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INFORMATION, SCHOOL CHOICE, AND ACADEMIC ACHIEVEMENT: EVIDENCE FROM TWO EXPERIMENTS

Justine S. Hastings
Jeffrey M. Weinstein

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Information, School Choice, and Academic Achievement: Evidence from Two Experiments
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ABSTRACT

There is growing empirical evidence that low-income parents place lower weights on academics when choosing schools, implying that school choice plans may have the smallest impact on the choices of the families they are targeting. This paper uses a natural experiment generated by the 2004 implementation of the No Child Left Behind Act in the Charlotte-Mecklenburg Public School District (CMS) and a field experiment we designed and implemented as part of the district's 2006 school choice plan to examine how transparent information on school-level academic performance affects the test scores of the schools parents choose and the subsequent impact on their children's academic outcomes. We find in both cases that providing parents with transparent information on the academic achievement at schools with their school choice forms results in significantly more parents choosing substantially higher-performing schools. We then use instrumental variables approaches, exploiting random variation generated by each experiment in the test score of the school attended to estimate the impact of attending a higher-scoring school on student academic outcomes. We find that attending higher-performing schools results in significant increases in their children's standardized test scores at the end of the first year. If the results we find represent permanent increases in student-level test scores, they suggest a small policy change that lowers information or decision making costs for these parents had a substantial monetary impact on their children's lifetime earnings, adding to growing evidence that small changes in information can greatly affect choices, program participation, and outcomes.

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

School choice plans are intended to improve both education quality and equity by providing incentives for schools to compete on academic achievement and by allowing broader access to quality public schools. Several urban public school districts are currently experimenting with public school choice plans, and the federal No Child Left Behind Act (NCLB) of 2001 includes a choice provision allowing students in failing schools to choose to attend non-failing schools outside of their neighborhood. The goal is to allow disadvantaged students to benefit academically from attending higher-performing schools and to increase pressure on failing schools to improve through the threat of losing students.

However, the incentives and outcomes generated by school choice depend to a large degree on parents' choice behavior. There is growing empirical evidence that parental preferences are very heterogeneous and that low-income parents place lower values on academic characteristics when choosing schools, implying that school choice plans may have the smallest impact on the choices of the families they are targeting (Fossey (1994), Armor and Peiser (1998), Schneider and Buckley (2002), Jacob and Lefgren (forthcoming)). In particular, Hastings, Kane, and Staiger (2006a, b) use data from the introductory year of school choice in the Charlotte-Mecklenburg Public School District (CMS) and show that low-income families place lower implicit weights on academics when choosing schools, which negatively impacts both marginal student outcomes and the pressure for school quality improvement in CMS.

It may be the case that, all else equal, low-income families place lower weights on school test scores because they rationally expect lower returns to education for their children. Alternatively, these families may face higher information or decision-making costs. They may place a high value on academic outcomes but find it more costly to act on those preferences, leading to a lower expressed preference for school test scores. Recent empirical research has shown that low-income households may have higher decision-making costs that may lead them to make "suboptimal" decisions when faced with complex decisions, such as picking the best school or investing in financial assets (Caskey (1994), Bertrand, Mullainathan, and Shafir (2006), Duflo et

al. (2006), McFadden (2006), Winter et al. (2006)). This paper uses a natural experiment and a field experiment in the Charlotte-Mecklenburg Public School District (CMS) to examine the degree to which transparent information on school-level academic performance impacts the schools that parents choose for their children, the importance they place on academic achievement when choosing schools, and the impact of having their children attend higher-performing schools on subsequent academic outcomes. We find in both cases that providing parents with transparent information on the academic achievement at schools with their school choice forms results in significantly more parents' choosing substantially higher-performing schools. We then provide evidence that attending high-performing schools results in significant increases in their children's standardized test scores at the end of the first year.

The natural experiment comes from the implementation of NCLB in the CMS school choice plan in the summer of 2004. Each spring since 2002 parents could participate in the CMS school choice plan by submitting their top three school choices for their children for the next school year. In the summer of 2004, CMS identified which schools would be classified as "Title I Improving" and therefore be subject to regulation under NCLB (an NCLB school).[1] The regulation required that parents of students at these schools be given the option to choose to attend another school and that they be given information on the academic achievement at the other schools in the district.[2] In order to comply with this regulation, CMS re-sent choice forms to each family with a child enrolled at a NCLB school, along with information on the percent of students who achieved grade level in reading and math (percent proficient) for every school in the district (a three-page spreadsheet printout), sorted alphabetically by school name. We compare the choices these families submitted in the spring without the NCLB information to the choices they made that same summer with the NCLB information and find that NCLB notification increased the fraction of parents choosing an alternative school by five percentage

[1] This was the first year that any school could be categorized as "Title I improving" under NCLB in CMS. 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 I status is determined in CMS in Section 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 CMS in Section 2. Under NCLB, parents cannot choose another Title I Improving school for their children. 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.

[2] Each state completed a Consolidated State Application Accountability Workbook for NCLB, and this particular implementation of NCLB requirements is part of the North Carolina state implementation. Other states may have varied in the specifics of how they would implement the broad goals of the regulation.

points, off a base of 11 percent and that these parents on average chose schools with 0.5 student-level standard deviation higher test scores than the schools they chose for their children just a few months earlier. This translates into approximately one standard deviation in the distribution of scores across schools in the district. [3]

To further examine the effects of transparent information on school choice, we conducted a field experiment in CMS for the 2006-2007 school choice process. CMS allowed us to provide information sheets with parents' school choice application forms to students in randomly selected schools that serve primarily low- to middle-income neighborhoods. These sheets were sent out with the CMS choice forms in the spring of 2006. The sheets provided either clear statistics on academic achievement for each school in the child's choice set or information on academic achievement coupled with estimated odds of admission. The information presented was simpler than the NCLB information in that it appeared in a one-page format, was sorted by the academic ranking of schools (instead of alphabetically), and contained only information on the schools relevant for the child's choice (e.g., only elementary schools for elementary school children). The simplified information was given to students at NCLB schools, as well as at non-NCLB schools (where parents do not receive the NCLB-mandated information with their choice forms).

We find that, among students at non-NCLB schools, receiving test score information led to a significant 6.6 percentage point increase in the probability of choosing a non-guaranteed school and a 0.10 student-level standard deviation increase in the average test score of the first-choice school relative to the guaranteed school option. Compared to receiving test score information alone, receiving information on admission odds and test scores had a slightly weaker impact on the probability of choosing a non-guaranteed school but a very similar average impact on the test score at the first-choice school. These results are similar in magnitude to those found in the 2004 NCLB natural experiment. However, the simplified information we provided had no significant impact on choice behavior for students at NCLB schools, where all families received the NCLB-

[3] 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.

mandated information, implying that our information sheets had no additional impact on the choices of parents at these schools.[4]

Together these findings suggest that parental choices respond more to receiving transparent information at the time of choice than to the format that the information appears in. In addition, in both experiments we find evidence of heterogeneity in individual choice response to receiving information. A large fraction of parents do not change their choices in response to information; however, a significant number change their choices drastically, choosing some of the highest-performing schools in the district. Among the factors that influence response rates are race, as African Americans are more likely to respond to information, and proximity to high-scoring alternative schools, since distance is an important component of choice.

After identifying the impact of information on the test scores of the schools parents choose, we turn to examine if the changed choices resulted in academic gains in the subsequent school year. We use instrumental variables approaches, exploiting random variation generated by each experiment in the test score of the school attended to estimate the impact of attending a higher-scoring school on student academic outcomes. In both experiments, we find large but marginally significant impacts of the test score of the school attended on own test scores. The point estimates imply gains in own test scores of 0.37 - 0.63 student-level standard deviations from attending a school with a one standard deviation higher average test score. While prior studies of the marginal impact of school choice have found little evidence of average test score gains as a result of attending a first-choice school (Cullen, Jacob, and Levitt (2006), Hastings, Kane, and Staiger (2006b), Cullen and Jacob (2007)), they have not had the opportunity to examine a setting in which a density of parents make very directed choices towards schools with substantially higher test scores. The magnitude of the results are similar to those found for the

[4] Hastings, Van Weelden, and Weinstein (2007) show, using a conditional logit model, that receiving simplified information on test scores doubled estimated parental preferences for test scores in the non-NCLB sample of students, effectively erasing the gap in preferences for academics between high- and low-income families. This effect is large in magnitude and only declines at relatively high-income levels. They also find evidence that the effect of receiving information on odds of admission with test scores increased preferences for test scores more among lower-income households than among higher-income households relative to receiving information on test scores alone, suggesting that lower-income households responded to odds information by choosing higher-performing schools, while higher-income households responded by choosing slightly lower-performing schools with better odds of admission.

upper tail of students choosing schools for academics in Hastings, Kane, and Staiger (2006b), and equivalent to value-added measures of the impact of a top-quartile teacher versus a bottom-quartile teacher on student test scores (Rockoff (2004), Rivkin, Hanushek, and Kain (2005), Kane, Rockoff, and Staiger (2006), Aaronson, Barrow, and Sander (2007)).

This paper is organized as follows. Section 2 explains the CMS school choice plan. Section 3 presents results from the NCLB natural experiment, and Section 4 discusses the findings from the field experiment. Section 5 analyzes the impacts on academic outcomes from each of the experiments, and Section 6 concludes.

2. CMS School Choice Plan

2.1. History of the Choice Plan

Before the introduction of a school choice plan in the fall of 2002, the Charlotte-Mecklenburg Public School District (CMS) operated under a racial desegregation order for three decades. For the 2002-2003 school year, CMS moved to a district-wide school choice plan in response to a court order to cease busing for racial integration. 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. This school was typically one of the closest schools to her, and she was guaranteed admission to this school. Admission to non-guaranteed schools was granted based on a lottery.

In the initial implementation of the school choice program, CMS underwent a large redistricting of home school assignments; approximately 50 percent of parcels lost property rights to the school they had rights to under busing. In the first year of choice, the district required every parent to submit a choice form, and it achieved a 95 percent compliance rate. In each year after the initial choice year, only parents of students in rising grades (K, 6^{th}, 9^{th}), new students to the district, students affected by the opening of new schools, and students who wished to change schools were required to submit choice forms.

