents	disciplinary incidents measure	9453	2.27	2.004	0.19	9.84

only 42% of these students attended their assigned schools. The balance of the sample enrolled in one of the many other public or private high schools and programs in the city.[10] Only 62% of students in the analytic sample graduated in 4 years. Other descriptive statistics on the analytic sample are shown in Table 2. A table of unimputed descriptive statistics on the analytic sample is included in Table 3.

Results

Linear Probability Estimates

The unconditional on-time graduation propensity gap between those who exercise high school choice and those who remain in their neighborhood schools is 11.2 percentage points (Table 4, Model 1).[11] This gap shrinks but

TABLE 4
Linear Probability Regression Coefficients of On-Time Graduation on School Choice, 2004

	1	2	3	4	5
Choice	0.112	0.086	0.079	0.042	0.035
	[14.67]**	[11.15]**	[10.22]**	[5.43]**	[4.57]**
White		0.131	0.072	0.025	0.002
		[10.11]**	[5.02]**	[1.74]†	[0.14]
Hispanic		0.087	0.062	0.035	–0.001
		[10.47]**	[6.69]**	[3.82]**	[0.10]
Asian		0.268	0.22	0.154	0.099
		[13.44]**	[10.75]**	[7.53]**	[4.88]**
Male		–0.145	–0.144	–0.141	–0.099
		[19.84]**	[19.85]**	[19.63]**	[13.74]**
Foster care		–0.106	–0.101	–0.077	–0.063
		[6.01]**	[5.76]**	[4.45]**	[3.69]**
Old for grade		–0.133	–0.122	–0.079	–0.072
		[13.67]**	[12.50]**	[8.07]**	[7.45]**
Elem chooser		0.05	0.045	0.037	0.034
		[6.50]**	[5.87]**	[4.83]**	[4.61]**
Num sch mvs		–0.069	–0.068	–0.063	–0.059
		[11.80]**	[11.71]**	[10.93]**	[10.46]**
SES			0.017	0.007	0.005
			[8.39]**	[3.13]**	[2.26]*
Conc poverty			–0.035	–0.031	–0.03
			[6.48]**	[5.72]**	[5.74]**
Math ach				0.089	0.075
				[21.90]**	[18.26]**
Self-efficacy					0.007
					[2.87]**
Disc incidents					–0.051
					[25.54]**
Constant	0.551	0.619	0.65	0.676	0.77
	[95.31]**	[68.09]**	[66.15]**	[69.23]**	[48.62]**
Observations	16532	16532	16532	16532	16532
R^2	0.01	0.08	0.09	0.12	0.15

Note. Absolute value of *t* statistics in brackets.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

remains statistically distinguishable from zero when additional covariates are controlled. Sociodemographic student characteristics associated with lower graduation rates include being male, in foster care, old for one's grade, and having high previous school mobility. Conversely, students who exercised school choice as an elementary school student had higher likelihoods of graduating on time (Model 2). Higher levels of SES are associated with higher graduation rates, whereas neighborhood concentrated poverty is negatively associated with graduation propensity, net of student SES and other sociodemographic characteristics (Model 3). Higher levels of previous test score achievement and higher levels of self-efficacy are associated with higher probabilities of graduating (Models 4 and 5). Higher levels of self-reported disciplinary incidents are

associated with lower graduation rates (Model 5). Conditional on sociodemographic characteristics, SES, neighborhood concentrated poverty, prior math achievement, self-efficacy, and disciplinary incidences, there are no graduation rate gaps between Blacks, Whites, and Hispanics. Being of Asian descent, however, remains associated with about a 10 percentage point increase in graduation propensity, net of other covariates. The final model shows that the unconditional on-time graduation propensity gap shrinks from 11.2% to 3.5% when other predictors of graduation are included. For reference, coefficients, standard errors, and odds ratios from logistic models are included in Table 5.

