NBER WORKING PAPER SERIES

NO CHILD LEFT BEHIND: ESTIMATING THE IMPACT ON CHOICES AND STUDENT OUTCOMES

Justine S. Hastings
Jeffrey M. Weinstein

Working Paper 13009
http://www.nber.org/papers/w13009

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2007

We would like to thank the Charlotte-Mecklenburg Public School District for making this project possible. We thank Joseph Altonji, Douglas Staiger, Ebonya Washington, and participants at the Yale University Labor-Public Finance Lunch and the Yale University Institution for Social and Policy Studies lunch for valuable comments. We gratefully acknowledge financial support from the Smith Richardson Foundation and the Yale University Institution for Social and Policy Studies. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

© 2007 by Justine S. Hastings and Jeffrey M. Weinstein. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

No Child Left Behind: Estimating the Impact on Choices and Student Outcomes
Justine S. Hastings and Jeffrey M. Weinstein
NBER Working Paper No. 13009
April 2007
JEL No. D8,I2

ABSTRACT

Several recent education reform measures, including the federal No Child Left Behind Act (NCLB), couple school choice with accountability measures to allow parents of children in under-performing schools the opportunity to choose higher-performing schools. We use the introduction of NCLB in the Charlotte-Mecklenburg School District to determine if the choice component had an impact on the schools parents chose and if those changed choices led to academic gains. We find that 16% of parents responded to NCLB notification by choosing schools that had on average 1 standard deviation higher average test scores than their current NCLB school. We then use the lottery assignment of students to chosen schools to test if changed choices led to improved academic outcomes. On average, lottery winners experience a significant decline in suspension rates relative to lottery losers. We also find that students winning lotteries to attend substantially better (above-median) schools experience significant gains in test scores. Because proximity to high-scoring schools drives both the probability of choosing an alternative school and the average test score at the school chosen, our results suggest that the availability of proximate and high-scoring schools is an important factor in determining the degree to which school choice and accountability programs can succeed at increasing choice and immediate academic outcomes for students at under-performing schools.

Justine S. Hastings
Yale University
P.O. Box 208264
New Haven, CT 06520-8264
and NBER
justine.hastings@yale.edu

Jeffrey M. Weinstein
Yale University
P.O. Box 208268
New Haven, CT 06520-8268
jeffrey.weinstein@yale.edu

1. Introduction

The federal No Child Left Behind Act (NCLB) of 2001 set out to reform public education by introducing accountability measures coupled with a public school choice requirement for all schools receiving federal Title I funds (Title I schools).[1] The public school choice component requires that districts allow parents of children at persistently under-performing schools the choice to send their child to a higher-performing school.[2] Public school choice provisions are included in several state-level accountability systems on which NCLB was in part based.[3] Such school choice and accountability programs are intended to provide all students the opportunity to obtain a high-quality education, as stated in the Department of Education's NCLB Public School Choice Guide: "When all students … are provided high-quality educational options, and when parents receive enough information to make intelligent choices among those options, public school choice can increase both equity and quality in education."[4] Thus the purpose of public school choice within an accountability program is two-fold. First, the choice provision offers parents the immediate option to send their child to a higher-performing school. Second, the threat of expanded parental choice may give schools a greater incentive to avoid regulation by improving student learning to reach stipulated academic achievement goals.

This paper empirically examines how effective school choice and accountability systems might be at accomplishing the first goal. We do so by estimating the impact of the NCLB school choice provision in the Charlotte-Mecklenburg School District (CMS) on parents' choices and subsequent student outcomes. The primary purpose of the choice provision is to give students at NCLB schools the opportunity to choose to attend schools with higher academic achievement

---

[1] Title I schools receive federal funds provided to school districts for assistance in improving the academic performance of students from low-income families. We will outline how Title 1 status is determined in the Charlotte-Mecklenburg School District in detail in Section 3.

[2] Title I Improving schools are Title I schools that have failed to make Adequate Yearly Progress (AYP) for two consecutive years. We will outline how AYP is determined in the Charlotte-Mecklenburg School District in detail in Section 3. Under NCLB, parents cannot choose another Title 1 Improving school for their child. The extent of school choices can vary by district, as NCLB set out broad provisions but allowed states and local districts flexibility in exactly how those mandates would be implemented.

[3] See Figlio and Rouse (2006), West and Peterson (2006), and Hanushek and Raymond (2004, 2005) among others for analyses of state-based accountability systems.

[4] U.S. Department of Education. (2004). *Public school choice*. http://www.ed.gov/policy/elsec/guid/schoolchoiceguid.pdf. (Page 9).

and to provide parents with clear information on academics to make an informed decision. The intent is that expanded choice and simplified information on academic performance (rating system) will allow these students to benefit from a higher-quality education. We find evidence that the NCLB choice provision in CMS accomplished this goal for a significant fraction of students. These families responded by choosing significantly better schools, and their children experienced academic gains as a result of being admitted to those schools. However, we show that that the proximity of schools with high test scores is an important factor determining the probability of choosing an alternative school in response to NCLB notification, the average test score at the alternative school chosen, and the subsequent impact of attending that school on academic outcomes. Thus it is not clear how successful the school choice provision would be in school districts with wide geographic densities of under-performing schools.

Because information on parental choice is often limited, and because NCLB just recently came into effect, most researchers have focused on estimating the incentives for marginal schools to improve to avoid regulation in states with accountability systems similar to those introduced under NCLB (Figlio and Rouse (2006), West and Peterson (2006), Hanushek and Raymond (2004, 2005)). There is some evidence that such accountability programs increase academic achievement at marginal schools; however, there is question as to whether the findings are generated by true gains in academic achievement or simply by gains in test score performance (Figlio and Getzler (2002), Jacob (2005), Chakrabarti (2005), Cullen and Reback (2006)). However, understanding parental choice response to regulation is a critical component in understanding the effect of school choice and accountability measures on school incentives and equilibrium outcomes. If regulation changes the population of families who choose to attend a school, schools may face weaker or stronger incentives to avoid regulation and may take different strategies to improve.

We use a unique policy experiment, the integration of NCLB into the newly-created school choice plan in CMS, in order to understand how NCLB notification affected parental choice. Starting in the 2002-2003 school year, CMS moved from a system of school assignment for racial integration to a district-wide public school choice plan. Each subsequent spring the school district elicited from parents the top three choices of where they wanted their child to go to

school and allocated slots at over-demanded schools by lottery. At the end of the 2003-2004 school year, CMS determined which schools failed to meet NCLB performance criteria. Students slated to attend these schools in the fall were re-issued choice forms that summer, along with notification that their school had failed to make AYP for two years in a row. They also received the NCLB-required simplified information on academic performance at their school and at all other schools in the district. This was a basic printout that listed every school in the district (regardless of grade level and program) along with its proficiency score (the percent of students who tested at or above grade level in reading and in math on the prior year's standardized tests).

We are able to compare the choices parents made in the spring (prior to notification) with the choices they made in that summer after receiving the NCLB notification letter. Because the two rounds of choice fall so closely together, we can compare choices within student, before and after the notification, thereby credibly holding all else equal (including grade level). In addition, after receiving the summer NCLB choice forms, the district assigned students to schools based on a lottery system. We can use these assignments to identify the effect of attending a chosen school on academic outcomes. We find that NCLB notification led to a significant change in the choice behavior of parents. Approximately 16% of parents who received notification responded by choosing a school different from their NCLB school. These parents chose schools with average test scores approximately 1 standard deviation higher than the schools they chose to attend just a few months earlier.[5] Thus, the NCLB notification with the information on academic performance facilitated the choice of a higher-performing alternative school for a significant fraction of parents.

