Technical Report

INFORMING SCHOOL-CHOOSING FAMILIES ABOUT THEIR OPTIONS: A FIELD EXPERIMENT FROM NEW ORLEANS

EDUCATION RESEARCH ALLIANCE FOR NEW ORLEANS

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Abstract

In order for school choice reforms to fulfill their potential, school choosers must be informed about their options. We conducted a randomized controlled trial during the school choice application period in New Orleans to assess the effects of providing information to parents. Families with children entering pre-K, kindergarten, or ninth grade were assigned to one of two treatment groups or a control group. A "performance" group received lists of the highest-performing schools or programs available (via U.S. mail, email, and text message). A "neighborhood" group received lists of the schools or programs in their home geographic zone. We find that the performance treatment made applicants significantly more likely to request high-performing schools, though the effects were concentrated among high school choosers. The performance treatment had especially strong effects among families of students with disabilities. The neighborhood treatment had only modest effects. We consider these findings in the context of questions about the role of information in school choice markets, as well as which families may be in particular need of support.

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INTRODUCTION

Over the last several decades, governments across the world have increasingly adopted private sector principles and practices. From "new public management" approaches that brought corporate management techniques to the public sector to the creation of markets and quasi-markets for social services, the relationships between governments and the people they serve have changed (Ferlie et al., 1996; Grand & Bartlett, 1993). This is certainly the case in education, as many countries have adopted market-based school reforms (Musset, 2012; OECD, 2017). In the United States, for example, the share of students attending charter schools, magnet schools, and voucher-supported private schools has increased sharply in recent years, with school choice programs now a core feature of many cities' education systems (Harris, Witte, & Valant, 2017).

The theory underlying these school choice reforms is straightforward. By encouraging families to choose from a collection of tuition-free, autonomous schools, the schools that provide the highest-quality options to families, most responsive to their needs, should thrive (Friedman, 1955; Chubb & Moe, 1990a; Chubb & Moe, 1990b). Yet the outcomes of school choice reform, while fiercely debated, have not lived up to many proponents' lofty expectations. Overall, charter school students perform similarly to matched peers in district schools (CREDO, 2013), while evidence on the academic effects of voucher programs is mixed at best (Dynarski, 2016).

Understanding why school choice in practice has not fulfilled the potential of school choice in theory has become a priority for education research and policy. The potential explanations are numerous and varied. Structural barriers such as inadequate student transportation, burdensome application and enrollment processes, or a shortage of seats in highly desirable schools may play a role. Parent preferences may play a role if parents prioritize factors

such as student demographics over academic performance. Another possible factor—and the focus of this study—is that parents may not have useful, high-quality information about their options. As market-based reforms have expanded, so, too, has the need to ensure that the public is well informed. Questions about how people use information—and how well informed they are as they make choices—has prompted research in policy areas such as education (e.g., Hastings & Weinstein, 2008), health care (e.g., Hibbard et al., 2002), housing (e.g., Bergman, Chan, & Kapor, 2020), and energy consumption (e.g., Davis & Metcalf, 2016).

This study reports on an information experiment we conducted in partnership with the public school district in New Orleans, the Orleans Parish School Board (OPSB). During the period in which families requested school placements for the 2019-20 school year, we randomly assigned potential applicants to one of three groups. A "performance" group received lists—via flyers, text messages, and emails—of the highest-performing schools they could request. For K-12 applicants, these were schools that performed best on newly released state measures of growth on state assessments. For pre-kindergarten (pre-K) applicants, these were programs that scored highest on a quality measure (the Classroom Assessment Scoring System, or CLASS) that the state had recently adopted for early childhood education. A "neighborhood" group received lists—also via flyers, text messages, and emails—of all of the schools in the applicants' home geographic zones. A control group received a parallel set of communications that did not highlight any particular schools.[1] New Orleans is an especially suitable setting for this type of study. The city no longer uses attendance boundaries, so all families engage in some form of school choice. Its unified enrollment system, OneApp, includes the vast majority of public

[1] For simplicity, we often refer to schools and programs as "schools" in the rest of the manuscript, although some pre-K programs in the study are not school-based.

schools and publicly funded early childhood programs in the city. Additionally, its placement algorithm was designed to elicit applicants' true rank-ordered preferences.

Our findings indicate that the performance treatment affected applicants' school choice behaviors. Those effects were concentrated among certain subgroups of applicants, and more evident on some outcomes than others. For instance, we observed stronger effects for high school applicants than early childhood and elementary school applicants, and the effects were strikingly large for families of students with disabilities. Notably, these materials were relatively likely to lead applicants to request more high-performing schools (or request at least one high-performing school)—and relatively unlikely to change applicants' first-choice requests and placements. The neighborhood treatment had only modest effects on applicants' requests for, and placements in, schools in applicants' home geographic zones.

This study contributes to the school choice literature in several ways. We attempted to design and administer the treatments in ways that attend to questions about other attempts to inform families about schools. For instance, due to fortunate timing with the state's first-ever release of student progress letter grades, we shared seemingly easy-to-interpret information that was likely useful to most families (since these measures show how much students learn, not what they know) and not previously well known to the public (since student growth was hard to observe prior to the release of these ratings). By partnering with the district and communicating similar information via flyers, text messages, and emails, we addressed challenges related to getting information into the hands of school-choosing families. By administering the experiment in New Orleans, we could see how families ranked their options relative to one another—and administer similar interventions across the early childhood, elementary/middle school, and high school spectrum. We believe the study's findings show the effects of a low-cost, well-

administered information intervention that reaches families citywide at the time they chose schools for their children. We think the magnitude of the effects on families of students with disabilities suggests a specific group that is not adequately supported during the school choice process.

The paper proceeds with a description of prior research on informing school-choosing families, followed by a description of the policy context, data, study design, methods, and results. We conclude by discussing the implications of these results for school choice policy, focusing on the potential impact of information provision to change school choice behaviors and outcomes.

PRIOR LITERATURE

Arguments for school choice reform have appealed to an assortment of principles, including equity, efficiency, liberty, and pluralism (Berner, 2017; Chubb & Moe, 1990a; Coons & Sugarman, 1978; Friedman, 1955). Friedman (1955) authored the most prominent argument, applying the logic of markets to schools. He proposed a system in which parents (consumers) could select from a collection of government-funded schools led by private school operators (producers). Government funding was justified, he argued, by the positive externalities of schooling, since a society benefits when its children are well educated. School choice policies would improve efficiency through the market interactions between parents (who know and care deeply about their children) and school leaders (who must attract parents to receive state funding). Parents would choose high-quality schools that fit their children well, and then both those children and society more generally would benefit.

However, research examining parents' school requests suggests that many do not prioritize school quality as it is commonly conceived in education research and policy, calling

into question whether a market for schooling will deliver on its theorized potential. For instance, studies of parents' school requests in cities across the US indicate that parents value academic performance but that other considerations—such as proximity to home and student demographics—often receive greater weight (Glazerman & Dotter, 2017; Denice & Gross, 2016; Frankenberg, 2018; Harris & Larsen, 2019; Hastings, Kane, & Staiger, 2009; Ladd & Turaeva, 2020). Insofar as parents incorporate school performance into their considerations, they may lean more heavily on achievement levels than on growth measures that are more directly attributable to schools (Abdulkadiroğlu et al., 2020; Glazerman & Dotter, 2017; Imberman & Lovenheim, 2016; Rothstein, 2006), although the evidence on this question is mixed (Houston et at., 2020; Houston & Henig, 2019).

Why so many families with school choice opportunities enroll their children in seemingly low-performing schools is a question of interest in education research and policy. Answering it requires attention to the many assumptions that underlie the market theory of school choice. For one, while Friedman assumed compatibility between what parents desire from their own children's schools and the social purposes of education, many parents may simply not care about conventional measures of school quality, whether because those measures are flawed or parents focus on narrower private interests (Levin, 1991; Schneider, 2017). However, at minimum, many parents certainly say they care about academic quality (Stein, Goldring, & Cravens, 2010) and surveys indicate similar preferences for schools from parents and the public (Valant & Newark, 2016). A second possibility is that parents may wish they could send their children to high-performing schools but confront barriers to enrollment, such as complex application processes (Gross, DeArmond, & Denice, 2015), a lack of transportation availability (Jochim et al., 2014), capacity constraints in desirable schools (Lincove, Valant, & Cowen, 2018), and logistical and

financial considerations that supersede academic considerations (Harris & Larsen, 2019). These barriers may be especially daunting for families in poverty—a possible explanation for why disadvantaged families' school requests imply less weight on academic quality criteria (Hastings, Kane, & Staiger, 2009).

Another possibility is that many families lack relevant information while choosing schools. If this is the case, then studies using school requests to examine parents' revealed preferences might underestimate how much they value academic quality (Bergman, Chan, & Kapor, 2020; Yettick, 2016). The potential reasons behind information shortcomings are plentiful. Parents might struggle to find information (Jochim, A. et al., 2014; Schneider, Teske, & Marschall, 2000), or struggle to identify useful information amid the "smog" of data available (Shenk, 1997). The information they find might already be known to them, or might not feel relevant to their children, which could be a particular challenge for certain subgroups such as students with disabilities (SWDs) (McKittrick et al., 2020). Their use or interpretation of school quality information and school profiles may depend on how that information is formatted (Jacobsen, Snyder, & Saultz, 2014; Glazerman et al., 2020). Moreover, many parents prefer word-of-mouth information—the quality of which varies across social networks (Schneider et al., 1997)—and may not see much value in formal information reports (Schneider, Teske, & Marschall, 2000; Valant & Newark, 2020). Others may not engage in much of a school search at all (Bell, 2009).

A few experimental studies have sought to identify the effects of providing school choice information to families. Hastings and Weinstein (2008) randomly assigned families in Charlotte-Mecklenburg to receive, in hard copy, lists of schools ordered by their average math and reading scores (along with, for some families, odds of admission). The information modestly increased

the probability of choosing a high-scoring school, at least for some applicants. Corcoran et al. (2018) conducted a field experiment in New York City in which they randomly assigned 8th-grade students in high-poverty middle schools to receive lists of nearby high schools with relatively high graduation rates (with some students receiving additional information). The treatment led students to request more schools from the lists provided. Treatment group students did not request schools with higher graduation rates, on average, but were less likely to match to schools with low graduation rates and more likely to match to their first-choice schools. Allende, Gallego, and Neilson (2019) found that presenting families of public pre-K students in Chile with an informational video and materials led them to choosing higher-performing elementary schools—and led their children to score higher five years later. Quasi-experimental studies from the Netherlands (Koning & van der Wiel, 2013) and Chile (Mizala & Urquiola, 2013) have found null to modest effects of school quality ratings on choice behavior.