The models presented in Table 4 assume that the effect of choice is constant across groups. This turns out to be an untenable assumption. Table 6 presents models that test the variability of the choice effect by math achievement for the entire analytic sample (Model 2); for selective enrollment choosers versus assigned school attendees (Model 3); and for nonassigned other public choosers versus assigned school attendees (Model 4). The nonassigned other public category includes programs that vary in their selectivity, such as magnet schools and programs, career academies, military academies, and charter schools. It displays the coefficients of the main and interaction effects only; the other noninteracted main effects are controlled but not shown. Model 2 shows that the strength of the choice effect varies by prior math achievement (the achievement × choice interaction effect is positive and significant at the .05 level in Model 2). The achievement–graduation–propensity gradient is steeper for students who exercise school choice than for those who remain in their assigned schools. At high levels of math achievement, those who exercise school choice tend to have higher graduation rates than students who remain in their assigned schools. At very low levels of math achievement (less than –1.137 below the mean, to be precise), this gap is reversed. In other words, students with very low test scores are less likely to graduate if they exercise school choice than if they do not. Model 3, which computes the same interaction term for selective enrollment choosers versus assigned high school attendees only, shows that the choice–math achievement interaction term is stronger than in Model 2. The achievement–graduation–propensity gradients for selective enrollment and assigned high school attendees plotted in Figure 1 show that high-achieving students (greater than +.05, which is the point of intersection of the two lines) have much higher predicted graduation rates if they attend selective enrollment high schools instead of their assigned high schools. Conversely, low-achieving students who attend selective enrollment high schools (of which there are very few[12]) are at great risk of failing to graduate on time relative to low-achieving students in assigned high schools. Model 4, which computes the choice–math achievement interaction for nonassigned other public choosers versus assigned high school attendees only, is statistically significant and positive but is much weaker than the interaction term in Model 3, suggesting that background achievement is not as salient for graduation chances for those students who are not elite school eligible.[13]

If the effect of unobservables on school choice is conditionally independent of the other covariates, the results presented in Tables 4 and 6 can be considered unbiased estimates of the causal effect of exercising school choice. The tenability of this assumption is next explored with propensity score analysis.

Propensity Score Estimates

The first step in conducting a propensity score analysis is to estimate a propensity model of treatment regressed on predictor variables. This study employs a strong set of covariates to model the high school selection process, including whether a student was "magnet eligible" (the student's math and reading test scores were high enough to qualify him or her for a selective enrollment magnet school), whether the student was old for his or her grade (an indicator of being retained in grade or starting school late), whether the student previously exercised elementary school choice, parental support, mother's education, self-efficacy, disciplinary incidences, and distance to choice and assigned high schools. A logistic regression model of choice regressed on a number of predictors is included in Table 7. This treatment propensity model indicates that the following student and neighborhood characteristics are negatively associated with exercising choice: being male, bilingual, poor,

TABLE 5
Odds Ratios and Logit Coefficients of On-Time Graduation on School Choice, 2004

	1	2	3	4	5
Choice	1.601	1.476	1.435	1.224	1.191
	0.47	0.389	0.361	0.202	0.175
	[14.54]**	[11.08]**	[10.19]**	[5.52]**	[4.67]**
White		1.851	1.416	1.149	1.029
		0.616	0.348	0.139	0.028
		[9.88]**	[5.04]**	[1.97]*	[0.39]
Hispanic		1.479	1.319	1.17	0.987
		0.392	0.277	0.157	–0.013
		[10.31]**	[6.49]**	[3.61]**	[0.28]
Asian		4.442	3.592	2.693	2.08
		1.491	1.279	0.991	0.732
		[12.29]**	[10.34]**	[7.90]**	[5.74]**
Male		0.52	0.518	0.518	0.618
		–0.654	–0.658	–0.657	–0.481
		[19.52]**	[19.54]**	[19.23]**	[13.55]**
Foster care		0.634	0.646	0.711	0.75
		–0.456	–0.437	–0.342	–0.287
		[5.78]**	[5.53]**	[4.25]**	[3.51]**
Old for grade		0.562	0.589	0.71	0.726
		–0.576	–0.529	–0.343	–0.321
		[13.30]**	[12.12]**	[7.62]**	[6.97]**
Elem chooser		1.263	1.24	1.203	1.193
		0.233	0.215	0.184	0.176
		[6.54]**	[5.98]**	[5.06]**	[4.74]**
Num sch mvs		0.736	0.737	0.749	0.753
		–0.306	–0.306	–0.289	–0.283
		[11.58]**	[11.52]**	[10.75]**	[10.31]**
SES			1.083	1.032	1.024
			0.08	0.031	0.023
			[8.40]**	[3.14]**	[2.30]*
Conc poverty			0.848	0.861	0.859
			–0.165	–0.149	–0.152
			[6.52]**	[5.82]**	[5.79]**
Math ach				1.525	1.449
				0.422	0.371
				[20.94]**	[17.74]**
Self-efficacy					1.032
					0.032
					[2.79]**
Disc incidents					0.787
					–0.239
					[24.14]**