We then use the lottery assignment of students to oversubscribed schools to determine if the choices caused by NCLB notification resulted in higher academic achievement. We measure academic achievement using observable student-level statistics such as absences, suspension rates, and standardized test scores. We find that students who were randomly admitted to the chosen school experienced a 9.9 percentage point decline in the probability of having a week or

[5] This is 1 standard deviation in the distribution of scores across schools in the district, which translates into about 0.5 student-level standard deviations. We will show that this change in choice behavior is not generated by the restriction that students exercising choice under NCLB cannot choose to attend other Title I Improving schools. Rather, parents who responded to the NCLB notification picked higher-scoring schools controlling for this choice set restriction.

more suspensions off of a mean of 25.8 percent. On average, students experience a positive but statistically insignificant increase in standardized test scores. We do find evidence that students who apply to schools that are substantially higher-scoring than their current school experience significant improvements in test scores. Among students with above-median differences between the chosen school's average test score and the score at their current NCLB school, admission to the chosen school increased test scores by 0.17-0.19 student-level standard deviations.

The remainder of this paper is organized as follows. Section 2 reviews the relevant NCLB literature. Section 3 describes the school choice plan and implementation of NCLB in CMS. Section 4 discusses the data and empirical results for the effect of NCLB notification on choices. Section 5 discusses the empirical results for the effect of NCLB notification on student outcomes. Section 6 concludes.

2. Literature Review

There is little empirical evidence to date on the impact of NCLB on parental school choices and subsequent student outcomes. Because the regulation is so recent and information on student choices is often not available, researchers have focused on the impacts of state accountability systems on academic achievement. Greene (2001), Figlio and Rouse (2006), and West and Peterson (2006) examine the effect of the Florida A+ Plan on school average test scores. They all find a relative improvement in test scores at low-performing schools that are faced with the threat of offering school vouchers (close to regulation, but not actually regulated). Figlio and Rouse (2006), however, provide evidence that these gains are mainly due to the stigma of being labeled a low-achieving school instead of voucher threats. Clark (2003) examines the 1990 Kentucky Education Reform Act (KERA) which consists of funding, academic, and accountability reforms. She does not find an impact on academic performance measures for white students but does find some evidence that KERA increased the performance of black students. Hanushek and Raymond (2004, 2005) and Carnoy and Loeb (2002) construct state-level panel data on accountability systems and find a positive relationship between average test scores and accountability measures. However, Figlio and Getzler (2002), Jacob (2005), Chakrabarti (2005), and Cullen and Reback (2006) all provide evidence that changes in school-

level average accountability measures may result from policy-induced behavior aimed at maximizing test scores that count in regulation rather than improving academic achievement in and of itself.

However, most of these studies do not examine how the choice component of accountability systems such as NCLB affects parents' choices and subsequent student achievement (West and Peterson (2006)). Hastings, Kane, and Staiger (2006a) examine parental choices and preferences using the introductory year of school choice in CMS. They find that low-income parents of students with low academic performance tend to place a low weight on academics when choosing schools. Hastings, Van Weelden, and Weinstein (2007) randomize simplified information on academic quality of schools across non-NCLB schools serving middle- to low-income students. They show that receiving simplified information with the school choice form causes an increase in active choice participation, an increase in the average test score of the chosen schools, and a doubling in the estimated preference for academic performance. They conclude that a lower preference for academics for low-income students is consistent with higher information and decision making costs. In a similar manner, NCLB requires that families be provided with simplified information on the academic quality of their own school as well as on other schools in the district. We find in CMS that a fraction of students respond by choosing much higher academically-performing schools.

Another line of research estimates the academic gains for students who exercise choice. These papers exploit random assignment of students to over-demanded schools in school choice or voucher programs to estimate the average treatment effect of attending a first-choice school, conditional on the school chosen (Witte, Sterr, and Thorn (1995); Greene, Peterson, and Du (1997), Witte (1997), Rouse (1998), Peterson, Myers, and Howell (1998), Mayer et al. (2002), Krueger and Zhu (2004), and Cullen, Jacob, and Levitt (2006)). These papers have been unable to find robust or significant academic gains from attending a first-choice school; however, Hastings, Kane, and Staiger (2006b) combine detailed information on student choices with lottery randomization into first-choice schools and show that students placing a high weight on academics when choosing schools experience significant academic gains when randomized into their first-choice schools. We will show that NCLB led some parents to choose schools with

substantially higher scores and that their children experienced some gains in traditional measures of academic outcomes as a result of gaining admission to those schools.

3. CMS School Choice Plan and No Child Left Behind

3.1. Overview of the CMS School Choice Plan

CMS introduced district-wide school choice in the fall of 2002. Prior to that, CMS operated under a racial desegregation order for three decades, busing students from discontinuous neighborhoods to achieve racial balance at schools across the district. In September 2001, the U.S. Fourth Circuit Court of Appeals ordered the district to dismantle the race-based student assignment plan by the beginning of the next school year. In December 2001, the school board voted to approve a new district-wide public school choice plan.

In the spring of 2002, parents were asked to submit their top three choices of school programs for each child. Each student was assigned a "home school" in her neighborhood, often the closest school to her, and was guaranteed a seat at this school. Magnet students were similarly guaranteed admission to continue in their current magnet programs. Admission for all other students was limited by grade-specific capacity limits set by the district. Students could choose any school in the district; however, busing transportation was only guaranteed to schools in a student's quadrant of the district (the district was split into 4 quadrants called "choice zones"). The district allowed significant increases in school enrollment size in the first year of the school choice program in an expressed effort to give each child one of her top three choices. In the spring of 2002, the district received choice applications for approximately 105,000 of 110,000 students. Admission to over-subscribed schools was determined by a lottery system as described in Hastings, Kane, and Staiger (2006b).

Once the first year of school choice was completed, students were required to submit choice forms in subsequent years only if they were new to the district, rising graders (Kindergarten, 6th, or 9th grade), affected by changes in home school boundaries due to new school openings, or

wanted to change schools from their current school assignment. In other words, once the first year of school choice was completed, and students were for the most part attending chosen schools, students had the guaranteed right to remain in that school until the terminal grade and did not need to submit a choice form unless they wanted to change schools again. In the following two years, CMS continued to experience near complete participation in the school choice plan. In each year, admission to over-subscribed schools was determined by a lottery system. However, after the first year of choice, CMS did not expand capacities at schools in an attempt to accommodate demand, and hence, the number of oversubscribed schools increased substantially.

3.2. Information on Schools

In order for parents to determine which schools to choose, CMS provided several resources. First, each family received a choice book. The choice book was approximately 100 pages long. It contained detailed instructions on how to complete the school choice form and how to submit it along with a brief description of the lottery process.[6] The bulk of the choice book was devoted to written descriptions of each school and program, from preschool through high school. There are approximately 120 elementary, 40 middle, and 30 high school choice options in the district. The descriptions were written by the schools, describing the positive features each school offered to students. Objective measures of school characteristics such as average test score performance, suspension rates, or racial compositions were not provided.

In addition, CMS provided a family application center that parents could phone or visit in order to ask questions about the school choice process. The staff members at the family planning center were instructed to emphasize the positive aspects of each school during their discussions with parents. In particular, staff members were supposed to respond to questions like "Which school is the best school?" by advising parents to discuss with their children what their needs were and then to visit the different school options in order to determine which school was the best for their children, since what a "good" school is depends on each individual child.

[6] Parents were not told how the lottery was run (e.g. first-choice maximizer) or how 'priority boosts' were implemented.

CMS also offers an extensive website. On this website, parents can review statistics for each school one-by-one. The school profiles provide statistics such as physical locations, standardized test score performances, suspension rates, racial compositions, and attendance rates. However, statistics for schools were reported as averages for the entire school even if different school programs were housed on the same campus (e.g. magnet program and non-magnet program). This aggregation may mask the true achievement rates of the separate school choice options on each campus. In addition, parents would have to view all statistics for each school separately, instead of viewing a statistic for all of their choice options on one simplified page. Hence, obtaining objective information on schools would involve a significant web search and comparison.