Each year a significant fraction of schools in the district is oversubscribed.[5] The lottery process for assigning students to oversubscribed schools has continued since the introduction of school choice in 2002. Under the lottery system, students are first assigned to priority groups by school and grade. The priority groups have varied from year to year but generally have given priority to higher-poverty and lower-performing students who choose lower-poverty, higher-performing schools.[6] Within each priority group, admission is determined by randomly assigned lottery number alone.

2.2. Information Provided to Parents

In order for parents to determine which schools to choose, CMS provided several resources. First, each year CMS provided a school choice guide that 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.[7] The bulk of the choice book was devoted to written descriptions of each school and program, from pre-school 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 included.

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 Application Center emphasized 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,

[5] In the first year, approximately one third of the schools were oversubscribed due to CMS's commitment to expand capacities at schools in an effort to give parents one of their top three choices. In subsequent years, two thirds of the schools have been oversubscribed as capacities were fixed.

[6] The priority group definitions were initially based on free- and reduced-lunch status and the concentration of free- and reduced-lunch recipients at a school. Since the use of lunch recipient status has recently received negative attention, CMS moved to use test score performance as a priority instead. Students performing below average on end-of-year exams are given priority for admission to schools performing at or above the district average on standardized exams.

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

since what a "good" school is depends on each individual child.[8] It is important to note that this advice may be correct, as the relationship between school average test scores and student achievement has not been strongly established. However, it does suggest to parents that identifying a "good" school takes a substantial and potentially daunting investment of time and effort.

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. 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 might involve a significant web search and comparison.

2.3. Information on Odds of Admission

Although the fraction of parents choosing non-guaranteed school options for their children has remained fairly steady across the years, the number of seats available for choice has decreased significantly at many schools. After the first year of choice, when CMS stopped expanding seats at high-demand schools, some schools effectively stopped admitting any students through the choice lottery. Parents expressed frustration at not gaining admission to any of their chosen schools.[9] In order to help guide parental choices and minimize frustration, CMS added to the choice book a description of schools (again pooled for campuses, not shown for individual programs) as "Low", "Medium", or "High" capacity utilization. The purpose was to categorize "schools according to their ability to provide space for non-guaranteed seats".[10] Thus, a "High" rating for capacity utilization would imply a low probability of admission through the choice plan. This page appeared at the back of the 2004-2005 CMS school choice book. The district did not experience a decline in the number of parents choosing over-subscribed schools for this choice year. For the 2005-2006 school year choice process, the district moved the capacity utilization page to page 14 in an attempt to make it more salient to parents; however, they still

[8] Information from interviews and conversations with Family Application Center staff.
[9] Hastings, Kane, Staiger, and Weinstein (2007) show that white parents who lost the lottery were significantly more likely to vote in school board elections.
[10] CMS 2004-05 Student Assignment Application Guide. (Page 90).

did not experience a decline in the choice of over-subscribed schools. In the absence of easy information on academic outcomes, parents may have interpreted low odds of admission as signals of high quality, which may have led to an increase in demand at oversubscribed schools.

2.4. Integration of NCLB into the CMS School Choice Plan

NCLB legislation was introduced in January of 2002. Beginning in the summer of 2004, CMS implemented NCLB in accordance with North Carolina state regulation that in turn was based on federal requirements. Title I schools face sanctions under NCLB if they fail to make Adequate Yearly Progress (AYP) for two years in a row. As defined by CMS, a school is a Title I school (receives federal Title I funds) if 75 percent 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.[11] If just one target was missed for one subgroup, then the school failed to make AYP.[12] Thus if a school *is both Title 1 for two years in a row and also fails to make AYP for two years in a row* it is classified as Title 1 Improving and thus subject to NCLB regulation.[13]

In each year since 2004, 16-21 schools have satisfied both constraints and entered into regulation under NCLB. The regulation implies that parents need 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) is required to supply with this notification information on the academic

[11] 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.

[12] 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 percent proficiency by the end of the 2013-2014 school year; this requirement can also be satisfied for a subgroup if its percent proficient falls within a 95 percent confidence interval for the target percent proficient), a minimum participation rate in each of the exams for each subgroup (95 percent in each year or averaged over the prior two or three years), and attendance rates for elementary and middle school students or graduation rates for high school students (an increase of 0.1 percentage points from the previous year or anything over 90 percent). For information on other means by which subgroups can make AYP, please see the Consolidated State Application Accountability Workbook for North Carolina (2005) which provides federal NCLB guidelines along with North Carolina's implementation of these guidelines.

[13] Thirteen schools were Title I in both the 2002-2003 and 2003-2004 school years but made AYP in at least one of those years and hence were not subject to regulation under NCLB. Twenty-one schools did not make AYP in any of the two school years but were not Title I (each school that was Title I in the 2002-2003 school year was also Title I in the 2003-2004 school year, and vice versa) and thus were also not subject to regulation under NCLB.

achievement of the schools that parents could select.[14] CMS provides a three-page spreadsheet printout, sorted by school name, with the percent proficient for every school in the district, as well as a list of Title I Improving schools, since students exercising choice under NCLB are not allowed to choose to attend another Title I Improving school.

Thus the NCLB legislation provided statistics to parents on the academic achievement at their school and at every other school in the district, as well as notification that their school had failed to meet AYP. Importantly, this information is only provided to parents of students slated to attend a NCLB school in the subsequent school year. Figure I presents a timeline of events for the reader to refer to throughout the discussion of the two experiments and results.

3. NCLB – A Natural Experiment in Transparent Information on Academics

3.1. Data and Research Design

At the end of the 2003-2004 school year, CMS compiled the test score outcomes for schools in the district and determined that 16 schools, 10 elementary and six middle schools (no high schools), were both Title I schools and had failed to make AYP for the prior two years. These schools were categorized as Title I Improving and entered regulation under NCLB. Because NCLB schools were identified in June 2004, at the end of the school year, CMS had to re-send choice forms in July, along with the NCLB notification to parents of students slated to attend NCLB schools in the fall. For families with children at these schools, we observe the choices they submitted in the spring of 2004 without the NCLB-mandated information and the choices they submitted in July of 2004 after receiving the information. Students of parents who chose alternative schools in July were then entered into a school choice lottery.

We have secure access to administrative data from CMS including choice form, demographic, and test score data 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, student demographics, and

[14] U.S. Department of Education. *Public school choice*. www.ed.gov/policy/elsec/guid/schoolchoiceguid.pdf. (Page 18).

student and school locations. 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 (NCLB) 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 NCLB because they either failed to make AYP both years but were not Title I (21 schools fall into this category) or because they were Title I but made AYP in at least one of the two years (13 schools fall into this category).[15] Because Title I status in CMS is defined as 75 percent or above free- or reduced-lunch concentration, NCLB schools have a substantially higher-than-average lunch-recipient rate. In addition, they have a higher proportion of African American students, a lower average neighborhood income level[16], and higher-than-average suspension rates.

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. Of 6,695 students in our sample who received NCLB notification, 1,149 responded by submitting a form in July.[17] 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 parents filled out a form in July and chose a school different than their current NCLB school first, and 5,603 parents either did not respond to NCLB notification or chose their NCLB first in the July lottery.

Given the number of responders alone, it appears that NCLB notification had a substantial impact on parental choice. It is important to note that students at NCLB schools are there mainly because their parents actively or passively (by default assignment) chose the NCLB school (despite the three-year-old school choice plan).[18] While this subgroup is disadvantaged relative

[15] Every school that was Title I in the 2002-2003 school year was also Title I in the 2003-2004 school year, and vice versa.

[16] Throughout this paper, income is measured as the median income in the 2000 Census for people of a student's own race living in a student's block group.

[17] We began with the 8,284 students who received NCLB notification, of which 1,363 responded by filling out a form in July. 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).

[18] 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. 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 percent versus 14.55 percent). Approximately two thirds of parents who

to the average family in the district, they are representative of families that many school choice plans, and NCLB in particular, are intended to help. Hence, understanding how transparent information affects their choices is important in and of itself. We will examine if these impacts hold more broadly using the 2006 field experiment.

3.2. The Effect of NCLB-Mandated Information on Parental Choice

Table II presents mean choice behavior for parents at NCLB schools before and after receiving NCLB notification. If we include all parents (those that chose their NCLB school in the spring and those that did not), we see that the fraction of parents that chose a school other than their NCLB school increased by 5.1 percentage points relative to a base of 11 percent after receiving NCLB notification. The average test score of the school chosen increased by a statistically significant 0.047 student-level standard deviations. If we consider only parents who chose their NCLB school in the spring, the fraction of parents choosing an alternative school after receiving the NCLB information increased from zero percent to 14.5 percent. The average test score of the school chosen increased by 0.088 student-level standard deviations.

The results shows that receiving the NCLB information made the average parent less likely to choose their NCLB school first, instead selecting schools with higher average test scores. Although the coefficients are statistically significant, they are small in magnitude.[19] 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 percent who submitted forms and chose a school other than their NCLB school first in July. Figure II 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. A small fraction of students chose schools that were close to or slightly worse performing than their NCLB school, while another minority of students

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did not choose their NCLB school first in the spring did not 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 parents who chose to take their children out of the NCLB school, we may in fact be underestimating the effect given the timing of the notification.

[19] About nine percent 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.

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.

This change in choice behavior was not mechanically generated by the fact that NCLB parents could not select another NCLB school in July. Table III shows the difference in the average test score at the first-choice school in the spring versus in July for parents that chose a non-NCLB school in July. It also shows how the average test score at schools within five miles changed due to the fact that the choice set was restricted to exclude NCLB schools. The average test score at local schools available for choice does increase due to the NCLB restriction but cannot account for the large change in choice behavior. These families were choosing schools with average test scores that were well below the local average in the spring (-0.502 vs. -0.322) and well above the local average in July (-0.017 vs. -0.247).