(continued)

TABLE 5 (continued)

	1	2	3	4	5
Constant	–	–	–	–	–
	0.205	0.507	0.658	0.789	1.258
	[8.53]**	[12.32]**	[14.52]**	[16.97]**	[15.88]**
Observations	16532	16532	16532	16532	16532

Note. Absolute value of *z* statistics in brackets. Replicates the linear probability models presented in Table 4 for reference. For each regressor, the odds ratio is displayed first, the logit coefficient second, and the absolute value of the *z* statistic (in brackets) third.
*Significant at 5%. **Significant at 1%.

TABLE 6
Linear Probability Regression Coefficients of On-Time Graduation on School Choice With Interactions, 2004

	1	2	3	4
	Entire Sample	Entire Sample With Interaction	Selective vs. Assigned	Other Public vs. Assigned
Choice	0.035	0.037	0.016	0.034
	[4.57]**	[4.86]**	[0.84]	[4.16]**
Math ach	0.075	0.061	0.06	0.061
	[18.26]**	[10.34]**	[9.80]**	[10.04]**
Choice × math ach		0.025	0.054	0.018
		[3.37]**	[3.57]**	[2.17]*
Constant	0.77	0.767	0.751	0.77
	[48.62]**	[48.31]**	[35.74]**	[45.05]**
Observations	16532	16532	8822	14698
R^2	0.15	0.15	0.17	0.13

Note. Models 1 and 2 are calculated from the entire analytic sample, Model 3 contains only selective enrollment choosers and assigned attendees, and Model 4 contains only other public choosers and assigned attendees. Absolute value of *t* statistics in brackets. Models also control for gender, foster care status, old for grade, elementary school chooser, number of school moves, self-efficacy, and disciplinary incidences.
*Significant at 5%. **Significant at 1%.

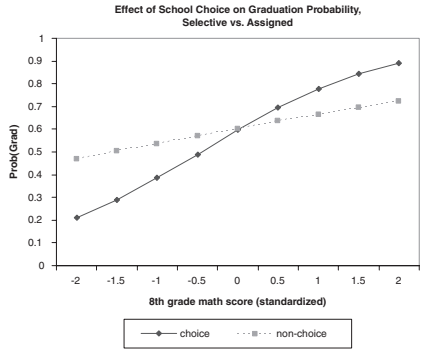

FIGURE 1. *Predicted graduation gradient by choice status and prior math score, selective versus assigned only, Chicago Public School eighth graders, 2004.*
Note. Computed from coefficients in Model 3 of Table 6.

old for grade, or White or Hispanic (vs. Black or Asian); having high previous school mobility or high disciplinary incidences; and living in neighborhoods served by few schools of choice or low concentrated poverty. High school choosers are more likely to have also been elementary school choosers and tend to have high test scores, high SES, high parental education, and high parental support. Choosers also tend to live in high SES neighborhoods and live farther away from their assigned high schools.[14]

Participants were divided into 12 balanced strata based on the predicted values of the propensity model.[15] *t* tests of treated and untreated participants indicate that these strata balance

TABLE 7
Odds Ratios From Logistic Propensity Model Predicting High School Choice, 2004