Research on the role of information in early childhood education (ECE) choice is more limited, but some recent work points to the potential for information interventions to affect parents' decisions. Information shortcomings and asymmetries shape many parents' perceptions of ECE program quality, sometimes even after enrolling in a program (Mocan, 2007). Over the last decade, most states have developed a Quality Rating and Improvement System (QRIS) to monitor the performance of ECE programs and communicate programs' ratings to parents (QRIS National Learning Network, 2017). Studies of these systems show that programs receiving lower ratings lose enrollment (Bassok, Dee, & Latham, 2019) and that providing rating information to parents can lead them to switch from parental to nonparental care (Herbst, 2018) and choose higher-performing options (Dechausay & Anzelone, 2016). Notably, these studies focus on private childcare programs, not school-based pre-K. Choosing a school-based pre-K program is

often a choice not only to attend a pre-K program but also the elementary school affiliated with that program. In this situation, parents may prioritize their preferences for the elementary program over any quality information specific to the pre-K.

Looking across these studies, we see reason to believe that families want and use information about schools, with potentially different responses from different groups of families (Corcoran et al., 2018; Hastings & Weinstein, 2008). We see less reason to believe that providing information is a surefire way to change behaviors or outcomes. Notably, though, how information interventions are designed and administered might shape their effects. For instance, the effects from Charlotte-Mecklenburg may have been mitigated if many families did not pay attention to their mail, or if reports of schools' average test scores—which, compared to student growth measures, tend to be more widely available, more highly correlated with student composition (Reardon, 2019), and less informative about school quality (Meyer, 1997)—did not lead to much updating of school choosers' opinions. The effects from New York City may have differed if the information had been directed to the parents of 8th-grade students, whose priorities might differ from those of their children (Ajayi, Friedman, & Lucas, 2017; Steinberg et al. 2009). It also could be that information affects families' school preferences but those effects are not evident in the data available to researchers (which may not show rank-ordered requests) or do not result in different school placements or student outcomes (e.g., because of capacity constraints in desirable schools).

We designed this study's treatments with the intent of approximating the strongest impact that one might realistically expect from an authentic, low-cost school choice information intervention. We partnered with a school district and provided information to all families for whom we had contact information—in a city in which choice is not restricted to the most

engaged families. We contacted families via US mail, text message, and email to increase the likelihood that we reached them and the number of ways in which they heard from us. For the performance group in particular, we provided simplified information with a useful metric (performance) that hardly any recipient likely knew before receiving our materials. We targeted materials to parents and guardians, in case adolescents' school preferences are erratic or unpredictable. Finally, we examined applicants' rank-ordered preferences, which provides a fuller picture of the interventions' effects on applicant behaviors and placements.

POLICY CONTEXT

School Choice in New Orleans

In the aftermath of Hurricane Katrina in 2005, the New Orleans public school system underwent drastic changes. The state of Louisiana quickly accelerated its efforts to take control of low-performing public schools in New Orleans, transferring control of the vast majority of schools from the city-controlled OPSB to the state-controlled Recovery School District (RSD). The RSD opted for a portfolio model whereby it would authorize, oversee, and provide support to schools while leaving school leaders with substantial operational autonomy. Rather than being assigned to schools based on where they live, families could request seats in schools across the city. Within a few years, what had been a conventional urban school district became the country's most decentralized, charter-based system.

The radical decentralization of the immediate post-Katrina school system created an assortment of problems, many of which related to the application and enrollment processes (Buerger & Harris, 2015). For example, families struggled to navigate a complex set of school-specific application processes. To address these problems, the city adopted a common application system that later evolved into the OneApp. It centralized both the application

process, by allowing parents to apply to multiple schools with one form, and the enrollment process, by assigning students to schools based on a placement algorithm.

OneApp

OneApp, like the broader school system in New Orleans, has evolved over time. At the time of this RCT (requests made for the 2019-20 school year), families could request up to 12 schools for K-12 grades and eight for early childhood, ranked in order of preference. The system had two rounds: the Main Round, when most requests and placements occurred (including virtually all placements in the most popular schools), and Round 2 (K-12 only) for families who did not get a placement or were not satisfied with the placement they received. After the OneApp was complete, OPSB also managed a process called Late Enrollment as a first-come, first-served option for schools with seats still available.

For K-12, families could request seats in virtually any public school in New Orleans through OneApp.[2] Income-eligible families could also request seats in private schools that participated in the Louisiana Scholarship Program, a statewide private school voucher program. Many schools gave priority access for a subset of their seats to students who lived in the school's geographic zone or within a half-mile walk zone. However, at least half of the open seats in every school did not offer any geographic priority. The algorithm also considered other priorities, such as having a sibling enrolled in the school. Importantly for the purposes of this paper, the algorithm was "strategy-proof" in the sense that applicants' had incentive to rank schools in their true order of preference rather than attempt to game the algorithm in some way (e.g., by ranking less-preferred, higher-probability schools first).

[2] A few public schools did not participate in OneApp at the time, choosing to manage their own enrollment process instead. We noted these schools in small print at the bottom of the performance and neighborhood group materials but did not include them in the lists of schools highlighted. Since the schools did not participate in OneApp, we do not have records of the applications they received.

For early childhood, families could request seats in an assortment of publicly funded programs, including school-based pre-Ks, Head Start programs, and private learning centers. These seats were available tuition-free only to families whose income did not exceed the qualifying limit. In nearly all schools, obtaining a seat in a school-based pre-K offered the additional perk of being guaranteed a seat in that school's kindergarten program in a subsequent year by virtue of being a continuing student. This early childhood choice process also happened through OneApp, with the need to demonstrate eligibility creating additional steps for applicants (Weixler et al., Forthcoming).

Application patterns from prior years suggest that information shortcomings may be an issue in New Orleans. In the year before our intervention, our data show that about half of Main Round applicants to entry grades (pre-K, kindergarten, and grade 9) applied to three or fewer programs, with 12 percent of the entry-grade applicants going unmatched. It is possible that these applicants were well informed and, for example, would only enroll in a few OneApp schools before looking for other options (e.g., private schools). However, earlier research on the OneApp found that most applicants who did not receive Main Round placements subsequently requested schools that they could have requested in the Main Round but did not (Harris, Valant, & Gross, 2015). Notably, too, many applicants do not request any high-performing, high-rated, or in-zone schools. For example, in the year prior to our intervention, only about one fourth of pre-K applicants and half of kindergarten applicants requested at least one high-performing school, while only about half of kindergarten applicants requested at least one school in their home geographic zone. These, too, could be intentional decisions that reflect parents' preferences. However, New Orleans has high rates of student mobility (a possible indicator of dissatisfaction), and applicants matched to a high-scoring or high-performing school in our data

were less likely than other students to switch schools before October 1.[3] It stands to reason that families could benefit from receiving more information about high-performing and close-to-home schools.

DATA

This study uses several datasets from OPSB and the Louisiana Department of Education (LDOE). With data from OneApp applications and school enrollments in prior years, the district identified the students who, with normal grade progression, would enter a four-year-old pre-K program, kindergarten, or ninth grade in the fall of 2019. This set of students formed our sample of potential pre-K, kindergarten, and ninth-grade applicants. We focused on these grades because these are the primary school entry grades in New Orleans (which has few standalone middle schools). Since the addresses and phone numbers in the district's database were recorded exactly as entered by parents, we dropped unusable records (e.g., phone numbers missing a digit) and corrected seemingly obvious mistakes in other records (e.g., slightly misspelled street names). We discarded mailing addresses known to be more than three years old due to concerns that the family might have moved since the data were collected.

After administering the study, we obtained deidentified, applicant-level OneApp records for the 2019-20 OneApp Main Round. These records included the schools that families in the study requested (ranked in order of preference), along with students' subsequent placements. These files also showed applicants' priority status, including any schools in which they were guaranteed a seat because they were enrolled in the prior year.

We merged these files with student-level records from LDOE from the 2018-19 school year (concurrent with the time of treatment being administered). We used the files to obtain

[3] High-rated refers to schools with A or B ratings in the state's School Performance Score system, which was primarily based on students' test score levels and, for high schools, graduation rates.

information on students' background characteristics. Specifically, we observe race/ethnicity, eligibility for free or reduced-price lunch (FRPL), special education status, and gender for students who had been enrolled in a Louisiana public school. For applicants who had not previously enrolled in a Louisiana public school, we supplemented LDOE's records with information available in the OneApp files. As part of the OneApp process, many parents reported their children's special education status and their family income (primarily to determine eligibility for publicly funded early childhood seats). We used a binary variable for special education status that includes all classifications except for gifted status.[4] For family income, we used LDOE's FRPL variable where available and supplemented it with FRPL eligibility that we calculated (as the district does) based on information that families reported about their household size and monthly income. The OneApp did not ask parents to report their children's race, ethnicity, or gender, so our only information on those characteristics comes from LDOE records.

Table 1 shows demographic information for students in our analytic sample, disaggregated by grade level and treatment condition. We have background information available for a little more than 80 percent of the incoming ninth grade students, since most of these students were enrolled in a Louisiana public school in the prior year.[5] Our background information is more limited for students entering kindergarten and pre-K, as many of these students were new to the public school system. Information on children's race/ethnicity and gender, which come only from LDOE records, is available for only about 17 percent of pre-K applicants and 61 percent of kindergarten applicants. Information on family income and special

[4] We identified students' special education status using LDOE records that the student had an Individualized Education Plan (IEP) and, in cases where that information was not available, whether parents reported on OneApp that their children had an IEP or Individualized Family Service Plan (IFSP).

[5] Ninth-grade students without demographic information had previously applied for a seat through OneApp but did not enroll, or did not have demographic data in an LDOE record that merged with their OneApp record.

education status is available for about three-fourths of pre-K applicants and 90 percent of kindergarten applicants.