3.3. Which Parents Responded Most to NCLB-Mandated Information?

Table IV examines which types of students were more likely to respond to NCLB notification by choosing a better school, and of those students, which ones were more likely to choose higher-scoring schools. Table IV present regressions of the form:

$$Y_i = \alpha + X_i\beta + \delta_j + \varepsilon_i \qquad (1)$$

where Y_i is an indicator for whether the parent chose a non-NCLB school or the average test score at the first-choice school, X_i is a matrix of student characteristics, δ are guaranteed school choice fixed effects, and ε_i is a random error term that allows for clustering at the NCLB school program and grade level. Columns 1 and 2 present results for the probability of choosing an alternative to the NCLB school in July (choosing out) for the entire sample receiving choice forms in July, as well as for the sub-sample of recipients who had chosen their NCLB school in the spring, respectively. The results show that proximity to high-scoring schools significantly increases the probability of responding to the NCLB-mandated information by choosing an alternative school. In addition, African American parents were more likely to choose out in July. Interestingly, both high-scoring students and those with past suspensions were more likely to choose out in July. Hence both high- and low-performing students sought to attend alternative schools. Unexcused absences, on the other hand, decrease significantly the probability of

choosing out. In addition, the results in column 1 imply that parents who chose out in the spring were also significantly more likely to choose out in July, as we would expect.

Columns 3 and 4 examine the determinants of the test score of the first-choice school conditional on choosing out in July; that is, conditional on choosing out, which families responded by choosing high-scoring alternatives? Proximity to high-scoring schools is a significant determinant of parental choice; increasing both the probability of responding and the test score of the school chosen. Even though African Americans were more likely to respond to the NCLB-mandated information by choosing out, the chosen schools are lower scoring than average. The same holds true of free-and-reduced-lunch recipients. This result may reflect the trade-off that African American parents must make between average school scores and racial compositions.[20]

Overall, the NCLB-mandated information facilitated the choice of a higher-performing alternative school for a significant fraction of parents. Our analysis also 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 as well as the average test score at the school chosen. In order to examine if there are similar effects of simplified information on parental choices outside of the NCLB sub-sample, we conducted a field experiment as part of the 2006-2007 school choice plan. The next section examines the impact that these information forms had on parental choice.

4. A Field Experiment in the 2006-2007 School Choice Plan

4.1. Distribution and Design of Information

Working with CMS, we designed simplified information sheets to attach to parents' school choice forms for the 2006-2007 school choice round to further investigate the effect of simplified information on parental choice. The experiment was limited by the district in several important ways. First, students attending the same school and living in the same choice zone had to receive the same type of information. Hence, information was randomized at the school and choice-zone

[20] However, it is important in interpreting these results to remember that our sample is almost 90 percent lunch subsidy recipients and over 77 percent African American.

level (school-zone).[21] Second, the set of schools was restricted to NCLB schools and non-NCLB schools serving low- to middle-income neighborhoods, and we were asked to limit the number of forms provided to non-NCLB students to a few thousand. As in prior years, choice forms were provided to all students slated to attend NCLB schools (to comply with the choice requirements of the law), to rising grade students (going into K, 6th, or 9th grade in the next school year), and to students whose home-school assignments for the 2006-2007 school year were affected by the opening of new schools. Our simplified information sheet was attached to this choice form, so grade restrictions held as well.

In addition, we were restricted to providing information on test scores and odds of admission. After the first year of school choice, a significant number of schools in the district were over-subscribed, admitting few, if any, students each year. Despite this fact, demand for these schools had not declined, leading the district to limit the available schools for choice to those with a positive probability of admitting students in order to mitigate parental discontent.[22] We combined odds of admission with test scores to examine how, if at all, parents would react to clear information on admit chance. We randomly selected school-zones to receive either Score forms (test score information only) or Odds forms (test scores coupled with odds of admission) subject to the constraints listed above. The randomization was done separately for each of the segments of schools: pre-K (rising kindergarten), 5th graders (rising 6th), 8th graders (rising 9th), and NCLB students. There were 6,328 non-NCLB students in 46 school-zones (39 schools) who were part of the field experiment, and 10,134 NCLB students in 31 school-zones (19 schools) who were part of the experiment.[23]

[21] The district was split into 4 quadrants called "choice zones". In prior years, parents could choose from any school in the district, but their child would only receive free transportation to schools in their choice zone. For the 2006-2007 school year, CMS significantly redrew the boundaries of the choice zones so that each zone contained a range of possible schools given the new restricted choice set. Hence, it was often the case that students attending a current school lived in different 2006-2007 choice zones, even though they would have been in the same choice zone under the prior choice zone boundary definition.

[22] Recall from Section 2 that the school choice booklet did provide information on capacity utilization to give parents an idea of which schools were likely to offer many versus few seats for choice. Despite the change in the schools offered for choice in the 2006-2007 school choice plan, there were many school options with a wide range of academic performance for families to choose from. For further discussion of the 2006-2007 school choice plan as well as summary statistics on the choice offerings, please see Hastings, Van Weelden, and Weinstein (2007).

[23] Note that the number of NCLB schools increased since 2004; however, they were all still elementary and middle schools. There were 19 NCLB schools expected for the 2006-2007 school year. After the 2006 test score results were completed in July 2006, two more schools ended up being classified as Title I Improving, resulting in 21 total NCLB schools for the 2006-2007 school year.

The simplified information forms were specialized for each child. They contained a list of schools in the student's choice set, which depended on the student's choice zone and her home school assignment for the 2006-2007 school year. Figures III and IV provide examples of the simplified information forms. The forms list the schools in the choice set, along with program-specific school average scores (and odds of admission where applicable).[24] The scores were calculated from the prior year's average performance of students in that school and program on standardized tests and then re-scaled to correspond to a percentage score that looks like a grade. The odds of admission were calculated based on the prior year's admission rates. The information sheets incorporated the CMS logo and its graphic themes and were designed to look as if they were made by and came from the school district. The school district approved the final design. The staff at the Family Application Center reported seeing parents with their simplified information sheets in hand, with notes written on the forms as they made their decisions.

4.2. Aggregated Reduced-Form Results

Table V gives descriptive statistics for students in the district as a whole as well as for students in the non-NCLB sample and the NCLB sample. The differences in student characteristics across the three columns is as expected; average test scores and neighborhood income levels decline as we move from the district mean in column 1 to the non-NCLB sample mean in column 2, to the NCLB sample mean in column 3.

Tables VI and VII examine the effect that simplified information had on aggregate choice behavior. The outcome measures of interest are aggregated or averaged at the school-zone level, that is, the level at which the treatment was assigned. Table VI reports regression adjusted differences in average baseline characteristics between school-zone combinations receiving Score forms, Odds forms, and those in the control group. The first three columns give the means of the dependent variables for the treatment and control groups. The last two columns give the

[24] Two types of each form were given. One with only numeric information on test scores and one with a graphical apple rating that represented the numeric scores in addition to the numbers themselves. The graphical addition was randomized within school and homeroom, since it technically added no new information. We did not find that further simplification affected choices, so we pool the choice forms with graphics and without graphics for this analysis.

coefficient from a regression of the dependent variable in each row on whether the school-zone received Score or Odds forms, controlling for randomization-block fixed-effects:

$$\bar{y}_S = \alpha + R_S\delta + \theta T_S^1 + \phi T_S^2 + \varepsilon_S \qquad (2)$$

where R_S are randomization-block fixed effects, and T_S^1 and T_S^2 are indicators for whether students in school-zone *S* received the Score form or the Odds form, respectively. Standard errors are reported in parentheses, and p-values are in brackets below each coefficient estimate. All of the coefficients are insignificant implying that baseline characteristics are balanced across school-zones that received simplified information and those that did not.

Table VII presents regression-adjusted mean differences in aggregated choice behavior between treatment and control groups in the fraction of parents listing a non-guaranteed school as their first choice and the average difference between the test score of the first-choice school and the test score at the guaranteed school (test score gain). Results are presented pooled and separately for the non-NCLB school-zones and the NCLB school-zones, since the information provided to the control groups differs across those two samples. Columns 1 and 2 provide ordinary least squares estimates of the treatment effects, while columns 3 and 4 provide weighted least squares estimates, where each school-zone observation is weighted by the number of students in that school-zone. Standard errors are reported in parentheses, and p-values are in brackets below each coefficient estimate.

The first panel of estimates shows no significant overall impact of transparent information on parental choice behavior pooled across the non-NCLB and NCLB observations. However, the second panel shows that among the non-NCLB school-zones, transparent information had a significant impact on choices. Receiving simplified information on test scores increased the fraction of parents who listed a non-guaranteed choice as their first choice by about seven percentage points off of a base of 31 percent. This implies that simplified information on test scores increased demand for non-guaranteed schools by an economically significant 23 percent. Receiving simplified information on odds of admission and test scores did not have a significant impact on the fraction of parents choosing non-guaranteed schools, although the point estimates

are similar in magnitude to the point estimates on Score form.[25] Both forms have similar effects on the average difference between the test score of the first-choice school and the test score at the guaranteed school; Score forms and Odds forms caused an increase of about 0.10 standard deviations (a 70 percent increase relative to the mean of 0.14). Hence, in the non-NCLB group, parents respond to information on scores or on scores coupled with odds of admission by choosing schools with significantly higher test scores than their guaranteed option.

Both of these results are similar in magnitude to the results from the 2004 NCLB natural experiment. There, by comparing the choices parents made before and after receiving simplified information, we found a significant increase in the fraction of parents choosing non-guaranteed schools and a significant increase in the score of the school chosen relative to the school they previously chose in the spring (typically their guaranteed school in July). This suggests that our information may have no additional effect over the NCLB-mandated information. Indeed, the third panel of results in Table VII presents estimates of the effects on aggregate choice behavior in the NCLB sample of school-zones. Here the control group received the NCLB-mandated information, and we find that our information had no significant impact on the treatment versus the control group. Insignificant results imply that our simplified information had no effect above and beyond the information already provided by the district as part of NCLB regulation. This suggests that parents are not too sensitive to the number of options listed or the format in which they are listed; what is important is that information on school characteristics come with the choice form used in making the decision.[26]

4.3. Which Parents Responded Most to Information?

Examining the student-level data from the non-NCLB panel in our field experiment, we find that the small average impact on the test scores of schools chosen is generated by a large group of

[25] Hastings, Van Weelden, and Weinstein (2007) find that simplified information on test scores also affected the average number of choices that parents listed. School-zones receiving Score forms had a significant 24 percent increase in the average number of choices listed relative to the control group. However, school-zones receiving Odds forms had no significant average increase in the number of choices listed relative to the control group. This implies that knowing the odds of admission along with the test score of each choice decreased the number of choices listed relative to receiving information on test scores alone. For further analysis of behavioral response to information, please see that paper.

