Leveling the Playing Field for High School Choice: Results from a Field Experiment of Informational Interventions*

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Abstract

We conducted a field experiment in 165 high-poverty New York City middle schools to help students choose and enroll in higher-performing high schools. The experiment provided simplified information about school performance as well as process-specific supports to complete tasks necessary for admission to selective and over-subscribed schools. While treated students did not apply to higher-performing schools on average, their application behavior changed in ways that led them to match to higher-performing schools and avoid very low-performing schools. Subgroup analysis finds comparatively advantaged students benefited more from the intervention, raising questions about whether informational interventions alone will reduce inequality in access to higher-performing schools.

I. Introduction

By allowing families to choose from an array of options, school choice policies aim to increase school productivity and reduce racial and socioeconomic gaps in access to high-performing schools. Their efficacy depends, however, on families' ability to navigate these options and gain admission to more effective schools. If lower-income and historically disadvantaged groups are less likely to have the information and supports needed to identify, apply, and enroll in high-performing schools, the effects of school choice on inequality may be limited.

Prior research finds that, on average, lower-income families put less relative weight on academic factors when choosing schools than their more affluent counterparts (Abdulkadiroğlu, Agarwal, & Pathak 2017; Abdulkadiroğlu et al. 2020; Hastings, Kane, & Staiger 2009). Less clear is the extent to which this is attributable to preferences, supply-side constraints, or other factors, such as information use (Burgess et al. 2014; Glazerman & Dotter 2017; Hastings & Weinstein 2008). School choice can be a time and resource-intensive activity, leading some families to be less informed than others about available options and application strategies. Like other public programs, school choice can involve administrative or procedural barriers that make it difficult to realize its full benefits (Gross et al. 2015; Jochim et al. 2014; Lareau et al. 2016). At the secondary level, low-income and immigrant students are more likely to make school choices alone with little adult direction (Condliffe et al. 2015; Sattin-Bajaj 2014).

New York City's universal high school admissions process is an ideal setting to examine whether information and other supports can reduce inequality in access to higher-performing schools. As part of the largest public school choice program in the country, the NYC Department of Education (NYCDOE) requires every 8th grader to apply to high school and rank up to 12 programs from more than 750 in 440 schools citywide (Abdulkadiroğlu, Pathak, & Roth 2005).[1] Academic programs vary in admissions method (academically selective vs.

[1] As we describe later, students apply to *programs*, not *schools*. Most schools offer only one program, but some offer multiple programs. We use these terms interchangeably when the distinction is unimportant.

not), admissions priorities, and curricular focus (e.g., STEM, health professions, humanities). They also vary widely in performance. In 2015, two out of every five NYC high schools had a four-year graduation rate below 65 percent. One in four had a graduation rate above 85 percent. While there are many good options to choose from, the system's scale and complexity raises the risk that less supported and informed students will make sub-optimal choices.

In this paper, we report the findings of a large school-based randomized trial designed to help students in high-poverty middle schools navigate the school choice process and enroll in higher-performing high schools. Our experiment was conducted in 165 NYC schools that together serve nearly 20,000 8th graders. Students in treatment schools received a one-page list of 30 geographically proximate high schools with a graduation rate at or above the city median ($\geq$70%). Lists were customized to each middle school and were designed to create awareness of high-performing schools where students' odds of admission was high. The materials explained admissions methods in plain language, facilitated an easy comparison of graduation rates, and reported estimated travel time by public transportation from their current school. In two of three treatment arms, we provided additional information and supports to help students assess curricular fit and to overcome procedural hurdles that impede access to more selective or over-subscribed schools.

The experiment had three primary aims. First, we sought to test whether providing targeted and comparative information about nearby options increased students' propensity to apply to, match, and enroll in higher-performing high schools, and to avoid schools with a very low graduation rate. Second, we wanted to assess whether information has a greater effect on school choices and placements when it is accompanied by supports to help students overcome barriers to admission at selective or over-subscribed schools. Third, we aimed to examine whether students varied in the extent to which they responded to the information we provided. Whether informational interventions can "level the playing field" depends on the degree to which historically disadvantaged students use and benefit from them.

Our study differs from previous K-12 informational interventions for school choice in several key ways. First, our experiment was conducted in a setting that is substantially more complex than those previously studied. NYC high school choice involves hundreds of school options that vary in admissions criteria and other priorities. The quality of school to which students *match* depends not only on their choices, but also their attention to admissions priorities and selectivity that affect odds of admission. An informational intervention in this context must go beyond a comparative list of schools and encourage applications to higher-performing schools with a realistic chance of matching. We created our informational tool with this context in mind, and two treatment arms provided additional supports—including text messages—to help students gain access to selective or over-subscribed schools. Second, unlike previous work, we directed our interventions to students themselves, who play a significant role in high school choice. Third, our study sample was large and diverse, enabling us to estimate heterogeneous treatment effects for populations that have more to gain from the interventions. These estimates provide an initial test of whether informational interventions for school choice can reduce inequality in access to high-performing schools.

We find that students in treatment schools used our customized lists when making choices and were more likely to apply to our specific school recommendations than students in control schools. Perhaps surprisingly, we did not find that students in treatment schools *applied* to schools with a higher graduation rate, on average.[2] They did, however, *match* to higher-performing schools by orienting their application toward schools where their odds of admission were higher. They were also much less likely to include schools with a low graduation rate (below 70%) on their application. Of the three treatment arms, the largest effect was for the simplest intervention, where students matched to high schools with a 1.7 percentage point higher graduation rate, a 0.12σ effect. Students in this treatment arm were 5.1 percentage points less likely to enroll in a low graduation rate high school. Additional supports, such as text message reminders, did not increase effects over the one-page list alone, and often resulted in a smaller impact.

[2]As we show later, the mean graduation rate for the control group's top three choices was already quite high, at 81%, roughly the 70th percentile in the distribution of NYC high schools at that time.

Importantly, while we find nearly all subgroups responded to our intervention, comparatively advantaged groups appeared to benefit more from them. For example, higher-achieving students in treatment schools applied to more schools on our lists than did lower-achieving students, and they saw greater reductions in their likelihood of matching to a low graduation rate school. Similarly, White and Asian students were more responsive to the intervention than were Black students. At the same time, students from non-English speaking households—who represent nearly half our sample—were among the most likely to draw choices from our lists and to avoid matching to low-performing schools as a result. This finding suggests the benefits of interventions like ours may be significant for families for whom English is not the dominant language spoken at home.

Taken together, this study finds that providing simplified and customized information to middle school students can impact their high school choices and increase the quality of schools they enroll in. Beyond simply inducing students to choose higher-performing schools, the intervention improved placements by incorporating odds of admission into its design and by limiting applications to the lowest-performing schools. Our findings also suggest that broad-based informational interventions alone may not reduce inequality in access to high-performing schools, since most subgroups responded to the information we provided. The longer-run effects on inequality will ultimately depend on whether lower-income and other historically disadvantaged groups see a larger return to matching to higher-performing schools. We will investigate those longer-term effects in future work, when data become available to assess students' high school and post-secondary outcomes.

II. Background

A. Information and other frictions in school choice

Research has identified multiple frictions associated with school choice and enrollment decisions. One relates to the cost of acquiring information. If families lack full information about their choices and opportunities—or if obtaining this information is costly—they may make sub-optimal decisions. A second friction relates to information and/or choice overload, where information is readily available, but the number or complexity of choices inhibits decision-making. A third relates to administrative or other procedural barriers that make it more difficult to actualize or reap the full benefits of an educational decision.

A large literature in economics, psychology, and education finds that, in some contexts, simplified information, choice architecture, and other behavioral "nudges" can reduce such frictions. These approaches aim to increase the salience of information, narrow the scope of choices, facilitate comparisons, and/or mitigate procedural barriers. Examples abound, from the selection of insurance plans (Abaluck & Gruber 2016; Johnson et al. 2013; Kling et al. 2012) to the choice of college and major (e.g., Hoxby & Turner 2013; Wiswall & Zafar 2015) to the claiming of financial aid or tax benefits (Bettinger, Long, & Oreopoulos 2012; Bhargava & Manoli 2015; Page, Castleman, & Meyer 2016).

Three randomized experiments in K-12 school choice have found that providing information about schools can improve choices and later student outcomes.[3] In a seminal study, Hastings and Weinstein (2008) mailed information about school performance and odds of admission to parents in Charlotte-Mecklenburg, North Carolina. They found providing this information increased the fraction of parents choosing high-performing schools by 5 to 7 percentage points on a baseline of 31%. Students whose families were induced into applying to a higher-performing school later earned higher test scores. Valant and Loeb (2014) conducted an experiment in Milwaukee, Washington, D.C., and Philadelphia in which families were given informational booklets about available schools citywide. They found that families applying to middle school selected higher-performing schools after being given the information while older students selected *lower*-performing schools. To explain the latter finding, they speculated that students applying to high school were more influenced by non-academic aspects of schools included in the booklet. Valant and Weixler (2020) sent mailers, emails,

[3]As part of an ongoing school-based informational experiment in Ghana, Ajayi, Friedman, and Lucas (2017) found that information sessions increased parents' involvement in the high school choice decision.

and texts to parents in New Orleans and found that families who received information about school performance were more likely to choose top-performing schools than families who received a list focused on school location. Positive effects were concentrated among high school choosers and families of students with disabilities.

Informational experiments have been much more common at the post-secondary level, targeting both application and enrollment behaviors. Hoxby and Turner (2013) randomly assigned high-achieving, low-income high school students to receive direct mailings about applying to college and their estimated net cost of attending a selective college or university. This information had a sizable effect on the number and selectivity of institutions to which lower-income students applied. This change in application behavior resulted in a higher rate of admission to selective institutions and an increased likelihood of attending a selective school. A more recent scale-up study did not see the same impact, however (Gurantz et al. 2021). One reason may be that the scaled-up program did not specifically "nudge" students to selective colleges, but rather provided information about a wider range of institutions. That study found treated students applied to more colleges, but not necessarily better ones.

The experiments described thus far were primarily focused on providing new information rather than narrowing choices or reducing choice overload. In NYC, the environment is information-rich but potentially overwhelming. School guidance counselors informed us that the primary tool students use is the district's printed high school directory, which spans more than 600 pages and includes detailed information about every school (Sattin-Bajaj et al. 2018). Students can also learn about high schools from websites, other printed guides, open houses, and by attending high school fairs where hundreds of school representatives are present.[4] A large literature finds that people have difficulty making choices when faced with a large and complex set of options, and they respond by using simplified strategies or by delaying their decision (Iyengar 2010; Shafir, Simonson, & Tversky 1993; Thaler & Sunstein

[4]In the year following our study, the NYCDOE introduced SchoolFinder, later renamed MySchools, a website that allows users to search for and obtain information about high schools. The site largely provides the same information as the printed high school directory and does not make direct head-to-head comparisons between schools. In 2020, the NYCDOE discontinued the full-length printed directory.

2008). The potential for this behavior is high our context.

With respect to administrative barriers, research finds small transaction costs and procedural barriers can have large effects on educational choices. These studies have mostly been carried out in higher education contexts. Bettinger et al. (2012), for example, found that information about financial aid eligibility was not sufficient for increasing application rates and college enrollment. Rather, FAFSA filing assistance paired with information was needed. Despite the large returns to college, Bulman (2015) found that students' propensity to take the SAT was sensitive to the availability of nearby testing centers, and to default registration. Pallais (2015) found that increasing the number of free score reports from 3 to 4 led ACT takers to apply to a wider range of colleges, and led lower-income students to attend more selective colleges.

Finally, a robust literature finds that text message reminders can help educational decision-makers overcome procrastination or aversion to administrative tasks (Castleman & Page 2015; Page, Castleman, & Meyer 2019). The efficacy of these interventions depend on context and design, however (Avery et al. 2021). In a K-12 setting, Weixler et al. (2020) found weekly text messages increased the likelihood that low-income families completed income verification for enrollment in an early childhood education program by 7 percentage points.

The general finding that emerges from this literature is that information that facilitates comparisons, narrows the choice set, and makes certain options more salient can influence choices. Process supports can also be effective at helping individuals follow through with actions associated with a choice. Our interventions were guided by this literature and by a pilot study we conducted in 2014-15. The pilot included trial interventions in nine schools and interviews with guidance counselors in approximately 17% of all 8th grade serving schools in NYC (Sattin-Bajaj et al. 2018). In the next section, we describe the NYC high school choice process in greater detail, highlighting ways in which its complexity makes informational and other supports potentially beneficial to students and their families.

B. High school choice in New York City

New York City employs a deferred acceptance algorithm that uses students' ranked choices, available space, and schools' own rankings and priorities to match students to high schools (Abdulkadiroğlu, Pathak, & Roth 2005, 2009). It is "strategy-proof" in that it is a dominant strategy for students to rank truthfully. In 8th grade, students submit an application ranking up to 12 high school programs, out of more than 750 offered. An application is required; no student is permitted to attend a default neighborhood school and avoid an active choice, and only in rare cases are they guaranteed admission to their first-choice school.[5]

High school applications are typically submitted in early December. Roughly 92% of applicants are offered a school placement in March or April in the first round of matching, and just over half are offered their first choice school. At the time of our study, students who did not receive a first round offer re-applied in a supplemental round to schools with open seats.[6] Students not matched in the supplemental round are administratively assigned, and those who enter late consult a counselor or Family Welcome Center staff to discuss available options. Subsequent appeals and transfers are possible but difficult, and students may apply again in 10th grade to schools with available seats.

Applicants choose from a wide variety of high school types, including small themed schools, large comprehensive schools, career academies, and performing arts schools.[7] The programs to which students apply have admissions methods and—in most cases—priorities that impact which students are admitted. Excluding the exam-based specialized high schools, there are seven admissions methods: four that are academically non-selective (unscreened,

[5]For example, students in combined middle-high schools are allowed to continue in their existing school if it is their first choice. Admission to one's zoned school is also guaranteed, where applicable, but zoned schools no longer exist in most parts of the city. Since 2002, more than 48 large, predominately zoned high schools have closed for poor performance (Kemple 2015; Quint et al. 2010). Our field experiment excluded middle schools where the opportunity to continue into 9th grade was available.

[6]The supplemental round was eliminated in 2020 and replaced with a system of waitlists.

[7]Specialized high schools, charter schools, and the LaGuardia Performing Arts High School are outside of the main high school choice process. The specialized high schools, which include the well-known Stuyvesant High School and Bronx Science, admit students on the basis of an entrance exam. At the time of this study, only 5.7% of 9th grade students were enrolled in charter schools.

limited unscreened, zoned, and screened for language), two that are selective (screened and audition), and one that is partially selective with an intentionally balanced test score distribution (educational option). Priorities give preference to students based on residential or middle school location, and in the case of limited unscreened schools, demonstrated interest. For example, priority admission may be given to residents of the same borough or geographic area, and/or to students who visit an open house or information session and sign in. Screened and audition programs rank students according to their own criteria. To maximize their odds of matching to a desired school, students must be attentive to program priorities and selective admissions requirements (Corcoran et al. 2017).