Across these grade levels, large majorities of the students observed are black and eligible for free or reduced-price lunch. This reflects New Orleans having disproportionate numbers of students of color and students in poverty—disproportionate even to the city's youth population, since a large share of white and non-poor students in New Orleans attend private schools (Weixler et al., 2017). An especially large share of pre-K applicants come from low-income families, since family income is a criterion for most early childhood seats available in the OneApp.

Table 1 also illustrates that the treatment and control groups are well balanced with respect to observable student characteristics. Columns showing the differences in the background characteristics between the performance and control groups, and neighborhood and control groups, reveal no differences significant at $p<.05$.

STUDY DESIGN

The basic design of this study is a randomized controlled trial (RCT) in which we randomly assigned families to one of three conditions: performance (emphasizing schools with strong performance measures), neighborhood (emphasizing schools close to home), or control. We sent flyers, text messages, and emails to parents/guardians in accordance with their treatment or control group assignment.

Treatment Conditions

We opted for performance- and neighborhood-related treatments in order to address questions of interest to district leaders in New Orleans and the broader education research and policymaking communities. Testing a performance-related intervention was important in the

context of unanswered questions about why parents' school requests do not suggest more interest in schools with strong academic records. District leaders were especially curious about the impact of the state's newly released progress scores. These local leaders also encouraged us to test a neighborhood condition, because of ongoing uncertainty about whether parents are more interested in—and would be better served by—schools close to home or higher-performing schools that are farther away.[6]

The "performance" group received lists of the highest-performing schools in New Orleans, as well as a note that enrolling in schools where students learn a great deal is very important to some families.[7] For pre-K, we identified the ten programs (available tuition-free to eligible families) that received the highest scores according to the CLASS. LDOE has adopted CLASS as its primary early childhood assessment program. CLASS measures the quality of teacher-child interactions in early childhood classrooms, drawing from the assessments of trained observers (Mashburn et al., 2008). For kindergarten and ninth grade, we benefited from fortuitous timing in the state's first-ever release of school letter grades based only on student progress. These grades showed how schools performed on state-defined measures of student growth in English language arts, mathematics, science, and social studies (using, in part, Louisiana's value-added model; LDOE, 2017). LDOE had previously released school letter grades—and did so again in this school year—but had not released a grade specific to student growth. Appendix 1 shows how the schools in this study compared on these measures. In general, they received better grades on the new progress measure, and many schools looked

[6] Another benefit of the study design is that it may enable tests of the effects of choosing a high-performing school versus a school close to home (on outcomes such as student mobility and test scores). This will come from comparisons of the performance and neighborhood groups. However, these outcome data will be more difficult to collect and interpret than we anticipated because of disruptions from the COVID-19 pandemic.

[7] Because of limited information about families' backgrounds, we were only able to send flyers in English.

considerably different on this measure (2018 progress score) than they did on the measure that had been available prior to its release (2017 School Performance Score).[8] The performance group's communications highlighted all schools that received an "A" grade in student progress. This was 17 schools offering kindergarten and six schools offering ninth grade.[9] Private pre-K programs received CLASS scores but private schools did not receive LDOE student progress scores, so we included private pre-K programs offering publicly funded tuition-free seats, but not private schools, in the materials.

The "neighborhood" group received lists of schools in their home geographic zones, as well as a note that enrolling in a school close to home is very important to some families. OPSB had divided the city into seven geographic zones. It used these zones to determine geographic preferences for the subset of schools—and the subset of seats within those schools—that incorporated those preferences. Families in the neighborhood group received a complete list of schools in their zone. For the flyers, we identified their zone by the address that we had on file (the address to which we sent the flyers). For the text messages, we invited families to reply with their zip code in order to receive a message showing the schools in that zip code's geographic zone.[10] For the emails, we displayed a zone map and a list of all of the schools in each zone. We chose not to prepopulate the school lists on the text messages and emails because we expected that some of our mailing addresses were no longer current and we would be sending list of schools in a zone where the family no longer lives.[11] In total, across the seven geographic zones,

[8] The School Performance Score incorporated student growth among a broader set of factors such as high school graduation rates and proficiency levels on state assessments.
[9] A few schools were available only during the Early Window portion of the Main Round and therefore appeared in the flyers but not the emails or text messages (which arrived after the Early Window closed). One of the 17 kindergarten programs and one of the six high schools that appeared on the flyers' lists of high-performing schools did not appear on the email or text message lists.
[10] Approximately 100 parents replied to these messages with a zip code.
[11] For the purposes of this study, the district identified students' zones based on prior years' information, and our analysis examines whether applicants listed additional schools in their prior zone. We cannot identify whether

78 pre-K, 55 kindergarten, and 27 ninth-grade programs appeared in the neighborhood group's materials.[12]

The control group received a set of flyers, text messages, and emails parallel to those sent to the performance and neighborhood groups, but the control group materials did not identify or highlight any specific schools. Rather, these materials reminded families of the OneApp deadline and website. We did this to neutralize effects from the performance or neighborhood treatments that might arise from just reminding (or notifying) people of the deadline. Of course, members of the control group—like members of the performance and neighborhood groups—might have utilized information other than what we provided through this study. Many families likely used "informal" sources of information, such as word-of-mouth recommendations from friends. In New Orleans, "formal" sources of school information included the OPSB and LDOE websites and the OneApp itself. The OPSB website showed school profiles with an assortment of information that included the geographic zone, state letter grade (not the student progress grade shown on the intervention materials), and lists of school offerings. The LDOE website had profiles with more detail on the letter grades, including the progress scores, and less detail on the school programs. The Urban League of Louisiana published guides to support families choosing early childhood education programs and high schools and hosted its annual school exposition. The OneApp application portal—which applicants saw, although sometimes only to enter already-determined choices—contained information as well. All applicants saw a list of schools within their geographic zone and, if the applicant added a school to her list of requests, a few details about that school, including its growth letter grade and overall letter grade. Although most

applicants moved before submitting this application. However, of the 1,952 applicants in our study who received a zone preference to a school on their application, 86 percent still resided in their prior zone.

[12] These materials noted two additional kindergarten programs and three additional ninth-grade programs that did not participate in OneApp.

applicants only saw this information from the OneApp portal very late in the process, it could have mitigated the effects of the treatments.

Figure 1 displays examples of the flyers. These two-sided, color flyers were identical on one side for the performance, neighborhood, and control groups (within the same grade). The other side contained group-specific information. This included a list of high-performing schools and their scores for the performance group, and a map and list of in-zone schools for the neighborhood group. Figure 2 displays the emails. We designed these emails to mirror, in style and content, the flyers. Figure 3 displays the text messages. These, too, align with the content of the other materials, although the limitations of text message communication kept us from mirroring the style and color. All of these materials were addressed from the district, not the research team.

Randomization Process

We conducted the randomization using the set of cleaned mailing addresses. This meant that we attempted to mail a flyer to every family in the study. If that family did not have a valid phone number or email address, they would not receive that type of communication. Randomization occurred at the family level such that all children living at the same address would have the same treatment assignment. If a family had multiple children in the same grade, we sent a single flyer, text message, and email. If a family had children in multiple grades, we sent a separate flyer, text message, and email for each grade level (e.g., performance condition materials for both pre-K and kindergarten).

Modes of Communication

We timed our communications to coincide with the OneApp Main Round application window, which is the most active and important part of the school choice calendar in New

Orleans (e.g., when the most seats are available in high-demand schools). The Main Round application was open from November 19, 2018 through February 22, 2019. A few schools with uncommon application criteria such as language assessments required applications to be submitted by an "Early Window" deadline of January 11. We sent flyers on December 14 and December 17, emails on January 17, and text messages on January 22. We removed schools with Early Window deadlines from the lists of schools in the emails and text messages.

SAMPLE

Table 2 shows how many students the district attempted to contact for the study (by the various modes of communication) and how many students ultimately had a OneApp Main Round application. Across all grades, we attempted to contact the families of 9,829 students. Of those students, 7,265 appeared in the Main Round. Those who did not appear might have enrolled in private school without submitting a OneApp, waited until after the Main Round to attempt to obtain a seat, or moved out of New Orleans, among other possibilities. Of the students who did appear, 7,067 had usable application records (e.g., applied for the grade level we anticipated), meaning that about 72 percent of the students initially contacted ultimately appeared in our analytic sample.[13] The share was larger for high school applicants (85 percent) than kindergarten and pre-K applicants (70 percent and 58 percent, respectively).

We used mailing addresses as the basis of our randomization, so the district sent a flyer to all of these families. It sent a text message to 99 percent of them, and the full set of correspondences—flyer, text message, and email—to about 80 percent (email records were more limited, especially for potential high school applicants). Since we cleaned the mailing addresses, phone numbers, and email addresses before attempting contact, only a small number of

[13] Of these 7,067 students, 5,987 were the only child in the study sample in their household, 1,014 shared a household with one other child in the sample, 54 with two other children, and 12 with three other children.

attempted communications bounced back to the district. Our records indicate that fewer than one percent of the flyers and five percent of the emails failed to send.[14] However, we cannot know what percentage of the intended recipients actually received and read the materials.

Another 2,897 students appeared in the OneApp data without us initiating contact (e.g., because the applicants were new to New Orleans public schools).[15] These numbers reflect the challenges of identifying, in advance, which families will apply for an upcoming year and having usable contact information for those families. This is a particular challenge for families with young children who have not previously applied for, or enrolled in, a public school or ECE program. Still, even for pre-K and kindergarten, over two-thirds of all applicants in the OneApp Main Round had been contacted for the study.

While the differences between which families the district contacted and which families actually applied does not threaten the RCT's internal validity, it is worth considering their implications for external validity. It could be, for example, that families that recently moved to New Orleans are underrepresented in our analytic sample, or that families in the sample are unusually familiar with the OneApp process and New Orleans schools (having applied or enrolled before). If the study's informational materials have stronger effects on families less familiar with their options, we might expect larger effects from the broader population of applicants than from this study sample. Additionally, a disproportionate share of the OneApp's early childhood seats had been available only to families living below the federal poverty line (typically those applying to Head Start programs). As a result, the district's contact information

[14] We do not have data on the bounce-back rates of the text messages.
[15] This excludes students who appeared in our dataset but did not have a OneApp ID number because they had applied for private schools outside of New Orleans via the Louisiana Scholarship Program.

for potential pre-K and kindergarten applicants likely consisted of a disproportionate number of families in poverty.