[26] Alternatively, it may be the case that this group of parents is relatively inert. Parents who would have responded to NCLB information and notification may have already responded by choosing out in prior years. Hence the remaining students have parents who are not responsive to information on academic outcomes.

parents who do not change their choice behavior, averaged with a group of families who choose drastically better schools for their children. Figure V shows a kernel density estimate of the test score of the school chosen relative to the guaranteed school for students who chose alternative schools by treatment and control groups. Because mean impacts of scores and odds were similar, we combine the treatments into "Received_Information", and plot the choices of those families versus the choices of control group families. There are two interesting things to note. First receiving information almost eliminates the density of parents who pick schools with lower average test scores than their guaranteed option (density to the left of zero). Second, receiving information more than doubles the density of parents choosing schools with average test scores more than one student-level standard deviation higher than their children's guaranteed school. This gain represents moving from a school in the lower quartile of the distribution of test scores across schools to one in the upper quartile.

As in the 2004 NCLB natural experiment, we can use student-level data to examine which families were more likely to respond to our information sheets by choosing substantially better schools. Tables VIII presents results for the non-NCLB sample from regressions of the form

$$y_i = X_i\beta + \delta R_i + \theta T_i + Z_i T_i \lambda + \varepsilon_i \qquad (3)$$

where X_i are student characteristics such as race and lunch recipient status, as well as characteristics of the student's choice set that might affect her choices, such as the score at her guaranteed school, the average distance to local schools, and the average test score at local schools. The choice set characteristics are somewhat ad hoc, but were chosen to roughly capture the importance of the choice set on the response to transparent information. The R_i are randomization-block fixed effects, T_i is an indicator for whether child received a Score form or an Odds form, and $Z_i T_i$ is a matrix of interactions between baseline characteristics that might affect treatment response as well as the size of the treatment effect. Standard errors are clustered at the school-zone and grade level.

Column 1 shows the interaction effects of receiving information on the gain in the score of the first-choice school relative to the guaranteed school, and column 2 shows the same regression restricted to the sample of students that chose a non-guaranteed school. Both columns show that the average impact of receiving information on the score of the school chosen is significantly

increasing in the average test score of schools within five miles, implying that distance is an important component of choice. Transparent information on school test scores only impacts choices among families who can feasibly send their children to those schools. If there are no high-scoring school alternatives, we would not expect information to have much of an impact on the schools parents choose. These results are similar to the results found using the 2004 NCLB natural experiment. Both columns imply that the effect of information on the score of the school chosen is decreasing in the distance to the guaranteed school. It is difficult to interpret the meaning of this coefficient; however, it is significant in both columns. Among the interactions with demographics, we find that African American parents are more likely to respond to transparent information on test scores by choosing higher-scoring schools. The opposite was true in the 2004 NCLB natural experiment, where African Americans and free-lunch recipients who selected alternative schools in response to NCLB information chose slightly lower-scoring schools than other responders. However, there is much more variation in socio-economic status in this sample than in the 2004 NCLB natural experiment which may generate the difference in results.

Taken together, the results from the 2004 NCLB natural experiment and the field experiment we conducted in 2006 imply that transparent information on academic achievement at schools has a large impact on parental choice. It increased demand for non-guaranteed schools by over 20 percent and significantly increased the test scores of the schools families chose. The fact that overall results were similar across the two experiments suggests that the degree of simplification is not as important as simply providing information on school test scores to parents at the time of choice. Furthermore, across the two samples, families with high-scoring schools in close proximity are the most likely to choose substantially better schools. This implies that lowering information costs has the biggest impact on choice where there are good options to choose from.

5. Impact on Test Scores

While simplified information on school test scores resulted in more parents' choosing higher-scoring schools, it is not clear if students benefit academically from attending those schools. A handful of recent papers have examined the impact of attending first-choice schools on academic

outcomes using lottery assignments in school choice plans to generate random variation in the test score at the school attended.[27] Cullen, Jacob, and Levitt (2006) examine lottery outcomes for high school choice in the Chicago Public Schools, and Cullen and Jacob (2007) examine similar lotteries for elementary school students. Neither finds a significant impact of attending a first-choice school on test scores or other measures of academic achievement. This may be in part caused by the fact that, on average, attending a first-choice school results in only small increases in the test score of the school attended. It also may be that parents are picking particular schools for different reasons, leading to heterogeneous treatment effects that average towards zero. Hastings, Kane, and Staiger (2006b) use lottery assignments from the first round of school choice (Spring 2002) in CMS and find that parents pick schools for different reasons and that students of parents who place high implicit weights on academic achievement experience significant gains in test scores as a result of attending those schools (approximately 0.1 student-level standard deviations for students in the highest end of the preference-for-academics distribution).

The two experiments examined in this paper provide an opportunity to examine the impact of attending substantially higher-scoring schools on own academic achievement for parents who were much more directed in choosing schools based on academic dimensions as a result of receiving transparent information on test scores. The experiments have two slightly different designs, so we will analyze them separately. In the field experiment, we will focus on the non-NCLB students and use the random assignment of information and its interactions with baseline characteristics that increase the response rate to information as instruments for the test score of the school attended. In 2004 NCLB natural experiment, treatment and control groups were the same set of families before and after receiving NCLB-mandated information, so we do not see eventual academic outcomes for both treatment and control groups as we do in the field experiment. However, since students of parents who chose alternative schools were subjected to a lottery admission process, we can use the lottery assignments as random variation in the score of the school attended. Here we will use winning a lottery and its interaction with the test score

[27] Angrist, Bettinger and Kremer (2006) examine a voucher program in Columbia that provided one-half of secondary school tuition by lottery to low-income high-school students. Continuation of the voucher after the first year was predicated on sufficient academic progress. They find significant impacts of winning a voucher lottery on graduation rates and other measures of academic performance.

of the school chosen relative to the NCLB school as instruments in a regression of own test scores on the test score of the school attended.

5.1. Analysis of the Field Experiment: Estimating the Effect of Receiving Information on Academic Outcomes

The most direct way to examine if simplified information led to improvements in test scores is to test if receiving simplified information in 2006 resulted in test score gains at the end of the 2006-2007 school year by causing students to choose and thus attend higher-scoring schools. Overall, we expect to be underpowered to detect an aggregate effect since receiving information resulted in an average gain in the test score of school chosen of only 0.1 student-level standard deviations. However, because this 0.1 is effectively an average of many non-respondents with a minority of respondents who chose substantially better schools, we may be able to estimate the effect of receiving information among those likely to respond by picking a better school.

Table IX presents effects of information on aggregated student outcomes, that is, the average over school-zone blocks (the level at which the randomization was done) of the change in student test score (average reading and math score in 2007 minus the 2006 test score), the change in the probability of having at least one suspension, and the change in the number of unexcused absences. This table is the outcome analog to Table VII for the non-NCLB panel. The first column shows the impact on the average test score of the school attended relative to the guaranteed school. The coefficient is marginally significant and half the magnitude of the effect on the test score of school chosen found in Table VII, reflecting the lottery admissions and normal attrition that impact the probability of attending the chosen school. The second column shows no significant impact on average test score gains between school-zones receiving information and the control group. Note the smaller sample size in the second column due to the fact that only students in grades 4-9 have baseline and outcome test scores. The third column of results implies that receiving information decreases suspension rates significantly. The size of the coefficient implies that students who received information and attended non-guaranteed schools must have experienced a complete decline in suspension rates. However, as is apparent in the constant term, suspension rates increased significantly overall in the 2006-2007 school year, implying the treatment group experienced a smaller rise in suspension rates relative to the

control group. It is important to note that impacts on suspensions may not just be generated by changes in student behavior but also by differences in suspension policies across different schools (Kinsler (2006)). The fourth column shows no significant impact of information on the number of unexcused absences but again a significant increase in unexcused absences over the 2006-2007 school year.

The aggregate results above may be underpowered to test for changes in test score outcomes from attending higher-scoring schools since many parents did not respond to information by choosing higher-scoring schools. We can use student-level data to test if students of parents who were most likely to respond to information by choosing much higher-scoring schools experienced academic gains. More specifically, we can use random assignment of information and its interaction with baseline characteristics as instruments for the test score of the school attended in 2006-2007 in order to test if attending a better school through receiving and subsequently responding to information results in higher test score outcomes.

Table X presents estimates from instrumental variables regressions of the form:

$$y_i = X_i\beta + \theta S_j + R_i\delta + \varepsilon_i \qquad (4)$$

Where X_i is a matrix of student baseline characteristics including characteristics of the choice set, S_j is the average test score of the school attended in the 2006-2007 school year measured in student-level standard deviation units, R_i are randomization-block fixed effects, and standard errors are clustered at the school-program and grade attended. The first-stage regression predicts the test score of the school attended as a function of receiving information, information interacted with baseline characteristics, Z_i, a subset of X_i.

$$S_j = X_i\lambda + \phi T_i + T_i Z_i\zeta + R_i\eta + \varepsilon_i \qquad (5)$$

Table X presents instrumental variables regressions results. The first column of results includes information, information interacted with choice set characteristics (distance to guaranteed school, guaranteed score, average distance to schools within five miles, and average score of schools within five miles) and information interacted with demographic characteristics (African American, free-lunch recipient, and single child family) as excluded instruments. These interactions are the interactions shown in Table VIII. The second column also includes

interactions between information and baseline academic achievement (average baseline test score, number of absences, and the number of out-of-school suspension) as excluded instruments. In both columns, the instruments are weak but jointly significant, with p-values of 0.032 and 0.050, respectively. Both columns imply a large and significant impact of the score of school attended on test score outcomes, controlling for baseline test scores. The estimates imply that increasing the score of the school attended by one student-level standard deviation results in an expected gain in own test score of 0.63 student-level standard deviations. The estimated effect is large, but the standard errors are large as well, with point estimates significant at the five percent level. Various studies measuring the value added of teachers suggest that moving from a bottom-quartile teacher to a top-quartile teacher raises student test scores by about 0.25 student-level standard deviations (Rockoff (2004), Rivkin, Hanushek, and Kain (2005), Kane, Rockoff, and Staiger (2006), Aaronson, Barrow, and Sander (2007)). This evidence suggests that choosing to attend a school that is 0.5 student-level standard deviations higher in test scores in response to transparent information has a similar impact on test scores as having a top-quartile teacher rather than a bottom-quartile teacher.