	OR	*SE*	*z*	$p > \|z\|$	95% Confidence Interval	
White	0.710	0.046	–5.26	0.000	0.625	0.807
Hispanic	0.457	0.022	–16.27	0.000	0.415	0.502
ln (distance to high school)	1.748	0.043	22.66	0.000	1.665	1.834
Male	0.886	0.030	–3.62	0.000	0.830	0.946
Free/red lunch	0.765	0.039	–5.22	0.000	0.691	0.846
Spec ed	0.960	0.049	–0.79	0.429	0.868	1.062
Old for grade	0.760	0.033	–6.41	0.000	0.699	0.826
Read ach	1.073	0.168	0.45	0.652	0.790	1.459
Read ach^2	0.974	0.022	–1.17	0.241	0.932	1.018
Read ach^3	1.003	0.001	2.58	0.010	1.001	1.005
Num sch w/in 3 mi	0.973	0.008	–3.16	0.002	0.957	0.990
Elem chooser	1.877	0.066	18.03	0.000	1.753	2.010
Num sch mvs	0.899	0.023	–4.21	0.000	0.855	0.945
Conc poverty	1.092	0.033	2.89	0.004	1.029	1.159
Conc poverty^2	0.960	0.015	–2.67	0.008	0.931	0.989
Self-efficacy	1.053	0.011	4.72	0.000	1.031	1.076
Neigh SES	1.137	0.041	3.53	0.000	1.059	1.221
Neigh SES^2	0.906	0.016	–5.53	0.000	0.874	0.938
Neigh SES^3	0.940	0.009	–6.61	0.000	0.922	0.957
Bilingual	0.882	0.055	–2.01	0.044	0.781	0.997
Parental support	1.060	0.013	4.84	0.000	1.035	1.085
Magnet eligible	2.090	0.092	16.78	0.000	1.918	2.278
ln (avg dist to choice high school)	0.308	0.036	–10.08	0.000	0.245	0.387
SES	1.027	0.010	2.69	0.007	1.007	1.048
Disc incidents	0.952	0.009	–5.55	0.000	0.935	0.968
Mother's education	1.069	0.017	4.09	0.000	1.035	1.103

participants on the propensity score. In addition, a total of 312 *t* tests (with a .01 significance level criterion) indicates that the participants are balanced on analysis covariates.[16] Panel A of Table 8 shows that average propensity increases with strata and that average propensities between treated and untreated participants within strata are nearly identical. The variance in propensity within strata depends on the size of the strata (narrow strata have lower propensity score variance). At low levels of propensity (Strata 1, 2, 3, 4), nonchoosers outnumber choosers. At high levels of propensity (Strata 9, 10, 11, 12), choosers outnumber nonchoosers. In all strata, there are adequate numbers of nontreated participants to compare with treated participants (Strata 12 has the fewest comparison participants at 43). The average effect on the treated, ATT, is the average within-strata difference between choosers and nonchoosers on graduation propensity, weighted by the proportion of treated participants across strata.[17] This results in an estimate of .036, with a standard error of .009 (see Panel B of Table 8). With a *t* statistic of 4.052, we can reject the null hypothesis of an equality of graduation propensities between choosers and nonchoosers. This result is virtually identical to the result in the final model of Table 4, providing evidence that the simpler linear probability model estimate may not be substantially inflated.

The graduation gap, however, does not appear to be homogeneous across strata; it is larger in many of the higher strata. To test the hypothesis that graduation likelihood increases as propensity to exercise school choice increases, block identifiers are included in a linear probability regression (see Table 9). Conditional on the student

TABLE 8
Propensity Score Results

Panel A: Graduation Rates by Choice Status and Propensity Strata, 2004

	Pr(Grad)		Avg(Propensity)		*SD*(Propensity)		*N*	
Strata	Nonchooser	Chooser	Nonchooser	Chooser	Nonchooser	Chooser	Nonchooser	Chooser
1	0.465	0.468	0.159	0.162	0.031	0.031	567	118
2	0.529	0.497	0.252	0.256	0.028	0.027	1171	401
3	0.518	0.528	0.350	0.353	0.029	0.029	1373	745
4	0.517	0.563	0.425	0.425	0.015	0.014	729	516
5	0.513	0.529	0.475	0.475	0.015	0.015	726	669
6	0.534	0.563	0.525	0.525	0.015	0.014	699	751
7	0.568	0.595	0.575	0.576	0.014	0.014	631	867
8	0.578	0.631	0.648	0.650	0.029	0.029	1058	1921
9	0.625	0.684	0.747	0.750	0.029	0.029	701	2132
10	0.729	0.732	0.825	0.825	0.015	0.015	239	1089
11	0.732	0.783	0.874	0.875	0.013	0.015	161	1050
12	0.750	0.808	0.910	0.912	0.008	0.007	43	503