C. The potential for an informational intervention in NYC

New York City high schools vary substantially in effectiveness. The average 4-year graduation rate for NYC high schools was 72% in 2014-15 (one year prior to our study), with a standard deviation of 16 percentage points. 1 in 4 high schools had a graduation rate of 61% or lower. While this variation is due in part to student sorting, a recent paper by Abdulkadiroğlu et al. (2020) estimated sizable causal effects of NYC high schools on math scores, graduation rates, and post-secondary outcomes. Other causal evidence strongly supports the claim that school effects are large (e.g., Deming, Hastings, Kane, & Staiger 2014; Jackson 2010; Jackson et al. 2020; Pop-Eleches & Urquiola 2013). Especially relevant to this paper is quasi-experimental evidence which found that attendance at a small, academically non-selective NYC high school has positive effects on the likelihood of graduation and subsequent educational attainment (Bloom & Unterman 2014). That study found larger effects for Black and academically at-risk students—including low-income and low-achieving students—leading us to expect that students in our study population stand to benefit from matching to higher-quality schools.[8] Another study found no benefits to attending a selective school in Chicago, but students

[8]Because their design relied on randomization via the matching algorithm, Bloom and Unterman (2014) excluded schools that were not oversubscribed. Our interventions highlight schools with high graduation rates—which tend to be oversubscribed—and thus the high-impact schools in their study appear regularly on our intervention materials.

who avoided high schools with very low graduation rates and attended higher-performing non-selective schools had better outcomes (Allensworth et al. 2017).

An analysis of students' high school choices and placements in the year prior to our study demonstrates the potential for an intervention to improve student choices. In Table 1 we report estimates from regressions that show mean differences in the graduation rates of 1st-3rd choice high schools and school matches by student background. These regressions use high school applications data from 2014-15, one year prior to our intervention. Columns (1)-(4) show regression-adjusted differences without controls for prior achievement, while (5)-(8) include these controls. The regressions without controls for prior achievement are relevant in that they show mean differences in the schools chosen and attended by different populations of NYC students. Of course, gaps are not necessarily due to preferences or access to information, since geographic priorities, capacity constraints, and academic screens affect the odds of admission to many schools. Both student choices and matches reflect these supply-side constraints. The regressions with controls for achievement address this to some extent by describing differences in the graduation rates of choices and matches for students with comparable prior achievement.

The coefficients in Table 1 show that lower-income, Black, Hispanic, male, and low-achieving students choose and are matched to lower-performing high schools than their higher-income, White, Asian, female, and higher-achieving counterparts. For example, the top three choices of students eligible for free lunch had a mean graduation rate almost 4 percentage points lower than the top choices of students not eligible for free lunch (col. 1). For reference, 4 percentage points is about 0.25σ in the distribution of high school graduation rates in NYC in 2014-15. Free lunch eligible students were also much more likely to rank schools with a graduation rate below 70% in their top three (+7.3 percentage points, col. 3). Above and beyond the income gap, Black and Hispanic students ranked schools among their top three with graduation rates at least 4 percentage points lower than Asian and White students, and their top choices included a much greater share of low graduation rate schools (+13-14 percentage points). Differences in *choices* in part explain why these groups *matched* to schools with lower graduation rates (cols. 2 and 4). Free lunch eligible students, for example, matched to high schools with graduation rates 4.2 percentage points below the matches of non-free lunch eligible students, and they were 10 percentage points more likely to match to a low graduation rate school. Black and Hispanic students were 20-22 percentage points more likely to be matched to a low graduation rate school ($< 70\%$) than White and Asian students. When conditioning on prior achievement (cols. 5-8), these gaps attenuate, but remain sizable. Controls for residential borough—which help to address spatial variation in the supply of higher-performing schools—also narrow the gaps, but all remain statistically significant and meaningful in size.

We find lower-income and historically under-served students are also less likely to take steps that would increase their chances of admission to desired schools. In NYC, priority admission to limited unscreened schools is often given to students who attend an open house or information session and sign in, or who sign in with a school representative at a high school fair. In 2014-15, we found that only 41% of students who ranked a limited unscreened school as their *first choice* had open house priority at that school (see Online Appendix Table D.1). The rate was lower still for low income students (38%), English learners (ELs; 33%), and students with disabilities (36%).[9] As we show elsewhere, having information session priority substantially increases the odds of admission to higher-performing limited unscreened schools that are oversubscribed (Corcoran et al. 2017). The small high schools found to have large effects in Bloom and Unterman (2014) were all of this type.

Though the regressions in Table 1 are descriptive, they are qualitatively consistent with structural estimates of preference parameters that find disadvantaged families place less implicit weight on academic quality when choosing schools (Abdulkadiroğlu, Agarwal, & Pathak 2017; Harris & Larsen 2015; Hastings, Kane, & Staiger 2009). To the extent vari-

[9] Open house priority is not a clean measure of actual open house *attendance*, since high schools self-report whether or not students attended an open house. In 2017-18, the NYCDOE eliminated the open house priority (Disare 2017).

ation in choices and admissions priority is in part driven by informational frictions and/or procedural hurdles, an intervention of the type we implemented could raise the quality of schools to which students match and enroll.

D. Description of the interventions

Informed by extant literature and our understanding of the challenges faced by NYC students choosing high schools, we developed a one-page informational tool called "Fast Facts," a customized list of 30 high schools for each middle school in our study. We designed Fast Facts to be an accessible starting point for students and a useful reference for school performance data and admissions requirements. The intent was not for students to limit their search to these schools, but rather to begin with an initially smaller set of choices and to become aware of lesser-known higher-performing schools in their vicinity. Proximity was important given preferences for closer schools (Glazerman & Dotter 2017; Harris & Larsen 2015) and the use of geographic admissions priorities.

Schools recruited to the study were randomized into three treatment arms and a control group (details provided later). Eighth grade students in the first treatment arm (FF1) received the one-page Fast Facts list described in the introduction and below. Students in the second treatment arm (FF2) received Fast Facts *and* a supplementary list of academically non-selective schools that give priority admission to students who attend an open house or information session and sign in. This group was also invited to receive weekly text message reminders about open house dates, time, and locations. Students in the third treatment arm (FF3) received Fast Facts *and* a supplementary list of high school programs organized by academic interest area. Both supplemental lists were attached to Fast Facts on the inside of a bi-folded sheet. All treatment schools received a separate one-page insert of "screened language" programs citywide that serve recent immigrants learning English.[10]

Intervention materials were delivered directly to students by trained research assistants

[10]We did this to ensure students in this special population received an appropriate school list. Additional details are provided in Online Appendix B.

via a 40-minute standardized lesson in a group setting, often, but not always, in a classroom. This lesson explained how to use the materials and emphasized the importance of graduation rates, admissions methods, and location when making school choices. All materials—including text messages—were available in English and Spanish, and lessons were delivered in Spanish when requested by the school guidance counselor. Control schools did not receive any materials until after the study was complete. (These schools were later provided Fast Facts lists that they could use with their 7th graders). School visits took place in September (43 visits), October (72 visits) and early November (5 visits).

Table 2 provides an overview of the treatment groups and intervention materials. Importantly, we generated Fast Facts and supplementary lists for *all* study schools, regardless of treatment or control assignment. Doing so provided "counterfactual" data that characterize the information each school would have received had they been in a particular treatment group. The same was done for weekly text message content. Among other things, this aids in estimating the treatment effect of the intervention on specific school choices. Additional details about the intervention materials follow, and examples are pictured in Online Appendix Figures B.1-B.4. For a full description of the methodology used to generate customized high school lists, see Online Appendix B.

Fast Facts: Fast Facts is a one-page list of 30 proximate high schools with a 4-year graduation rate at or above the city median (≥70%). Unique lists were generated for each middle school. Except in rare cases, Fast Facts high schools were within a 45-minute trip by public transportation from the middle school. New schools (which lack graduation rates) and academically screened high schools were included but limited to a maximum of 10 each.[11] Our

[11] A list of the highest-performing high schools in an area could be dominated by screened schools, which are selective and have lower odds of admission. We limited the number of these schools to make the list relevant to a heterogeneous student population. NYC has a large number of recently-opened high schools that did not yet have a graduating cohort as of 2015-16, and we did not wish to exclude these entirely. As explained in Online Appendix B, we used data on 9th grade credit accumulation to predict graduation rates for these schools. The imputed graduation rate did not appear on Fast Facts; rather, the graduation rate reads "*new school" with a note explaining that it had recently opened and therefore lacked a graduation rate.

procedure for selecting Fast Facts high schools prioritized shorter travel times, and schools were listed in descending order by graduation rate (as in Hastings & Weinstein 2008). The front of the sheet listed the 30 high schools, along with their borough, graduation rate, travel time, page in the High School Directory, and admissions method(s). The reverse side explained admissions methods in plain language and offered guidance for applying to schools of each type ("What do I need to do?"). Students were encouraged to list 12 choices on their application to increase their odds of a match. Descriptive characteristics of high schools appearing on the Fast Facts sheets are reported in Online Appendix Table B.1. The mean graduation rate of schools appearing on the average Fast Facts list was 81.5%.

Academically non-selective school supplement: The non-selective school supplement was intended to increase student participation in open houses and information sessions for high schools that give priority admission to students who attend and sign in. Unique supplementary lists were generated for each middle school. Programs on the supplementary list were drawn from schools on the Fast Facts sheet (of the limited unscreened type) and more were added when necessary. A total of 18-25 limited unscreened programs appeared on each list.[12] The criteria for inclusion was the same as Fast Facts, and programs were listed in descending order by their school's graduation rate. This supplement list emphasized the importance of attending and signing in at open houses, and it included a calendar on which to write dates and times of scheduled admissions events. Parents and students were invited to sign up for weekly text message reminders about upcoming information sessions and fairs.[13] Descriptive statistics for high schools appearing on the (combined) Fast Facts and non-selective school supplement are reported in Online Appendix Table B.2.

[12] Unlike Fast Facts, which did not include program codes (since schools may offer multiple programs), the non-selective school supplement listed the 4-character program code required for the student application. The graduation rate was not shown on the supplement, since in most cases it appeared on the front page (Fast Facts).

[13] Schools in the FF2 treatment arm were given a 3-digit numeric code that they could text to our project phone number to sign up. This code was prominently displayed on the non-selective school supplement, and posters were given to FF2 treatment schools to advertise the text messaging opportunity. See Online Appendix B for details on our process for generating text messages.

Schools by academic interest area: The supplementary list of schools by interest area was intended to help students identify high schools that match their academic or career interests and to emphasize the importance of auditions and other requirements for selective schools. Unique lists were generated for each middle school. A total of 49 programs were featured on each list, with seven in each of the following categories: Performing & Visual Arts; Health Professions; "Academically Selective"; STEM; Humanities & Global Studies; Law, Government, Civics, & History; and Business & Communication. The criteria for inclusion were the same as Fast Facts. The supplement showed the school and program name, borough, program code, and admissions method. Multiple programs from the same school were permitted. Descriptive statistics for high schools represented on the (combined) Fast Facts and interest area supplement are reported in Online Appendix Table B.3.

Text message reminders: Two text message reminders were sent weekly to FF2 students and/or family members who opted in. In most cases, these texts contained information about an open house taking place that week at an academically non-selective high school. There was no central calendar for these events, so we compiled them ourselves from high schools and other sources.[14] Reminders were specific to the receiving school and were chosen using an algorithm that prioritized schools with higher graduation rates and fewer scheduled open house opportunities. Roughly 40% of the high schools referenced in the text messages appeared on the school's non-selective high school supplement. The remaining 60% were drawn from other schools in the borough holding open houses that week.[15] Further details on the text messaging intervention are provided in Appendices B and C.

Control condition: As noted earlier, the NYC context is information-rich, and, in theory, students can access an abundance of resources about high schools. These include fairs, school

[14]If too few open houses were scheduled in a given week, we sent general information about a high school instead (e.g., its name, location, and public transit access). Our team compiled a list of 762 open house dates via the High School Directory, the NYCDOE website, individual school websites, and weekly phone calls to high schools. See Online Appendix B for details.

[15]It was not possible to populate open house reminders each week exclusively from a middle school's non-selective supplement, so we drew from a wider set of schools.

counselors and other middle school personnel, friends and family, websites and apps, and the printed directory. Students in our control group have access to these resources, and thus estimated treatment effects are relative to "business as usual." While there was nothing to prevent treatment schools from sharing our materials with control schools, we actively discouraged this and emphasized the customized nature of the information for their school. (The middle school name was prominently displayed at the top of each school list).

It is worth reiterating that our intervention materials were delivered to *students* rather than to parents or school counselors. This decision was based on existing literature which finds disadvantaged students often make school choices with little adult guidance (Sattin-Bajaj 2014; Valant & Loeb 2014; Ajayi et al. 2017). The materials were designed with students in mind, but we expected and encouraged them to share with their families. Counselors were also encouraged to integrate the tools into their advising. Materials were delivered in the form of colorful printed fliers, rather than via digital means.[16] This was done to reduce comparison friction (Kling et al. 2012), to facilitate head-to-head comparisons of school graduation rates, and to support students with limited or no access to the internet at home.

Finally, we opted for graduation rates as our school performance measure rather than alternatives, such as value-added, for several reasons. First, value-added is an inherently complicated concept for students and families, and we wanted a familiar measure of quality that aligned with other resources available to them. Second, using our own estimates of school value-added on high school graduation, we found 73% of schools appearing on a Fast Facts list had both above average graduation rates and positive value-added (see Online Appendix Figure D.1). Finally, estimation of value-added in contexts where there is not a comparable baseline measure is still an emerging area in the literature. It is unclear whether controlling for prior test scores is a sufficient control for causal estimates of value-added on graduation (e.g. Abdulkadiroğlu et al. 2020; Deming et al. 2014; Jackson et al. 2020).

[16] In 2016-17, we conducted a "scale-up" experiment that tested the relative benefits of paper versus digital access to Fast Facts. Results from the scale-up study will be available at a later date.

III. Data and Experiment Design

A. Data sources

For this analysis we used school and student-level administrative data from the NYCDOE.[17] Our main analytic dataset was constructed by matching student records from the 2015-16 high school admissions process with demographic and other student background information. Characteristics of high schools, including graduation rates, were obtained from the NYCDOE 2015-16 High School Directory and Demographic Snapshot.