5.2. 2004 NCLB Natural Experiment

The 2004 NCLB natural experiment provides a second opportunity to examine if there are academic gains for children of parents that chose substantially better schools in response to receiving transparent information on school test scores. As mentioned earlier, we will use the summer lottery assignments of NCLB respondents to schools in order to examine the effect of the score of the school attended on own academic outcomes. Hence, this regression tests if attending the higher-scoring schools chosen as a result of receiving information indeed resulted in higher student test scores.

Once choice forms were submitted in July 2004, admissions were determined by lottery. The lottery was run based on the number of seats made available for each grade and school-choice combination. 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 an alternative school.

We will use 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 in order to examine the effect of attending a school with higher test scores on own academic achievement. This leaves us with a very small sample of 227 students, since many students were in priority groups for which everyone was either admitted or denied admission for that particular grade and choice. For further discussion of the lottery process and the construction of this sample, see Hastings and Weinstein (2007).

Table XI verifies that baseline characteristics are not caused by winning a lottery to attend a chosen school within the subgroup of students for whom lottery number alone determined admission. The regressions control for lottery fixed effects included to account for the fact that different lotteries have different odds of admission (Rouse (1998)). Standard errors are clustered at the attended school program and grade level. None of the coefficients on won lottery are significant, indicating that lottery winners and losers are balanced on baseline characteristics.

Table XII shows that winning the lottery to attend a chosen school increased the test score of the school attended by 0.24 student-level standard deviations. In addition, the score of the school attended increases by about half the score of the school chosen, reflecting the fact that winning the lottery increases the probability of attending the chosen school by about 50 percent. Here we can make a direct comparison between the size of treatment in prior lottery choice studies and this study. Hastings, Kane, and Staiger (2006b) examine lotteries for which the average effect of winning a lottery on the score of school attended is around 0.12, which is about half the effect here. Cullen, Jacob, and Levitt (2006) and Cullen and Jacob (2007) also analyze lotteries with similarly small average effects of winning a lottery on tests score measures at the school attended.[28]

We can use winning the lottery and winning the lottery interacted with the test score at the first-choice school relative to the NCLB school as instruments for the test score of the school

[28] Cullen, Jacob, and Levitt (2006) find that lottery winners, on average, attended schools where the fraction of ninth graders scoring at or above national norms is larger by 0.025 relative to a base of 0.349. Cullen and Jacob (2007) find that lottery winners, on average, attended schools 2.3 points higher relative to a standard deviation of 12.1 points.

attended. Because lottery number is randomly assigned, it, along with its interaction with the test score of the school chosen serves as a valid instrument that substantially increases the test score of the school attended. Table X presents IV regressions of the form:

$$y_i = \alpha + \theta S_j + X_i\beta + L_i\delta + \varepsilon_i \qquad (6)$$

Where X_i is a matrix of student baseline characteristics including score of the school chosen relative to the guaranteed school score, S_j, is the average test score of the school attended in the 2004-2005 school year measured in student-level standard deviation units, L_i are lottery fixed effects, and standard errors are clustered at the school-program and grade attended. The first stage regression predicts the test score of the school attended as a function of winning the lottery, and winning the lottery interacted with, T_i, the score of school chosen.

$$S_j = \mu + \phi W_i + \lambda W_i * T_i + X_i\beta + L_i\gamma + \varepsilon_i \qquad (7)$$

The first column of Table XIII shows a large and (marginally) significant impact of the test score of school attended on the own test score outcomes. The coefficients imply that attending a school with one student-level standard deviation higher test scores results in a 0.37 student-level standard deviation increase in own test scores. This result is similar in magnitude to the results from the 2006-2007 information field experiment, and is marginally significant despite the small sample size. The point estimate is slightly smaller, but both results imply a substantial impact of attending better schools on student test score outcomes.

The second column shows impacts on suspensions. The results are not significant at the ten percent level, but the point estimates suggest a substantial reduction in suspension rates from attending a higher scoring school. The point estimate represents a 70 percent reduction in the probability of having at least one out-of-school suspension as a result of attending a school with one standard deviation higher test score.

Taken together, the results of these two experiments imply that providing clear information on school test scores within a choice plan increases the proportion of parents choosing substantially better schools for their children. These changed choices appear to have generated measurable improvements in academic outcomes for these students. It is important to note that these results do not imply that moving a random low-achieving child to a school with high average test scores

will result in that child gaining academically. In both cases we identified the impact of the test score of the school attended off of likely responders or actual responders to transparent information. On the other hand, these two information experiments changed the school choices for enough parents by a substantial amount to allow us to examine the marginal impact of choice for students in lower-income communities whose parents were able to easily act on their desire to sent their child to an academically challenging school, as a result of receiving transparent information on school test scores at the time of choice.

6. Conclusion

One of the most important topics in education policy is school choice and its effect on competition, student outcomes, and student sorting. School choice programs are gaining support as potential ways to introduce market forces into public education, by forcing schools to more efficiently produce higher quality education for all students. A key component of NCLB is the requirement that students at failing schools be given the option to choose to attend another non-failing schools. The goal is to allow lower-income students to benefit academically from attending higher-performing schools in other neighborhoods and to increase pressure on failing schools to improve through the (limited) introduction of market forces.

However, the extent to which market forces successfully lead to improved educational outcomes for all socio-economic groups depends on parents' choice behavior. As researchers have focused on the determinants of parental choice, there is mounting evidence that many parents, particularly from lower-income backgrounds, do not choose schools for academic achievement. However, there is little evidence as to why. This paper analyzed data from two experiments that provided transparent information on school test scores to low-to-middle income families in the CMS school choice plan. We find that receiving transparent information on school test scores resulted in significantly more parents choosing substantially better schools for their children, implying that information and decision-making costs are important factors driving the observed choices of these families.

Furthermore we exploit the random variation in the test score of the school attended generated by the information experiments to test if attending a higher-scoring school resulted in increased test scores for these children. Our estimates imply a 0.37-0.63 gain in own test scores as a result of a one student-level standard deviation increase in the test scores at the school attended. Back-of-the-envelope calculations of the impact of increases in student test scores on expected lifetime earnings imply that a 1 standard deviation increase in a child's test score corresponds to a $100,000-$200,000 gain in lifetime earnings (Murnane, Willett, and Levy (1995), Neal and Johnson (1996)), Kane and Staiger (2002)). If the results we find represent permanent increases in student-level test scores, they suggest that high information and decision making costs for these parents had a substantial monetary impact on their children's lifetime earnings. Research on financial investment decisions has also shown that small changes in information presentation or defaults have large impacts on future wealth that seem much larger in magnitude than the investment needed to calculate the optimal decision (for example see Madrian and Shea (2001), Duflo et al. (2006), Choi et al. (2007)).

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Table I: Summary Statistics for NCLB Schools in June 2004

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)
% 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)
% 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.

Table II: Effect of 2004 NCLB-Mandated Information on Parental Choice Behavior

	Spring 2004 Choice Round	July 2004 NCLB Choice Round	Difference: July - Spring
All NCLB Parents			
Fraction Choosing Alternative School	0.112	0.163	0.051***
	(0.004)	(0.005)	(0.006)
Test Score of First-Choice School	0.053	0.100	0.047***
Relative to NCLB School	(0.003)	(0.003)	(0.004)
Number of Students	6,695	6,695	6,695
Parents Who Chose NCLB School in Spring			
Fraction Choosing Alternative School	0	0.145	0.145***
	--	(0.005)	(0.005)
Test Score of First-Choice School	0	0.088	0.088***
Relative to NCLB School	--	(0.003)	(0.003)
Number of Students	5,945	5,945	5,945

Notes: Standard errors are in parentheses. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table III: Differences in Choice Characteristics Between Spring and July Choices

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 Five Miles			
Spring 2004	-0.322	-0.328	-0.277
July 2004	-0.247	-0.253	-0.206
Number of Students	1,092	963	129

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

Table IV: Characteristics that Influenced Response to NCLB Information

	Dependent Variable: Chose Non-NCLB School First		Dependent Variable: Average Test Score at First-Choice School	
	(1)	(2)	(3)	(4)
Choice Set Characteristics				
Distance to NCLB School	0.003	0.005	-0.015**	-0.016**
	(0.002)	(0.003)	(0.005)	(0.005)
Ave. Distance to Schools Within Five Miles	0.001	-0.007	-0.011	-0.004
	(0.004)	(0.005)	(0.015)	(0.014)
Ave. Score of Schools Within Five Miles	0.093**	0.111**	0.233***	0.186***
	(0.034)	(0.042)	(0.057)	(0.052)
Student Characteristics				
Black	0.055*	0.065**	-0.079*	-0.081*
	(0.028)	(0.024)	(0.033)	(0.045)
Free- or Reduced-Lunch Recipient	0.018	0.026	-0.069**	-0.082**
	(0.018)	(0.016)	(0.027)	(0.038)
Female	-0.004	-0.007	-0.027	-0.025
	(0.009)	(0.009)	(0.025)	(0.025)
Rising Grader	0.033**	0.034**	-0.053	-0.039
	(0.013)	(0.015)	(0.033)	(0.032)
Demeaned Income	-0.0002	-0.0002	-0.001	-0.001
	(0.0003)	(0.0004)	(0.001)	(0.001)
Baseline Unexcused Absences	-0.002***	-0.002**	-0.003*	-0.004**
	(0.001)	(0.001)	(0.002)	(0.002)
Baseline Number of Suspensions	0.002***	0.003***	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Baseline Test Score	0.015**	0.014**	0.025	0.032
	(0.007)	(0.007)	(0.018)	(0.020)
Chose Non-NCLB School in Spring	0.134***	--	0.104***	--
	(0.027)	--	(0.022)	--
Constant	0.109***	0.121***	0.313***	0.299***
	(0.042)	(0.039)	(0.056)	(0.057)
Mean of Dependent Variable	0.176	0.155	0.618	0.608
Number of Students	4,646	4,022	818	623
Adjusted R-Squared	0.039	0.026	0.139	0.141

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

Table V: Summary Statistics for Students in Field Experiment

Variable	District (N = 125,313)	Non-NCLB Panel (N = 6,328)	NCLB Panel (N = 10,134)
Fraction Black	0.457	0.600	0.688
	(0.498)	(0.490)	(0.463)
Fraction Lunch Recipients	0.461	0.608	0.837
	(0.498)	(0.488)	(0.369)
Average Income	56764	48767	35194
	(26915)	(21278)	(14943)
Average Test Score[†]	0.006	-0.339	-0.518
	(0.948)	(0.850)	(0.827)
Average Number of Unexcused Absences	4.116	3.449	4.387
	(7.152)	(6.004)	(6.002)
Fraction With at Least One Out-of-School Suspension	0.120	0.141	0.184
	(0.325)	(0.348)	(0.388)

Note: Standard deviations are in parentheses.