3.3. The Implementation of NCLB in CMS

NCLB legislation was introduced in January 2002. Beginning in the summer of 2003, CMS implemented NCLB in accordance with North Carolina state regulation that in turn was based on federal requirements. Each year, all schools are required to make Adequate Yearly Progress (AYP). However, only Title I schools face sanctions under NCLB if they fail to do so. As defined by CMS, a school is a Title I school (receives federal Title I funds) if 75% or more of its students qualify for federal lunch subsidies. As defined by North Carolina under NCLB compliance, a school needs to satisfy certain academic targets for 10 subgroups of students in order to make AYP.[7] Each subgroup needed to have forty or more students for it to be included in the determination of school AYP. If just one target was missed for one subgroup, then the school failed to make AYP. Targets include the percentage of students scoring proficient on North Carolina standardized tests for math and reading for each subgroup (with the percentage needed to make AYP gradually increasing over time in order to meet the federal requirement of 100% proficiency by the end of the 2013-14 school year)[8], a minimum participation rate in each of the exams for each subgroup (95% in each year or averaged over the prior two or three years),

[7] For North Carolina, the subgroups are the entire school, Asian, American Indian, Black, Hispanic, Multi-racial, White, economically disadvantaged, limited English proficiency, and students with disabilities.
[8] This requirement can also be satisfied for a subgroup if its percent proficient falls within a 95% confidence interval for the target percent proficient.

attendance rates for elementary and middle school students (an increase in 0.1% from the previous year or anything over 90%), and graduation rates for high school students (an increase in 0.1% from the previous year or anything over 90%).[9]

At the end of the 2003-2004 school year, CMS compiled the test score outcomes for schools in the district and determined that sixteen schools, ten elementary and six middle schools, were both Title I schools and had failed to make AYP for the past two years. These schools were categorized as Title I Improving and entered regulation under NCLB. The regulation implied that parents needed to be notified of the NCLB status of their school and offered the choice to attend an alternative school. In addition, the district (as part of a federal requirement) was required to supply with this notification information on the academic achievement of the schools that parents could select.[10] CMS provided information on the percent of students in the NCLB school who made grade level in reading or math (percent proficient) for every school in the district (a three page spread-sheet print-out), as well as list of Title I Improving schools since students exercising choice under NCLB were not allowed to choose to attend another Title I Improving school. Thus the NCLB legislation provided simplified information to parents on the academic achievement at their school and at every other school in the district along with notification that their school had failed to make AYP and that they therefore had a right to choose to send their child to another non-NCLB school.

Parents who received the NCLB notification had submitted (along with all other parents) choice forms in the spring of 2004 for the 2004-2005 school year. Because NCLB schools were identified in June 2004, at the end of the school year, CMS re-sent choice forms in July along with the NCLB notification to parents of students slated to attend NCLB schools in the fall. These forms had a similar format to the typical CMS choice form and allowed parents to submit three choices for what school they would like their child to attend in the 2004-2005 school year. Because of the timing of the choice plan and determination of NCLB schools, we are able to observe how choices change for parents receiving NCLB forms. We observe their choices

[9] For information on other means by which subgroups can make AYP, please see the Consolidated State Application for North Carolina (2005) which provides federal NCLB guidelines along with North Carolina's implementation of these guidelines.
[10] U.S. Department of Education. (2004). *Public school choice*. http://www.ed.gov/policy/elsec/guid/schoolchoiceguid.pdf. (Page 18).

submitted in the spring of 2004 for which school they would like their child to attend in the fall. After parents received the NCLB notification letter, we observe their choices submitted in July 2004 for which school they would like their child to attend in the fall given this new information on their school's relative academic performance.

Once the NCLB choice forms were received, parents had approximately one month to submit them. Students were then entered into a choice lottery. CMS made spaces available at previously full schools (partly through normal summer attrition of current students) in order to accommodate NCLB students. Students were sorted by priority group and a randomly assigned lottery number.[11] Admission to schools was thus determined first by priority group and then by lottery number. Priority groups were based on academic achievement (above- or below-grade level) and lunch-recipient status. This was done to satisfy the federal requirement that the poorest and lowest-achieving students be allowed the first opportunity to attend an alternative school.

The lottery was not run as a first-choice maximizer as was typically done in the spring lotteries for the district as a whole. Instead, all of a student's choices were evaluated at once when her lottery number came up. Parents, as usual, did not know the lottery process, and in this case they were not informed of the NCLB-specific priority groupings either. If a student did not gain admission to any of her choices, her parents were allowed to check a box that stated that they would like the district to attempt to place their child administratively in a non-NCLB school with an open slot instead of returning her to her NCLB school. The district would then supply an assigned school in August, and parents could accept or reject that school over their current NCLB school at that point. Of the students used in our analysis, 1,092 out of 6,695 submitted a choice form in July and chose a school other than their current NCLB school. Of these, 615 checked the box saying that they did not wish to return to their NCLB school if they did not get any of their choices. Of these, 208 did not receive one of their choices and were offered administrative placement. Of these, only 50 ended up attending the administratively placed school.

[11] The random number was assigned by a computer using an algorithm that we verified with CMS computer programmers.

4. Data and Regression Analysis

4.1. Data Description

We have secure access to administrative data from CMS including choice form information for every student who submitted a form in the Spring 2004 school choice round and the July 2004 NCLB choice round, student-level lottery numbers for each choice round, school assignments, attendance records, test score outcomes, and student demographics. We also have information on student and school locations. We use the student-level data to construct school characteristics. These characteristics include percent black, percent of students receiving lunch subsidies, and average standardized test scores - each student's test score is standardized by grade level, and the average of these standardized scores over the students at a school choice is the school choice's test score measure. We focus on elementary and middle schools (students in K-8th grades), since there were no Title 1 Improving high schools in CMS.

Table I describes the schools that were designated Title I Improving at the end of the 2003-2004 school year. The 16 Title I Improving schools had on average significantly lower test scores than the district average. However, there were elementary and middle schools with average test scores in this range that were not Title I Improving because they either failed to make AYP both years but did not make Title I classification for at least one year (21 schools fall into this category) or because they were Title I both years but made AYP in at least one of the two years (13 schools fall into this category). Because Title I status in CMS is defined as 75% or above free- or reduced-lunch concentration, Title I Improving schools have a substantially higher-than-average lunch-recipient rate. In addition, they have a higher proportion of black students, a lower average neighborhood income level, and higher-than-average suspension rates. In addition, according to school-level capacity data from CMS, these schools are on-average under-demanded (below capacity) and have a smaller student body population.

4.2. The Effect of NCLB on Student Choices

We began with the 8,284 students who received NCLB notification, of which 1,363 responded by filling out a form in July. Parents were told, just like in the regular spring lottery, that if they

wanted to remain at their current school, they did not have to fill out a form. We exclude from the analysis students who were not active in CMS at the time of the spring lottery (221 students), students with special needs or those being retained (1,245 additional students), and students who had missing demographic information (123 additional students). This left us with a sample of 6,695 students who received NCLB notification, of which 1,149 responded by submitting a form in July. Of the parents who did fill out a form in July, 57 of them listed their current NCLB school as their first choice which they did not need to do in order to remain at their NCLB school. Thus, 1,092 students filled out a form in July and chose a school different than their current NCLB school first, and 5,603 students either did not respond to NCLB notification or chose their NCLB first in the July lottery.

Student were slated to attend a NCLB school in the fall of 2004 for one of two reasons: their parents chose that school in the spring (either actively or through default), or their parents chose a different school, did not win admission, and the student was assigned to the NCLB school. Table II shows the cross tabulation of two indicators: if the parent chose the NCLB school in the spring, and if they chose a school other than their NCLB school in July. Parents who did not choose their NCLB school first in the spring but still got placed in the NCLB school were twice as likely to respond by choosing a school different than their NCLB school first in July (30.31% versus 14.55%). Approximately two thirds of parents who did not choose their NCLB school first in the spring did not decide to select another school in July. This may be because parents find it more difficult to change schools in the middle of the summer, not long before the new school year. Therefore, while NCLB notification doubled the fraction of students choosing to leave their NCLB school (865 versus 749), we may in fact be underestimating the effect given the timing of the notification.