Applications data include each student's ranked high school choices (up to 12 programs), his or her priority group for each choice, rankings of the student by the school in cases where screening is permitted, and their final school assignment. From this information we created outcome variables characterizing students' choices, matches, and related outcomes. These variables fall into three sets: (1) school presence on the Fast Facts and/or supplementary lists, (2) school characteristics, and (3) other admissions process and enrollment outcomes. Examples of the second set include the school's 4-year graduation rate, whether the graduation rate was below 70%, travel time from the middle school to the high school, location in the same borough as the middle school, applications per seat in the prior year (a measure of demand), and admissions method (e.g. screened or limited unscreened). All of these variables were created for the 1st, 1st-3rd, and all choices, as well as the matched and enrolled school. Examples in the third set of outcomes include the number of choices submitted; open house priority status for limited unscreened programs; ranking of the student by screened programs; whether or not the student was matched to his or her 1st, 1st-3rd, or any choice in the first round; participation in the second round after a successful match; and matriculation to the matched school in 9th grade. Other outcome variables include measures of within-application variability in the graduation rate of school choices and consistency in academic interest area. Within-application variability was calculated as the difference between the highest and low-

[17] Access to de-identified and confidential data was possible through a secure data use agreement with the Research Alliance for New York City Schools.

est graduation rate of schools on a student's application. Within-application consistency in interest area was calculated as the highest percentage of choices from the same interest area.

Demographic and other student characteristics measured prior to our experiment include race/ethnicity, gender, free or reduced price lunch eligibility, special education services received, EL and foreign born status, and 7th grade English Language Arts (ELA) and mathematics test scores standardized to mean zero and standard deviation one by subject.[18] These student-level variables were used as covariates to increase precision of our impact estimates and for estimating effects by subgroup. Aggregates of these variables at the middle school level were also included as covariates. Means for the full study sample of 19,109 students are reported in Table 3. Notably, students in the study sample are majority Hispanic (55%) and free lunch eligible (74%). 16% of students are classified as ELs, and 49% speak a language other than English at home. The average student scored 0.29-0.31σ below the city average in 7th grade ELA and math, and 11% were missing 7th grade test scores, suggesting they were not enrolled in the NYC public schools in the prior year.

B. Recruitment and treatment assignment

We recruited 165 schools from the more than 500 schools serving 8th grade students in NYC, focusing on some of the highest-poverty schools in the city.[19] Online Appendix Tables A.1 and A.2 report mean characteristics and prior choice outcomes for schools in our study, schools in the sampling and recruitment frames, and all schools citywide. Schools in the study were disproportionately located in the Bronx and Brooklyn and enrolled a higher share of Hispanic students, ELs, and free lunch eligible students than the citywide average. Two thirds were in the highest two quartiles of poverty in NYC (by students' residential census tract), and the remaining were in the next highest quartile. In 2014-15, students in study schools applied to high schools with lower graduation rates, on average, than did students

[18] We also have 8th grade ELA and math test scores, but admission to academically screened programs is dependent upon 7th grade performance.

[19] A detailed description of our sampling procedure is provided in Online Appendix A. We excluded combination middle-high schools, special education schools, and schools in Staten Island.

in the full population, and a larger share of schools on their application were non-selective. 23 schools participated in our 2014-15 pilot study, although just 8 of these received a Fast Facts list in that year.

To increase power over a simple cluster randomized trial in which schools are randomly assigned to treatment conditions, we randomized schools within blocks of similar schools located in the same borough. Blocks were matched quadruplets of schools selected using a Mahalanobis distance measure of difference between schools (see Bruhn & McKenzie 2009; King et al. 2007). School variables used in the matching procedure included prior choice outcomes (e.g., the mean graduation rate of first round matches in 2013-14), prior achievement, economic disadvantage, and school size.[20] The 23 schools that participated in our pilot study were blocked separately. By matching on observable characteristics before randomization and controlling for the randomization blocks in our regressions, we increase the likelihood that the schools assigned to the treatment and control groups are similar at baseline, and account for some of the extant variation in our outcomes.

Online Appendix Tables A.4 and A.5 provide descriptive statistics and results of balance tests for school characteristics in our three treatment arms and control group. Other than pilot study participation, there are no statistically significant differences in mean school characteristics across groups, indicating the randomization was successful. Additionally, there are no statistically significant differences in the mean characteristics of high schools appearing on the Fast Facts and supplementary lists generated for these groups (Online Appendix Tables B.1-B.3).

C. Estimating treatment effects

For a student i in middle school j, we are interested in the effect of each of the three treatment arms (indicator variables FF_{1j}, FF_{2j}, and FF_{3j}) on an outcome Y_{ij}. These outcomes, described in Section III.A, include applications to schools listed on Fast Facts and the supple-

[20] See Online Appendix A for details. 2013-14 was the most recent data on choices available at the time of matching.

mentary school lists, characteristics of high school choices, final match and enrolled school, and other outcomes of the admissions process. β_1-β_3 are the parameters of interest for the casual effect of the three treatments. We include controls for randomization block W_b and a vector of student and school demographic characteristics measured prior to the intervention (X_i and S_j respectively). Standard errors are adjusted to allow for clustering at the middle school level, the unit of randomization. The estimating equation is therefore:

$$Y_{ij} = \beta_1 FF_{1j} + \beta_2 FF_{2j} + \beta_3 FF_{3j} + \gamma X_i + \delta S_j + \sum_{b=1}^{39} \alpha_b W_{bj} + \epsilon_{ij} \tag{1}$$

To remove bias due to student mobility, we assigned students to their middle school of record in October of 8th grade. Recall that "treatment" here consists of the provision of materials via a short in-school presentation (and in the case of FF2, the offer of text message reminders). Beyond inference from systematic differences in the specific choices of treatment and control group students, we cannot observe whether students actually received or used the materials. Thus, our main impact estimates are best interpreted as intent-to-treat (ITT).

To examine variability in the effects of the intervention, we estimate equation (1) separately for select student subgroups. Our primary subgroups of interest are economic disadvantage (eligibility for free or reduced price meals), language spoken at home, prior achievement, and race/ethnicity. Results for other subgroups, such as gender and SWD status, are provided in Online Appendix D.

A description of our approach to analyzing text messaging effects on choices is provided in Online Appendix C. Because the causal effect of text messaging alone is not identified by our design, we relegate our discussion of this aspect of the intervention to that section.

IV. Main Results

A. Impact on high school choices

Tables 4 - 7 provide our main impact estimates. Given the experimental design and our interest in variability across treatments, we focus our discussion on the separate estimates by treatment arm. However, we also report pooled impact estimates in these tables and in related tables in Online Appendix D. While we find a number of notable and meaningful differences across the treatment arms, few differences were precise enough to be significant. Consequently, any differences in point estimates across treatment groups should be taken as suggestive.

Table 4 reports estimates of the intervention's effects on specific high school choices. Understanding whether students used the lists we provided is complicated by the fact that high schools with higher graduation rates—like those on Fast Facts—receive more applications in general. To address this, we also created Fast Facts and supplementary lists for control schools and calculated the percent of their choices that were drawn from these "counterfactual" lists. The top panel of Table 4 shows the estimated impact of the three interventions on the percent of 1st, 1st-3rd, and all choices drawn from the custom lists. Each impact estimate pertains to the list(s) of high schools actually received by the treatment group.

We found students in all three treatment arms were significantly more likely to apply to schools on their custom lists than were students in the control group. For example, students who received Fast Facts alone (FF1) were 9.3 percentage points more likely to rank a Fast Facts school as their first choice than students in the control group. Similarly, the percent of top three and all choices from Fast Facts were 10.4 percentage points higher among FF1 students. Each of these estimates is statistically significant at the 0.001 level, and the effects are large given that 37.2 (33.5) percent of control students' top three (all) choices appeared on their "counterfactual" Fast Facts list.[21] With a control group standard deviation of 32.6,

[21] Because Fast Facts consists of high-performing high schools within a short travel distance from the middle school, it is not surprising that more than 1 in 3 of the control group's choices are these schools.

a 9-10 percentage point increase in choices from Fast Facts represents a $0.28 - 0.31\sigma$ effect. To put this in perspective, a 10.4 percentage point increase in top three choices is equivalent to 1 in 3 students listing an additional Fast Facts school among their application's top three.

The extent to which students used the Fast Facts list depended on whether supplementary lists were provided. Students who received Fast Facts alone (FF1) chose comparatively more schools from their customized list than students who received additional information (FF2 and FF3). In contrast to FF1, students in the FF2 and FF3 treatment arms were only 3.3 and 4.4 percentage points more likely to rank a school from their combined lists as their first choice than students in the control group, though only the latter is statistically different from zero. The percent of top three choices from their respective lists were 5.5 percentage points higher among FF2 and FF3 students (in both cases $p < 0.01$ and an effect size of 0.17σ). The percent of *all* choices from these lists were 6.2 and 6.7 percentage points higher, respectively ($p < 0.001$ and an effect size of $0.27 - 0.29\sigma$). In sum, it appears our interventions had measurable effects on students' propensity to choose high schools from our custom lists. The point estimates suggest greater usage among students who received Fast Facts alone—without supplemental information—but we cannot reject the hypothesis of an equal effect across treatment arms.

While students in all treatment arms were more likely to apply to schools on their intervention-specific lists, these lists each included a minimum of 30 schools. We examined which of the Fast Facts schools students included on their application and where they ranked them. Figure 1 (top panel) shows that students' top-ranked choices were more influenced than their lesser choices. This is important, as the majority of students are matched to one of their top choices. Figure 1 (bottom panel) shows the FF1 treatment had a larger impact on students' propensity to apply to the *last 15* high schools (of the 30) than the first 15. This finding is intuitive when considering schools were sorted in descending order by graduation rate, and the first 15 represent more popular, better known schools. The last 15—which were also above our graduation rate threshold—appear to have been more novel options. This is noteworthy in a context where a surplus of applications to the city's highest-performing schools can result in congestion and limit the general equilibrium effect of applying to quality schools.

Finally, students in the FF2 treatment arm were significantly more likely to apply to schools appearing on the academically non-selective school supplement than students in the control group, by 6.7 percentage points (Online Appendix Table D.10). While not statistically different from FF1 and FF3, the point estimate for FF2 is nearly twice as large, suggesting FF2 students were receptive to the schools highlighted on their supplemental list. At the same time, we noted earlier the seemingly smaller effect of FF2 on students' overall application relative to FF1. As we show later, these together reflect a shift away from academically screened choices toward non-selective school choices. We did not observe a significant difference between FF3 and control students in the propensity to apply to schools specifically listed on the academic interest area supplement.[22]

B. Impact on graduation rate of choices and matches

Table 5 reports the estimated effects of our interventions on the graduation rates of students' top three high school choices and match. Perhaps surprisingly, students in the three treatment arms did not apply to schools with higher graduation rates, on average, than students in the control group. This may be because the control group mean was already high at 81%, approximately the 70th percentile in the distribution of NYC high schools in 2015-16 and the average for schools appearing on Fast Facts. Students in FF1 were, however, 3.0 percentage points less likely to rank schools with a low graduation rate (<70%) in their top three ($p < 0.10$). With a control group standard deviation of 31.6 points, this represents an effect size of 0.09σ. The effects for students in FF2 and FF3 were smaller and statistically insignificant.

All treatment arms reduced within-application *variability* in graduation rates by 1.8 to 2.1 percentage points relative to the control group. The largest point estimate was for FF2

[22] One exception is a small, statistically significant effect on the propensity to list a school from the interest area supplement as a first choice (not shown).

($p < 0.01$), with a reduction of 2.1 percentile points in graduation rate variability, an effect size of 0.16σ. Remarkably, the mean within-application range in graduation rates in the control group exceeded 30 percentage points. The impact of the interventions on variability appears to have come from the reduced propensity to apply to low graduation rate schools.

While choices are important, the match itself is more consequential for students' long-run outcomes. We find that students who received Fast Facts alone (FF1) *matched* to schools with a higher average graduation rate than students in the control group, by 1.7 percentage points, a statistically significant effect ($p < 0.01$) and an effect size of 0.12σ. Moreover, students in two of the three treatment arms were significantly less likely to match to a high school with a graduation rate below 70%. Students who received Fast Facts alone (FF1) were 6.3 percentage points less likely to match to a low graduation rate school, a 14.6 percent reduction relative to the control group. Students in the other two treatment arms (FF2 and FF3) were 5.1 percentage points ($p < 0.10$) and 3.3 percentage points (statistically insignificant) less likely to match to a low graduation rate high school, respectively. Each of these point estimates are meaningful in size, given that 43 percent of students in the control group matched to a low graduation rate school.

We also estimated treatment effects on the graduation rate of students' *enrolled* school in 9th grade, which can differ from their final match if the student appeals their match, moves, enrolls in a charter or specialized high school, or are grade retained. The point estimate for the impact of FF1 on the graduation rate of enrolled schools is smaller than that for the matched school (1.1 vs. 1.7 points), though we cannot reject equal effects. The same pattern holds for enrollment in a low graduation rate school versus match. The attenuated point estimates indicate that the effect of informational interventions can be muted when there are competing alternatives outside the main assignment process.

C. Impact on other school choice characteristics and outcomes

Table 6 shows that students in treatment schools were 3.1 to 3.5 percentage points more likely to be matched to their first choice high school (a 7-8% increase over the control group, in which 44.6% received their first choice), and 2.1 to 3.5 percentage points more likely to be matched to one of their top three choices (a 3-5% increase over the control group, in which 73.3% received a top three choice). These effects appear to be driven in part by students in treatment schools applying to schools where their odds of admission were higher. Table 7 shows that treated students were more likely to apply to academically non-selective schools, schools located in the same borough as their middle school, and schools in lower demand, all factors associated with a greater odds of admission. To provide an example, FF2 and FF3 students applied to programs with roughly 10% fewer applications per seat (as measured in 2014-15) than the programs on control students' applications. Students in the FF2 treatment group applied to 23 percent more limited unscreened high schools in their top three (7.9 percentage points on a baseline of 34.7%), consistent with their receipt of the non-selective school supplement. The increase in applications to non-selective programs appears to have come at the expense of applications to screened programs, which fell in all treatment groups, with a statistically significant decline in FF1 and especially FF2 (3.0 and 5.7 percentage points, respectively).