[†]Only students in grades three through eight take these exams, so this variable is based on 62,759 students in the district, 3,920 students in the non-NCLB panel, and 6,117 students in the NCLB panel.

Table VI: Average Baseline Characteristics

	Sample Means and Standard Deviations			Regression Adjusted Mean Differences	
	Score Form	**Odds Form**	**Control Group**	**Score Form**	**Odds Form**
Black	0.664 (0.153)	0.576 (0.186)	0.651 (0.209)	-0.008 (0.042) [0.843]	-0.039 (0.064) [0.545]
Lunch Recipient	0.763 (0.128)	0.748 (0.144)	0.716 (0.229)	0.046 (0.039) [0.242]	0.029 (0.059) [0.627]
Income	41725 (8996)	41573 (8456)	43334 (13598)	-1726 (2301) [0.456]	-2392 (3522) [0.499]
Test Score[†]	-0.404 (0.248)	-0.452 (0.191)	-0.426 (0.281)	0.016 (0.059) [0.784]	-0.125 (0.082) [0.133]
At Least One Out-of-School Suspension	0.192 (0.145)	0.150 (0.149)	0.165 (0.166)	0.007 (0.029) [0.811]	-0.047 (0.045) [0.296]
Number of Unexcused Absences	3.932 (1.994)	4.139 (2.192)	4.220 (2.673)	-0.520 (0.462) [0.265]	0.417 (0.707) [0.557]
Number of Observations	33	11	33	77	77

Notes: Adjusted differences report the coefficient on whether the school-zone received Score forms or Odds forms from separate OLS regressions with each variable in the first column as the dependent variable, controlling for randomization-block fixed effects. For adjusted differences, standard errors are in parentheses, and p-values are in brackets. Asterisks indicate significance (*=.10, **=.05, ***=.01).
[†] Only students in grades three through eight take these exams, so this variable is based on 28 school-zones that received Score forms, 11 school-zones that received Odds forms, and 25 school-zones that were in the control group.

Table VII: Regression-Adjusted Mean Differences in Aggregated Choice Behavior

	Ordinary Least Squares		Weighted Least Squares	
	Fraction Choosing Non-Guaranteed School	**Average Score Gain at First-Choice School**	**Fraction Choosing Non-Guaranteed School**	**Average Score Gain at First-Choice School**
Panel 1: Pooled Results for All School-Zones				
Scores	0.025	0.039	0.007	0.013
	(0.028)	(0.026)	(0.026)	(0.025)
	[0.370]	[0.136]	[0.805]	[0.603]
Odds	0.007	0.055	-0.0004	0.023
	(0.043)	(0.040)	(0.039)	(0.037)
	[0.863]	[0.173]	[0.991]	[0.535]
Mean	0.258	0.136	0.237	0.142
N	77	77	77	77
Panel 2: Results for Non-NCLB Observations Only				
Scores	0.066*	0.098***	0.073*	0.101***
	(0.036)	(0.029)	(0.037)	(0.027)
	[0.074]	[0.002]	[0.053]	[0.001]
Odds	0.045	0.108**	0.079	0.105**
	(0.057)	(0.046)	(0.060)	(0.043)
	[0.434]	[0.025]	[0.194]	[0.021]
Mean	0.319	0.142	0.305	0.136
N	46	46	46	46
Panel 3: Results for NCLB Observations Only				
Scores	-0.028	-0.038	-0.028	-0.032
	(0.044)	(0.042)	(0.038)	(0.038)
	[0.526]	[0.375]	[0.456]	[0.417]
Odds	-0.047	-0.022	-0.040	-0.021
	(0.065)	(0.063)	(0.054)	(0.055)
	[0.481]	[0.724]	[0.463]	[0.699]
Mean	0.168	0.128	0.195	0.146
N	31	31	31	31

Notes: Columns present separate OLS regressions of aggregated choice behavior on information treatment controlling for randomization-block fixed effects. Standard errors are in parentheses, and p-values are in brackets. Weighted regressions weight by the number of students in each school-zone block. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table VIII: Reduced Form Relationship Between Choice Set Characteristics and Response to Information

Dependent Variable: Test Score at First-Choice School – Test Score at Guaranteed School	**All Students** (1)	**Chose Non-Guaranteed School** (2)
Main Effect		
Received Information	0.012	0.182
	(0.047)	(0.142)
Interactions with Choice-set Characteristics		
Information * Distance to Guaranteed Choice	-0.015*	-0.045***
	(0.008)	(0.015)
Information * Ave. Distance to Schools in 5 Miles	0.015	0.027
	(0.010)	(0.018)
Information * Ave. Test Score at Schools in 5 Miles	0.111*	0.248*
	(0.060)	(0.122)
Information * Test Score at Guaranteed School	0.028	-0.057
	(0.071)	(0.125)
Interactions with Demographics		
Information * African American	0.065***	0.026
	(0.023)	(0.075)
Information * Lunch Recipient	0.035	0.036
	(0.038)	(0.071)
Information * Single Child in CMS	0.025	0.014
	(0.015)	(0.037)
Interactions with Baseline Academic Performance		
Information* Combined Test Score	0.015	0.01
	(0.022)	(0.030)
Information* Number of Absences	-0.002	-0.0004
	(0.002)	(0.006)
Information* Number of Suspensions	-0.001	0.002
	(0.002)	(0.004)
Number of Students[†]	3,920	1,222
Adjusted R-Squared	0.17	0.33

Notes: Each regression controls for full set of level effects, as well as baseline test score, an indicator for whether the student has at least one suspension, number of absences, and randomization-block fixed effects. Standard errors are in parentheses and are clustered at the school-zone and grade level. Asterisks indicate significance (*=.10, **=.05, ***=.01).

[†] Because we include baseline test scores, the sample only includes students in grades three through nine.

Table IX: Regression-Adjusted Mean Differences in Aggregated Outcomes for Non-NCLB Students

	Score of School Attended – Guaranteed Score	Change in Student Test Score	Change in Suspension Rate	Change in Number of Unexcused Absences
Received Information	0.054*	0.006	-0.046**	0.039
	(0.027)	(0.049)	(0.021)	(0.627)
Constant	0.016	0.040	0.079***	2.318***
	(0.020)	(0.039)	(0.016)	(0.458)
N	46	33	46	46

Notes: Columns present separate OLS regressions for non-NCLB students controlling for randomization-block fixed effects. Standard errors are in parentheses. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table X: IV Estimates of Effect of Score of School Attended on Own Test Score

Dependent Variable: Mean of Reading and Math Test Score in 2007	**Model 1**	**Model 2**
Score of School Attended	0.626**	0.613**
	(0.317)	(0.305)
Baseline Test Score	0.722***	0.722***
	(0.025)	(0.025)
Baseline Number of Unexcused Absences	-0.00002	-0.00002
	(0.002)	(0.002)
Baseline Indicator if at Least One Out-of-School Suspension	-0.042*	-0.042*
	(0.025)	(0.025)
Baseline Number of Out-of-School Suspensions	0.003	0.003
	(0.002)	(0.002)
Baseline Number of In-School Suspensions	-0.009	-0.009
	(0.010)	(0.010)
African American	-0.034	-0.034
	(0.022)	(0.022)
Lunch Recipient	-0.055*	-0.056**
	(0.028)	(0.027)
Demeaned Income	0.0001	0.0001
	(0.001)	(0.001)
Magnet Student	-0.077	-0.074
	(0.075)	(0.073)
Number of Students	3,280	3,280
R-Squared	0.663	0.665
Excluded Instruments:	Received Information Info * Choice Set Chars. Info * Demographics	Received Information Info * Choice Set Chars. Info * Demographics Info * Baseline Academics
Joint P-Value for Excluded Instruments	0.032	0.045

Notes: Regressions include choice set characteristics, as well as randomization block fixed-effects. Levels for all interacted variables are included. Standard errors are in parentheses and are clustered at the attended school-program and grade level in the 2006-2007 school year. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table XI: Baseline Characteristics by Lottery Winners and Lottery Losers

Baseline Characteristic	Won Lottery	Lost Lottery	Adjusted Difference
African American	0.878	0.875	0.057 (0.052)
Lunch Recipient	0.965	0.973	-0.018 (0.013)
Female	0.461	0.518	-0.001 (0.068)
Income	30,207	28,658	2024 (1793)
Number of Unexcused Absences	5.009	5.875	-1.787 (1.207)
Number of Suspensions	3.626	3.732	0.704 (1.044)
Test Score	-0.796	-0.886	-0.051 (0.056)
Number of Students[†]	115	112	223

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, controlling for lottery-block fixed effects. Standard errors are clustered at the attended school-program and grade level in the 2004-2005 school year. 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.

Table XII: Effect of Winning the 2004 NCLB Lottery on Score of School Attended

	Score of School Attended	Score of School Attended
Won Lottery	0.238***	-0.179
	(0.071)	(0.125)
Won Lottery * (Score of School Chosen - Guaranteed Score)	--	0.523***
	--	(0.172)
Observations	169	169
Adjusted R-squared	0.46	0.50

Notes: This table reports the coefficient on whether the student won the lottery on the score of the school attended in the 2004-2005 school year, controlling for lottery-block fixed effects and the following baseline covariates: black, lunch recipient, female, baseline test score, demeaned income, an indicator for greater than or equal to five unexcused absences, number of unexcused absences, an indicator for greater than or equal to one suspension, and number of suspensions. Standard errors are clustered at the attended school-program and grade level in the 2004-2005 school year. Asterisks indicate significance (*=.10, **=.05, ***=.01).