Panel B: Average Treatment Effect on the Treated (ATT) Weighted by Strata Size

ATT	*SE*	*N*(Treated)	*N*(Control)
0.036	0.009	10762	8097

characteristics that predict graduation (from the final model presented in Table 4), participants in Blocks 9 through 12 have higher graduation propensities (results statistically significant at the .05 level; Block 7 also has higher graduation propensities, but this result is statistically significant only at the .10 level; results from Model 2). Model 3 shows that a one-unit increase in propensity (going from a null probability of choice to a perfect probability of choice) increases graduation probability by about 10 percentage points. The propensity-adjusted regression estimate of the choice effect is .027, slightly lower than the ATT estimate of .036 from the propensity analysis. Including a propensity–choice interaction term reveals that the slope of the choice propensity–graduation propensity gradient among the choosers is steeper than the same gradient among the nonchoosers (Model 4).

Catchment Area Fixed-Effect Estimate

Thus far, the models presented assume that the choice participation graduation gap is constant across the city. Some scholars, however, have argued that the choice effect could vary by the quality of the assigned neighborhood school (Neal, 1997). In other words, observable neighborhood high school characteristics, such as teacher turnover and school poverty, and unobservable neighborhood high school characteristics, such as teacher quality, might confound the choice effect, making the choice benefit stronger for students who leave low-quality schools and weaker for students who leave high-quality schools. One way to address this threat to validity is to include fixed effects for k–1 neighborhood high schools in the district. A fixed-effects formulation involves the following thought experiment: Take two students in the same neighborhood, each with the same observable background characteristics, and compare the graduation propensities of those who exercised school choice and those who did not. Including fixed effects provides a pooled within-neighborhood-high-school-attendance-area estimator (averaged across attendance areas) for the cohort.

A regression with the same variables in Table 4 and 48 school fixed effects for the n–1 high schools with attendance areas in Chicago produces an estimate of high school choice on graduation probability of .043 ($t = 4.04$, $p < .01$), which is slightly higher than the estimate without high school fixed effects. In other words, taking into account assigned high school quality, the benefit of choice is slightly stronger than when assigned high school quality is uncontrolled,

TABLE 9
Linear Probability Regression Coefficients of On-Time Graduation With Interactions, 2004

	1	2	3	4
Choice	0.035	0.026	0.027	–0.024
	[4.57]**	[3.26]**	[3.34]**	[1.05]
Block 2		0.024		
		[1.02]		
Block 3		0.019		
		[0.85]		
Block 4		0.027		
		[1.08]		
Block 5		0.009		
		[0.37]		
Block 6		0.033		
		[1.31]		
Block 7		0.043		
		[1.72]†		
Block 8		0.039		
		[1.59]		
Block 9		0.052		
		[2.04]*		
Block 10		0.076		
		[2.69]**		
Block 11		0.1		
		[3.42]**		
Block 12		0.089		
		[2.60]**		
Prop score			0.099	0.051
			[3.35]**	[1.43]
Choice × prop score				0.092
				[2.35]*
Constant	0.77	0.733	0.716	0.74
	[48.62]**	[27.37]**	[31.01]**	[29.41]**
Observations	16532	15809	15812	15812
R^2	0.15	0.14	0.14	0.14

Note. Absolute value of *t* statistics in brackets. Models also control for race/ethnicity, socioeconomic status, math test score, neighborhood concentrated poverty, gender, foster care status, old for grade, elementary school chooser, number of school moves, self-efficacy, and disciplinary incidences.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

which suggests that the previous estimates may be downwardly biased by negative selection into school choice (i.e., students with lower assigned school quality are more likely to exercise choice but less likely to graduate).

Multilevel Random-Effects Estimates

A fixed-effects formulation averages across high schools to provide a single estimate of the regression slope. An alternative approach is to specify a random-effects model in which both the intercept (average graduation rate) and the choice slope (the effect of choice on graduation propensity) vary across schools with an interaction between the choice effect and assigned high school poverty and average achievement. Such a model is presented in Table 10; Model 1 includes just student-level covariates from the final model in Table 4. In this specification, variables are group mean centered on the neighborhood high school attendance area averages.[18] This specification, which is similar to entering catchment area fixed effects, removes the effect of observable and unobservable between-school differences, tests the hypothesis that graduation propensity and the choice effect vary between schools, and models this variation.