If we assume that parents who did not fill out a form in July wanted their child to attend the NCLB school, then we can estimate the average impact that NCLB had on parents' choices. Table III presents a reduced-form regression of the effect that receiving a NCLB form had on the characteristics of students' first-choice schools. The data include each student's first-choice school in the spring and in July, where we assume that a student chose her current NCLB school first in July if she did not fill out a form. The average effect is the average within-student change

in the characteristics of the first-choice school before (Spring) and after (July) receiving the NCLB notification. Table III shows that receiving the NCLB form made parents less likely to choose their NCLB school first, and their first-choice schools had higher average test scores, lower percent black, and lower lunch subsidy rates. Although the coefficients are statistically significant, they are small in magnitude. For example, NCLB form receipt caused an average increase in the scores of schools chosen first of only 0.047 standard deviations – approximately one to two percentile point ranks. However, 84% of parents in our sample did not return the form or returned the form but chose their NCLB school first, therefore choosing a school with no change in average test scores.[12] The significant but small in absolute value increase suggests that NCLB had a very large impact on the characteristics of the first-choice school for the 16% who submitted forms and chose a school other than their NCLB school first in July.

Table IV shows the differences in the characteristics of first-choice schools between spring and July choice rounds for students who responded in July by choosing a school other than their NCLB school first. The first two rows show that students chose schools first that were on average .5 student-level standard deviations higher in average test scores in July versus the spring. This is more than a 15 percentile point rank increase in scores and is 1 standard deviation higher based on the distribution of school average test scores. We break down these numbers by race; however, there is not a substantial difference by race. In addition, the schools chosen first in July had a substantially lower percent black and a substantially lower free- and reduced-lunch concentration. In the spring, the students' first-choice schools were in the 78th percentile of the distribution of school percent black and the 82nd percentile of the distribution of school percent free and reduced lunch of the schools they could choose. In July, the students' first-choice schools were in the 53rd percentile of the distribution of school percent black and the 47th percentile of the distribution of school percent free and reduced lunch of the schools they could choose. Because students were not allowed to choose other NCLB schools in the July lottery, we may be concerned that the increase in the score of the first-choice school is partly generated by the fact that students were prohibited from choosing some of the schools with low performance. Rows 3 and 4 show the average test score of schools within five miles of the student (an

[12] As shown in Table II, about 9% of the students who chose their NCLB school first in July (by virtue of not submitting a form or by submitting a form and putting down their NCLB school first) chose a different school first in the spring and did not get in. For these students, the difference in test scores would not be zero.

approximation for relevant schools in the choice set). The average score of available schools increases from Row 3 to Row 4 due to the restriction that students could not select another NCLB school. If we compare Rows 1 and 2 to Rows 3 and 4, we see that students' first-choice schools had lower-than-average test scores given their choice set in the spring and had higher-than-average test scores given their choice set in July. Hence, the increase in the scores at the first-choice school was not mechanically generated by the choice set restriction placed by NCLB.

The statistics presented in Table IV showed that a fraction of families who received NCLB notification responded by choosing substantially better schools. However, most families did not respond. We can empirically examine what types of families were more likely to respond by choosing a school other than their NCLB first in the July lottery; for example, did NCLB notification affect the choices of higher-achieving students more than it did for lower-scoring students? Tables V and VI both examine the decision to respond to the NCLB notification as a function of student and school characteristics. Table V presents simple differences in mean student attributes across these students who responded by choosing a school other than their NCLB school first in July versus students who chose their NCLB school first in July (the latter group includes students who did not fill out a form), controlling only for NCLB school fixed effects. The first two columns of statistics in Table V give the straight means for the two groups of students for each variable of interest. The final column reports the difference in means between the students who responded by choosing a school different from their NCLB school first in July versus students who chose their NCLB school first in July, adjusting for NCLB school fixed effects.

The coefficients show that students who chose a school other than their NCLB school first in the spring, but did not gain admission to that school, were more likely to choose a school different from their NCLB school first again in July. This reflects the cross tabulations seen in Table II. In addition, students in rising grades were more likely to choose to attend a different school in July. This makes sense since many parents may find it easier to switch their child's school at this time. We also find that black students and students who are in magnet programs in their NCLB schools are marginally statistically significantly more likely to choose a school other than their NCLB school first in July. There are no significant differences for most of the other student-level

demographics such as gender, lunch-recipient status, baseline test score, and income. However, parents of students who had more suspensions and who had fewer unexcused absences in the baseline year were more likely to respond to the NCLB notification by choosing a different school first in July.

There are two other characteristics that are on average significantly different across the two groups of students. The distance from the family to the NCLB school and the average test score of local schools all significantly differ across students who responded by choosing a school other than their NCLB school first in July versus students who chose their NCLB school first in July. These variables would increase or decrease the attractiveness of the NCLB school relative to other potential schooling options. Students who responded by choosing a school other than their NCLB school first in July lived on average further from their NCLB school and also had higher average scoring schools to choose from within a five-mile driving distance. Hence alternatives to the NCLB school may have been slightly more attractive for these students, increasing the probability that they would respond to the NCLB notification by choosing a different school first.

Table VI presents a conditional (fixed-effects) logit of the probability of responding to the NCLB notification by choosing a school other than the NCLB school first as a function of the baseline characteristics presented in Table V. Again, the fixed effects are at the NCLB school level. The logit results are for the most part consistent with the mean differences in Table V. Students who chose to attend a school other than their NCLB school in the spring, but were assigned back to their NCLB school, were significantly more likely to respond to the NCLB notification. The coefficient implies a 114% increase in the odds of responding (exp(.759) – 1), which translates into an 11.0 percentage point increase in the probability of responding to the NCLB notification by choosing a school other than the NCLB school first in July. In addition, an increase in baseline test score significantly increases the odds of choosing a school different from the NCLB school first in July. A one standard deviation increase in baseline test scores increases the odds of choosing out of the NCLB school by 11%, which translates into a 1.5 percentage point increase in the probability of responding by choosing out. Again we find that an increase in baseline number of unexcused absences significantly decreases the probability of responding,

while an increase in suspensions significantly increases the probability of responding. For this sample of students, the raw correlations between test scores, unexcused absences and suspensions are small (between 0.18 and 0.22 in absolute value), implying that the effects do not end up canceling each other out for a typical student. Taken together, there seem to be two types of students who responded to NCLB notification by choosing a school other than their NCLB school first in July: those who were doing on average better in school (higher test scores and fewer unexcused absences) and those who were having problems (more suspensions).

The final significant determinant of responding to the NCLB notification is the average test score of nearby schools. If the average test score at nearby schools increases by one student-level standard deviation, there is an 84% increase in the odds of responding by choosing a school other than the NCLB school first. This translates into an 8.8 percentage point increase in the probability of responding. Thus, a key determinant of response is the availability of high-achieving alternatives in the surrounding area. If there are not better alternatives available within a reasonable distance, parents may not choose out of failing schools simply because there are no better alternatives to choose from.

Table IV showed that students who responded to the NCLB notification chose on average substantially better schools. There is, however, a large variation across responders in the average test scores of their chosen schools. Figure 1 shows a kernel density estimate of the difference in the test score of the first-choice school listed on the July choice form and the NCLB school for students who chose a school other than their NCLB school first in July. On average, responders selected first-choice schools with 0.62 student-level standard deviation higher test scores than their NCLB school. However, a small fraction of students chose schools that were close to or slightly worse performing than their NCLB school, while another minority of students chose some of the highest-performing schools in the district; schools that outperformed their NCLB school by over one student-level standard deviation in test scores.

Of students who chose a school other than their NCLB school first in July, what types of students chose substantially better schools? Table VII presents regression results of the test score at the July first-choice school on baseline student characteristics for those who chose a school different

from their NCLB school first in July, controlling for NCLB school fixed effects. Students who did not choose their NCLB school first in the spring chose on average higher-scoring schools in July. Black students chose schools with slightly lower test scores, as did students who were entitled to receive federal lunch subsidies and students with unexcused absences. Students enrolled in magnet programs at NCLB schools chose higher-scoring schools.

The biggest observable determinant of the average score of the first-choice school in July seems to be proximity to higher-scoring schools. Increasing the average test score of schools in a five-mile radius by one student-level standard deviation increases the average score at the first-choice school by 0.229. Hence the proximity and availability of much higher-scoring schools seem to determine both the probability of choosing to leave the NCLB school as well as the test score at the first-choice school in July. This is consistent with the strong weight parents place on proximity when choosing schools as found in Hastings, Kane, and Staiger (2006a). Choice is most effective when there are many options in close proximity, all else equal.