Several other aspects of treated students' choices may have contributed to a higher likelihood of matching to their top choices. First, students in the FF1 treatment arm included more same-borough schools in their top three than the control group (by 6.6 percentage points, a 0.18σ effect; $p < 0.01$). Given the use of geographic admissions priorities, this may have improved their odds of a match. We did not find a significant difference between treatment and control groups in travel time to their top three choices, despite our prioritizing of proximate schools when generating high school lists. Second, students in FF1 and FF3 were more likely to have been ranked by a screened or audition school. (Students at a minimum must be ranked by a selective program to be considered for admission. Schools may not rank students that do not meet their criteria.) Conditional on listing at least one screened or audition program in their top three, FF1 students were ranked by 6.7 percent more of these choices than the control group ($p < 0.001$), and FF3 students were ranked by an additional 4.7 percent ($p < 0.05$). These are small to modest-sized effects, given the control group standard deviation of 43.0. The mechanism behind this increased propensity is less clear. Students may have been more attentive to the admissions criteria of selective programs, something we emphasized on our intervention materials (especially FF3). Alternatively, the interventions may have altered the composition of screened schools to which students applied (perhaps to a less selective set) or altered the composition of students applying to screened schools, if the marginal student was induced to apply to more non-selective schools. Third, there is some suggestive evidence that students in treatment schools were more likely to have open house priority at non-selective limited unscreened schools ranked in their top three. These point estimates are positive—and largest for students in the FF2 treatment which emphasized open house attendance in its printed materials and text messages—but none is statistically significant.[23] While our experiment was not designed to separately identify the effects of FF2 materials and text messaging, the data do not suggest a strong response to the specific messages we sent, despite moderate take-up (see Online Appendix C for details).

The informational interventions did not impact the *number* of submitted choices, despite our explicit encouragement to list 12 schools (Table 6). In fact, there is some evidence that the FF2 and FF3 treatments reduced their average number of choices by approximately 0.6. This had no apparent effect on the odds of matching, however, and as noted, treated students were in fact more likely to receive their first choice.[24] Students in the FF1 and FF2 treatment arms were less likely to participate in the second round of high school admissions conditional on being matched in the first round (Table 6), although the point estimates are below the threshold of statistical significance. If negative, this might indicate greater satisfaction with

[23]We did not find that the FF3 treatment yielded more internal consistency in the academic interest areas of school choices; if anything, treated students' choices appear to be less concentrated in one interest area than students in the control group.

[24]More than 93% of students in the control group were matched in the first round.

their initial match. We find the opposite effect for students in the FF3 treatment arm, where the percentage of students returning to the second round was 1.6 percentage points higher than that in the control group ($p < 0.10$).

Roughly 88 percent of students in control schools ultimately enrolled in their matched school in 9th grade. We find the FF1 and FF3 interventions increased students' likelihood of doing so, by 2.6 and 1.8 percentage points, respectively ($p < 0.01$ and $p < 0.10$). This potentially indicates greater satisfaction with their match. Notably, we find that FF1 students were 1.5 percentage points less likely to enroll in a charter school for 9th grade than students in the control group. This is a relatively large effect when compared to the 5 percent of students in control schools that enrolled in a charter high school. The difference is important, as it tends to weaken the intervention's effect on enrolled school performance. (Charter high schools in 2015-16 had higher average graduation rates than schools in the traditional process to which our study students matched).

D. Sensitivity analyses

We conducted several sensitivity analyses for our main impact estimates. First, for the graduation rate models we imputed graduation rates for schools that did not yet have a graduating cohort, using the same method applied when generating Fast Facts lists (see Online Appendix B). The graduation rates used in Table 5 were missing for schools projected to have a graduation rate above 70% that were included on Fast Facts lists—as well as those projected to have a graduation rate below 70%—to which some students applied. The use of imputed graduation rates increased the number of students used in the estimation, but had little effect on the impact estimates (see Online Appendix Table D.2). Second, we re-estimated all models excluding schools that participated in our pilot study (Online Appendix Table D.3), and excluding charter schools, which may counsel students differently about high school choice by steering them to charter schools (Online Appendix Table D.4). The results were again very similar.

Finally, we examined the impact of our interventions on other measures of high school quality beyond graduation rates, including a 9th grade "on-track" indicator, a measure of college readiness, and the percent of students who feel safe at the high school (Online Appendix Table D.5). The first two are strongly correlated with four-year graduation rates and thus yielded estimates qualitatively similar to those in Table 5. An exception is a negative and statistically significant effect of the FF3 treatment on the mean on-track and college readiness rates at students' choices and matches, which corresponds to the negative FF3 point estimate on graduation rates in Table 5. We find a small but statistically significant *negative* effect of the interventions on the perceived safety at treatment students' choices and matches (the latter only significant for FF3, $p < 0.10$) For example, FF2 and FF3 led to a 0.84 and 0.92 percentage point reduction in the mean safety rating of students' top three choices, respectively ($p < 0.05$ and $p < 0.01$). These are moderate effects given the control group standard deviation of 6.8. The mechanism behind this change is less clear, although it is consistent with the treatment students in our study applying to more schools in their home borough (largely Bronx and Brooklyn, where schools have lower average safety ratings).

V. Impact heterogeneity

Our interventions were motivated by the observation that traditionally under-served populations in NYC—including free lunch eligible, Black and Hispanic, and students who do not speak English at home—are more likely to choose and subsequently match to schools with lower graduation rates, even conditional on prior achievement. While we targeted high-poverty middle schools for this experiment, students in our participating schools exhibit heterogeneity by income, achievement, race/ethnicity, EL and disability status, and other factors (see Table 3). Our large study sample permitted us to estimate impacts separately by subgroup. Results for our main subgroups of interest are shown in Tables 8-9, with additional results reported in Online Appendix D.

We focus here on four key outcomes: the percent of top three choices from the intervention-specific school lists, match to a first-choice high school, graduation rate of the matched high school, and match to a low graduation rate school. Separate estimates are reported by family income (FRPL eligibility), language spoken at home (English, Spanish, or other non-English), prior achievement (bottom or top quartile in 7th grade math), not present in a NYC public school in 7th grade, and race/ethnicity.[25] We include not present in 7th grade as a subgroup since they are plausibly least familiar with local schools and the school choice process. It is important to note that these subgroups are not mutually exclusive; White and Asian students in our data are more likely to be higher-achieving, for example.

We find nearly all subgroups responded to our intervention by applying to more schools on our lists. However, lower-income, lower-achieving, and Black students were no more likely to draw from them than their higher-income, higher-achieving, White, and Asian counterparts, and in some cases, usage was greater among the latter subgroups. Comparatively advantaged students were also more likely to benefit from the interventions through receiving their first choice. No subgroup responded by applying to schools with a higher average graduation rate, but nearly all saw reductions in their likelihood of matching to a low graduation rate school.

The first three columns of Table 8 report estimated impacts on the percent of 1st-3rd choices from the Fast Facts and supplementary lists. The effect on top three choices was positive and statistically significant for nearly every subgroup and treatment arm. However, Asian, White, and Hispanic students in treatment schools drew considerably more choices from these lists than Black students.[26] The intervention also had a larger impact on higher-achieving students' choices than lower-achieving students' choices. This is most striking for FF1, where the percentage of top three choices from Fast Facts increased by 16 percentage points for top quartile students (relative to the control group) but only 6.8 percentage points

[25]Subgroup estimates with a pooled treatment effect are reported in Online Appendix Tables D.6 and D.7. Impact estimates by gender, immigrant status, EL status, disability status, and other groups are reported in Online Appendix Tables D.8 and D.9.

[26]It is important to note that students identified as "White" in our study sample come from a wide variety of backgrounds. 23 percent were born outside of the U.S., with the largest shares born in Yemen, Uzbekistan, Russia, Algeria, and Egypt. Only 46% of White students spoke English at home.

for bottom quartile students. Notably, the interventions had larger effects on the choices of students who speak a language other than English at home than the choices of students in English-speaking households. For example, we find the percentage of top three choices drawn from Fast Facts was 12.7 and 13.7 percentage points higher for Spanish-speaking and other non-English language students in FF1, respectively, versus the control group. For English-speaking students, the effect was 6.2 percentage points.

The latter three columns of Table 8 report the impact of the interventions on the propensity to receive a first-choice match. These point estimates are often larger for higher-income and higher-achieving students. For example, non-FRPL students saw a positive effect of the FF1 and FF2 interventions on receiving their first choice match while FRPL students did not. (The opposite was true for FF3). In the same way, students in English-speaking households saw larger effects on receiving their first choice than students who speak another language at home, and top quartile students (in FF1 and FF3) saw larger effects than students in the bottom quartile. It may be that higher-achieving students face fewer barriers to admission when they apply to more selective schools on our list. This is one possible explanation for why large treatment effects on applications fail to translate into higher match rates for students who do not speak English at home. First choice match effects of FF1 and FF2 on students new to the district are very large (14-21 percentage points), though this subgroup is comparably small (N=801).

We find little evidence the interventions led any group to *apply* to schools with a higher average graduation rate, and for some groups the effect was negative.[27] Despite this, Table 9 shows that FF1 had a positive effect on the *matched* school graduation rate for most subgroups, including FRPL and non-FRPL students, students who speak English or Spanish at home, bottom quartile students, and Black, Hispanic, and Asian students. The largest point estimate was for students new to the school district (5.4 percentage points). Given the standard errors on these subgroup estimates, we cannot reject the hypothesis that they are

[27]These are not shown in Table 9, but see the pooled estimates in Online Appendix Tables D.6 and D.7.

the same. However, these results show the overall positive effect of FF1 on matched school graduation rates was not driven by any particular subgroup.

The final three columns of Table 9 show that most subgroups in treatment schools were less likely to match to a school with a graduation rate below 70%. However, point estimates tend to be larger for higher-achieving, White, and Asian students. For example, FF1 students in the top quartile of achievement were 11.3 percentage points less likely to match to a low graduation rate school than those in the control group, compared to 6 percentage points for students in bottom quartile. (The difference is larger for FF3, but smaller for FF2). Point estimates for FRPL and non-FRPL students are comparable. Students who speak a language other than English at home generally saw larger reductions in their likelihood of matching to a low graduation rate school than English-speaking students, particularly when assigned the FF1 treatment.

To summarize, students in nearly all subgroups and treatment arms responded to the informational interventions by applying to specific schools on our customized lists. Most subgroups in FF1 were more likely to receive their top choice and matched to schools with a higher average graduation rate. Similarly, most subgroups across treatment arms were less likely to match to low graduation rate schools. For all key outcomes, however, effects appeared largest for higher-achieving, White, and Asian students. Effects were often smallest for Black and lower-achieving students. Large effects on matched school quality for students new to the district and students speaking other languages at home are noteworthy.

VI. Discussion

School choice processes are complex and cognitively demanding. This is especially true in a large city like New York, which has both a very large number of options and multiple admissions mechanisms that require distinct actions. To increase their prospect of enrolling in a high-performing school, applicants and their families must be informed of their available choices and be attentive to admissions methods, screening criteria, and other priorities that affect their odds of admission. While public opportunities exist to learn about the process and available schools, prior evidence suggests that families with limited time and resources will nevertheless struggle with the system's scale and complexity.

Our field experiment was designed to address these challenges. Using a set of standardized rules, we created custom one-page lists of high schools for students in each participating middle school. Our aim was not for them to restrict their search to these schools, but to begin with an initially smaller set of choices with salient performance and admissions information. Recognizing that many students in our study would not be competitive for admission at academically selective schools, our lists included a mix of selective and non-selective schools where students' odds of admission were higher. All had four-year graduation rates at or above the citywide median. Two treatment arms provided additional supports (including text messages) to help students assess curricular fit and/or gain access to selective or over-subscribed schools. Unlike prior work, our intervention was focused on students as the key decision maker.

We found that students in schools receiving our informational interventions responded by applying to specific schools on their lists. Further, they made choices that resulted in a higher-performing high school match. This is not because they applied to higher-performing schools on average, but because they avoided low graduation rate schools and applied to schools where their odds of admission were higher. These included less academically selective schools, schools with fewer applications per seat, and schools where the student was more likely to have geographic priority. We also found that students engaged more with the process, as captured by their likelihood of attending an open house or meeting the requirements to be ranked by an audition or screened school.

Our results demonstrating the efficacy of directly providing curated information to low-income students opens multiple pathways for future intervention. Engaging parents of adolescents is challenging, a fact which has been well established in multiple bodies of literature

(Crosnoe & Ressler 2019). Adding to this challenge is that approximately 1 in 4 parents of school-aged children are immigrants whose dominant language is not English (Batalova, Hanna, & Levesque 2021). This is especially true in immigrant gateways like NYC, where more than half do not speak English at home. Given their presence in school buildings, students are more accessible targets for intervention, but prior work has raised questions about the efficacy of directing school information interventions to adolescents (Valant & Loeb 2014).

To be sure, there are students who could be more ambitious with their top-ranked choices. Under deferred acceptance, there is no penalty to ranking "reach" schools highly, and students should list schools in order of their true preference. On the other hand, it is common for students to mix well-known—and often highly selective—high schools with markedly lower-performing ones on their application.[28] The odds of admission are low at the former, leading to a probable match at the latter. In this case the student would be better served by giving more consideration to schools that are performing well but have higher odds of admission. One recent study took a different approach, embedding an alert system into school choice platforms in Chile and New Haven, finding that explicit warnings about low odds of admission led to more choices and a higher likelihood of match (Arteaga et al. 2021).

It is natural to be concerned about the general equilibrium implications of an informational intervention like ours at scale. In a city in which the supply of seats at high-performing schools is limited, an intervention that encourages more students to apply to already-oversubscribed schools will not necessarily improve equity in access to school quality. This is particularly true in NYC, where nearly a third of all high school programs use academic or other screening mechanisms for admission, rather than pure lotteries. We are encouraged that our intervention in 118 treatment schools did not congest applications at a small subset of schools. In fact, students in all treatment groups were more likely to receive their first choice. Of course, it is difficult to extrapolate to wider dissemination. For similar

[28]Recall that in control schools, the mean within-application range in graduation rates was more than 30 percentage points (Table 5).

reasons, it is difficult to determine whether the intervention would pass a cost-benefit test at scale. The intervention described in this paper was labor intensive, since we delivered a short presentation in each school at a cost of approximately $13 per treated student. In ongoing work, we are evaluating a lighter-touch intervention we implemented at a larger scale in 2016 and 2017 (Cohodes et al., 2022).

Our experiment was conducted in high-poverty middle schools serving students our prior research indicated fared worse in the high school choice process. Because families vary in their access to and use of information, we expected the interventions would reduce socioeconomic and racial disparities in choice behaviors and outcomes. Our results suggest reasons to be wary of this claim, since lower-income, lower-achieving, and Black students were no more likely to use our tools than other students in the same schools. Moreover, higher-achieving students were better positioned to benefit from them, since their odds of matching to academically selective schools were higher. It may be that supports for high school choice in high-poverty schools are more aligned with lower-achieving students, creating an opportunity to intervene for higher-achieving students in these schools (Avery & Pathak 2019). The implication is that informational interventions for school choice are not necessarily inequality-reducing. Their longer-run effect on inequality will depend on whether less-advantaged students benefit more from marginal changes in school quality. In future work we will estimate effects on these interventions on achievement, attainment, and other outcomes.