Model 1 shows that the choice effect, .040, is statistically different from zero and varies across schools.[19] Neighborhood high schools (i.e., those with attendance areas) were divided into quintiles by school poverty level. Low-poverty schools are defined as those in the lowest quintile of school poverty (45.4% to 83.2% poor). High-poverty schools are defined as those in the highest quintile of poverty (95.3% to 98.3% poor). Model 2 shows the results of including these two variables on the intercept and slope of the model. School poverty is not associated with the overall graduation rate (the intercept), but it is associated with the effect of choice on graduation propensity (the choice slope). Specifically, living in a low-poverty neighborhood is associated with a higher choice effect than living in a neighborhood with medium or high poverty. The marginal choice effect of living in a neighborhood with medium poverty is 3.9%. The marginal effect of living in a low-poverty neighborhood on the choice effect is 9.2% (3.9 + 5.3 = 9.2%). Including neighborhood school average achievement does little to reduce the low poverty–choice effect, but average achievement is positively related to the overall graduation rate. In other words, there is evidence to support the hypothesis that students living in neighborhoods with high-achieving high schools are more likely to graduate, but there is no

TABLE 10
Linear Probability Regression Coefficients of On-Time Graduation on School Choice and Neighborhood Characteristics, Multilevel Random-Effects Models, 2004

	1	2	3
	Fixed Effects		
Intercept	0.623	0.623	0.625
	[55.474]**	[57.221]**	[62.890]**
Low-poverty HS		0.027	–0.004
		[0.978]	[–0.139]
High-poverty HS		–0.015	–0.018
		–0.531	[0.916]
Average HS ach			0.003
			[2.759**]
Choice slope	0.0399	0.039	0.041
	[3.864]**	[3.939]*	[4.209]**
Low-poverty HS		0.053	0.053
		[2.298]**	[2.025]**
High-poverty HS		0.012	0.013
		[0.444]	[0.477]
Average HS ach			0.000
			[0.125]
	Random Effects		
Intercept (U0)	0.00524	0.00520	0.0042
	[377.786]**	[345.592]**	[272.369]**
Choice slope (U1)	0.0025	0.0024	0.0025
	[88.015]**	[80.845]**	[80.562]**

Note. Models also control for the covariates shown in Table 4. HS = high school.
*Significant at 5%. **Significant at 1%.

evidence to support the hypothesis that students in neighborhoods with high-achieving schools get more (or less) benefit from exercising school choice (the high school achievement–choice interaction effect is not statistically different from zero). Therefore, these results contradict the assumption that the benefits of choice are greater for students served by disadvantaged schools. Those students served by non-high-poverty schools are more likely to benefit from choice than those students served by higher poverty schools.

Conclusion

Traditionally in the United States, parents and students had choice over school enrollment in two senses: choice of neighborhood ("school choice by mortgage") and choice of sector ("private school choice"). For public school attendees whose parents were not changing residences based on school composition or quality, administrative authorities assigned students to schools based on geography and, in some cases, racial characteristics. With the expansion of school choice programs in the past three decades, the family's role in making school choice has become more important. This shift has arguably placed greater responsibility for ensuring educational opportunity on students and parents and reduced the responsibility of local education authorities. In short, some students may enjoy the benefits of well-informed and well-considered family decisions and others may suffer the consequences of poorly informed and poorly considered family decisions about schooling. This study shows that the decision of whether to exercise choice in Chicago may be a consequential one. This study examines whether exercising high school choice increases student probabilities of on-time high school graduation. Using administrative data from a large cohort of eighth-grade students in Chicago who attended a wide variety of high schools including selective enrollment, magnet, charter, career and military academies, magnet programs, and assigned comprehensive high schools, this study finds a graduation benefit of exercising school choice. The unconditional effect of exercising choice on high graduation propensity is 11.2 percentage points. Adjusting for race, gender, prior achievement, SES, and other background characteristics shrinks the choice benefit to 3.5 percentage points. Adding assigned high school fixed effects to control for neighborhood high school quality increases the choice benefit to 4.3 percentage points. A propensity score analysis using the principal stratification approach (Frangakis & Rubin, 2002; Hong & Raudenbush, 2005; Rosenbaum & Rubin, 1983) results in an estimate of 3.6 percentage points.