Table XIII: IV Estimates of Effect of Score of School Attended on Student Academic Outcomes from 2004 NCLB Assignment Lottery

Dependent Variable: Mean of Reading and Math Test Score in 2005	Ave. Test Score in Reading and Math	At Least One Suspension
Score of School Attended	0.372*	-0.374
	(0.206)	(0.253)
Baseline Test Score	0.932***	-0.245*
	(0.110)	(0.123)
Baseline >=5 Unexcused Absences	0.076	-0.104
	(0.123)	(0.111)
Baseline >=1 Suspension	0.016	0.215*
	(0.093)	(0.108)
Baseline Num. of Suspensions	-0.003	0.016***
	(0.007)	(0.005)
Baseline Num. of Unexcused Absences	-0.015*	0.010
	(0.007)	(0.007)
African American	-0.024	0.093
	(0.137)	(0.125)
Lunch Recipient	0.013	-0.895***
	(0.323)	(0.246)
Female	0.117	-0.023
	(0.076)	(0.073)
Demeaned Income	0.00001	0.002
	(0.004)	(0.004)
Score of School Chosen – Score of Guaranteed School	0.303	0.088
	(0.312)	(0.319)
Number of Students	161	169
R-squared	0.67	0.28
Excluded Instruments:	Won Lottery Won Lottery * (Score of School Chosen-Guaranteed Score)	
Joint P-Value for Excluded Instruments:	0.002	

Notes: Standard errors are clustered at the attended school-program and grade level in the 2004-2005 school year. Asterisks indicate significance (*=.10, **=.05, ***=.01). All regressions include lottery fixed effects.

Figure I: Timeline of Events

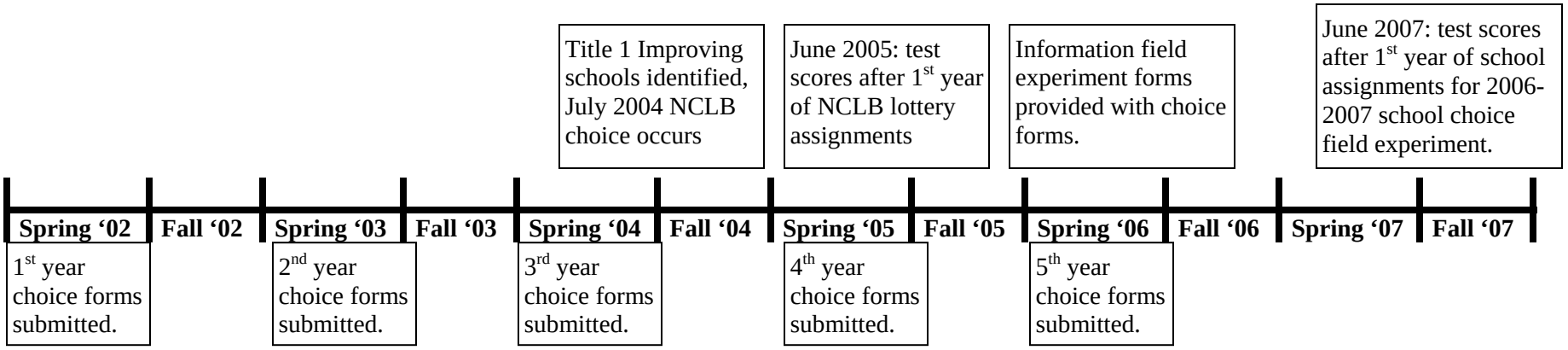
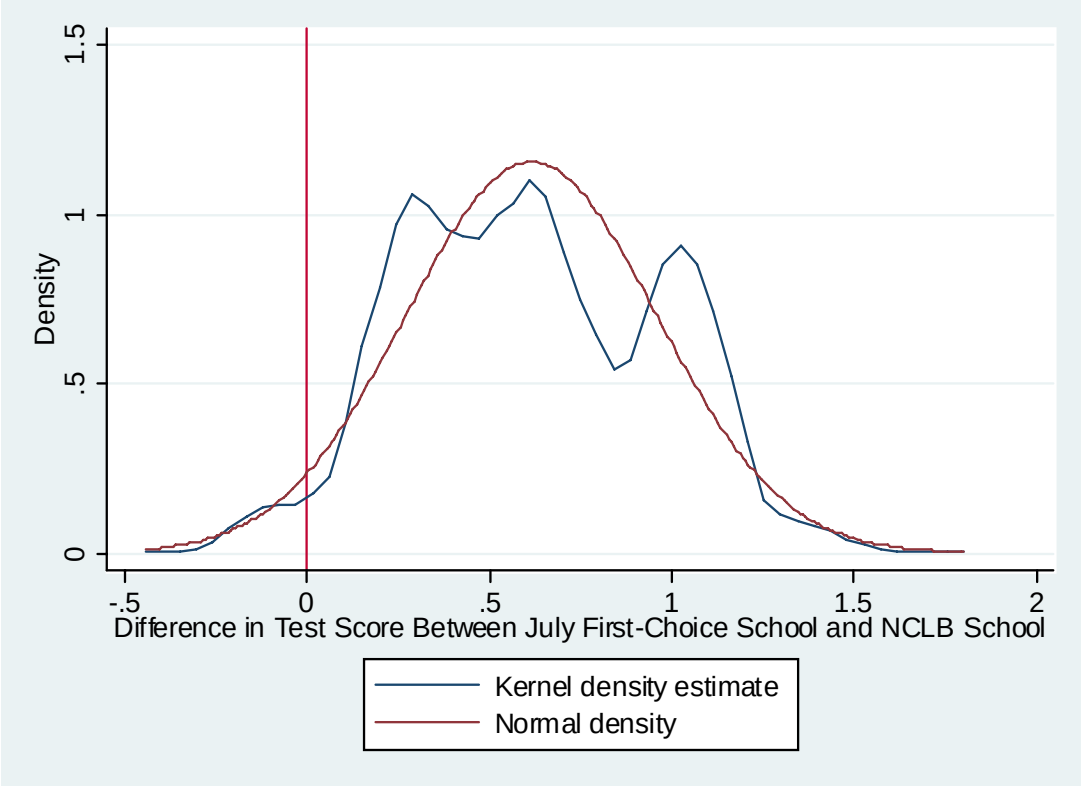
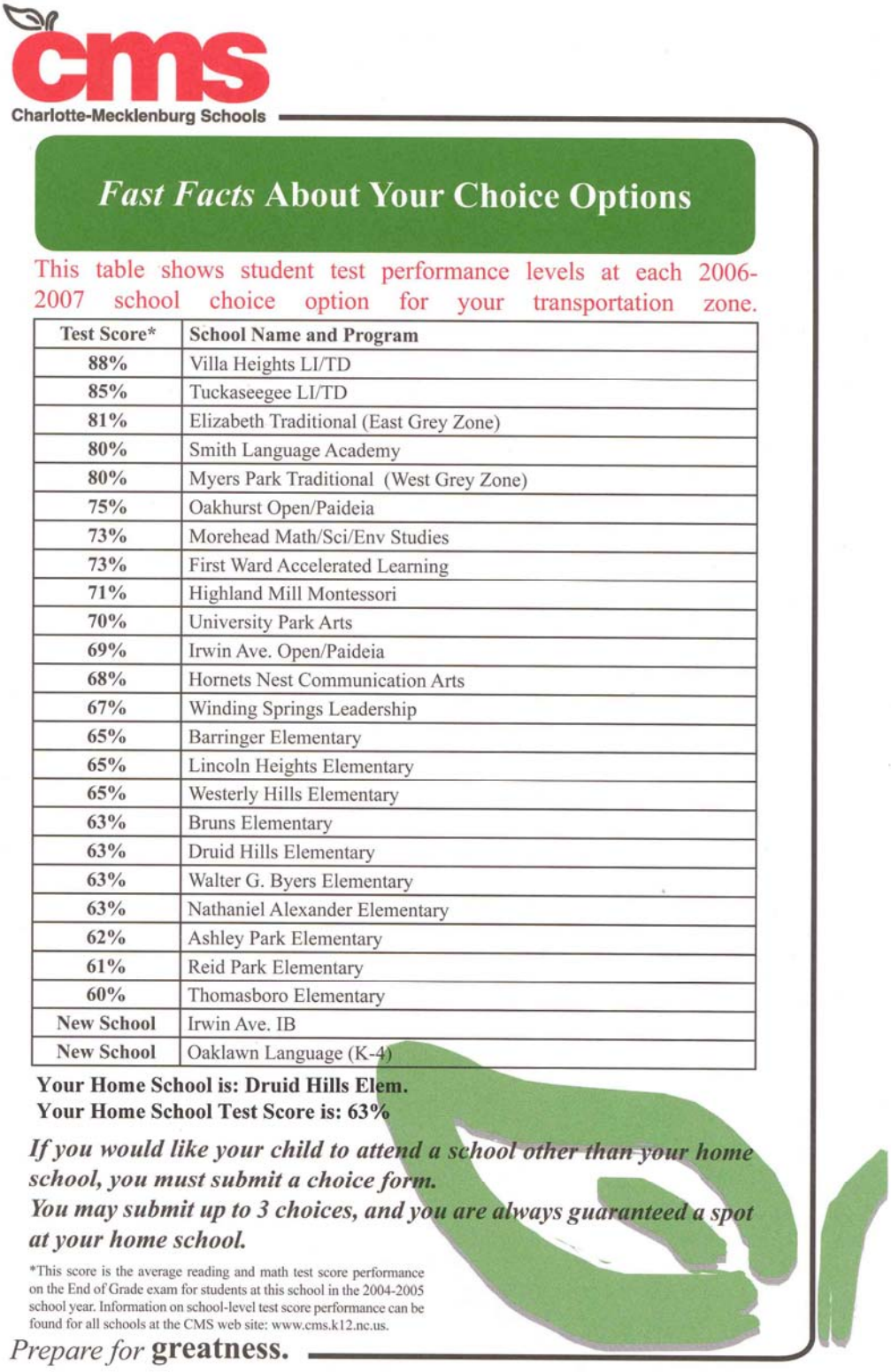


Figure II: Kernel Density Estimate of Difference in Test Score Between July First-Choice School and NCLB School for Families Who Did Not Choose NCLB School First in July



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

Figure III: Example of a Score Form



cms
Charlotte-Mecklenburg Schools

Fast Facts About Your Choice Options

This table shows student test performance levels at each 2006-2007 school choice option for your transportation zone.