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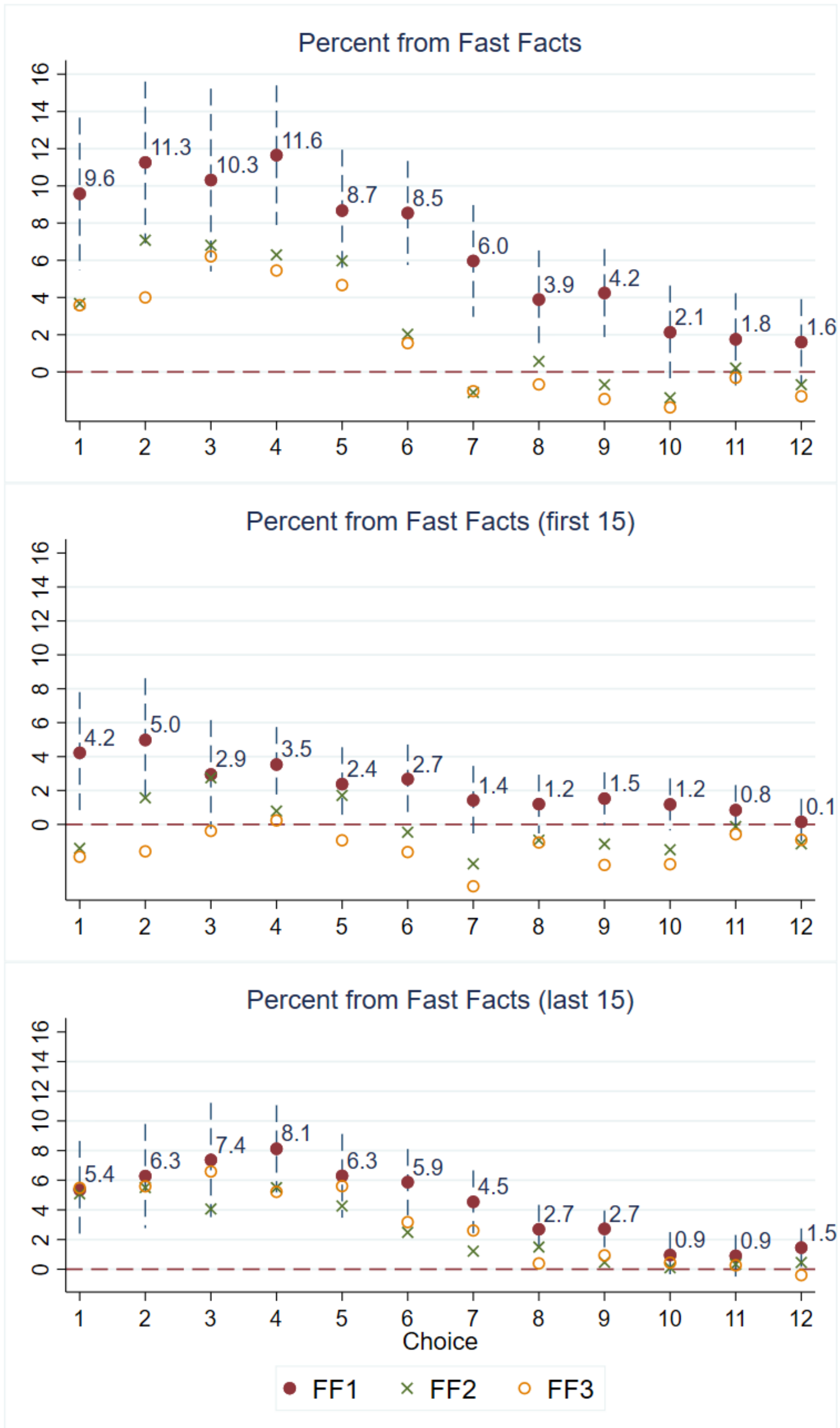
Figure 1: Impact of informational intervention on students' propensity to choose Fast Facts schools



Notes: each point estimate comes from a separate regression where the outcome is an indicator equal to one if the student chose a Fast Facts school as their kth choice (k=1 to 12). For clarity of presentation, a 95% confidence interval is shown for the FF1 point estimate only.

Table 1: Regression-adjusted differences in the graduation rates of high school choices and matches, by student background, 2014-15

	Without test score controls:				With test score controls:			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Graduation rate		% Low graduation rate		Graduation rate		% Low graduation rate	
	Choices 1-3	Match	Choices 1-3	Match	Choices 1-3	Match	Choices 1-3	Match
Free lunch eligible	-3.668***	-4.237***	7.275***	10.12***	-2.032***	-2.353***	4.159***	5.638***
	(0.106)	(0.150)	(0.308)	(0.521)	(0.101)	(0.143)	(0.303)	(0.514)
Reduced price eligible	-0.823***	-1.060***	0.201	1.867*	-0.531***	-0.630**	-0.0992	1.042
	(0.168)	(0.236)	(0.485)	(0.821)	(0.157)	(0.223)	(0.469)	(0.800)
Spanish at home	-1.558***	-1.308***	3.413***	4.811***	-0.703***	-0.522**	1.524***	2.816***
	(0.129)	(0.177)	(0.373)	(0.614)	(0.121)	(0.167)	(0.363)	(0.600)
Other language at home (not English)	-0.661***	-0.297	0.351	0.144	-0.203	0.285	-0.843*	-1.427*
	(0.138)	(0.195)	(0.398)	(0.678)	(0.130)	(0.186)	(0.387)	(0.665)
Black	-3.902***	-6.149***	13.99***	21.66***	-0.778***	-2.391***	7.749***	12.53***
	(0.141)	(0.196)	(0.407)	(0.680)	(0.136)	(0.190)	(0.406)	(0.681)
Hispanic	-3.692***	-5.850***	12.90***	20.24***	-1.305***	-2.907***	8.225***	13.16***
	(0.156)	(0.218)	(0.452)	(0.758)	(0.148)	(0.209)	(0.444)	(0.748)
Asian	2.450***	0.135	-0.982*	5.627***	1.700***	-0.226	0.315	6.400***
	(0.158)	(0.227)	(0.458)	(0.789)	(0.149)	(0.215)	(0.445)	(0.769)
Female	1.511***	2.087***	-4.283***	-6.683***	1.056***	1.519***	-3.258***	-5.190***
	(0.0817)	(0.113)	(0.236)	(0.394)	(0.0773)	(0.108)	(0.231)	(0.388)
Quadratic in reading and math and math z-scores	NO	NO	NO	NO	YES	YES	YES	YES
N	69,058	57,326	69,058	57,326	69,058	57,326	69,058	57,326

Notes: descriptive regressions using data from the 2014-15 high school admissions process. The regression models also include controls for special education status, "other race," and "other language spoken at home" (coefficient estimates not shown). Models in columns (5)-(8) include a quadratic function of 7th grade ELA and math z-scores as controls. Sample sizes are smaller for match regressions, as a greater proportion of matched high schools versus chosen schools lack published graduation rates (i.e., are newer schools). Standard errors in parentheses, * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table 2: Overview of treatment arms

Treatment group	Fast Facts	EL insert	Supplementary list	Other
FF1	Yes	Yes	None	None
FF2	Yes	Yes	Nonselective schools (18-25)	Opt-in to weekly text message reminders about open houses
FF3	Yes	Yes	Programs by academic interest area	None
Control	No	No	No	None

In all three treatment groups, trained research assistants delivered the materials via a 40-minute standardized lesson in a group setting. All materials were available in both English and Spanish.

Table 3: Student-level descriptive statistics, full study sample 2015-16

	%		Mean	Std. Dev.
Student characteristics:		Student achievement:		
White	5.7	7th grade ELA z-score	-0.285	0.924
Black	30.0	7th grade math z-score	-0.313	0.913
Hispanic	54.9	Missing 7th grade ELA (%)	9.3	–
Asian	8.5	Missing 7th grade math (%)	7.2	–
Other race/ethnicity	0.9			
Female	49.0	School characteristics:		
Free lunch eligible	74.2	Grade 8 enrollment	199.7	178.3
Reduced price eligible	3.7	% Female	48.5	4.2
Special education	21.7	% Asian	8.7	12.2
English learner (EL)	16.1	% Black	29.7	25.6
Immigrant	16.0	% Hispanic	54.9	24.5
		% White	5.7	9.4
Language spoken at home:		% SWDs	21.8	5.6
Spanish	36.7	% ELs	16.9	9.4
Other non-English	12.7	% Free/reduced price lunch	90.4	10.9
English	50.6	8th grade math	287.3	17.1
		8th grade ELA	288.5	11.0
Borough of middle school:		Missing 8th grade math (%)	11.0	–
Brooklyn	34.9	Missing 8th grade ELA (%)	10.2	–
Manhattan	11.8			
Queens	16.6	School in pilot study (%)	10.6	–
Bronx	36.7	Charter school student (%)	4.2	–

Notes: Authors' calculations using data from the NYCDOE. N=19,109

Table 4: Impact of informational interventions on choices from Fast Facts lists

	Treatment groups			
	FF1	FF2	FF3	Pooled
List of schools:	Fast Facts only	Fast Facts + nonselective supplement	Fast Facts + interest area supplement	Fast Facts only
% of choices from intervention-specific list:				
1st choice	9.268***	3.257	4.410*	5.689**
	(2.082)	(2.055)	(2.148)	(1.735)
1st-3rd choices	10.430***	5.503**	5.482**	7.425***
	(2.112)	(2.051)	(1.957)	(1.739)
All choices	10.430***	6.159***	6.740***	8.228***
	(1.835)	(1.782)	(1.754)	(1.559)
Control group mean [SD]:				
1st choice	40.5	41.0	43.4	40.5
1st-3rd choices	37.2	37.9	40.5	37.2
	[32.6]	[32.6]	[32.8]	[32.6]
All choices	33.5	34.2	37.3	33.5
	[23.1]	[23.1]	[23.1]	[23.1]

Notes: each *cell* in the top panel is the estimated effect of the FF1, FF2, FF3, or pooled treatment on the percent of 1st, 1st-3rd, or all choices from the intervention list of schools. The FF1-FF3 columns relate to the intervention-specific school lists. Pooled estimates relate to the list of schools common to all treatment groups (Fast Facts). Means and standard deviations for the control group are shown in the bottom panel. N=19,109 student observations in each regression. All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table 5: Impact of informational interventions on graduation rate of choices and matches

	Treatment groups				Control group	
	FF1	FF2	FF3	Pooled	Mean	SD
Graduation rate:						
1st-3rd choices (mean)	0.326	-0.367	-1.019+	-0.339	80.9	11.2
	(0.480)	(0.665)	(0.553)	(0.447)		
Final matched school	1.664**	0.526	-0.066	0.742	73.4	13.7
	(0.571)	(0.662)	(0.596)	(0.488)		
9th grade enrolled school	1.066+	0.298	-0.154	0.425	74.3	14.2
	(0.582)	(0.678)	(0.619)	(0.501)		
Graduation rate <70%:						
1st-3rd choices (mean)	-3.008+	-0.755	0.363	-1.210	23.1	31.6
	(1.718)	(2.221)	(2.080)	(1.684)		
Final matched school	-6.274*	-5.147+	-3.346	-4.914*	42.9	49.5
	(2.418)	(2.959)	(2.865)	(2.322)		
9th grade enrolled school	-5.133*	-3.768	-3.072	-4.034+	40.7	49.1
	(2.422)	(2.918)	(2.864)	(2.320)		
Within-application variability in gradrate:						
All choices (range)	-1.780**	-2.051**	-1.803**	-1.857***	30.2	13.7
	(0.647)	(0.665)	(0.618)	(0.503)		

Notes: each *row* reports estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Sample sizes vary from 16,075 (9th grade enrolled school) to 19,090 (variability in graduation rates). All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table 6: Impact of informational interventions on admissions and enrollment outcomes

	Treatment groups				Control group	
	FF1	FF2	FF3	Pooled	Mean	SD
Round 1:						
Number of Round 1 choices	-0.067	-0.570*	-0.603*	-0.390+	8.7	3.0
	(0.227)	(0.266)	(0.255)	(0.209)		
Matched to 1st choice	3.104+	3.530+	3.539*	3.370*	44.6	–
	(1.651)	(1.794)	(1.655)	(1.351)		
Matched to 1st-3rd choice	2.116	2.725+	3.499*	2.773*	73.3	–
	(1.437)	(1.590)	(1.527)	(1.233)		
Round 2 and later:						
Participation in Round 2 after main round match	-1.432	-1.355	1.640+	-0.304	12.6	–
	(0.906)	(1.001)	(0.913)	(0.720)		
9th grade enrollment in matched school	2.577**	-0.289	1.787+	1.558+	88.0	–
	(0.954)	(1.023)	(1.034)	(0.810)		
Enrolled in a charter high school	-1.506+	-0.207	-0.666	-0.869	5.2	–
	(0.774)	(0.852)	(0.769)	(0.650)		

Notes: each *row* represents estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Sample sizes vary from 18,019 (participation in supplemental round conditional on first round match) to 19,109 (match to 1st choice). All regression models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table 7: Impact of informational interventions on other characteristics of chosen schools

	Treatment groups				Control group	
	FF1	FF2	FF3	Pooled	Mean	SD
School location:						
Travel time (minutes),	-1.160	0.855	1.394	0.281	32.4	13.1
1st-3rd choices	(1.052)	(1.071)	(0.877)	(0.797)		
Percent in same borough,	6.577**	1.017	2.758	3.771*	76.9	35.8
1st-3rd choices	(2.014)	(2.190)	(1.841)	(1.572)		
School popularity:						
Demand (apps per seat),	-0.319	-0.751+	-0.796*	-0.602*	14.1	7.7
1st-3rd choices	(0.312)	(0.407)	(0.340)	(0.281)		
Admissions methods and priority:						
Limited unscreened,	2.811+	7.875***	2.591	4.032**	34.7	35.0
1st-3rd choices	(1.637)	(1.869)	(2.015)	(1.434)		
Screened,	-3.040+	-5.676**	-2.577	-3.547*	35.1	35.1
1st-3rd choices	(1.765)	(1.757)	(2.089)	(1.453)		
Limited unscreened, % 1st-3rd	2.513	2.906	1.466	2.268	51.4	43.2
choices with open house priority	(1.825)	(2.063)	(2.147)	(1.662)		
Screened, % 1st-3rd	6.653***	0.223	4.669*	4.228**	41.0	43.0
choices where ranked by school	(1.686)	(1.963)	(1.860)	(1.499)		
Interest areas:						
Percent largest interest area	-2.721**	-1.268	-0.790	-1.650+	55.0	18.4
category (all choices)	(0.913)	(1.105)	(1.109)	(0.911)		

