To Choose or Not to Choose:
High School Choice and Graduation in Chicago

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Introduction

The recent growth of public and private school choice programs (Wirt *et al.*, 2004) has injected additional urgency to 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 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 *et al.*, 1996; Reay & Ball, 1997; Reay, 2003). A 2002 review found that a sizeable majority of studies found beneficial effects of competition, although the sizes of the reported effects were generally small (Belfield & Levin, 2002). Studies of the impact 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 (Mayer et al 2002; Peterson and Howell, 2004; Barnard et al 2003; Howell and Peterson, 2002; Greene 2000; Greene, Peterson, and Du, 1997; Rouse 1998) and others showing no effects (Wolf et al, 2006; Krueger and Zhu, 2004, Witte 2000).

This paper tests whether students who exercise high school choice in an environment with a myriad of public school alternatives are more likely to graduate, an outcome of critical importance for adult employment prospects, earning potential, and transition to postsecondary education. Though 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 attempted to estimate the effects on high school graduation of exercising school choice in such a wide range of school types represented in this study.

This study examines the effect of school choice in Chicago, a city with a large public school choice program in which over 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 (Author, 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. Consequently, 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.

Background and Related Literature

In order 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. While 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 policymakers 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 *et al.* (1986) noted that there is wide between-school variation in dropout rates even once student differences are controlled. Bryk & Thum (1989) found that high levels of internal differentiation and weak school normative environments were associated with increasing the probability of dropping out. Rumberger & Thomas (2000) found that while 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 & Burkam (2003) found that curriculum, school size, and positive student-teacher relationships affected school drop out rates.

Research on Catholic schools helped sharpen the theoretical and methodological issues at stake in estimating school effects. Coleman and colleagues (Coleman *et al.*, 1982; Coleman & Hoffer, 1987) 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 *et al.* (1993) noted that prior Catholic school research had been limited by under-specified mechanisms, and sought to remedy this by conducting case studies of ten 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) 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 while 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 (Green *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 non-robust, effects for low-income black males at only a few grade levels (Gill, 2007). In a reanalysis of a private voucher experiment in New York City, Krueger & 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 taking into account when coding student's race.

Though 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 et al 2008). A much larger student lottery experiment found that students who attended a Chicago magnet high school/program were no more likely than non-attendees to benefit academically (i.e., they were no more likely to graduate, have higher test score gains, have higher credit accumulation rates), but were less likely to be arrested and were less likely to report disciplinary incidents (Cullen *et al.*, 2006). An observational study covering an earlier time period and with a larger sample size found that while students who attend a non-neighborhood 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 non-choosers) (Cullen *et al.*, 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 address 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 (while 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 the Chicago and provides confirmatory evidence from Florida, but ignores other types of school choice programs which serve substantially more students in Chicago (in the spring of 2001, for example, charter school secondary enrollment comprised less than two percent of total 9th 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 eleven percent of the 2000 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. In order 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 student's priority ranking, their score on the entrance exam (30%), their 7th grade reading and math test results (30%), 7th grade reading, math, science, and social

[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.

studies grades (30%), and 7th 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 eleven 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 non-selective programs within the same school, and about one in six had no selection criteria, but required an application for admission.[2]

The past forty years of school effects research provide several lessons for the present study. First, estimating such effects is methodologically complex, with the issue of selection looming large. Though 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 non-equivalent over time. A second lesson from school effects research is that treatment effects can vary across sub-populations. 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

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

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 was not available for this study, this study uses a variety of techniques to estimate the causal effect of choice on 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, admission criteria, operating lotteries and conducting admissions takes 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 the 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 while 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 2000 8th 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 their neighborhood schools. Hispanic students were more likely to remain in their neighborhood schools. 10% 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 8th graders opted to attend a private high school for 9th grade. The students who did

[3] Charter school enrollement 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.

so were disproportionately white and high achieving, although not as high achieving as the students who enrolled in the selective enrollment magnets.