5. The Effect of Choices on Student Outcomes

NCLB notification succeeded in changing choices for some parents at Title I Improving schools. The simplified information led a significant fraction of parents to choose to send their children to higher-performing schools. However, it is not clear if changing the choices that parents make improves their child's academic outcomes. It is therefore important to estimate the impact that these choices had on subsequent measures of academic achievement. We will use academic outcomes at the end of the first year after NCLB notification to test for the presence of academic gains as a result of attending a newly-chosen school.

Once choice forms were submitted, admissions were determined by lottery process. The lotteries were run based on the number of seats made available for each grade and choice combination. As described earlier, the lottery number was the concatenation of two priority numbers followed by a random number. Priority was given to students performing below grade level and to students who qualified for free or reduced lunch. This was done to satisfy the NCLB requirement

that the lowest-performing and poorest students be given the first right to attend a school other than their "failing" school.

We present two different methods for estimating the effect of winning versus losing the lottery on outcomes. Method 1 uses only the priority group (if any) in each grade and choice combination for which some students won and some students lost that lottery; that is, we include only students for whom lottery number alone determined admission to that grade and choice combination. Method 2 uses all students, regardless of priority group, whose first-choice school and grade is one in which some students won and some students lost the lottery for admission to that school. Of the 1,092 students who filled out a form in July and chose a school different than their current NCLB school first, 227 students are in the sample defined by Method 1, and 562 students are in the sample defined by Method 2.

For the sample defined by Method 1, admission is determined solely by lottery number, so the impact of winning or losing the lottery is well identified. On the other hand, the sample size is small, which may imply that the results do not hold more broadly. Hence, we also use the sample defined by Method 2, more than doubling the sample size. However, here admission is determined by priority group and lottery number; students with lower tests scores and/or who are free- or reduced-lunch recipients have lower (better) priority numbers and hence a better chance of admission. We will control for student baseline characteristics, as well as first-choice and grade fixed effects, in the regression analyses that follow.

Table VIII reports mean baseline characteristics for lottery winners and losers, as well as regression adjusted differences from an OLS regression including fixed effects for the school choice program and grade for which the lottery is being conducted (lottery-block fixed effects).[13] The first three columns use the sample defined by Method 1, and the second three columns use the sample defined by Method 2. Coefficients in column 3, rows 1 through 7, show that lottery winners and losers do not have significantly different baseline demographic or academic baseline characteristics for students in the Method 1 sample. Hence lottery numbers do not predict

[13] For the Method 1 sample, note that lottery-block fixed effects span priority-group fixed effects. We must control for lottery-block fixed effects since the odds of admission change across each lottery.

baseline characteristics for students in the Method 1 sample, as we would expect if they were indeed randomly assigned. Recall that the lottery was not run as a first-choice maximizer; rather, it evaluated a student's choices when the student's lottery number came up. To construct the Method 1 sample, we treat a student as choosing a school if she listed it as any of her choices. It may be the case that students who listed a school second are less likely to be admitted since they may have had a higher (worse) priority group and/or higher (worse) lottery number to have not been admitted to their first-choice school. The third column of the final row shows that winners were no more or less likely to have listed the choice as their first choice than losers were. We fail to reject the null hypothesis that there is a differential impact of winning versus losing the lottery on the probability of listing the choice first.[14]

The last column of Table VIII shows that baseline characteristics do vary significantly across lottery winners and losers for students in the Method 2 sample. Students who won the lottery were more likely to be black, were more likely to receive free or reduced lunch, had lower incomes, had more suspensions, and had lower baseline test scores. These differences are mechanically generated by the priority group definitions, and they are most significant for lunch-recipient status and baseline test scores (the two variables that define priority groups). We control for these baseline characteristics when estimating the effect of winning versus losing the lottery on student outcomes.

We now want to test if there are significant differences in end-of-year outcomes for students who won the lottery to attend their chosen school versus those who lost the admission lottery. Since we do not have data on outcome variables for students who were not enrolled in CMS for the 2004-2005 academic year, it is important to look at attrition. Table IX shows the effect of winning versus losing the lottery on whether the student was not enrolled in any CMS school in the 2004-2005 school year. This estimate gives the differential attrition between lottery winners and lottery losers. The first two columns use the sample defined by Method 1, and the second two columns use the sample defined by Method 2. The average attrition rate is 6.2% for students

[14] In the Method 1 sample, 172 students listed the school as their first choice, and 55 listed the school as their second choice. There were none who listed it as their third choice. However, given the many different choices and the availability of those choices, this may not affect our sample considerably.

in the Method 1 sample, and 5.0% for students in the Method 2 sample.[15] For the Method 1 sample, the estimated effect of winning versus losing the lottery on attrition is small (0.057) but marginally significant (p-value = 0.081). However this has the opposite sign than what we would expect. The positive value implies that, if anything, lottery winners were more likely to attrit than lottery losers, instead of the other way around. For the Method 2 sample, the estimated effect of winning the lottery on attrition is much smaller (0.012) and is not statistically significantly different from zero.

Table X presents the reduced-form estimates of winning versus losing the lottery on measures of academic achievement. The first four columns use the sample defined by Method 1, and the second four columns use the sample defined by Method 2. The results are presented for all students on average, as well as for those with above- and below-median expected "treatment", where treatment is defined as the difference between the test score at the chosen school and the test score at the current NCLB school. For the Method 1 sample, the median difference in the test score of the chosen school and the test score at the current NCLB school is 0.893 student-level standard deviations. For the Method 2 sample, the median difference in the test score of the first-choice school and the test score at the current NCLB school is 0.642 student-level standard deviations. All regressions include fixed effects for each grade and choice combination, as well as student baseline characteristics: black, lunch recipient, female, demeaned income, an indicator for greater than or equal to five unexcused absences, an indicator for greater than or equal to one unexcused absence, an indicator for greater than or equal to five suspensions, an indicator for greater than or equal to one suspension, total number of unexcused absences, and total number of suspensions. Results for student standardized test score outcomes also include baseline student test scores, lowering the sample size to 161 students in the Method 1 sample and 451 in the Method 2 sample, as only students in third through eighth grades take these exams. Standard errors are clustered at the school-choice and grade level in all regressions.

The first row presents the effect of winning versus losing the lottery on the average test score of the school attended. For students in the Method 1 sample, lottery winners attended schools with

[15] These rates are lower than typical, but it is probably because students in our sample are observed half way through the summer so we would expect them to be lower if most moving occurs in the first half of the summer.

on average 0.324 student-level standard deviation higher test scores than lottery losers. Students who chose schools with substantially better (above-median) test scores than their current NCLB schools attended schools with on average 0.375 student-level standard deviation higher test scores if they won the lottery. Students who chose schools with below-median differences in test scores attended schools that had 0.166 higher average test scores if they won the lottery; however, the coefficient is not significant. For students in the Method 2 sample, lottery winners attended schools with on average 0.146 student-level standard deviation higher test scores than lottery losers, which is marginally significant (p-value = 0.070). Students who chose schools with substantially better (above-median) test scores had a significant 0.256 increase in the score of the school attended as a result of winning the lottery. This point estimate is -0.032 and insignificant for students winning lotteries to attend schools that with moderately (below-median) higher scores than their NCLB school. Given that we find a small and insignificant effect of winning versus losing the lottery on the test score of the school attended for students with a below-median "treatment", we may not expect to see significant academic gains for these students as a result attending their first-choice school.

The second row of Table X shows that winning versus losing the lottery does not have a statistically significant impact on the probability of having a week or more ("serious") unexcused absences. This result holds across both samples. However, we do find that, for the Method 1 sample, the probability of having at least one unexcused absence decreases by 21% (0.174 off of a base of 0.845) when students choose, and gain admission to, schools that have above-median increases in test scores over their current NCLB schools. Across both samples, the point estimates imply that students who choose schools with below-median differences in test scores have an increased likelihood of having at least one unexcused absence if they won, and the effect is statistically significant for the Method 2 sample.