Notes: each *row* represents estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Sample sizes vary from 11,826 (percent of screened choices where ranked by school) to 19,109 (percent in same borough). All regression models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table 8: Subgroup impact estimates: usage and match rates

	Usage: % of 1st-3rd choices from intervention-specific list			Matched to 1st choice			N
	FF1	FF2	FF3	FF1	FF2	FF3	
Full study sample	10.43***	5.503**	5.482**	3.104+	3.530+	3.539*	19109
	(2.112)	(2.051)	(1.957)	(1.651)	(1.794)	(1.655)	
FRPL eligible	10.45***	4.974*	5.344**	2.905	1.616	3.769*	14822
	(2.143)	(2.092)	(1.916)	(1.771)	(1.963)	(1.750)	
Not FRPL eligible	9.815***	7.187**	5.224*	3.704+	9.380***	2.791	4224
	(2.301)	(2.382)	(2.417)	(2.170)	(2.319)	(2.190)	
Spanish spoken at home	12.65***	5.830**	8.016***	0.0137	-1.474	4.396+	7022
	(2.078)	(2.174)	(1.956)	(2.081)	(2.505)	(2.343)	
Other non-English spoken at home	13.72***	14.00***	8.856**	0.762	5.644	-0.729	2419
	(3.183)	(4.167)	(2.908)	(4.334)	(4.433)	(3.372)	
English spoken at home	6.219***	3.942*	1.836	4.128*	3.957*	2.571	9668
	(1.827)	(1.795)	(1.815)	(1.783)	(1.803)	(1.887)	
7th grade math bottom quartile	6.812**	5.041*	6.157**	1.714	2.601	5.281*	6018
	(2.136)	(2.131)	(1.874)	(2.212)	(2.316)	(2.151)	
7th grade math top quartile	16.05***	8.420**	6.842*	6.988+	-0.181	9.400*	2128
	(3.211)	(3.149)	(3.024)	(3.551)	(3.821)	(3.656)	
Not present in 7th grade	9.087*	4.904	5.641	14.25**	20.62***	-1.498	801
	(3.973)	(4.559)	(3.535)	(5.384)	(5.474)	(5.282)	
White	13.06***	8.578*	19.25***	12.26**	13.01*	-0.900	1091
	(3.359)	(4.184)	(3.683)	(4.346)	(5.819)	(4.968)	
Black	4.528*	2.638	0.302	5.254*	4.988*	3.431	5718
	(1.785)	(1.745)	(1.575)	(2.108)	(2.201)	(2.373)	
Hispanic	11.34***	5.798**	6.934***	0.321	-0.412	2.536	10454
	(2.143)	(2.075)	(1.799)	(1.872)	(2.098)	(1.961)	
Asian	14.20**	17.50**	10.93**	14.22**	16.92**	-0.383	1612
	(4.613)	(5.652)	(3.494)	(5.423)	(5.648)	(4.584)	

Notes: Each row and column set (FF1-FF3) represents estimates from a separate regression for the indicated subgroup. Student and school covariates and block effects included (as in earlier tables). Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table 9: Subgroup impact estimates: graduation rates of matched school

	Graduation rate matched school			Graduation rate: below 70% matched school			N
	FF1	FF2	FF3	FF1	FF2	FF3	
Full study sample	1.664**	0.526	-0.066	-6.274*	-5.147+	-3.346	16657
	(0.571)	(0.662)	(0.596)	(2.418)	(2.959)	(2.865)	
FRPL eligible	1.690**	0.635	0.0213	-6.577*	-5.483+	-3.071	12949
	(0.604)	(0.712)	(0.619)	(2.565)	(3.124)	(2.944)	
Not FRPL eligible	1.518*	0.430	-0.099	-5.493+	-4.695	-4.959	3659
	(0.688)	(0.658)	(0.795)	(2.814)	(3.016)	(3.696)	
Spanish spoken at home	1.415*	-0.334	-0.368	-6.290**	-2.716	-3.654	6180
	(0.623)	(0.752)	(0.854)	(2.352)	(3.121)	(3.172)	
Other non-English spoken at home	0.936	2.828*	-2.114+	-9.456*	-19.53***	1.488	2069
	(1.050)	(1.334)	(1.153)	(4.084)	(4.964)	(4.664)	
English spoken at home	1.309*	0.586	0.0836	-3.755	-4.679	-1.923	8408
	(0.653)	(0.721)	(0.615)	(2.409)	(2.862)	(2.515)	
7th grade math bottom quartile	1.700*	1.146	-0.404	-6.036*	-8.132**	-0.756	5313
	(0.701)	(0.771)	(0.711)	(2.442)	(2.978)	(2.740)	
7th grade math top quartile	2.055	0.632	1.233	-11.29*	-7.377	-11.12*	1693
	(1.263)	(1.475)	(1.477)	(4.877)	(5.301)	(5.160)	
Not present in 7th grade	5.430*	0.837	2.756	-12.95+	2.554	-10.34+	675
	(2.189)	(2.201)	(1.857)	(6.752)	(7.935)	(6.219)	
White	0.896	2.493	0.0262	-10.27+	-16.01+	-18.29**	947
	(1.635)	(2.308)	(1.846)	(5.696)	(9.270)	(6.961)	
Black	1.330+	0.149	-0.380	-3.935	-4.025	1.230	5032
	(0.744)	(0.803)	(0.732)	(2.472)	(2.862)	(2.578)	
Hispanic	1.801***	0.570	0.182	-6.680**	-4.606	-4.858+	9147
	(0.514)	(0.691)	(0.674)	(2.135)	(2.942)	(2.697)	
Asian	4.304*	1.781	0.945	-18.56**	-20.13*	-11.75+	1344
	(1.810)	(2.268)	(1.934)	(6.860)	(7.747)	(6.730)	

Each row and column set (FF1-FF3) represents estimates from a separate regression for the indicated subgroup. Student and school covariates and block effects included (as in earlier tables). Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Online Appendix

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A Sampling procedure and treatment assignment

1. Sampling

To construct our initial middle school sampling frame, we began with two school-level datasets from the NYCDOE: the 2014-15 Demographic Snapshot, and the LCGMS extract from March 22, 2015. The latter is a file updated daily showing all NYC schools in operation, allowing us to identify school status changes since the Demographic Snapshot was released. From these two files we identified **566** schools that enrolled a minimum of 30 students in 8th grade *or* had zero 8th graders but at least one student in 7th grade. (This second condition retained some newer schools that did not serve 8th grade in 2014-15 but may have in 2015-16). We retained charter schools but excluded District 75 (special education) schools.

All 16 schools in Staten Island were dropped from this initial set, as were 109 schools that enrolled at least one 9th grader. We excluded Staten Island middle schools because they were comparatively more advantaged than those targeted by this study. Their effective set of high school choices is also more limited due to Staten Island's lower population density and geographic isolation. Schools that enrolled 9th graders were excluded because 8th graders in those schools frequently remain there for 9th grade. An additional three schools that served unusually high proportions of students with disabilities (>50%) or English language learners (>90%) were dropped. **438** eligible schools remained after these drops.

Geocoded student residential addresses from 2012-13 were used to calculate for each middle school the percent of 8th graders living in low-income Census tracts, defined using the population share with income below 150% of the poverty line from the American Community Survey. (2012-13 was the most recent address file available at the time this sampling frame was produced).[29] The working sample of 438 schools was split into quartiles based on this poverty measure.

[29]Schools in the working sample not observed in the 2012-13 data were geocoded to Census tracts. In place of the student average measure, we used the 5-year (2009-2013) poverty estimate for the Census tract in which the school was located.

Schools in the top two quartiles of poverty comprised our "high-poverty" recruitment pool (N=217). We sorted these in random order and began recruiting from the top of this list (see the following subsection for details on recruitment). When it became apparent we would need schools beyond this list, we created a "mid-poverty" recruitment pool consisting of the next quartile of schools (N=108).

Table A.1 provides mean characteristics of: (1) all NYC schools that served 8th grade in 2014-15 *or* served 7th grade in 2014-15 with the potential to serve 8th grade in 2015-16 (N=592); (2) all NYC schools in the baseline sampling frame (N=438); (3) all schools in the high-poverty recruitment pool (N=217); (4) all schools in the mid-poverty recruitment pool (N=108); and (5) all schools that participated in the study (N=165). (The fifth group is described later). Notably, the recruitment pools and study sample include a greater share of schools located in the Bronx and Brooklyn relative to the full population of schools serving 8th graders. Study schools also enrolled a higher share of Hispanic students, English language learners, and (by design) low-income students. They also tended to be smaller, and were less likely to be charter schools than the full population.

Table A.2 provides mean outcomes of the high school admissions process in 2013-14 for the same five groups of schools. (These outcomes are observed two years prior to the study, and were the latest available at the time of sampling). The sample sizes reported in this table are smaller than those in Table A.1, as not all schools had 8th graders participating in the admissions process in 2013-14. In that year, 8th graders in our study schools applied to high schools with lower graduation rates, on average, than did students in the full population. A larger share of high schools on their application used the limited unscreened admissions method, and a smaller share of students were unmatched after the main round.

2. Treatment assignment

As of August 12, 2015, 167 schools had agreed to participate in the study. We dropped two schools that we learned were screened middle schools that required an exam for admission, and a third that shared a guidance counselor with another recruited school.[30] This left 164 schools: 61 in the Bronx, 58 in Brooklyn, 29 in Manhattan, and 16 in Queens (Table A.3). Of these we aimed to assign 39 to each treatment arm (117 total) and 47 to the control group. The top panel of Table A.3 shows how these counts are divided by borough.

We randomly assigned schools to treatment and control groups within matched blocks of similar schools. Blocks were formed within the eight strata shown in the bottom panel of Table A.3. These include four borough strata, a fifth stratum of eight schools in the geographically isolated Rockaways section of Queens, and three strata of schools (23 total) that participated in our 2014-15 pilot study. Because pilot schools had previously been offered a treatment, had prior interactions with our team, and agreed to participate again, they likely differ systematically from other recruited schools. We therefore blocked these schools separately within borough. We also wished to ensure these schools received a treatment as a reward for past participation, so all of them were forced into one of the three treatment groups, chosen at random. Nearest neighbor matches were drawn from the borough at large to serve as controls for the pilot schools. (This explains why the total number of schools in the pilot blocks [32] exceeds the number of pilot schools [23]).

Within each matched block we aimed to have one school assigned to each treatment arm (FF1, FF2, and FF3) and at least one school assigned to the control group. Since there were more schools planned for the control group than any single treatment arm, some blocks have more than one control. In total, 39 blocks were formed, 8 of which were blocks consisting primarily of pilot schools.[31]

Mahalanobis nearest-neighbor matching was used to form blocks of similar schools within

[30]Students in the two screened schools fare well in the high school choice process and a large fraction are admitted to the city's specialized high schools. We dropped these schools since they are outside the target population for this study. The third school was dropped because it would be impossible to randomly assign schools that share a guidance counselor to different treatment conditions and maintain compliance. As described later, these three schools were returned to the study sample after randomization.

[31]The pilot study blocks consist of schools in the same borough, but not necessarily the same randomization block from the pilot study. In forming the 8 pilot study blocks we aimed to group schools that were in the same randomization block from the pilot study, or the same geographic school district when the former option was not possible.

each of the eight blocking strata listed in Table A.3. This procedure began by sorting schools randomly within strata. The first school was drawn and its three nearest neighbors identified. These four schools were removed, and the next school was drawn along with its three nearest neighbors, and so on. The school variables used in the matching procedure were as follows:

- Percent of high school applicants in 2013-14 with no main round match
- Mean graduation rate of students' top three choices in 2013-14 (main round)
- The percent of top three choices in 2013-14 (main round) that were limited unscreened schools
- Mean scale score of 8th grade students in 2013-14 in English language arts (ELA) and mathematics
- Grade 8 enrollment in 2013-14 (or if none, grade 7)
- Percent eligible for free or reduced price meals in 2013-14 (school-wide)

Means for several of these variables were reported in Table A.1-A.2. Some schools lacked 8th grade scale scores or choice outcomes from 2013-14 if they did not have an 8th grade class in that year. In these cases we imputed using the mean for other recruited schools in the same borough. After matched blocks were formed, we used the original random number to assign schools to the three treatment and control conditions. One school that was originally dropped because it shared a guidance counselor with another recruited school was added back at this point, and assigned to the same block and treatment as its companion school. This brought the total number of study schools to **165**.

After forming matched blocks, we ran several tests for balance. First, we estimated a set of regression models in which the dependent variables differed but the same set of explanatory variables were used (*mvreg* in Stata). Explanatory variables included the three treatment group indicators, an indicator for pilot study participation, and block fixed effects. A p-value was obtained for the joint hypothesis that coefficients on the treatment indicators were zero across all regression models. Next, in separate models we regressed treatment group assignment (FF1, FF2, or FF3) on a full set of school covariates. These covariates included all of the matching variables listed above, as well as the percent English language learners, percent with disabilities, percent female, percent by race and ethnicity, percent of students scoring at the lowest level in ELA (Level 1), percent scoring at the lowest level in mathematics (Level 1), and a charter school indicator. (These same variables were used as dependent variables in the first balance test). In these regressions a p-value was obtained for the joint significance of school characteristics in explaining treatment assignment.

We had no reason to expect the first iteration of matching and blocked randomization to yield the "best" possible balance. In the interest of identifying an *ex ante* well-balanced set of treatment assignments, we executed the above blocked randomization procedure—beginning with nearest neighbor matching—50 times. We then looked for iterations with the largest p-values and few (if any) statistically significant associations between treatment assignment and school characteristics. Of the 50 iterations, we chose a randomization with $p = 0.66$ for the first balance test and $p = 0.78$, 0.96, and 0.86 for the second balance tests. Coefficients from the latter three regression models are reported in Table A.4. The only explanatory variable that has a statistically significant association with treatment assignment is pilot study participation, which is expected given that pilot schools were purposefully assigned to a treatment. Results are similar, with p-values of 0.70, 0.99, and 0.82, when the pilot study indicator was omitted from the regressions.

Table A.5 reports the mean characteristics of schools in our study's treatment and control groups. Three additional schools volunteered to participate in our study, and the two recruited academically selective schools that were originally dropped were added back as control schools, increasing the number of participating schools to 170. However, these five schools (2 control, 2 FF1 and 1 FF3) are not included in Tables A.4-A.5 since they were not part of the original block randomization. Only students from the 165 schools in the original blocked random assignment are used in the main results of this paper.