ANALYSIS

As a randomized controlled trial with two distinct treatment conditions and a control condition, the empirical models for this analysis are straightforward. We obtain intent-to-treat (ITT) estimates of the effects of disseminating information about schools. Although we have some data related to how many messages were returned to sender (very few), we cannot observe the consumption of these materials in a way that would allow us to estimate treatment-on-the-treated (TOT) effects.

We examine an assortment of outcome variables related to the structure of applicants' ranked requests and subsequent placements. For each treatment, we are especially interested in whether applicants requested and received placements in the schools presented to them (high-performing schools for the performance treatment; in-geographic-zone schools for the neighborhood treatment).[16] We examine both the average number of highlighted schools that applicants requested and the proportion of applicants that requested at least one highlighted school. While we might see effects on the former outcome from a small number of applicants reacting strongly to the materials provided (i.e., requesting several additional highlighted schools), we likely would not see effects on the latter outcome unless a large number of applicants acted on the information provided.

[16] An alternate approach would be to include performance and neighborhood treatment indicators in the same model. We opted against this for presentation clarity, since we focus on performance-related outcomes for the performance treatment and neighborhood-related outcomes for the neighborhood treatment. The results from these models are substantively identical to those presented (with very little change in precision). We do not see evidence that the performance treatment affected the neighborhood-related outcomes or vice versa.

We also test whether the information led applicants to change their first-choice request to one of the listed schools. First-choice requests were especially important, since the OneApp's strategy-proof algorithm considered applicants for their first-choice requests before moving to their second choice and subsequent requests. Additionally, we test whether the intervention led applicants to request more schools overall—regardless of whether they came from the list of highlighted schools—to assess whether additional requests for highlighted schools seem to supplement or replace other school requests. Finally, we examine applicants' Main Round school placements.

Our model for estimating ITT effects is:

$$Y_{ij} = \alpha + \beta\left(Treat_j\right) + \theta(Block_j) + X_{ij}\delta + \varepsilon_{ij} \qquad (1)$$

where Y_{ij} represents the outcome for student *i* in family *j* (with family defined as students living at the same address), and $Treat_j$ indicates whether the student was in the treatment group (the performance group for performance-control comparisons; the neighborhood group for neighborhood-control comparisons). We include randomization-block fixed-effects, $Block_j$, to account for the fact that we conducted randomization separately for each of the seven geographic zones. We run models with and without a vector of covariates, represented by X_{ij}. These covariates consist of students' race/ethnicity, family income (based on eligibility for free or reduced-price lunch), special education status, gender, and grade level. In models with covariates, we code missing data as a distinct category to keep from dropping students with missing background information. We cluster the standard errors by family (home address) to account for some students in the study being members of the same family and therefore sharing the same treatment assignment.

In addition to presenting estimated effects for the full sample, we present estimates for notable subsamples. We show results for applicants who did not have a guaranteed seat in any school. Typically, students currently enrolled in a school—for example, students in a school-based pre-K that offers kindergarten—would have a guaranteed seat in that school in the following year. We disaggregate results for this group because applicants without a guaranteed seat might be less certain about their plans or familiar with their options, and therefore more likely to use the study's informational materials. We also test for heterogeneity in treatment effects across students of different backgrounds. We do this in two ways. First, we test for significant interactions between student background characteristics and treatment status for each of the demographic variables, treatments, and outcomes. (These results appear in the Results section text but not tables.) Second, we report treatment effects disaggregated by students' family income, special education status, race/ethnicity, and gender.

For ease of interpretation, we describe results from OLS regression models even for dependent variables with binary outcomes (and report results from logistic regression models in an appendix table). The OLS and logit models produce similar results in sign and significance.

RESULTS

We examine how the performance treatment affected performance-related outcomes and the neighborhood treatment affected neighborhood-related outcomes.[17] Specifically, we assess: whether applicants requested any of the (high-performing or in-zone) schools listed in the informational materials they received, how many of the listed schools they requested, whether their first-choice school was listed, and whether they were assigned to a listed school. In general,

[17] We see little evidence that the performance treatment affected neighborhood-related outcomes or the neighborhood treatment affected performance-related outcomes.

we find the strongest effects on whether applicants requested listed schools and how many of those schools they requested.

Interpreting these findings would have been more complicated if the treatments affected whether families submitted a OneApp. That was not the case. We observed no significant differences in the probabilities that applicants assigned to the performance, neighborhood, and control conditions submitted a Main Round application.[18] If the performance and neighborhood materials affected the probability that families would submit an application in the first place (e.g., by reminding parents of the upcoming deadline), it appears that the control materials succeeded in neutralizing that effect.

Full Sample (and by Grade Level)

Table 3 provides the estimated effects of the performance treatment (comparing performance to control). Results are presented in separate columns for models with and without covariates, and separate panels for the full sample of applicants (Panel A) and the subset that did not have a guaranteed seat in any school (Panel B). Although one might expect more precise estimates from models with covariates and stronger effects for students without a guaranteed seat (who must choose a new school and might be more drawn to informational materials), the results are similar. Given that, this section presents results from the most straightforward analyses: full-sample results from models without covariates.

Across all grade levels, the performance treatment led to a 2.7 percentage point (4.7 percent) increase in the probability of requesting at least one high-performing school ($p<.1$). It led applicants to request 0.09 (8.5 percent) more high-performing schools on average ($p<.05$),

[18] Specifically, 2,450 of 3,284 (74.6 percent) of those assigned to the performance group, 2,402 of 3,281 (73.2 percent) of those assigned to the neighborhood group, and 2,413 of 3,264 (73.9 percent) of those assigned to the control group had a Main Round application record. None of the differences between groups is statistically significant.

while requesting about 0.2 (4.9 percent) more schools overall ($p<.1$). These effects came largely from high school applicants. High school applicants in the performance group were 3.8 percentage points (4.7 percent) more likely to request at least one high-performing school, requested 0.12 (9.0 percent) more high-performing schools on average, and were 4.1 percentage points (16.5 percent) more likely to be assigned to a high-performing school ($p<.05$ in all cases). We do not observe statistically significant effects, for the full sample or any particular grade, on the probability of requesting a high-performing school as the applicant's first choice. We also do not observe any significant effects on the applications for pre-K or kindergarten seats.

Table 4 presents a parallel set of estimates for the neighborhood treatment (comparing neighborhood to control). Here, too, the results are similar regardless of whether the models include covariates and the sample is restricted to students without a guaranteed seat. We see very little impact from the neighborhood treatment. The only significant effect is on the probability that treated students requested at least one in-zone school. For the full sample, the neighborhood treatment led to a 2.8 percentage point (4.2 percent) increase in the probability of requesting at least one in-zone school ($p<.05$). By grade level, this was statistically significant only for kindergarten applicants ($p<.1$), although we observe positive coefficients for pre-K and ninth grade as well.

Appendix 2 shows results from logistic regression models for the binary outcome variables (requested high-performing/in-zone school, first choice was high-performing/in-zone school, and placed in high-performing/in-zone school). These estimates, which we report in odds ratios, are very similar in sign and significance to the results from Tables 3 and 4.

Heterogeneity of Effects

Next, we assess possibility heterogeneity in treatment effects across student subgroups. The specific subgroups we examine—by family income, special education status, race/ethnicity, and gender—correspond to the groups reported in Table 1, except where sample sizes are too small to have meaningfully precise estimates. In general, we find relatively consistent effects across subgroups with one notable exception: the performance treatment had very strong effects on the application behaviors of families of students with disabilities.

We first tested for statistically significant interactions between students' background characteristics and treatment status, looking separately at each treatment and outcome. Every interaction coefficient that is significant at $p<.05$ comes from the comparison of students with and without disabilities. Relative to families of students without disabilities, families of SWDs reacted more strongly to the performance treatment in the probability of requesting at least one high-performing school ($p<.05$), the number of high-performing schools requested ($p<.05$), and the total number of schools requested ($p<.01$). The neighborhood treatment also produced a stronger effect on the total number of schools requested by families of SWDs relative to other families ($p<.05$).

Table 5 shows the effects of the performance treatment (Panel A) and neighborhood treatment (Panel B) disaggregated by subgroups. In this table, unlike the analyses described in the preceding paragraph, asterisks indicate that the treatment estimate for a particular subgroup is statistically significant from zero—not that it is significantly different from the treatment estimates for other subgroups.

Here, too, the most striking effects come from families of SWDs. The performance treatment produced a 12.6 percentage point (23.7 percent) increase in the probability that

families of SWDs requested at least one high-performing school ($p<.01$). On average, it increased the number of high-performing schools requested by 0.41 (45.3 percent), and the total number of schools requested by 1.15 (30.2 percent). All of these effects were significant at $p<.01$. It also led to a 7.3 percentage point (45.4 percent) increase in the probability that SWDs were assigned to a high-performing school ($p<.1$). As we discuss in the Conclusion, these strong effects for families of SWDs may reflect that information about student growth is especially useful for these families relative to more commonly available information about student proficiency rates.

Aside from the differences by special education status, other evidence of heterogeneity is limited. We observe more statistically significant effects for black students than white or Hispanic students, but this might just reflect that the sample of black students is about ten times as large as each of the other samples. Tests of the interactions between student race/ethnicity and treatment status do not produce statistically significant effects on any outcomes. Table 5 also suggests potentially stronger effects on male students than female students (especially from the performance treatment). Again, however, we do not observe statistically significant interactions, and the fact that a disproportionate number of male students have SWD status may explain the gender differences visible in Table 5.

Finally, it is notable that even when disaggregating results we have little evidence of effects from the neighborhood treatment. We see some evidence that families of SWDs used the neighborhood group materials—and that families of SWDs were more likely to receive a placement in an in-zone school ($p<.1$)—but the subgroup effects for the neighborhood treatment are consistently modest or insignificant.