[Table 1 about here]

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 while 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 four 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 four years (or less), Y=0 if the student dropped out of school or was still in school after four years. Let Z, our treatment variable, take on two values: Z=1 if a student exercised school choice by attending any public or private high school other than their assigned neighborhood school, and Z=0 if they attended their assigned neighborhood school. The empirical strategy employed in this paper 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 compare the results obtained from four different kinds of models. First, the study estimates a variety of linear probability models of the general form

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

where **X** is a vector of student observables such as race/ethnicity, gender, 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.

While 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 one, which is strongly *likely* to exercise school choice; and group two, 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 non-comparable (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 (Rosenbaum & Rubin, 1983; Frangakis & Rubin, 2002; Hong & Raudenbush, 2005). This technique first estimates a propensity model in which choice is regressed on a number of covariates predicting treatment group assignment:

$$p(X) \equiv \Pr[Z = 1 \mid X] = E[Z \mid X] \quad (2)$$

The propensity score, *p(x)* , is then used to break subjects into strata in which those who got the "treatment" (choosers in my case) and those who did not (non-choosers) are balanced on the propensity score itself and measurable covariates. Thus, this method creates sub-groups of subjects who are good "matches" for each other and compares the treatment effect among just these subjects. While 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, that propensity score modeling produces unbiased causal effect estimations (Rosenbaum & Rubin, 1983).

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

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

where *I(q)* denotes the set of units in block *q* and N_q1 and N_q0 are numbers of treated and control subjects in block *q*, respectively. The ATT estimator in principal stratification is

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

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:

$$Y_{ij} = \beta_0 + \beta_1 Z_{ij} + \beta_2 \boldsymbol{X}_{ij} + \beta_3 S_j + \varepsilon_{ij} \quad (5)$$

where $\mathbf{S}_j$ is a vector of *n-1* fixed effects and e_{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 multi-level HLM 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, varies by the quality or poverty level of the student's default alternative. The base model is

$$Y_{ij} = \beta_0 + \beta_1\left(Z_{ij} - \bar{Z}.j\right) + \beta_2\left(X_{ij} - \bar{X}.j\right) + r_{ij}$$

$$\beta_{0j} = \gamma_{00} + u_{0j}$$

$$\beta_{1j} = \gamma_{10} + u_{1j} \quad (6)$$

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 8th grade classroom during the spring of 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 non-assigned 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 the spring of 2004. Students who graduated from high school in the spring of 2004 (or earlier) were coded as 1 on the dependent variable. Those who dropped out or were still enrolled as of the fall of 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 the spring of 2001, were included in the analysis.[6]

Because these administrative data do not contain information about important pre-treatment 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-

[4]Omitted from this analysis are students who moved out of the city of Chicago between 8th and 9th grade (7.5%), those who were retained in 8th 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 a high school with an 8th grade, or due to death – 10%). Censoring these groups limits the generalizability of the study to the original 8th 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 8th and 9th grade and whose graduation outcome could be observed due to enrollment in the public system at some point during their secondary education.

[5] Most 8th graders in Chicago are enrolled in a 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 CPS 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 9th 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 9th grade school choice. The private school returnees comprise only one percent 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 paper.

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. While 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, non-poor, 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 8th graders. These students were administratively assigned to one of 49 public high schools,[9] but only 59% of these students attended their assigned school. The balance of the public high school attendees enrolled in 21 other public high schools.[10] Descriptive statistics on the analytic sample are shown in table 2. A table of unimputed descriptive statistics on the analytic sample is included in the appendix in table 8.

[Table 2 about here.]

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

[7]Self-efficacy and SES are positively scaled variables, with higher values denoting higher levels of efficacy and SES. 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 datasets produced by the multiple imputation process.

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

[10]The 25% of private school returnees went to about 60 private schools, four-fifths of which were Catholic schools.