Test Score*	School Name and Program
88%	Villa Heights LI/TD
85%	Tuckaseegee LI/TD
81%	Elizabeth Traditional (East Grey Zone)
80%	Smith Language Academy
80%	Myers Park Traditional (West Grey Zone)
75%	Oakhurst Open/Paideia
73%	Morehead Math/Sci/Env Studies
73%	First Ward Accelerated Learning
71%	Highland Mill Montessori
70%	University Park Arts
69%	Irwin Ave. Open/Paideia
68%	Hornets Nest Communication Arts
67%	Winding Springs Leadership
65%	Barringer Elementary
65%	Lincoln Heights Elementary
65%	Westerly Hills Elementary
63%	Bruns Elementary
63%	Druid Hills Elementary
63%	Walter G. Byers Elementary
63%	Nathaniel Alexander Elementary
62%	Ashley Park Elementary
61%	Reid Park Elementary
60%	Thomasboro Elementary
New School	Irwin Ave. IB
New School	Oaklawn Language (K-4)

Your Home School is: Druid Hills Elem.
Your Home School Test Score is: 63%

If you would like your child to attend a school other than your home school, you must submit a choice form.
You may submit up to 3 choices, and you are always guaranteed a spot at your home school.

*This score is the average reading and math test score performance on the End of Grade exam for students at this school in the 2004-2005 school year. Information on school-level test score performance can be found for all schools at the CMS web site: www.cms.k12.nc.us.

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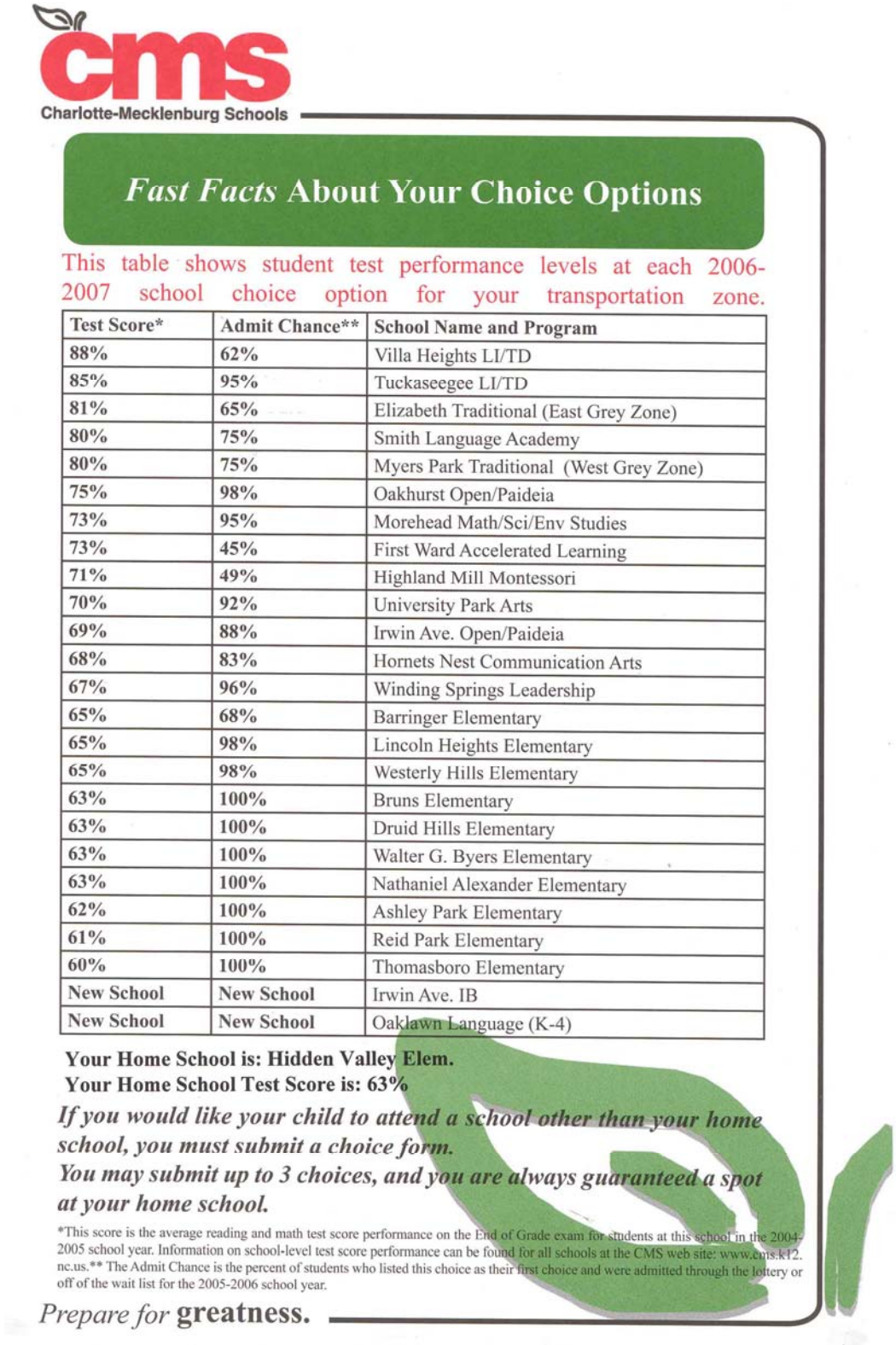
Figure IV: Example of an Odds Form



cms
Charlotte-Mecklenburg Schools

Fast Facts About Your Choice Options

This table shows student test performance levels at each 2006-2007 school choice option for your transportation zone.

Test Score*	Admit Chance**	School Name and Program
88%	62%	Villa Heights LI/TD
85%	95%	Tuckaseegee LI/TD
81%	65%	Elizabeth Traditional (East Grey Zone)
80%	75%	Smith Language Academy
80%	75%	Myers Park Traditional (West Grey Zone)
75%	98%	Oakhurst Open/Paideia
73%	95%	Morehead Math/Sci/Env Studies
73%	45%	First Ward Accelerated Learning
71%	49%	Highland Mill Montessori
70%	92%	University Park Arts
69%	88%	Irwin Ave. Open/Paideia
68%	83%	Hornets Nest Communication Arts
67%	96%	Winding Springs Leadership
65%	68%	Barringer Elementary
65%	98%	Lincoln Heights Elementary
65%	98%	Westerly Hills Elementary
63%	100%	Bruns Elementary
63%	100%	Druid Hills Elementary
63%	100%	Walter G. Byers Elementary
63%	100%	Nathaniel Alexander Elementary
62%	100%	Ashley Park Elementary
61%	100%	Reid Park Elementary
60%	100%	Thomasboro Elementary
New School	New School	Irwin Ave. IB
New School	New School	Oaklawn Language (K-4)

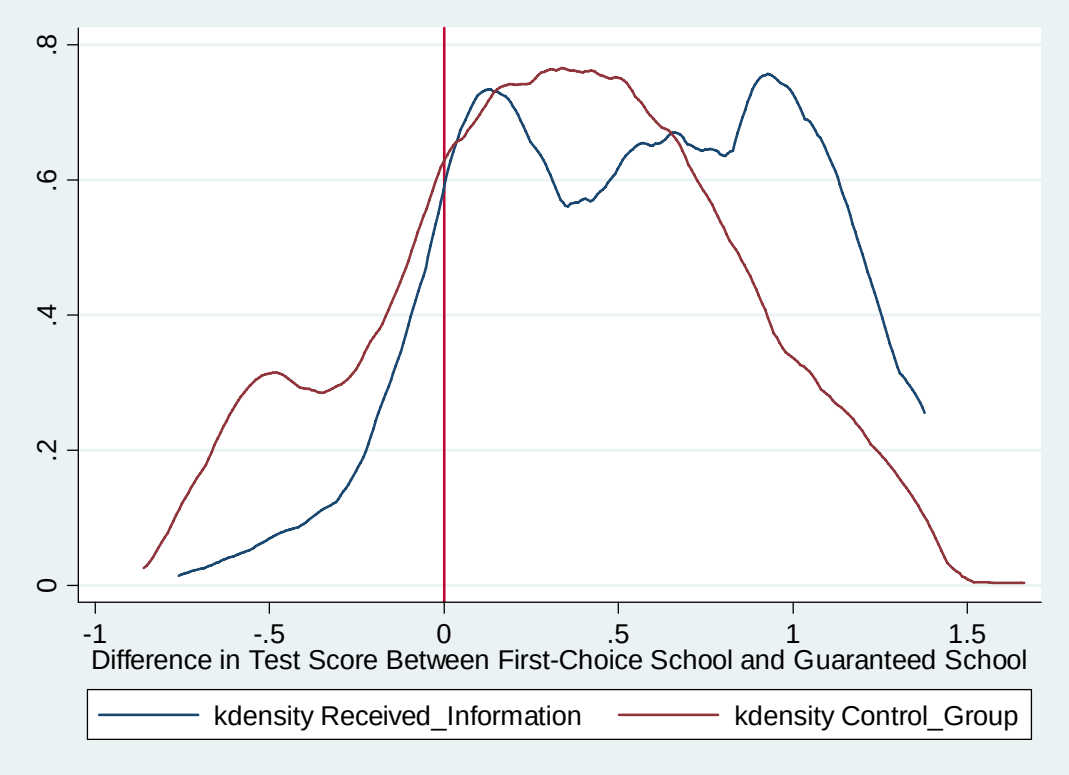
Your Home School is: Hidden Valley Elem.
Your Home School Test Score is: 63%

If you would like your child to attend a school other than your home school, you must submit a choice form.
You may submit up to 3 choices, and you are always guaranteed a spot at your home school.

*This score is the average reading and math test score performance on the End of Grade exam for students at this school in the 2004-2005 school year. Information on school-level test score performance can be found for all schools at the CMS web site: www.cms.k12.nc.us.** The Admit Chance is the percent of students who listed this choice as their first choice and were admitted through the lottery or off of the wait list for the 2005-2006 school year.

Prepare for **greatness.**

Figure V: Kernel Density Estimate of Difference in Test Score Between First-Choice School and Guaranteed School for Families Who Did Not Choose Guaranteed School First



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