CONCLUSION

Market-based theories of school choice assume that families have and use high-quality information. If families are uninformed or misinformed about their options, or how the choice process works, they might not request and receive seats in the schools they would prefer. This could help to explain differences between applicants' stated preferences (which suggest an intense focus on academic quality) and revealed preferences (which do not). Limitations in information quality and availability could be undermining choice processes in early childhood and K-12 education—with negative consequences for students and school choice markets.

For this study, we partnered with school district leaders in New Orleans to conduct a school choice information RCT. We attempted to design and administer the treatments in ways that address the various challenges related to informing families (and measuring the effects). We sent materials directly to families using multiple modes of communication—US mail, email, and text messages. The information was simple in format but, in the case of the performance treatment, more reflective of program quality than most other information available. Due to fortuitous timing, the performance information was essentially new to all families, with potential to lead many parents to update their impressions of schools. Moreover, our data allow us to observe the full structure of applicants' preferences, from their top choice through their 12th choice, and disaggregate by students' background characteristics.

We found significant intent-to-treat effects from the performance treatment, with the performance group more inclined that the control group toward high-performing schools. However, those effects were highly concentrated among certain subsets of families and on certain types of outcomes. The effects were stronger for high school applicants than early childhood and elementary school applicants (for whom we saw little impact). For example, high school applicants in the performance group were 3.8 percentage points (4.7 percent) more likely

to request a high-performing school, requested 0.12 (9 percent) more high-performing schools on average, and were 4.1 percentage points (16.5 percent) more likely to be assigned to a high-performing school, compared to control group applicants.

However, the largest, most notable effects were for families of students with disabilities. For these families, being in the performance group increased the probability of requesting at least one high-performing school by 12.6 percentage points (23.7 percent). It also produced a 45 percent increase in both the number of high-performing schools requested and the probability of being placed in a high-performing school. The strong reaction from families of SWDs seems consistent with recent qualitative research indicating that these families face particular challenges in finding relevant information about schools (McKittrick et al., 2020). For example, parents of SWDs might be especially interested in seeing where students grow the most, regardless of those students' starting points, especially for SWDs who may be well below grade level. The performance materials from this intervention spoke more directly to that question than most information that had been publicly available. The results from this study suggest the possibility of unmet needs in the school choice information available to families of SWDs.

The neighborhood treatment had minimal effects, although it produced a 2.8 percentage point (4.2 percent) increase in the probability that applicants requested at least one school in their home geographic zone. We expected more modest effects from the neighborhood treatment than the performance treatment, since the neighborhood information did not seem as novel or meaningful—and many parents are likely familiar with the schools close to home. Still, the contrast between the performance and neighborhood treatment effects serves as a useful reminder that families do not want, and react to, just any information about schools. They want information that addresses their needs and priorities.

One intriguing finding from this study is that while we generally found significant effects on whether applicants requested any of the highlighted schools, we did not find significant effects on their first-choice requests. This was the case even for student with disabilities. It could be that families are much clearer about their top choices than the schools that come lower on their lists. If that is the case, then informational materials such as these could change parents' requests without changing their placements, especially if most families are placed in their first-choice option.

Another intriguing finding is that the effects were so concentrated among high school applicants. Prior research suggests that high school applicants respond in unpredictable ways to school choice information, perhaps because of the role that children play in choosing their own high schools (Ajayi, Friedman, & Lucas, 2017; Corcoran et al., 2018; Valant, 2014). Future research could help to reveal the mechanisms by which this information operates. Researchers tend to describe school choice information as filling gaps in families' knowledge about schools and the choice process. Applicants, in other words, have preferences and alternatives, and information reveals to them which alternatives best satisfy their preferences. However, this is not the only way that information can affect choices. For example, presenting information that emphasizes academic quality could lead applicants to weigh academic quality more heavily (e.g., if they see a signal of what they should value). This reconfiguration of their decision criteria could result in different choices even if their opinions of individual schools are unaffected. Another possibility is that directing school choice materials to parents might have shifted the balance of some intra-family decisionmaking from children to their parents. If parents have different preferences than their children (e.g., greater concern about school academic

performance), then a change in their roles in the decision-making process could yield different choices even without changes to any individual's knowledge or criteria.

Although we cannot be certain about why the information had little effect on pre-K and kindergarten applications, some explanations seem plausible. Parents selecting pre-K programs in New Orleans are in many cases selecting their child's school through eighth grade, and for that reason may have little interest in choosing based on a pre-K-specific rating. Many of the New Orleans programs that scored highly on the state's pre-K rating are not located in popular elementary schools, and parents may not have been willing to risk an undesirable elementary school placement for a more favorable pre-K placement. Additionally, parents selecting both pre-K and kindergarten programs have practical considerations (e.g., transportation, distance, and sibling enrollment) that can weigh heavier for a parent of young children as compared to a parent of a high school student, and may limit the role of school quality in decision making (e.g., Harris and Larsen, 2019).

With so much variation in school choice policies and settings, it is important to consider the generalizability of any information intervention. New Orleans has a choice-based system without attendance boundaries that essentially requires all families to choose schools. On one hand, this suggests the families in this study might be especially adept and experienced with school choice, and perhaps unusually well informed about their options. On the other hand, the universality of school choice in New Orleans means that this intervention is not speaking to only the most engaged, savvy, or disillusioned families in the city—a subset of active choosers we might expect to find in other cities. Rather, the results come from a large proportion of the city's public school population. Other considerations for generalizability include whether a city has a

unified enrollment system with rank-ordered applications (as New Orleans does) and what types of options, and undersubscribed options, families have available.

Stepping back, we see this RCT's results as further demonstration that providing information about academic quality can affect applicant behaviors, especially for certain groups of families—and that information interventions can affect how citizens make decisions about market-oriented government services. At the same time, we see reason to be realistic about how much influence these types of low-cost information inventions will have, and how likely they are to have a major impact on school choice markets. This holds even truer for more passive efforts to disseminate information, such as placing content on websites that many applicants will never visit. The reality is that school-choosing families—and particularly families in poverty—confront barriers that extend well beyond information issues. These include transportation challenges, burdensome application processes, and an inadequate supply of seats in desirable schools. Information can only go so far if that information serves to make families aware of the schools in which they cannot, or would not wish to, enroll their children.

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Table 1. Descriptive Statistics and Randomization Balance.

	Full sample			Pre-K			Kindergarten			Grade 9		
	Control	Perform. - Control	Neigh. - Control	Control	Perform. - Control	Neigh. - Control	Control	Perform. - Control	Neigh. - Control	Control	Perform. - Control	Neigh. - Control
Free/Reduced lunch												
Yes	0.625	-0.017	0.003	0.656	-0.024	0.011	0.657	-0.040*	-0.019	0.578	0.011	0.017
		(0.015)	(0.015)		(0.030)	(0.029)		(0.024)	(0.023)		(0.023)	(0.023)
No	0.199	0.005	-0.004	0.096	0.026	-0.009	0.234	0.020	0.011	0.228	-0.018	-0.012
		(0.012)	(0.012)		(0.019)	(0.018)		(0.022)	(0.021)		(0.019)	(0.020)
Unknown	0.176	0.012	0.000	0.248	-0.002	-0.002	0.109	0.020	0.007	0.195	0.007	-0.005
		(0.011)	(0.011)		(0.027)	(0.027)		(0.016)	(0.015)		(0.018)	(0.019)
Special education												
Yes	0.085	0.011	-0.008	0.068	0.004	-0.007	0.081	-0.007	-0.015	0.097	0.027*	-0.001
		(0.009)	(0.008)		(0.016)	(0.015)		(0.014)	(0.013)		(0.014)	(0.014)
No	0.752	-0.023*	0.007	0.699	-0.007	0.012	0.817	-0.013	0.005	0.723	-0.032	0.007
		(0.013)	(0.013)		(0.029)	(0.028)		(0.020)	(0.019)		(0.021)	(0.021)
Unknown	0.163	0.011	0.000	0.233	0.004	-0.005	0.101	0.019	0.010	0.180	0.004	-0.006
		(0.011)	(0.011)		(0.026)	(0.026)		(0.016)	(0.015)		(0.018)	(0.018)
Race/Ethnicity												
Black	0.486	0.003	-0.015	0.129	-0.002	-0.014	0.473	0.008	-0.014	0.703	-0.019	-0.007
		(0.015)	(0.015)		(0.021)	(0.020)		(0.025)	(0.024)		(0.021)	(0.022)
White	0.052	0.004	0.002	0.028	-0.006	-0.010	0.070	0.005	0.005	0.049	0.009	0.007
		(0.007)	(0.007)		(0.010)	(0.009)		(0.013)	(0.013)		(0.010)	(0.010)
Hispanic	0.042	0.002	0.008	0.006	0.000	0.002	0.051	0.006	0.015	0.054	0.000	0.007
		(0.006)	(0.006)		(0.005)	(0.005)		(0.011)	(0.012)		(0.010)	(0.011)
Other	0.013	0.003	0.002	0.006	-0.006*	0.002	0.017	0.002	0.006	0.013	0.008	-0.002
		(0.003)	(0.003)		(0.003)	(0.005)		(0.007)	(0.007)		(0.006)	(0.005)
Unknown	0.408	-0.012	0.003	0.832	0.013	0.020	0.388	-0.021	-0.013	0.182	0.002	-0.005
		(0.014)	(0.015)		(0.023)	(0.023)		(0.024)	(0.024)		(0.018)	(0.018)
Gender												
Female	0.309	-0.001	-0.010	0.087	-0.017	-0.020	0.306	0.014	0.005	0.438	-0.017	-0.012
		(0.014)	(0.014)		(0.017)	(0.016)		(0.023)	(0.023)		(0.022)	(0.023)
Male	0.285	0.012	0.008	0.079	0.006	0.006	0.309	0.005	0.005	0.381	0.013	0.017
		(0.013)	(0.013)		(0.017)	(0.017)		(0.023)	(0.023)		(0.022)	(0.023)
Unknown	0.406	-0.011	0.002	0.834	0.011	0.014	0.385	-0.019	-0.009	0.181	0.003	-0.006
		(0.014)	(0.015)		(0.023)	(0.023)		(0.024)	(0.024)		(0.018)	(0.018)
Observations	2,346	4,733	4,680	541	1,082	1,094	860	1,664	1,717	945	1,987	1,869

Note. Standard errors appear in parentheses. Table shows results of OLS regressions conducted at student level with standard errors clustered by home address. All values reported as proportions (of students). Special education classifications include students with an Individualized Education Program (IEP) or Individualized Family Service Plan (IFSP) but exclude those with gifted status. *p<.10; **p<.05; ***p<.01.