(table 3, model 1).[11] This gap shrinks, but remains statistically distinguishable from zero when additional covariates are controlled. Socio-demographic 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 socio-economic status are associated with higher graduation rates, while neighborhood concentrated poverty is negatively associated with graduation propensity, net of student SES and other socio-demographic 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 socio-demographic 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 ten 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 the appendix in table 9.

[Table 3 about here.]

The models presented in table 3 assume that the effect of choice is constant across groups. This turns out to be an untenable assumption. Table 4 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 non-assigned other

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

public choosers versus assigned school attendees (model 4). The non-assigned other public category includes programs which 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 non-interacted 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 2 shows 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 non-assigned other public choosers versus assigned high school attendees only is statistically significant and

[12]Most students who attend selective enrollment magnet schools have high test scores. The median standardized 8th grade math score is 1.07. Only 8.6% of students had scores below +.05.

positive, but is much weaker than the interaction term in model three, suggesting that background achievement is not as salient for graduation chances for those students who are not elite school eligible.[13]

[Table 4 about here.]

[Figure 1 about here.]

If the effect of unobservables on school choice is conditionally independent of the other covariates, the results presented in tables 3 and 4 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. A logistic regression model of choice regressed on a number of predictors is included in the appendix (table 10). This treatment propensity model indicates that the following student and neighborhood characteristics are negatively associated with exercising choice: being male, bilingual, poor, old for grade, or white or Hispanic (versus 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

[13]Three other interaction effects were tested, but none reached conventional levels of statistical significance. There is some evidence of a differential ses-graduation gradient between choosers and non-choosers, although this finding is not statistically significant at the conventional .05 level. Though the main effect is statistically significant, the ses-graduation gradient among non-choosers is essentially flat. This gradient for choosers is weakly positive, suggesting that graduation propensity gains from choice are greater for choosers than for non-choosers. While 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 were found.

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 school.[14]

Subjects were divided into 12 balanced strata based on the predicted values of the propensity model.[15] T-tests of treated and untreated subjects indicate that these strata balance subjects on the propensity score. In addition, a total of 312 t-tests (with a .01 significance level criterion), indicate that the subjects are balanced on analysis covariates.[16] Panel A of table 5 shows that average propensity increases with strata, and that average propensity between treated and untreated subjects 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), non-choosers outnumber choosers. At high levels of propensity (strata 9, 10, 11, 12), choosers outnumber non-choosers. In all strata, there are adequate numbers of non-treated subjects to compare with treated subjects (strata 12 has the fewest comparison subjects at 43). The average effect on the treated, ATT, is the average within-strata difference between choosers and non-choosers on graduation propensity, weighted by the proportion of treated subjects across strata.[17] This results in an estimate of .036, with a standard error of .009 (see panel B of table 5). With a t-statistic of 4.052, we can reject the null hypothesis of an equality of graduation propensities between choosers and non-choosers. This result is

[14]Interestingly, the effects of neighborhood concentrated poverty and neighborhood SES are both non-linear. 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 non-significant neighborhood SES term and a positive, and significant, effect of concentrated poverty.

[15]Subjects 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 non-treated subjects to compare with subjects that 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 subject.

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

virtually identical to the result in the final model of table 3, providing evidence that the simpler linear probability model estimate may not be substantially inflated.

[Table 5 about here.]

The treatment effect, however, does not appear to be homogenous across strata. The effect of choice on graduation propensity appears to be 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 (table 6). Conditional on the student characteristics that predict graduation (from the final model presented in table 3), subjects in blocks 9-12 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 ten 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 non-choosers (model 4).

[Table 6 about here.]

Catchment Area Fixed Effect Estimate

Thus far, the models presented assume that the treatment effect of choice on graduation propensity 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 3 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 0.043 ($t=4.04$, $p<.01$), 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, 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 8, Model 1 includes

just student level covariates from the final model in table 3. In this specification variables are group mean centered on the neighborhood high school attendance area averages.[18] This specification, which is mathematically identical 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 HLM 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 evidence to support the hypothesis that students in neighborhoods with high achieving schools

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

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 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.