Table A.1: Mean school characteristics, 2014-15

	(1) All schools	(2) Baseline sampling frame	(3) High-pov. recruit. pool	(4) Mid-pov. recruit. pool	(5) Study schools
N	592	438	217	108	165
Charter school	0.147	0.105	0.124	0.093	0.079
Brooklyn	0.331	0.340	0.341	0.481	0.358
Manhattan	0.215	0.199	0.258	0.157	0.176
Queens	0.184	0.199	0.028	0.204	0.097
Bronx	0.243	0.263	0.373	0.157	0.370
Staten Island	0.027	–	–	–	–
High-poverty recruitment pool	0.341	0.461	0.931	–	0.630
Mid-poverty recruitment pool	0.182	0.247	–	1.000	0.327
Pilot study participant	0.039	0.052	0.101	0.000	0.139
% Female	49.5	49.2	49.1	48.6	48.7
% Male	50.5	50.8	50.9	51.4	51.3
% Asian	9.4	10.2	4.3	13.1	5.0
% Black	37.2	35.9	36.8	40.5	37.4
% Hispanic	41.1	42.4	55.3	34.5	51.6
% Other race	1.5	1.4	0.9	1.5	1.1
% White	10.8	10.1	2.6	10.3	4.8
% SWD	20.2	20.5	23.1	19.7	22.9
% EL	11.0	12.3	16.2	11.2	14.8
% FRPL	80.3	82.1	89.7	84.3	88.9
Census tract residential poverty	38.2	38.3	49.1	34.6	45.0
Mean 8th grade math scale score	291.7	291.6	284.1	293.8	284.7
Mean 8th grade ELA scale score	294.4	293.5	284.6	295.5	286.8
Enrollment	591.2	576.7	426.6	647.0	473.2
Grade 8 enrollment	123.5	134.6	98.8	153.0	116.0
Grade 9 enrollment	19.0	0.0	0.0	0.0	0.0

Notes: authors' calculations using data from the NYCDOE and American Community Survey (for Census tract poverty rates). School enrollment and demographic data come from the 2014-15 NYCDOE Demographic Snapshot.

Table A.2: Mean high school admissions process outcomes, 2013-14

	(1) All schools	(2) Baseline sampling frame	(3) High-pov. recruit. pool	(4) Mid-pov. recruit. pool	(5) Study schools
N	530	382	189	98	147
Graduation rates:					
1st choice	83.3	82.8	80.0	83.5	80.7
1st-3rd choices	82.2	81.6	79.1	82.2	79.7
All choices	81.0	80.4	78.0	80.8	78.5
Final matched school	76.5	75.5	72.2	76.2	72.9
9th grade enrolled school	76.9	75.7	72.3	76.4	73.0
Variability in gradrate (range)	22.4	24.3	27.5	23.3	26.5
Graduation rates <70%:					
1st choice	14.1	15.5	20.1	13.9	19.5
1st-3rd choices	16.3	17.7	22.5	16.2	21.7
All choices	18.9	20.6	25.7	18.9	24.8
Final matched school	30.6	34.0	41.3	31.3	39.7
9th grade enrolled school	30.2	33.8	41.3	31.0	39.9
Number of main round choices	7.0	7.7	8.4	7.3	8.1
Matched to 1st choice	48.3	44.6	46.2	43.6	45.6
Matched to 1st-3rd choice	75.1	73.6	75.6	72.4	75.3
Participation in R2 after main round match	9.7	10.3	10.7	11.3	10.9
9th grade enrollment in matched school	88.2	89.9	88.9	91.5	89.4
Enrolled in a charter high school	0.1	0.0	0.1	0.0	0.1
Percent in same boro, choices 1-3	79.1	79.3	75.0	81.2	76.3
Limited unscreened, choices 1-3	34.8	35.8	45.6	31.3	44.1
Screened, choices 1-3	38.4	35.8	29.2	38.2	30.4

Notes: authors' calculations using 2013-14 high school admissions data from the NYCDOE (the most recent available at the time of randomization to treatment assignment).

Table A.3: Counts of schools by treatment group, borough, and blocking group

	Treatment groups:				
	FF1	FF2	FF3	Control	Total
Borough totals:					
Bronx	14	14	14	19	61
Brooklyn	14	14	14	16	58
Manhattan	7	7	7	8	29
Queens	4	4	4	4	16
Total	39	39	39	47	164
Blocking group totals:					
Bronx	10	10	10	15	45
Brooklyn	11	11	11	13	46
Manhattan	6	6	6	7	25
Queens	2	2	2	2	8
Queens (Rockaways)	2	2	2	2	8
Bronx (pilot)	4	4	4	4	16
Brooklyn (pilot)	3	3	3	3	12
Manhattan (pilot)	1	1	1	1	4
Total	39	39	39	47	164

Notes: This table shows our planned assignment of 164 recruited schools to treatment and control groups. Schools were randomly assigned to treatments within matched blocks of similar schools. Blocks were formed within the eight strata of schools listed in the bottom panel. 23 pilot study schools were blocked separately within borough, and all pilot schools were assigned to one of three treatments (none to control). Nearest neighbor (non-pilot) matches from the same borough were selected as controls for the pilot study blocks. One additional non-pilot school in Brooklyn was added to the Brooklyn (pilot) group to balance one of the blocks. After treatment groups were assigned, the 165th school (which shared a guidance counselor with another participating school) was forced into the same treatment group.

Table A.4: Balance test: predicting treatment assignment using school characteristics

	(1) FF1 vs C	(2) FF2 vs C	(3) FF3 vs C
Percent with no R1 match	-1.602	1.958	1.644
	(-0.482)	(0.624)	(0.574)
Graduation rate of top 3 choices	0.068	-0.012	-0.074
	(1.186)	(-0.225)	(-1.459)
Percent of top 3 choices limited unscreened	0.018	0.016	-0.012
	(1.054)	(1.058)	(-0.780)
Mean 8th grade math score	0.053	-0.015	0.019
	(1.362)	(-0.420)	(0.516)
Mean 8th grade ELA score	-0.015	0.019	0.039
	(-0.355)	(0.390)	(1.005)
Grade 8 enrollment	0.003	-0.002	-0.001
	(1.783)	(-1.077)	(-0.741)
% Free or reduced price lunch	0.008	-0.006	0.003
	(0.487)	(-0.473)	(0.238)
% EL	-0.015	-0.002	0.014
	(-0.540)	(-0.068)	(0.675)
% SWD	0.006	-0.003	0.034
	(0.235)	(-0.105)	(1.527)
% Female	0.003	-0.022	0.022
	(0.111)	(-0.611)	(1.029)
% Black	0.024	-0.017	-0.015
	(1.198)	(-0.866)	(-0.634)
% White	0.045	-0.014	-0.044
	(1.358)	(-0.493)	(-1.287)
% Hispanic	0.021	-0.021	-0.020
	(1.162)	(-1.163)	(-0.874)
Charter school	0.360	0.225	0.402
	(0.604)	(0.508)	(0.709)
Percent ELA level 1	0.010	0.015	0.007
	(0.355)	(0.420)	(0.263)
Percent Math level 1	0.032	-0.011	0.015
	(1.122)	(-0.402)	(0.495)
Pilot study	0.962*	1.046**	0.910*
	(2.496)	(2.940)	(2.378)
Constant	-21.997	3.008	-11.469
	(-1.500)	(0.148)	(-0.660)
N	86	86	87
Joint p-value	0.780	0.964	0.863

Notes: t-statistics in parentheses. $* = p < 0.05$ $** = p < 0.01$. All regressions include randomization block fixed effects.

Table A.5: Mean school characteristics by treatment group, 2014-15

	Treatment groups:			
	FF1	FF2	FF3	Control
N	39	39	40	47
Charter school	0.077	0.103	0.075	0.064
Brooklyn	0.359	0.359	0.375	0.340
Manhattan	0.179	0.179	0.175	0.170
Queens	0.103	0.103	0.100	0.085
Bronx	0.359	0.359	0.350	0.404
In high poverty sampling frame	0.667	0.513	0.700	0.638
In mid poverty sampling frame	0.308	0.410	0.275	0.319
Pilot study participant	0.205	0.205	0.175	0.000
% Female	48.7	48.6	48.6	48.7
% Male	51.3	51.4	51.4	51.3
% Asian	4.3	4.9	5.8	5.0
% Black	37.1	40.1	38.3	34.7
% Hispanic	52.0	49.1	50.8	54.1
% Other race	1.2	0.9	1.1	1.2
% White	5.4	4.9	3.9	4.9
% SWD	23.5	22.3	23.6	22.2
% EL	13.9	13.7	15.6	15.7
% FRPL	89.7	87.1	90.3	88.7
Census residential poverty	45.0	42.8	46.3	45.6
Mean 8th grade math scale score	283.7	284.1	285.3	285.4
% Level 1 math	51.9	50.2	48.6	48.3
% Level 4 math	2.2	2.8	3.4	2.9
Mean 8th grade ELA scale score	285.6	287.0	287.4	287.1
% Level 1 ELA	42.5	39.5	39.1	39.1
% Level 4 ELA	2.7	2.7	3.2	3.5
Enrollment	498.0	462.5	414.9	511.1
Grade 8 enrollment	132.4	99.6	112.8	118.6
Grade 9 enrollment	0.0	0.0	0.0	0.0
Mean graduation rate - all choices	79.4	79.5	79.0	79.7
Mean graduation rate - top 3 choices	80.6	80.5	80.2	80.7
Mean graduation rate - 1st choice	81.3	81.1	81.1	81.7
Percent of all choices limited unscreened	46.0	45.2	41.6	42.5
Percent of top 3 choices limited unscreened	45.5	44.4	39.5	42.0
Percent of 1st choices limited unscreened	45.3	43.6	38.4	41.7
Percent with SPHS offer	0.0	0.0	0.0	0.0
Percent with LGA offer	0.0	0.0	0.0	0.0
Percent with no R1 match	0.1	0.1	0.1	0.1

Notes: One recruited school that shared a guidance counselor with a second recruited school was omitted from the original block randomization and later added back to FF3 (the group to which its companion school was randomly assigned). This explains why FF3 includes 40 schools instead of the original 39 from Table A.3. High school choice outcomes in the bottom section of the table are from 2013-14.

B Production of intervention materials

Study schools were randomized into three treatment arms and a control group. Schools in the first treatment arm (FF1) received a "Fast Facts" list of proximate high schools. Schools in the second treatment arm received Fast Facts and a supplementary list of academically non-selective "limited unscreened" schools that give priority admission to students who attend an open house. This group was also invited to receive text message reminders about these open houses. Schools in the third treatment arm received Fast Facts and a supplementary list of high school programs organized by academic interest area. All treatment schools received a one-page insert of "screened language" programs citywide that exclusively serve recent immigrants new to the English language.

The procedure we used to generate Fast Facts and supplementary lists drew from three primary data sources:

- The 2015-16 NYC High School Directory, which includes (among other things) graduation rates, program interest areas, and admissions methods. The graduation rate pertained to the cohort graduating in 2013-14, the most recent available at the time of printing.
- Imputed graduation rates for high schools that had not yet had a graduating cohort.[32]
- Travel time by walking or public transit from every middle school to every high school in NYC, calculated using the Google Maps API during August 2015.

Our starting point for creating Fast Facts was a list of all middle-high school combinations with their travel time by public transit (N=256,082). We dropped high schools that primarily served continuing 8th graders, reducing the list to 234,986 cases. For each high school we

[32]We predicted graduation rates for these high schools using a quadratic function of their 9th grade "on track" indicator (the percent completing 10 or more credits in 9th grade). The prediction model used all high schools with non-missing graduation rates and 9th grade "on track" indicators from 2014-15. The upper limit of the 95% prediction interval was used as the imputed graduation rate for schools lacking this information. High schools that were so new that they lacked both performance measures were omitted from the list.

retained information about its graduation rate (using the imputed version where necessary), admissions methods, interest areas, and directory page number.

Importantly, we produced these three lists for *all* study schools, regardless of their actual treatment assignment. Doing so provided a "counterfactual" Fast Facts list for schools that were not selected to receive one (or were assigned to receive a different version).

1. Fast Facts

Fast Facts sheets were provided to students in every treatment arm (FF1, FF2, and FF3). Each consisted of a list of 30 high schools. Our procedure for creating Fast Facts was as follows. For every middle school we identified all high schools with a graduation rate of 70% or higher that were within a 45-minute commute from that middle school.[33] This list was sorted by travel time (ascending), graduation rate (descending), and school name (ascending, to break ties and to ensure replicability). The first 10 high schools in this ordered list were immediately flagged for inclusion on Fast Facts. We then successively added schools as long as the cumulative number of *screened* schools was ≤ 10, the number of *new* schools was ≤ 10, and (in select cases) the number of schools located in a different borough was ≤ 10.[34] Schools that would put the Fast Facts list over these limits were skipped. Once 30 schools was reached, the list was finalized. In cases where 30 schools could not be identified with this procedure, we relaxed the graduation rate and commuting time restrictions.[35] High schools were listed on Fast Facts in descending order by graduation rate and (in the case of ties) alphabetically by school name. The imputed graduation rate was used in the sorting order for new schools, although the imputed rate was not displayed on the sheet. (Rather, the graduation rate reads "*new school").

[33]For schools in the Rockaways section of Queens we relaxed the commuting time requirement to 60 minutes.

[34]This restriction was imposed for 27 middle schools where we observed students very rarely applying to high schools outside of their own borough.

[35]In the Rockaways, the relaxed criteria were a graduation rate of 65% and a maximum commuting time of 75 minutes. For all other schools the relaxed criteria were a graduation rate of 65% and a maximum commuting time of 60 minutes.

To summarize, Fast Facts was a list of the closest 30 high schools within a given commute (45 minutes) that are above a graduation rate floor (70%). The list capped the number of new, screened, and (in some cases) out-of-borough schools that appeared. If necessary for producing a list of 30 schools, the maximum commuting time and/or minimum graduation rate was relaxed. A sample Fast Facts is pictured in Figure B.1.

2. Academically non-selective school supplement

Schools in the FF2 treatment arm were given Fast Facts and a supplementary list of academically non-selective high schools that give priority admission to students who attend an open house. The 18-25 high school programs featured on this supplement use the "limited unscreened" admissions method, which means they do not screen students using grades or other academic criteria. They do, however, give priority admission to students who attend an open house or information session. Schools on the supplement were drawn from Fast Facts or were added when Fast Facts did not generate at least 18 non-selective programs. Our procedure for creating this list was as follows. For each middle school we counted the number of limited unscreened *programs* offered by schools on Fast Facts. (We counted programs rather than schools, as some schools offered multiple programs). When there were >25 limited unscreened programs on Fast Facts, we identified 20 with the highest graduation rates and used these as the non-selective school supplement. When there were $18 \leq x \leq 25$ limited unscreened programs on Fast Facts, we retained them all for the non-selective school supplement. When there were <18 limited unscreened programs on Fast Facts, we retained these and drew additional programs until there were 20. (Schools were drawn using the same minimum criteria and sort order used for Fast Facts).