Table 2. Sample Sizes by Grade.

	Total	Pre-K	K	Grade 9
Contacted for study	9,829	2,795	3,598	3,436
Submitted OneApp in Main Round	7,265	1,819	2,531	2,915
Had usable application data	7,067	1,635	2,521	2,911
District sent flyer	7,067	1,635	2,521	2,911
District sent flyer and text message	6,986	1,621	2,495	2,870
District sent flyer, text message, and email	5,626	1,599	2,461	1,566
Not contacted for study but applied	2,897	809	1,231	857

Note. Sample sizes reported in number of students. Students with and without guaranteed seats included in sample. Analytic sample consists of those in "Had usable application data" row.

Table 3. Effects of Performance Treatment.

	Control				Performance minus control (no covariates)				Performance minus control (covariates)			
	All	Pre-K	K	Grade 9	All	Pre-K	K	Grade 9	All	Pre-K	K	Grade 9
Panel A. All students												
Any high-perf. schools requested	0.579	0.216	0.555	0.807	0.027*	-0.029	0.018	0.038**	0.019	-0.028	0.021	0.039**
					(0.015)	(0.024)	(0.025)	(0.017)	(0.013)	(0.024)	(0.024)	(0.017)
# of high-perf. schools requested	1.06	0.29	1.23	1.34	0.09**	-0.02	0.10	0.12**	0.09**	-0.02	0.12	0.12**
	[1.33]	[0.63]	[1.72]	[1.01]	(0.04)	(0.04)	(0.09)	(0.05)	(0.04)	(0.04)	(0.08)	(0.05)
First choice is high-perf. school	0.299	0.055	0.394	0.355	0.008	-0.002	-0.010	0.022	0.007	0.000	-0.009	0.020
					(0.014)	(0.014)	(0.024)	(0.021)	(0.013)	(0.014)	(0.024)	(0.021)
Assigned to high-performing school	0.233	0.070	0.319	0.248	0.014	0.012	-0.022	0.041**	0.015	0.014	-0.022	0.040**
					(0.012)	(0.016)	(0.022)	(0.020)	(0.012)	(0.016)	(0.022)	(0.020)
Total schools requested (#)	4.09	3.27	3.19	5.37	0.20*	-0.22	0.28	0.19	0.15	-0.26	0.33**	0.20
	[3.42]	[2.68]	[3.40]	[3.42]	(0.10)	(0.17)	(0.17)	(0.16)	(0.10)	(0.17)	(0.16)	(0.16)
Observations (max.)	2,346	541	860	945	4,733	1,082	1,664	1,987	4,733	1,082	1,664	1,987
Panel B. Students without guaranteed seat												
Any high-perf. schools requested	0.745	0.370	0.828	0.840	0.022	-0.052	0.015	0.030*	0.013	-0.047	0.017	0.029*
					(0.015)	(0.038)	(0.027)	(0.016)	(0.014)	(0.038)	(0.028)	(0.016)
# of high-perf. schools requested	1.43	0.50	2.14	1.42	0.09*	-0.04	0.12	0.11**	0.09*	-0.03	0.14	0.10**
	[1.42]	[0.76]	[2.01]	[0.99]	(0.05)	(0.06)	(0.14)	(0.05)	(0.05)	(0.06)	(0.14)	(0.05)
First choice is high-perf. school	0.342	0.093	0.497	0.359	0.016	-0.007	0.006	0.021	0.012	-0.003	0.008	0.018
					(0.017)	(0.024)	(0.036)	(0.022)	(0.016)	(0.025)	(0.035)	(0.022)
Assigned to high-performing school	0.245	0.118	0.334	0.247	0.029*	0.020	-0.007	0.044**	0.028*	0.024	-0.009	0.044**
					(0.015)	(0.027)	(0.032)	(0.021)	(0.015)	(0.028)	(0.032)	(0.021)
Total schools requested (#)	5.44	4.87	5.35	5.69	0.12	-0.31	0.28	0.14	0.10	-0.30	0.30	0.13
	[3.29]	[2.45]	[3.68]	[3.33]	(0.12)	(0.21)	(0.26)	(0.16)	(0.12)	(0.21)	(0.26)	(0.16)
Observations (max.)	1,623	314	428	881	3,335	618	851	1,866	3,335	618	851	1,866

Note. Standard deviations appear in brackets. Standard errors appear in parentheses. Table shows results of OLS regressions conducted at student level with standard errors clustered by home address. All values reported as proportions unless otherwise indicated (with number sign). Covariates consist of race/ethnicity, family income (FRPL status), special education status, gender, and grade level.
*p<.10; **p<.05; ***p<.01.

Table 4. Effects of Neighborhood Treatment.

	Control				Neighborhood minus control (no covariates)				Neighborhood minus control (covariates)			
	All	Pre-K	K	Grade 9	All	Pre-K	K	Grade 9	All	Pre-K	K	Grade 9
Panel A. All students												
Any in-zone schools requested	0.661	0.637	0.635	0.697	0.028**	0.013	0.041*	0.027	0.028**	0.016	0.040*	0.026
					(0.014)	(0.030)	(0.023)	(0.020)	(0.014)	(0.030)	(0.023)	(0.020)
# of in-zone schools requested	1.21	1.34	1.21	1.13	0.06	0.01	0.08	0.06	0.06	0.02	0.08	0.06
	[1.41]	[1.64]	[1.60]	[1.02]	(0.04)	(0.10)	(0.08)	(0.04)	(0.04)	(0.10)	(0.08)	(0.04)
First choice is in-zone school	0.444	0.498	0.532	0.336	0.006	0.014	0.012	-0.006	0.003	0.015	0.008	-0.007
					(0.015)	(0.032)	(0.025)	(0.020)	(0.014)	(0.032)	(0.025)	(0.020)
Assigned to in-zone school	0.411	0.451	0.472	0.333	0.003	-0.014	0.023	-0.009	0.000	-0.015	0.018	-0.009
					(0.014)	(0.030)	(0.024)	(0.021)	(0.014)	(0.030)	(0.024)	(0.021)
Total schools requested (#)	4.09	3.27	3.19	5.37	0.03	0.07	-0.01	0.07	0.06	0.11	0.04	0.06
	[3.42]	[2.68]	[3.40]	[3.42]	(0.10)	(0.17)	(0.16)	(0.16)	(0.10)	(0.17)	(0.16)	(0.16)
Observations (max.)	2,346	541	860	945	4,680	1,094	1,717	1,869	4,680	1,094	1,717	1,869
Panel B. Students without guaranteed seat												
Any in-zone schools requested	0.730	0.774	0.719	0.719	0.021	-0.017	0.043	0.019	0.020	-0.014	0.036	0.018
					(0.015)	(0.033)	(0.030)	(0.020)	(0.015)	(0.033)	(0.030)	(0.020)
# of in-zone schools requested	1.51	1.96	1.84	1.19	0.06	-0.09	0.10	0.05	0.05	-0.07	0.06	0.05
	[1.54]	[1.82]	[1.99]	[1.02]	(0.05)	(0.14)	(0.13)	(0.05)	(0.05)	(0.14)	(0.13)	(0.05)
First choice is in-zone school	0.414	0.537	0.499	0.335	-0.003	-0.008	0.002	-0.007	-0.005	-0.005	0.001	-0.007
					(0.017)	(0.040)	(0.035)	(0.020)	(0.017)	(0.040)	(0.036)	(0.020)
Assigned to in-zone school	0.373	0.462	0.383	0.336	-0.004	-0.054	0.031	-0.010	-0.006	-0.054	0.023	-0.010
					(0.017)	(0.038)	(0.033)	(0.022)	(0.017)	(0.038)	(0.033)	(0.022)
Total schools requested (#)	5.44	4.87	5.35	5.69	-0.04	-0.02	-0.11	0.01	-0.03	-0.01	-0.12	-0.01
	[3.29]	[2.45]	[3.68]	[3.33]	(0.12)	(0.20)	(0.25)	(0.16)	(0.12)	(0.20)	(0.25)	(0.16)
Observations (max.)	1,623	314	428	881	3,272	651	865	1,756	3,272	651	865	1,756

Note. Standard deviations appear in brackets. Standard errors appear in parentheses. Table shows results of OLS regressions conducted at student level with standard errors clustered by home address. All values reported as proportions unless otherwise indicated (with number sign). Covariates consist of race/ethnicity, family income (FRPL status), special education status, gender, and grade level.
*p<.10; **p<.05; ***p<.01.

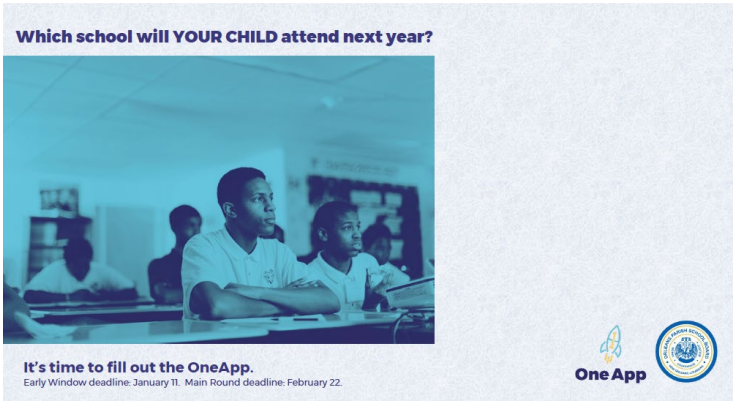
Table 5. Heterogeneity of Effects.