[Table 7 about here.]

Conclusion

Traditionally in the U.S., 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 last 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 8th 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

consistent 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, socioeconomic status, 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 (Rosenbaum & Rubin, 1983; Frangakis & Rubin, 2002; Hong & Raudenbush, 2005) 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 (Hoxby & Rockoff, 2004; Cullen *et al.*, 2005, 2006; Booker *et al.*, 2008) 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 benefitting at least some students. While 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, including the elite selective enrollment schools, which enrolled approximately 10% of this 8th 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 impacts for disadvantaged students (Friedman, 1955; Neal, 1997; Morgan, 2001). 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 effect of choice on graduation propensity for those with low probabilities of exercising choice is essentially zero. The effect of choice on graduation propensity 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 works best for those with high test scores and low neighborhood poverty suggests that school choice is 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 is 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 their school choice program more equitably, with greater reliance on lotteries and less reliance on selective criteria. Moreover, if access to high level course-work, 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

[20] For a study of charter school graduation and college enrollment effects see Booker et al (2008).

generalizable to other school districts with different approaches to student and parental choice of schools.

The research design emphasizes coverage of an entire 8th 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. This study does not employ an experimental design and the findings reported in this study 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. Omitting such covariates, however, would only 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 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.

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Appendix

[Table 8 about here.]

[Table 9 about here.]

[Table 10 about here.]

Figure 1: Predicted Graduation Gradient by Choice Status and Prior Math Score, Selective versus Assigned only, Chicago Public School 8th Graders, 2004. Note: Computed from coefficients in model 3 of table 4