The fourth and fifth rows of Table X show the impact of winning versus losing the lottery on the probability of having at least one week of suspensions ("serious") or at least one suspension ("mild"), respectively. Across both samples, we do not find any evidence that winning versus losing the lottery affected the probability of having a "mild" suspension. For the Method 1 sample, we find, on average, that the likelihood of having "serious" suspensions decreases by a

statistically significant 38% (0.099 off of a mean of 0.258) when students win the lottery. While the decrease is larger for students in the above-mean "treatment" group, it is not statistically significant. The coefficient is closer to zero and insignificant for students in lotteries to attend schools with below-median differences in test scores. For the Method 2 sample, the impact of winning versus losing the lottery on "serious" suspensions is not statistically significant, both overall and for the above- and below-median "treatment" students. Taken together, we do find some evidence of an overall decrease in the probability of "serious", but not "mild" suspensions, for lottery winners versus lottery losers.

The final row presents results for standardized test scores. The students' test scores are the average of the math and reading scores for the North Carolina End of Grade tests, standardized by grade level. For the students in the Method 1 sample, on average, winning versus losing the NCLB lottery had a positive but insignificant effect on student test scores. For the students in the Method 2 sample, the overall effect of winning versus losing the lottery is also insignificant, but larger in magnitude than in the Method 1 sample. However, across both samples, for the students who choose schools with substantially higher test scores than their current NCLB school, there is a statistically significant and large effect of winning versus losing the lottery on test scores. Across both samples, students who won versus lost these lotteries experienced a gain of approximately 0.17 to 0.19 standard deviations on their exams. This is a substantial improvement in cognitive outcomes, equivalent to approximately 5-6 percentile rank points on the End of Grade exams. For both samples, students applying to schools that were only slightly better than their current NCLB schools did not experience gains on test scores. The point estimate is insignificant for the two samples, slightly negative in the case of the Method 1 sample and slightly positive in the case of the Method 2 sample.

6. Conclusion

The school choice component of NCLB is aimed at allowing parents of children in under-performing schools the opportunity to choose to send their children to schools with higher academic achievement. The immediate goal is to allow these students the opportunity to achieve

higher academic outcomes. This paper examined the introduction of NCLB in CMS to determine if NCLB had an impact on the school choices parents made and if those choices led to gains in academic outcomes.

Using detailed school choice data from CMS, we are able to compare the choices parents made prior to NCLB notification with the choices they made after receiving notification. We find that NCLB notification led to an overall small but significant change in the choice behavior of parents. However, approximately 16% of parents who received notification responded by choosing schools with test scores that were on average 1 standard deviation higher than the schools they chose to attend just a few months earlier. Thus, the NCLB notification with the required simplified information on academic performance of schools in the district facilitated the choice of a higher-performing alternative school for a significant fraction of parents. The NCLB notification increased the fraction of parents choosing to attend a school other than their NCLB school by more than 100%.

We then use the lottery admissions of students to schools to determine if the choices caused by NCLB notification resulted in higher academic achievement. We find some evidence that winning admission to a chosen school leads to a significant decline in serious suspension rates but no significant average gain in standardized test scores. We do find some evidence that students who apply to schools that are substantially higher-scoring than their current NCLB school do experience significant improvements in test scores. Among students with above-median differences between the chosen school's average test score and the score at their current NCLB school, admission to the chosen school increased test scores by 0.17 – 0.19 student-level standard deviations.

However, our analysis shows that availability of and proximity to high-performing schools is a key factor in determining the probability of responding to NCLB notification by choosing an alternative school, the average test score at the school chosen, and hence the estimated test score gain from attending the chosen school. Our results suggest that the availability of proximate and high-scoring school alternatives is an important factor in determining the degree to which the NCLB choice component can be successful at both allowing access through choice to alternative

schools and increasing academic outcomes for the students exercising choice. School districts with large geographic densities of low-performing schools may therefore face more difficulty in experiencing positive effects on student choice and subsequent outcomes as a result of the NCLB school choice provision.

REFERENCES

Carnoy, Martin, and Susanna Loeb. (2002). Does external accountability affect student outcomes? A cross-state analysis. *Educational Evaluation and Policy Analysis*, 24(4), 305-331.

Chakrabarti, Rajashri. (2005). Do public schools facing vouchers behave strategically? Evidence from Florida. *Manuscript, Harvard University*.

Clark, Melissa A. (2003). *Education reform, redistribution, and student achievement: Evidence from the Kentucky Education Reform Act*. Princeton, NJ: Mathematica Policy Research, Inc.

Cullen, Julie Berry, Brian A. Jacob, and Steven Levitt. (2006). The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica*, 74(5), 1991-1230.

Cullen, Julie Berry, and Randall Reback. (2006). Tinkering toward accolades: School gaming under a performance accountability system. *NBER Working Paper No. 12286*.

Figlio, David N., and Cecilia Elena Rouse. (2006). Do accountability and voucher threats improve low-performing schools? *Journal of Public Economics*, 90(1-2), 239-255.

Figlio, David N., and Lawrence S. Getzler. (2002). Accountability, ability and disability: Gaming the system. *NBER Working Paper No. 9307*.

Greene, Jay P. (2001). *An evaluation of the Florida A-Plus accountability and school choice program*. New York: Manhattan Institute for Policy Research.

Greene, Jay P., Paul E. Peterson, and Jiangtao Du. (1997). *The effectiveness of school choice: The Milwaukee experiment*. Cambridge, MA: Program on Education Policy and Governance, Harvard University.

Hanushek, Eric A., and Margaret E. Raymond. (2004). The effect of school accountability systems on the level and distribution of student achievement. *Journal of the European Economic Association*, 2(2-3), 406-415.

Hanushek, Eric A., and Margaret E. Raymond. (2005). Does school accountability lead to improved student performance? *Journal of Policy Analysis and Management*, 24(2), 297-327.

Hastings, Justine S., Richard Van Weelden, and Jeffrey M. Weinstein. (2007). Preferences, information, and parental choice behavior in public school choice. *Manuscript, Yale University*.

Hastings, Justine S., Thomas J. Kane, and Douglas O. Staiger. (2006a). Parental preferences and school competition: Evidence from a public school choice program. *NBER Working Paper No. 11805*. May 2006 version available at: www.econ.yale.edu/~jh529/Hastings_Kane_Staiger_QualityCompetition.pdf.

Hastings, Justine S., Thomas J. Kane, and Douglas O. Staiger. (2006b). Preferences and heterogeneous treatment effects in a public school choice lottery. *NBER Working Paper No. 12145*. June 2006 version available at: www.econ.yale.edu/~jh529/Hastings_Kane_Staiger_Preferences&SchoolChoice.pdf.

Jacob, Brian A. (2005). Accountability, incentives and behavior: The impact of high-stakes testing in the Chicago Public Schools. *Journal of Public Economics*, 89(5-6), 761-796.

Krueger, Alan B., and Pei Zhu. (2004). Another look at the New York City school voucher experiment. *American Behavioral Scientist*, 47(5), 658-698.

Mayer, Daniel P., Paul E. Peterson, David E. Myers, Christina Clark Tuttle, and William G. Howell. (2002). *School choice in New York City after three years: An evaluation of the School Choice Scholarships Program*. Princeton, NJ: Mathematica Policy Research, Inc.

North Carolina State Board of Education. (2005). *Consolidated state application accountability workbook*. Available at http://www.ncpublicschools.org/docs/nclb/federal/consolidated/20050630workbook.pdf.

Peterson, Paul. E., David Myers, and William G. Howell. (1998). *An evaluation of the New York City School Choice Scholarship Program: The first year*. Cambridge, MA: Mathematica Policy Research and the Harvard Program on Education Policy and Governance.

Rouse, Cecilia Elena. (1998). Private school vouchers and student achievement: An evaluation of the Milwaukee Parental Choice Program. *Quarterly Journal of Economics*, 113(2), 553-602.

U.S. Census Bureau. *Census 2000, Summary File 3*; using American FactFinder. Retrieved January 10, 2005 from http://factfinder.census.gov.

U.S. Department of Education. (2004). *Public school choice*. Available at http://www.ed.gov/policy/elsec/guid/schoolchoiceguid.pdf.

West, Martin R., and Paul E. Peterson. (2006). The efficacy of choice threats within school accountability systems: Results from legislatively induced experiments. *Economic Journal*, 116(510), C46-C62.