These results suggest that for some students, exercising school choice is an important pathway to opportunity. Previous research on school choice in Chicago using a variety of research designs and student outcomes (Booker et al., 2008; Cullen et al., 2005, 2006; Hoxby & Rockoff, 2004) finds some positive benefits of school choice for certain populations of students attending particular types of schools. Therefore, the accumulated evidence seems to

suggest that at least in Chicago, school choice is benefiting at least some students. Whereas previous studies of school choice in Chicago examined the benefits of choice for students in only particular programs, the contribution of this study is that it examines the wide variety of high school choice programs in Chicago, which served more than half of the study sample, including the elite selective enrollment schools, which enrolled approximately 10% of this eighth-grade cohort.

A separate matter is whether the choice benefit is greater for advantaged or disadvantaged student subgroups. This is a key question in school choice research because scholars have argued that school choice may have larger effects for disadvantaged students (Friedman, 1955; Morgan, 2001; Neal, 1997). This study, however, finds no evidence of racial-ethnic differences or SES differences in the choice benefit but does find evidence that students with low test scores are less likely to benefit from choice. In fact, there is some evidence to suggest that students with low scores are better off if they remain in their assigned schools. The propensity score analysis provides further evidence that choice may work best for those most likely to exercise it. The school choice participation graduation gap for those with low probabilities of exercising choice is essentially zero. The choice participation gap for those with high probabilities of exercising choice is generally positive. Moreover, results from a multilevel random-effects model show that students living in high-poverty neighborhoods get less benefit from exercising choice than students living in low-poverty neighborhoods. The finding that choice seems to work best for those with high test scores and low neighborhood poverty suggests that school choice may be more of a pathway to opportunity for those who have already had access to high-quality educational experiences during early childhood and elementary school and may be less of an opportunity for those who have had less than adequate prior educational experiences.

Because it is quite likely that the effect of school choice on student outcomes is a matter of policy design, further research is needed to fully understand how the features of school choice programs relate to improved outcomes for students. This study estimates the aggregate choice participation effect. An important next step is to examine the choice participation effect for different high school treatments—selective enrollment high schools, charter schools,[20] and career academies, for example. An important feature of Chicago's choice program is that it allows students with high test scores to escape to schools with enriched programs. It could be that another district could design its school choice program more equitably, with greater reliance on lotteries and less reliance on selective criteria. Moreover, if access to high-level coursework, teachers, and programs were equitably distributed across schools in such a district, we might expect these factors to minimize the choice effect. This suggests that these results may not be generalizable to other school districts with different approaches to student and parental choice of schools.

The research design emphasizes coverage of an entire eighth-grade cohort and the entire range of choice options in Chicago rather than zeroing in on a particular subpopulation with an experimental or quasi-experimental design. Because this study does not employ an experimental or regression discontinuity design, the findings reported here are only suggestive of causal effects. This study uses a variety of techniques including propensity score analysis, catchment area fixed-effects, and random-effects models to attempt to control for observable characteristics. The possibility remains that those who chose a high school alternative may be different on unobservables after controlling for observables. Therefore, this study cannot distinguish between the consequences of choice and the act of choosing. Omitting unobserved covariates, however, would bias the results presented here only if such covariates were negatively correlated with the propensity scores. Given that many of the choice options available to students in this study are academically selective, omitted covariates that one typically considers in studies of school effects (motivation, ability, parental support for learning) are likely to be positively, rather than negatively, correlated with the propensity score. Therefore, the results presented here represent reasonable estimates of the graduation effect of high school choice participation.

Given the paucity of research on the effects of school choice participation on graduation rates, there is a pressing need for both nonexperimental and experimental research to inform policy. Further research could determine the mechanisms behind the positive choice effect, whether the effect varies in different types of programs and across different locales and student subpopulations, and why high-achieving students seem to benefit more. Moreover, this study and the work of Cullen et al. (2005) provide nonexperimental evidence that makes a large-scale randomized experiment to test the graduation benefits of choice more attractive.

Notes

[1]Special provisions are made to promote inclusion of English language learners and students with special educational needs and/or disabilities into selective enrollment schools. For example, students in this category are considered eligible to apply if their test score stanines sum to 10.

[2]I thank Elizabeth Duffrin for sharing these data with me.

[3]Charter school enrollment in Chicago has greatly expanded since 2000. Between 1997 and 2008, 30 charter entities with 65 campuses opened. In addition, the district authorized dozens of charter-like entities called performance and contract schools that trade some administrative flexibility for increased accountability.