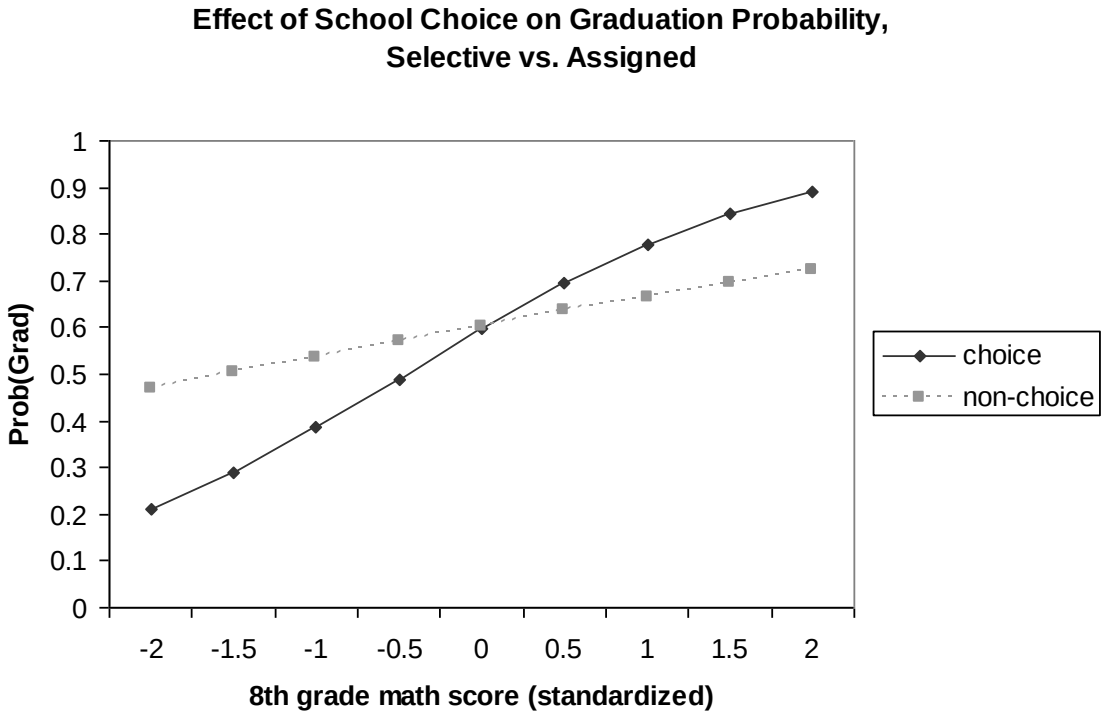

Table 1. Percentage distribution of 2000 enrollment, racial, gender, and poverty status composition, and average test scores, by high school type, Chicago, 2000.

	Pct of Enroll	Pct Black	Pct Hispanic	Pct White	Pct Asian	Pct Male	Pct 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.

Table 2: Analytic Sample Descriptives, 2004.

Variable	Description	Mean	Std. Dev.	Min	Max
Grad	Graduated in four 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 8th 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 last 3 yrs	0.37	0.628	0.00	5.00
SES	Socio-economic 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	8th 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: 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-squared	0.01	0.08	0.09	0.12	0.15

Absolute value of t statistics in brackets;
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 4: Linear Probability Regression Coefficients of On-time Graduation on School Choice with Interactions, 2004.

	1	2	3	4
	Entire Sample	**Entire Sample w/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-squared	0.15	0.15	0.17	0.13

Note: Models 1 and 2 is 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; + significant at 10%; * significant at 5%; ** significant at 1%; Models also control for gender, foster care status, old for grade, elementary school chooser, number of school moves, self-efficacy, and disciplinary incidences;

Table 5: Propensity Score Results

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

Strata	Pr(Grad)		Ave(Propensity)		SD(Propensity)		N	
	Non-Chooser	Chooser	Non-Chooser	Chooser	Non-Chooser	Chooser	Non-Chooser	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	Std. Err.	N(Treated)	N(Control)
0.036	0.009	10762	8097

Table 6. 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-squared	0.15	0.14	0.14	0.14

Absolute value of t statistics in brackets

+ significant at 10%; * significant at 5%; ** significant at 1%

Models also control for race/ethnicity, ses, 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.

Table 7: 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 [55.474]**	.623 [57.221]**	.625 [62.890]**
Low Poverty HS		.027 [.978]	-.004 [-.139]
High Poverty HS		-.015 -.531	-.018 [.916]
Average HS Ach			.003 [2.759**]
Choice Slope	.0399 [3.864]**	.039 [3.939]*	.041 [4.209]**
Low Poverty HS		.053 [2.298]**	.053 [2.025]**
High Poverty HS		.012 [.444]	.013 [.477]
Average HS Ach			.000 [.125]
	Random Effects		
Intercept (U0)	.00524 [377.786]**	.00520 [345.592]**	.0042 [272.369]**
Choice slope (U1)	.0025 [88.015]**	.0024 [80.845]**	.0025 [80.562]**

Note: Models also control for the covariates shown in table 3.

Table 8: Non-Imputed Analytic Sample Descriptives, 2004

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
Grad	Graduated in four 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 8th 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 last 3 yrs	16532	0.37	0.628	0.00	5.00
SES	Socio-economic 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	8th 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

Table 9: 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]**
Constant	-	-	-	-	-
	0.205	0.507	0.658	0.789	1.258
	[8.53]**	[12.32]**	[14.52]**	[16.97]**	[15.88]**
Obs	16532	16532	16532	16532	16532

Absolute value of z statistics in brackets

+ significant at 10%; * significant at 5%; ** significant at 1%

Notes: Replicates the linear probability models presented in table 3 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.

Table 10: Odds Ratios from Logistic Propensity Model Predicting High School Choice, 2004

	Odds Ratio	Std. Err.	z	P>|z|	[95% Conf. 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 HS)	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(Ave Dist to Choice HS)	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