Witte, John F. (1997). School choice and student performance in public schools. In Paul Thurston (Ed.), *Advances in Educational Administration*. Raleigh, NC: JAI Press.

Witte, John F., Troy D. Sterr, and Christopher A. Thorn. (1995). *Fifth-year report: Milwaukee Parental Choice Program*. Madison, Wisconsin: University of Wisconsin.

Table I: Summary Statistics for NCLB and Non-NCLB Schools

Variable	All Schools (N = 117)	Non-NCLB Schools (N = 101)	NCLB Schools (N = 16)
School characteristics			
Average Test Score	-0.069	0.026	-0.672
	(0.457)	(0.414)	(0.163)
Has Academic Magnet	0.350	0.356	0.313
	(0.479)	(0.481)	(0.479)
% Lunch Recipients	52.977	47.261	89.061
	(28.089)	(25.853)	(6.060)
% Black	46.547	41.645	77.489
	(24.666)	(22.367)	(13.670)
Average Income†	52,334	55,551	32,022
	(17,330)	(16,361)	(5,307)
% Capacity	104.470	105.700	96.699
	(15.612)	(15.078)	(17.164)
% Students With at Least One Suspension	13.772	11.443	28.476
	(12.690)	(10.232)	(16.740)
% AYP Requirements Met	95.841	98.003	82.194
	(7.888)	(3.798)	(12.444)
Number of Students	708.145	738.228	518.250
	(294.770)	(299.504)	(171.373)

Notes: Data are from the 2003-2004 school year. Standard deviations are in parentheses. All Schools and Non-NCLB Schools do not include high schools or alternative schools since every NCLB school is an elementary or middle school.

†Income for a student is calculated as the median block-group household income level from the 2000 Census for households of a student's own race for the block group that the student lives in. We average this over students at a school to get school average income.

Table II: Cross Tabulation of Parents Who Chose Non-NCLB Schools in Spring and July

	Did Not Choose NCLB School First in July	Chose NCLB School First in July	Total
Did Not Choose NCLB School First in Spring	227 (30.31%)	522 (69.69%)	749 (100%)
Chose NCLB School First in Spring	865 (14.55%)	5,081 (85.45%)	5,946 (100%)
Total	1,092 (16.31%)	5,603 (83.69%)	6,695 (100%)

Note: Numbers in parentheses give the relative frequency of each cell in its row.

Table III: Regression Adjusted Treatment Effect

Variable	Spring Mean	July Mean	Treatment Effect
Chose NCLB School First	0.888	0.837	-0.051*** (0.006)
Average Test Score at First-Choice School	-0.580	-0.533	0.047*** (0.004)
% Black at First-Choice School	77.069	74.256	-2.814*** (0.204)
% Lunch Recipients at First-Choice School	83.863	80.279	-3.584*** (0.241)
Number of Observations	6,695	6,695	13,990

Notes: Data are from the 2003-2004 school year. Treatment Effect reports the coefficient on an indicator for July 2004 from separate regressions with each variable in the first column as the dependent variable, controlling for student fixed effects. Standard errors adjust for clustering at the student level. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table IV: Differences in Choice Characteristics for Students Who Chose Out in July

Variable	All Students	Black	Not Black
Average Test Score at First-Choice School			
Spring 2004	-0.502	-0.513	-0.421
July 2004	-0.017	-0.034	0.108
Average Test Score of Schools Within 5 Miles			
Spring 2004	-0.322	-0.328	-0.277
July 2004	-0.247	-0.253	-0.206
% Black at First-Choice School			
Spring 2004	75.557	76.588	67.857
July 2004	49.351	50.278	42.426
% Lunch Recipients at First-Choice School			
Spring 2004	78.565	79.128	74.358
July 2004	43.350	43.867	39.495
Number of Students	1,092	963	129

Note: Data are from the 2003-2004 school year.

Table V: Comparison of Student Characteristics: Chose Out Versus Chose NCLB School

Variable	Did Not Choose NCLB School First in July	Chose NCLB School First in July	Adjusted Difference
Did Not Choose NCLB School First in Spring	0.208	0.093	0.105*** (0.018)
Black	0.882	0.832	0.045* (0.025)
Lunch Recipient	0.879	0.861	0.019 (0.014)
Female	0.493	0.498	-0.006 (0.012)
Test Score	-0.554	-0.571	0.031 (0.021)
Income	32,134	32,043	278.633 (426.203)
Number of Unexcused Absences	5.168	5.760	-0.620** (0.240)
Number of Suspensions	2.896	2.162	0.313** (0.137)
Rising Grader	0.316	0.228	0.064*** (0.019)
In Magnet Program	0.154	0.115	0.018* (0.009)
Distance to NCLB School	3.497	2.859	0.405*** (0.119)
Average Distance to Schools Within 5 Miles in July Choice Set	3.318	3.313	0.014 (0.014)
Average Test Score at NCLB School	-0.632	-0.633	-- --
Average Test Score of Schools Within 5 Miles in July Choice Set	-0.247	-0.266	0.017** (0.008)
Number of Students†	1,092	5,603	6,695

Notes: Data are from the 2003-2004 school year. Adjusted difference reports the coefficient on whether the student did not choose her NCLB school first in July from separate regressions with each variable in the first column as the dependent variable, controlling for NCLB school fixed effects. Standard errors adjust for clustering at the level of the NCLB school. Asterisks indicate significance (*=.10, **=.05, ***=.01).

†Student test score is the average of reading and math scores on the North Carolina End of Grade exams. Only students in grades three through eight take these exams, so this variable is based on 818 students who did not choose their NCLB school first in July and 3,828 who chose their NCLB school first in July.

Table VI: Conditional Logit of Probability a Family Chose Out in July

Variable	Coefficient	Standard Error	P-value
Did Not Choose NCLB School First in Spring	0.759***	0.124	0.000
Black	0.448*	0.248	0.071
Lunch Recipient	0.131	0.162	0.419
Female	-0.031	0.071	0.660
Test Score	0.106**	0.049	0.030
Demeaned Income	-0.002	0.002	0.383
Number of Unexcused Absences	-0.019***	0.007	0.006
Number of Suspensions	0.014***	0.004	0.000
Rising Grader	0.203**	0.087	0.020
In Magnet Program	0.142**	0.062	0.022
Distance to NCLB School	0.019	0.012	0.120
Average Distance to Schools Within 5 Miles in July Choice Set	0.009	0.029	0.748
Average Test Score of Schools Within 5 Miles in July Choice Set	0.608***	0.208	0.003
Mean of Dependent Variable	0.176		
Number of Students	4,646		
Log Pseudolikelihood	-2,029.688		

Notes: Data are from the 2003-2004 school year. Conditional (fixed-effects) logit estimation with NCLB school fixed effects. Standard errors adjust for clustering at the level of the NCLB school. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table VII: Determinants of Test Score at Chosen School in July

Dependent Variable: Test Score at July First-Choice School	Coefficient	Standard Error	P-value
Did Not Choose NCLB School First in Spring	0.103***	0.021	0.000
Black	-0.082**	0.028	0.011
Lunch Recipient	-0.069***	0.022	0.006
Female	-0.027	0.017	0.136
Test Score	0.025	0.019	0.208
Demeaned Income	-0.001	0.001	0.439
Number of Unexcused Absences	-0.003*	0.002	0.067
Number of Suspensions	0.001	0.001	0.376
Rising Grader	-0.052	0.038	0.189
In Magnet Program	0.077***	0.016	0.000
Distance to NCLB School	-0.015**	0.005	0.016
Average Distance to Schools Within 5 Miles in July Choice Set	-0.010	0.014	0.468
Average Test Score of Schools Within 5 Miles in July Choice Set	0.229***	0.052	0.001
Mean of Dependent Variable	0.019		
Number of Students	818		
Adjusted R-squared	0.1354		

Notes: Data are from the 2003-2004 school year. Linear regression estimation with NCLB school fixed effects. Standard errors adjust for clustering at the level of the NCLB school. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table VIII: Baseline Characteristics by Lottery Winners and Lottery Losers