[4]Omitted from this analysis are students who moved out of the city of Chicago between eighth and ninth grade (7.5%), those who were retained in eighth grade for academic reasons (2%), and those whose choices were otherwise predetermined (by being in jail, in a school for pregnant teens, in a school for students with special educational needs, or in a high school with an eighth grade or due to death; 10%). Censoring these groups limits the generalizability of the study to the original eighth-grade cohort population. Therefore, the study generalizes to the population of students who remained in Chicago who were in a position to make a high school choice between eighth and ninth grade and whose graduation outcome could be observed due to enrollment in the public system at some point during their secondary education.

[5]Most eighth graders in Chicago are enrolled in K-8 elementary schools rather than middle or junior high schools.

[6]Those students who attended private high schools and then did not return to Chicago Public Schools are omitted from the analysis because their graduation status was unobserved. The graduation status of 78% of students who attended a private high school in ninth grade was unobserved for this reason. Because 22% of private school attendees returned to the public system, the graduation status of these students was observed and these students were retained in the analysis because they met the selection criteria of a ninth-grade school choice. The private school returnees make up only 1% of the final analysis sample, but they are less likely to graduate than other students. Retaining these students in the analytic sample is likely to exert a slight downward bias to the school choice participation effect reported in this article.

[7]Self-efficacy and socioeconomic status are positively scaled variables, with higher values denoting higher levels of efficacy and socioeconomic status. These variables correlate positively with choice and graduation rate. Disciplinary incidence is negatively scaled, with higher values denoting more self-reported disciplinary incidences. This variable is negatively correlated with choice and graduation rate.

[8]Multiple imputation was carried out with SAS's proc mi procedure. Analysis was conducted on one of the resultant data sets produced by the multiple imputation process.

[9]The entire city is divided into high school attendance areas, so every street address has an assigned neighborhood high school.

[10]Private school returnees went to about 60 private schools, four fifths of which were Catholic schools.

[11]Note that because these are linear probability coefficients, the value of .112 reported in Table 2 represents the graduation gap between choosers and nonchoosers on a proportion scale (i.e., 0–1).

[12]Most students who attend selective enrollment magnet schools have high test scores. The median standardized eighth-grade math score is 1.07. Only 8.6% of students had scores below +.05.

[13]Three other interaction effects were tested, but none reached conventional levels of statistical significance. There is some evidence of a differential socioeconomic status–graduation gradient between choosers and nonchoosers, although this finding is not statistically significant at the conventional .05 level. Although the main effect is statistically significant, the socioeconomic status–graduation gradient among nonchoosers is essentially flat. This gradient for choosers is weakly positive, suggesting that graduation propensity gains from choice are greater for choosers than for nonchoosers. Although the sign of the choice–neighborhood concentrated poverty interaction term is negative, it is not statistically significant at the .05 or the .10 level. No evidence of race/ethnicity–choice interaction effects was found.

[14]It is interesting that the effects of neighborhood concentrated poverty and neighborhood socioeconomic

status (SES) are both nonlinear. The effect of concentrated poverty is increasing and convex. The effect of neighborhood SES is increasing but concave. A propensity model without the higher order terms results in a nonsignificant neighborhood SES term and a positive, and significant, effect of concentrated poverty.

[15]Participants in the two strata with the highest propensities to exercise school choice (13 and 14) were eliminated from the analysis due to a paucity of nontreated participants to compare with participants who had exercised school choice. In addition, an analysis covariate failed to balance in Strata 13.

[16]Less than 1% of *t* tests (2 of 312 tests) resulted in accepting a null hypothesis of a difference of means on a covariate between treated and untreated participants.

[17]To compute the average effect on the treated (ATT) and create balanced blocks, this study used STATA ADO programs pscore and atts written by Becker and Ichino (2002).

[18]Instead of entering X_i, one enters $(X_i - X.)$. Coefficients of the student-level predictors are not shown but are available from the author upon request.

[19]The fixed-effects result, .043, and the random-effects result, .040, differ slightly because the HLM estimation removed some schools from the analysis due to insufficient data for estimation.

[20]For a study of charter school graduation and college enrollment effects, see Booker, Sass, Gill, and Zimmer (2008).

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