	Free/Reduced lunch		Special education		Race/Ethnicity			Gender	
	Yes	No	Yes	No	Black	White	Hispanic	Female	Male
Panel A. Performance treatment and outcomes									
Any high-performing schools requested	0.022	0.015	0.126***	0.013	0.042**	0.047	0.041	0.027	0.058**
	(0.019)	(0.031)	(0.049)	(0.017)	(0.020)	(0.062)	(0.069)	(0.025)	(0.026)
# of high-performing schools requested	0.07	0.02	0.41***	0.04	0.14***	0.17	-0.12	0.08	0.17**
	(0.05)	(0.10)	(0.13)	(0.05)	(0.05)	(0.18)	(0.16)	(0.06)	(0.07)
First choice is high-performing school	0.007	-0.017	0.014	0.003	0.004	0.039	0.068	0.025	0.003
	(0.017)	(0.031)	(0.045)	(0.016)	(0.019)	(0.061)	(0.069)	(0.024)	(0.026)
Assigned to high-performing school	0.008	0.021	0.073*	0.007	0.015	0.024	0.036	0.014	0.028
	(0.016)	(0.029)	(0.038)	(0.015)	(0.018)	(0.058)	(0.072)	(0.023)	(0.025)
Total schools requested (#)	0.18	0.11	1.15***	0.07	0.35**	0.43	-0.33	0.11	0.50***
	(0.13)	(0.22)	(0.35)	(0.12)	(0.15)	(0.39)	(0.49)	(0.19)	(0.19)
Observations (max.)	2,918	956	428	3,505	2,309	253	203	1,458	1,379
Panel B. Neighborhood treatment and outcomes									
Any in-zone schools requested	-0.016	0.039	0.048	-0.007	0.021	0.067	-0.046	0.013	0.015
	(0.018)	(0.032)	(0.052)	(0.017)	(0.020)	(0.063)	(0.066)	(0.025)	(0.026)
# of in-zone schools requested	-0.06	0.16	0.19	-0.02	0.05	0.16	-0.15	0.05	0.04
	(0.05)	(0.11)	(0.14)	(0.05)	(0.05)	(0.18)	(0.17)	(0.06)	(0.06)
First choice is in-zone school	-0.009	0.015	0.014	-0.005	-0.008	0.072	-0.063	0.011	-0.016
	(0.016)	(0.032)	(0.047)	(0.015)	(0.019)	(0.064)	(0.067)	(0.024)	(0.025)
Assigned to in-zone school	-0.009	-0.003	0.077*	-0.019	0.005	-0.001	-0.083	0.014	-0.011
	(0.015)	(0.029)	(0.041)	(0.014)	(0.018)	(0.060)	(0.067)	(0.023)	(0.024)
Total schools requested (#)	-0.05	0.36	0.79**	-0.02	0.20	0.65	-0.50	-0.06	0.35*
	(0.13)	(0.23)	(0.37)	(0.12)	(0.15)	(0.45)	(0.48)	(0.19)	(0.19)
Observations (max.)	2,932	925	379	3,536	2,242	246	215	1,421	1,353

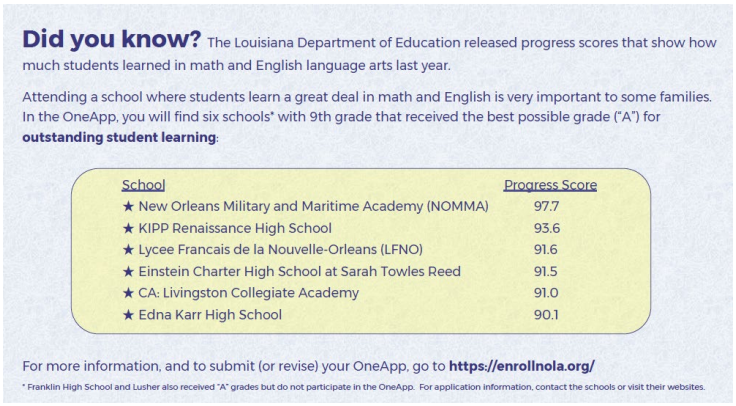
Note. Standard errors appear in parentheses. Table shows results of OLS regressions conducted at student level with standard errors clustered by home address. All values reported as proportions unless otherwise indicated (with number sign). Students with and without guaranteed seats included in sample. Models do not include covariates.

*p<.10; **p<.05; ***p<.01.

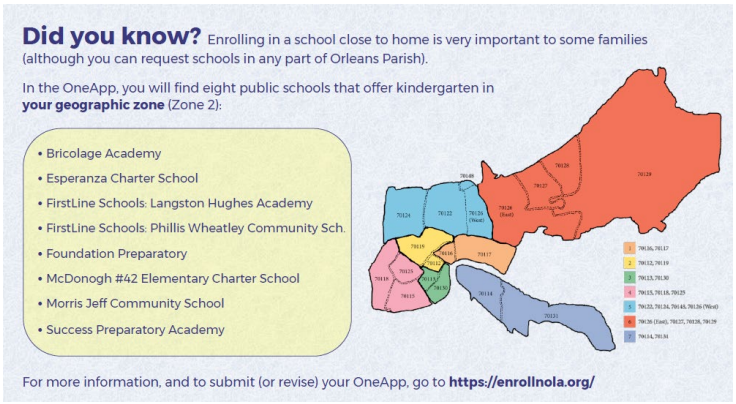
Figure 1. Samples of Flyers (Ninth Grade).



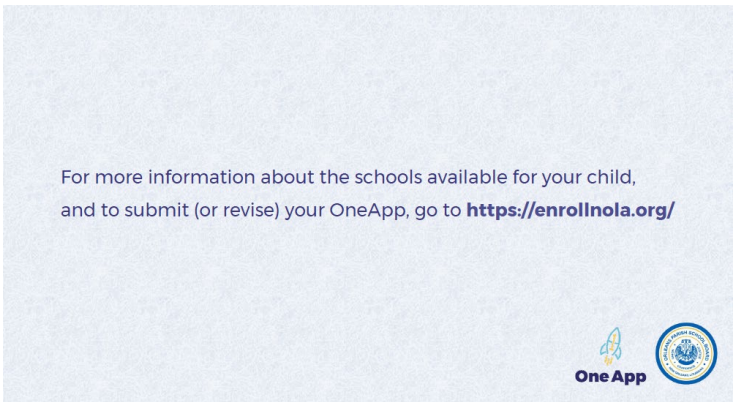
Front of flyer—All conditions



Back of flyer—Performance condition



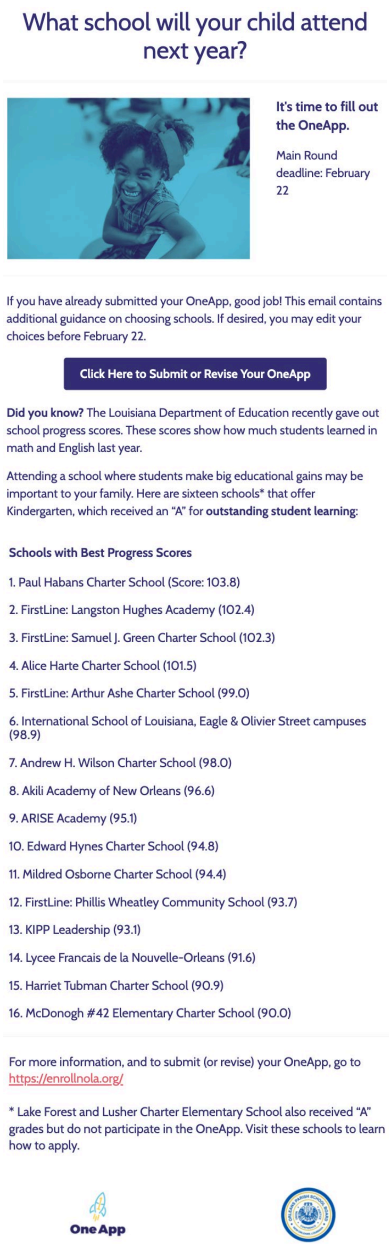
Back of flyer—Neighborhood condition



Back of flyer—Control condition

Figure 2. Samples of Emails (Kindergarten).

Performance condition



What school will your child attend next year?

It's time to fill out the OneApp.

Main Round deadline: February 22

If you have already submitted your OneApp, good job! This email contains additional guidance on choosing schools. If desired, you may edit your choices before February 22.

Click Here to Submit or Revise Your OneApp

Did you know? The Louisiana Department of Education recently gave out school progress scores. These scores show how much students learned in math and English last year.

Attending a school where students make big educational gains may be important to your family. Here are sixteen schools* that offer Kindergarten, which received an "A" for **outstanding student learning**:

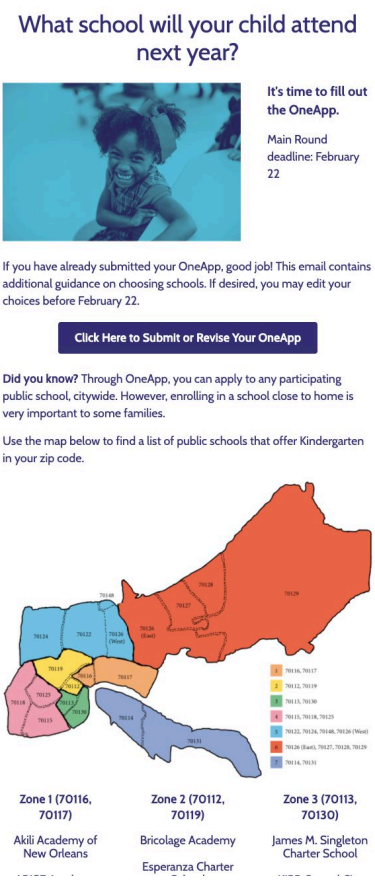
Schools with Best Progress Scores

1. Paul Habans Charter School (Score: 103.8)
2. FirstLine: Langston Hughes Academy (102.4)
3. FirstLine: Samuel J. Green Charter School (102.3)
4. Alice Harte Charter School (101.5)
5. FirstLine: Arthur Ashe Charter School (99.0)
6. International School of Louisiana, Eagle & Olivier Street campuses (98.9)
7. Andrew H. Wilson Charter School (98.0)
8. Akili Academy of New Orleans (96.6)
9. ARISE Academy (95.1)
10. Edward Hynes Charter School (94.8)
11. Mildred Osborne Charter School (94.4)
12. FirstLine: Phillis Wheatley Community School (93.7)
13. KIPP Leadership (93.1)
14. Lycee Francais de la Nouvelle-Orleans (91.6)
15. Harriet Tubman Charter School (90.9)
16. McDonogh #42 Elementary Charter School (90.0)

For more information, and to submit (or revise) your OneApp, go to https://enrollnola.org/

* Lake Forest and Lusher Charter Elementary School also received "A" grades but do not participate in the OneApp. Visit these schools to learn how to apply.