	Method 1 Sample			Method 2 Sample		
Baseline Characteristic	**Won Lottery**	**Lost Lottery**	**Adjusted Difference**	**Won Lottery**	**Lost Lottery**	**Adjusted Difference**
In Priority Group for First Choice	0.713	0.804	-0.079 (0.073)	--	--	-- --
Black	0.878	0.875	0.049 (0.065)	0.902	0.871	0.105** (0.049)
Lunch Recipient	0.965	0.973	-0.013 (0.013)	0.925	0.853	0.181*** (0.041)
Female	0.461	0.518	0.015 (0.084)	0.481	0.526	-0.067 (0.078)
Income	30,207	28,658	1,892.770 (2,029.906)	30,846	31,351	-3,961.591* (2,055.373)
Number of Unexcused Absences	5.009	5.875	-1.634 (1.140)	5.762	4.920	0.371 (0.680)
Number of Suspensions	3.626	3.732	0.863 (1.302)	4.743	3.540	2.732** (1.037)
Test Score	-0.796	-0.886	-0.066 (0.061)	-0.874	-0.366	-0.990*** (0.056)
Number of Students†	115	112	227	214	348	562

Notes: Adjusted Difference reports the coefficient on whether the student won the lottery from separate regressions with each variable in the first column as the dependent variable. Lottery-block fixed effects are included for specifications using Method 1 Sample, and grade and first-choice combination fixed effects are included for specifications using Method 2 Sample. Standard errors adjust for clustering at the level of the fixed effects. Asterisks indicate significance (*=.10, **=.05, ***=.01).

†Test score is the average of reading and math scores on the North Carolina End of Grade exams. Only students in grades three through eight take these exams, so this variable is based on 89 students who won the lottery and 91 students who lost the lottery in the Method 1 Sample, and 182 students who won the lottery and 317 students who lost the lottery in the Method 2 Sample.

Table IX: The Impact of Winning Versus Losing the Lottery on Attrition

	Method 1 Sample		Method 2 Sample	
	Mean	**Regression Adjusted Difference: Lottery Winners Versus Lottery Losers**	**Mean**	**Regression Adjusted Difference: Lottery Winners Versus Lottery Losers**
Not Present in CMS at Beginning or End of 2004-2005 School Year	0.062	0.057* (0.032)	0.050	0.012 (0.016)
Number of Students	227	227	562	562

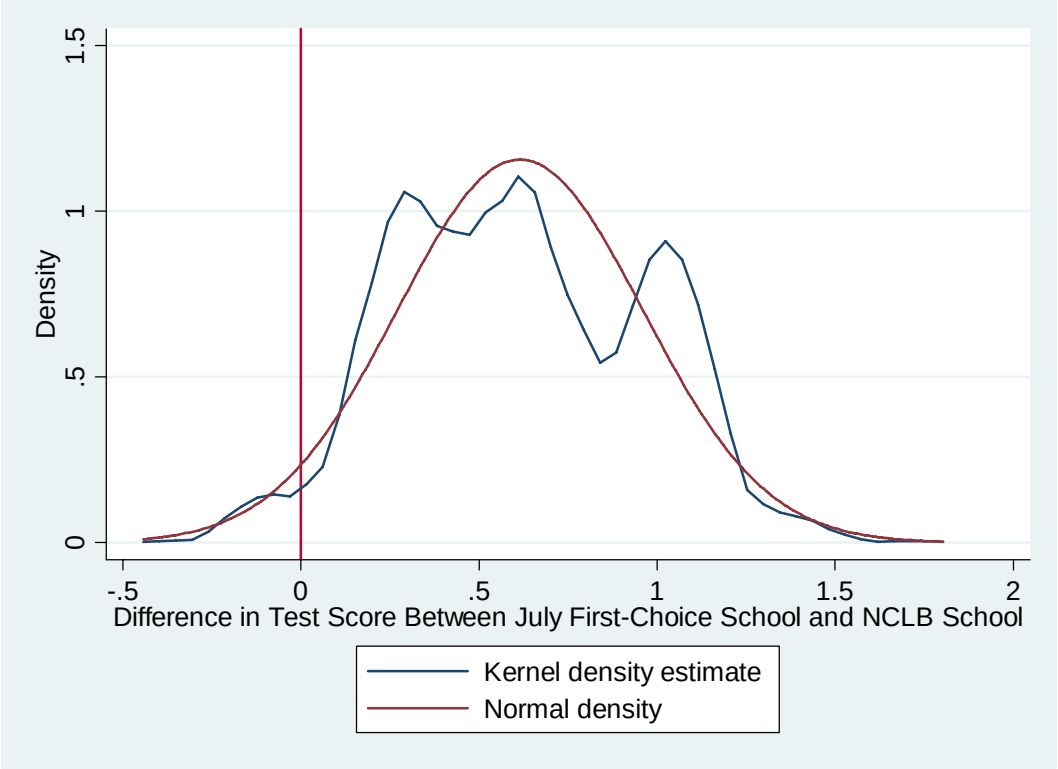
Notes: Regression Adjusted Difference is from a regression of an indicator of attrition on whether the student won the lottery, controlling for the following baseline covariates: black, lunch recipient, female, demeaned income, an indicator for greater than or equal to five unexcused absences, an indicator for greater than or equal to one unexcused absence, an indicator for greater than or equal to five suspensions, an indicator for greater than or equal to one suspension, total number of unexcused absences, and total number of suspensions. Lottery-block fixed effects are included for specifications using Method 1 Sample, and grade and first-choice combination fixed effects are included for specifications using Method 2 Sample. Standard errors adjust for clustering at the level of the fixed effects. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table X: The Impact of Winning Versus Losing the Lottery on Outcomes

	Method 1 Sample				Method 2 Sample			
2004-2005 School Year Outcome	**Mean**	**All Students**	**Above-Median Score Difference**	**Below-Median Score Difference**	**Mean**	**All Students**	**Above-Median Score Difference**	**Below-Median Score Difference**
Average Test Score at School Attended	-0.240	0.324***	0.375***	0.166	-0.284	0.146*	0.256***	-0.032
		(0.080)	(0.101)	(0.100)		(0.079)	(0.088)	(0.056)
≥ 5 Unexcused Absences	0.427	-0.154	-0.205	-0.101	0.410	0.020	0.002	0.051
		(0.108)	(0.123)	(0.179)		(0.057)	(0.076)	(0.054)
≥ 1 Unexcused Absence	0.845	-0.086	-0.174**	0.051	0.822	-0.001	-0.065	0.126**
		(0.077)	(0.072)	(0.102)		(0.068)	(0.088)	(0.049)
≥ 5 Suspensions	0.258	-0.099*	-0.125	-0.060	0.288	-0.018	-0.023	-0.018
		(0.050)	(0.097)	(0.084)		(0.040)	(0.056)	(0.046)
≥ 1 Suspension	0.451	-0.076	-0.063	-0.078	0.472	0.006	-0.026	0.095
		(0.076)	(0.127)	(0.073)		(0.048)	(0.065)	(0.060)
Number of Students	213	213	107	106	534	534	271	263
Student's Test Score	-0.805	0.046	0.169**	-0.070	-0.565	0.114	0.189*	0.047
		(0.081)	(0.076)	(0.139)		(0.076)	(0.104)	(0.076)
Number of Students	161	161	82	79	451	451	213	238

Notes: Each column reports the coefficient on whether the student won the lottery from separate regressions with each variable in the first column as the dependent variable, controlling for the following baseline covariates: black, lunch recipient, female, demeaned income, an indicator for greater than or equal to five unexcused absences, an indicator for greater than or equal to one unexcused absence, an indicator for greater than or equal to five suspensions, an indicator for greater than or equal to one suspension, total number of unexcused absences, and total number of suspensions. The Student's Test Score specification includes baseline student test score. Lottery-block fixed effects are included for specifications using Method 1 Sample, and grade and first-choice combination fixed effects are included for specifications using Method 2 Sample. Standard errors adjust for clustering at the level of the fixed effects. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Figure I: Kernel Density Estimate of Difference in Test Score Between July First-Choice School and NCLB School for Families Who Did Not Choose NCLB School First in July (N = 1,092)



Note: We use the Epanechnikov kernel and the optimal width as computed by default in Stata.