Neighborhood condition



What school will your child attend next year?

It's time to fill out the OneApp.

Main Round deadline: February 22

If you have already submitted your OneApp, good job! This email contains additional guidance on choosing schools. If desired, you may edit your choices before February 22.

Click Here to Submit or Revise Your OneApp

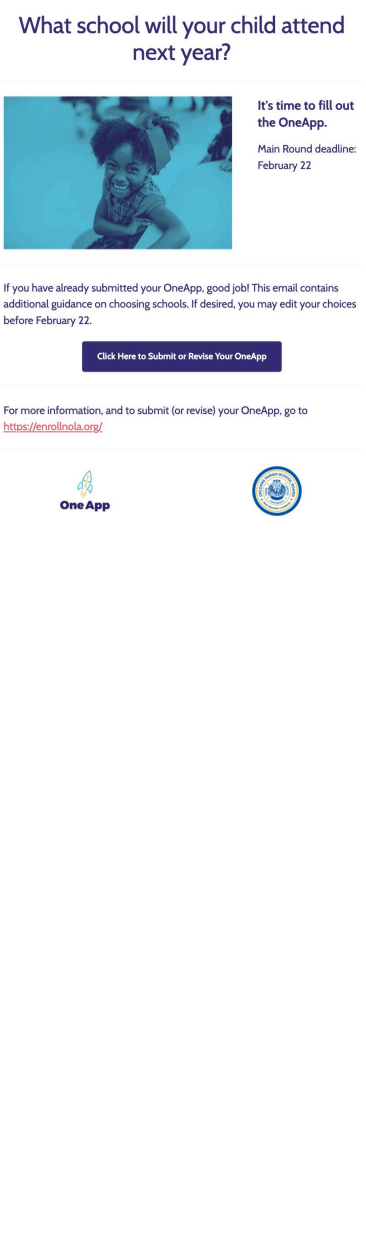
Did you know? Through OneApp, you can apply to any participating public school, citywide. However, enrolling in a school close to home is very important to some families.

Use the map below to find a list of public schools that offer Kindergarten in your zip code.

Zone 1 (70116, 70117)	Zone 2 (70112, 70119)	Zone 3 (70113, 70130)
Akili Academy of New Orleans	Bricolage Academy	James M. Singleton Charter School
	Esperanza Charter	

(image cut off to fit page)

Control condition



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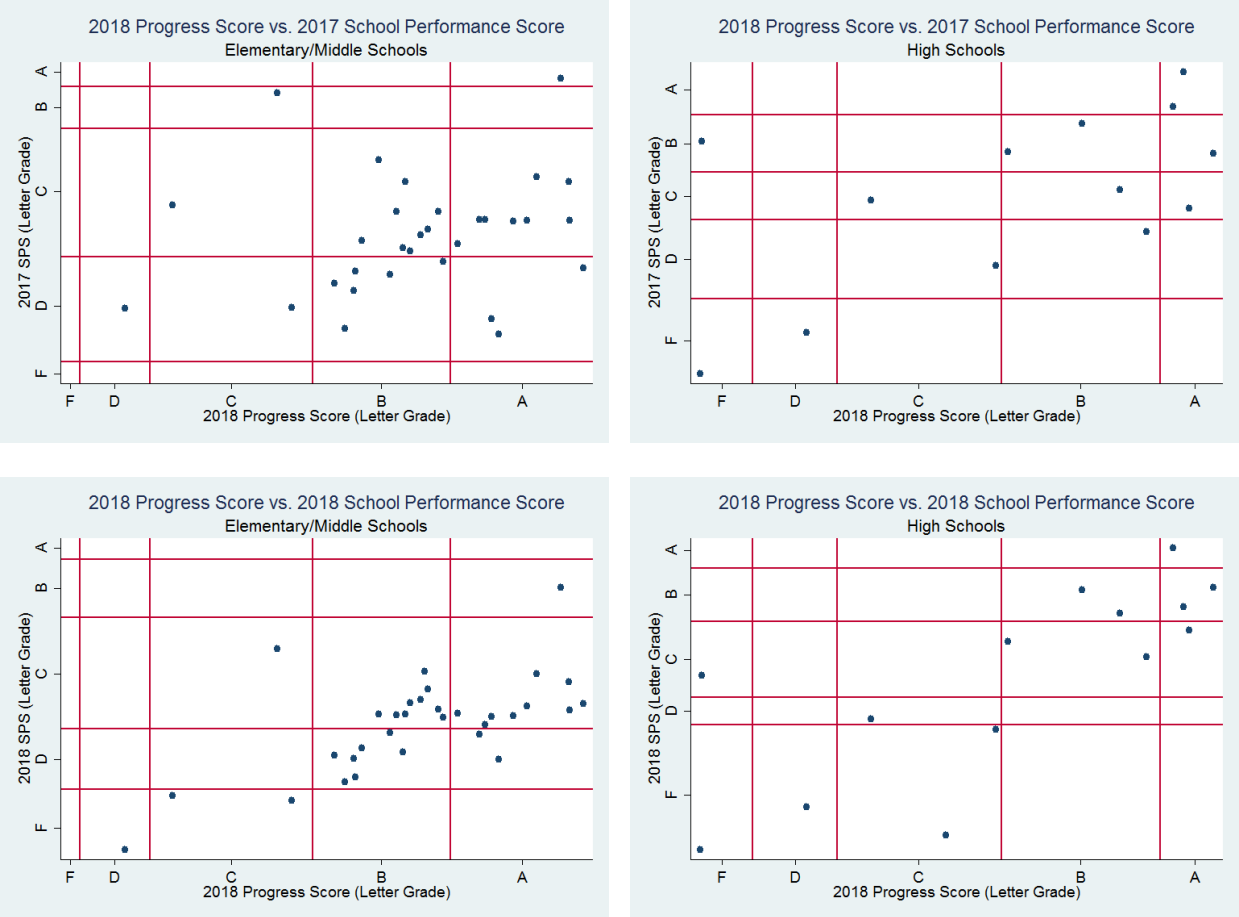
Click Here to Submit or Revise Your OneApp

For more information, and to submit (or revise) your OneApp, go to https://enrollnola.org/

Figure 3. Samples of Text Messages (All Grades).

Send Date & Time	**Grade/Condition**	**Content**
1/22 @ 9AM	Grade=Pre-K Group= Performance	The OneApp is open! If you haven't made your pre-Kindergarten choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22! Interested in the highest-rated PK programs? Hynes, Capdau-Avery Alexander, Behrman, Mc 42, ENCORE, Lafayette, Eisenhower, Open Minds Open Hearts Daycare, Langston Hughes, and Joseph Craig!
1/22 @ 9AM	Grade=Pre-K Group= Neighborhood	The OneApp is open! If you haven't made your pre-Kindergarten choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22! If you would like a list of free pre-K programs in your area, respond with your zip code!
1/22 @ 9AM	Grade=Pre-K Group=Control	The OneApp is open! If you haven't made your pre-Kindergarten choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22!
1/22 @ 9AM	Grade=K Group= Performance	The OneApp is open! If you haven't made your school choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22! Interested in elementary schools that have the best progress scores? Habans, Langston Hughes, Sam Green, Harte, Arthur Ashe, ISL, Wilson, Akili, ARISE, Hynes, Osborne, Phillis Wheatley, KIPP Leadership, Lycee Francais, Tubman, and Mc 42!
1/22 @ 9AM	Grade=K Group= Neighborhood	The OneApp is open! If you haven't made your school choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22! If you would like a list of schools with kindergarten in your area (in the OneApp), respond with your zip code!
1/22 @ 9AM	Grade=K Group=Control	The OneApp is open! If you haven't made your school choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22!
1/22 @ 9AM	Grade=9 Group= Performance	The OneApp is open! If you haven't made your school choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22! Interested in high schools that have the best progress scores? NOMMA, KIPP Renaissance, Einstein, Livingston, and Edna Karr!
1/22 @ 9AM	Grade=9 Group= Neighborhood	The OneApp is open! If you haven't made your school choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22! If you would like a list of schools with 9th grade in your area (in the OneApp), respond with your zip code!
1/22 @ 9AM	Grade=9 Group=Control	The OneApp is open! If you haven't made your school choices (or want to change them), go to https://enrollnola.org/. Deadline is Feb 22!

Appendix 1. Comparison of School Letter Grades on Assorted State Measures.



Note. Each point represents a school. Points plotted by scores on respective numerical measures (e.g., 2018 progress index) that correspond to letter grades. Schools represented are those in the analytical sample, excluding schools with "T" ratings (for schools transitioning to new leadership) and "combination" schools (with both elementary/middle and high school grades).

Appendix 2. Effects of Performance and Neighborhood Treatments on Binary Outcome Variables (Odds Ratios).

	All	Pre-K	K	Grade 9
Panel A. Performance treatment and outcomes				
Any high-performing schools requested	1.124*	0.827	1.081	1.325**
	(0.070)	(0.130)	(0.116)	(0.167)
First choice is high-performing school	1.044	0.955	0.954	1.115
	(0.072)	(0.266)	(0.106)	(0.118)
Assigned to high-performing school	1.084	1.199	0.898	1.245**
	(0.077)	(0.280)	(0.101)	(0.133)
Observations (max.)	4,733	1,082	1,664	1,987
Panel B. Neighborhood treatment and outcomes				
Any in-zone schools requested	1.147**	1.059	1.210*	1.164
	(0.077)	(0.141)	(0.133)	(0.132)
First choice is in-zone school	1.026	1.059	1.054	0.963
	(0.067)	(0.139)	(0.112)	(0.113)
Assigned to in-zone school	1.013	0.942	1.100	0.957
	(0.064)	(0.122)	(0.112)	(0.101)
Observations (max.)	4,680	1,094	1,717	1,869

Note. Standard errors appear in parentheses. Table shows results of logistic regressions conducted at student level with standard errors clustered by home address. Students with and without guaranteed seats included in sample. Estimates reported as odds ratios.
*p<.10; **p<.05; ***p<.01.