For presentation on the academically non-selective school supplement, programs were sorted in descending order by their school's graduation rate, and (in the case of ties) alphabetically by program name. Schools that already appeared on Fast Facts were introduced with the text, "These are some of the limited unscreened schools from your Fast Facts list."

Any added schools not on Fast Facts were introduced with the text, "Here are a few more limited unscreened programs to consider." Unlike Fast Facts, the non-selective school supplement provided the 4-character program code and program (rather than school) name. A sample non-selective school supplement is pictured in Figure B.2.

3. Schools by academic interest area

Schools in the FF3 treatment arm were given Fast Facts and a supplementary list of high schools grouped by academic theme or interest area. The 49 high school programs featured on this list were drawn from Fast Facts or were added when Facts Facts did not generate enough programs in each category. Our procedure for creating this list was as follows. For each middle school we identified seven programs in each of these categories: Academically Selective (all screened programs); Business & Communications; Health Professions; Humanities; Law, Government, Civics & History; Performing and Visual Arts; and STEM.[36] In each interest area we took the first seven programs that appeared after applying the same minimum criteria and sort order used for Fast Facts.[37] By using the original sort order, schools featured on Fast Facts were the first to be listed in their respective interest area. Fast Facts was often not sufficient to populate seven programs in each category. In these cases, we drew additional programs until each interest area was filled.

For presentation on the academic interest area supplement, programs were sorted in descending order by their school's graduation rate. (Again, listing first programs in schools that appeared on Fast Facts, and then added programs.) Unlike Fast Facts, the interest area supplement provided the 4-character program code, admissions method, and program name. (For example: "PPA HS: Musical Theatre," "PPA HS: Dance," and "Union Square Academy

[36] The categories were consolidated from a larger number of interest areas used by the NYCDOE in its High School Directory. "Academically Selective" is not an interest area *per se*, but a way to distinguish schools that screen on the basis of grades, test scores, or other criteria.

[37] For the academic interest area supplement we relaxed the maximum commute time to 80 minutes, or 90 minutes in the Rockaways. This was done to ensure a minimum number of schools in each interest area. We also modified the sort order so that programs that screened for English language learners were listed last in the case of ties.

for Health: Dental"). A sample academic interest area supplement is pictured in Figure B.3.

4. Screened language insert

All treatment schools received a one-page insert identifying 42 higher-performing schools citywide that offered "screened language" programs for English language learners and recent immigrants. This insert was the same for all treatment schools, with schools listed separately by borough. School names were listed, along with program names (e.g., Bilingual Haitian Creole Institute), 4-character program code, language of instruction, and directory page number. All of these schools had a 6-year graduation rate of 70% or higher. The front of the insert was printed in English, while the back was printed in Spanish. A sample screened language insert is pictured in Figure B.4.

5. Fast Facts and supplementary list descriptives

Tables B.1-B.3 report descriptive statistics for the high schools appearing on our intervention materials. Table B.1 summarizes the Fast Facts lists given to all treatment schools in the study. Table B.2 summarizes the (pooled) Fast Facts list and academically non-selective school supplement; FF2 was the only group of schools that actually received both of these lists. Table B.3 summarizes the (pooled) Fast Facts list and academic interest area supplement; FF3 was the only group of schools that actually received both of these lists. Again, we generated Fast Facts—and the two supplementary lists—for *all* study schools, regardless of treatment assignment. Doing so provided "counterfactual" lists that characterize information a school would have received, had they been in a particular treatment group.

Table B.1 shows that the typical Fast Facts list consisted of 30 high schools with an average graduation rate of 81.5% and average commuting time (middle school to high school) of 25.3 minutes. An average of 57.4% of schools on Fast Facts offered a limited unscreened program, 25.1% offered a screened program, and 23.3% offered *only* screened programs.[38] An

[38] Admissions methods used by a school are not mutually exclusive. A school can offer, for example, a

average of 26.3% were new schools that as of 2015-16 had not had a published graduation rate, and 78.9% of listed high schools were located in the same borough as the middle school.

Tables B.2 and B.3 show how the materials produced for the FF2 and FF3 schools compare to the typical Fast Facts lists. The combined Fast Facts and academically non-selective school supplement included an average of 32.4 unique schools (versus 30 on Fast Facts alone), while the combined Fast Facts and academic interest area supplement included an average of 42.9 schools. The average graduation rate of schools on the former (81.2%) was comparable to Fast Facts alone, while the latter (82.6%) was higher. (The interest area supplement required drawing more schools onto the list, including a minimum of seven screened programs, which tend to have higher graduation rates). As expected, the combined Fast Facts and academically non-selective school supplement included a higher share of schools offering limited unscreened programs than Fast Facts alone (61.2% vs. 57.4%). The average travel time on the two set of materials was higher (26.1 and 31.6 minutes, respectively) and a smaller share of schools was located in the same borough as the middle school (76.7% and 65.3%). (These differences reflect the need to draw additional schools onto the supplementary lists).

As a test for whether the intervention materials produced were balanced across treatment and control groups, the rightmost column in Tables B.1-B.3 report the p-value from a regression of the listed high school characteristic on a set of treatment group indicators and randomization block fixed effects. In only one case is the p-value less than 0.05, providing confidence that the schools appearing on the intervention materials are comparable, on average, across middle schools in the experiment.

6. Open house data and text message reminders

Our master list of open houses was compiled from the 2015-16 High School Directory, the NYCDOE online calendar, visits to tables at the city and borough-wide school fairs, and weekly calls by our research team to limited unscreened high schools. Because open house

screened program and a limited unscreened program.

dates were regularly added, canceled, and re-scheduled, this data collection continued until the last batch of text messages were sent. By that time we had assembled a list of **762** open house dates. The number of open houses varied by high school; some offered as few as one open house during the fall semester, for example, while others held weekly or bi-weekly open houses.

We scheduled **11 weeks** of text messaging, with information about **two** high schools sent to participants every Sunday evening. The first batch of messages was sent on September 20, 2015, and the last on November 29, 2015 (Table B.4). The content of the messages changed weekly and was customized to each receiving middle school. Our weekly procedure for selecting high schools for inclusion in the text message reminders was as follows.

For each middle school we identified all limited unscreened high schools that met our original criteria for inclusion on Fast Facts.[39] From this set we flagged schools with *scheduled* open house dates as of that week. Based on these dates we allocated open house reminders to 22 available slots (over 11 weeks), prioritizing high schools with fewer total open house opportunities and with higher graduation rates. For example, if a high school had a total of *one* scheduled open house, we assigned a text message reminder for it on the Sunday before the open house. Up to two of these could be scheduled in one week. If more than two such open houses were identified in a single week, we prioritized school(s) with higher graduation rates. When high schools had *two or more* scheduled open houses, we attempted to assign a text message reminder for the first of these. If that week was full, we attempted to schedule a message for the week of the second open house, and so on. Finally, after all schools with scheduled open houses were assigned a text message slot (subject to the limit of 2 per week), we filled unassigned slots with a general message with information about a limited unscreened high school not already covered above (again prioritizing higher graduation rates).

Because the open house calendar was dynamic, this weekly routine sometimes led to

[39] For most middle schools this included high schools with a graduation rate of 70% or higher and a maximum commuting time of 45 minutes. For schools in the Rockaways, we included high schools with a graduation rate of 70% or higher and a maximum commuting time of 70 minutes. The latter is relaxed somewhat from the Fast Facts criteria to ensure a sufficient number of schools.

repeat messages. To see this, suppose high school K123 had one open house scheduled as of October 25. Given its limited open house opportunities at the time, we would have prioritized a text message reminder for that week. If K123 later scheduled more open houses, it would re-appear on our list (with regularity if it is a high graduation rate school). We therefore monitored the results of our algorithm to minimize duplication. When we observed a middle school was scheduled to receive a repeat text message reminder for the same high school, we often manually forced them to receive a different reminder (for the next school in their text message priority list). We were less likely to do this in the first few weeks of text messaging, since most users had not yet signed up for the service.

Table B.4 reports the number of open house and general text message reminders sent in each week of the study. In the early weeks (1-3), the two messages tended to include one open house reminder and one general school message. In later weeks—during the peak open house period—both weekly text messages were open house reminders.

Examples of the open house reminder and general text messages are shown below. (These were sent in English or Spanish, depending on user preferences). When recipients wanted more information about a school, they were given the opportunity to text back "1" for information about the first school and "2" for information about the second school. The examples below include the responses to these requests.

> `Open House this week @ Urban Assembly School for Law & Justice on Sat 12/12 @ 11am txt 1 for more info`
>
> `UALaw&Just is @ 283 Adams St, Brooklyn, 718-858-1160; bus: B103 B25 B26 B38 B41 B45 B54 B57 B61 B62 B63 B65 B67 B69; train: G, 2 3 4 5 R, M, A C F, B Q`

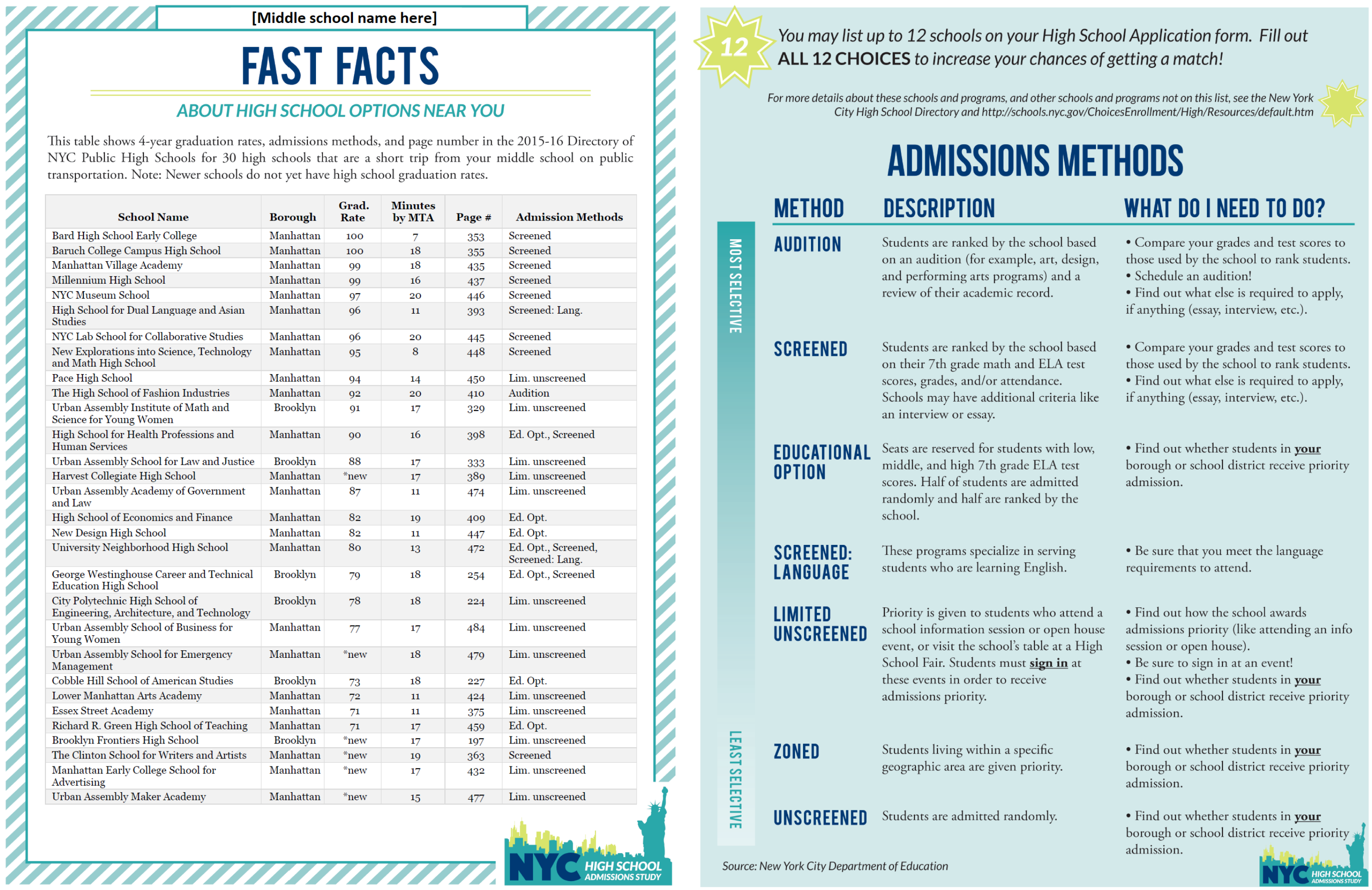
> `Interested in Bronx River HS? Call 718-904-4210 to schedule a visit txt 1 for more info`
>
> `Bx River is @ 3000 East Tremont Ave, Bronx, 718-904-4210; bus: Bx21 Bx24`

```
Bx31 Bx4 Bx40 Bx42 Bx4A Bx8; train: 6
```

When a school offered multiple open houses in one week, our text message accommodated this. For example:

```
Open House this week @ Murray Hill Academy on Thur 11/12, Sat 11/14 @ Thur
4-5:30pm; Sat 9:30-11am & 11:30am-1:00pm txt 2 for more info
```

As we did with the Fast Facts and supplementary school lists, we generated "counterfactual" text messages for middle schools that were not assigned to the non-selective school supplement treatment group (FF2). These were generated using the same rules as those used to produce the actual text messages sent to participating families in the FF2 treatment arm.



[Middle school name here]

FAST FACTS

ABOUT HIGH SCHOOL OPTIONS NEAR YOU

This table shows 4-year graduation rates, admissions methods, and page number in the 2015-16 Directory of NYC Public High Schools for 30 high schools that are a short trip from your middle school on public transportation. Note: Newer schools do not yet have high school graduation rates.

School Name	Borough	Grad. Rate	Minutes by MTA	Page #	Admission Methods
Bard High School Early College	Manhattan	100	7	353	Screened
Baruch College Campus High School	Manhattan	100	18	355	Screened
Manhattan Village Academy	Manhattan	99	18	435	Screened
Millennium High School	Manhattan	99	16	437	Screened
NYC Museum School	Manhattan	97	20	446	Screened
High School for Dual Language and Asian Studies	Manhattan	96	11	393	Screened: Lang.
NYC Lab School for Collaborative Studies	Manhattan	96	20	445	Screened
New Explorations into Science, Technology and Math High School	Manhattan	95	8	448	Screened
Pace High School	Manhattan	94	14	450	Lim. unscreened
The High School of Fashion Industries	Manhattan	92	20	410	Audition
Urban Assembly Institute of Math and Science for Young Women	Brooklyn	91	17	329	Lim. unscreened
High School for Health Professions and Human Services	Manhattan	90	16	398	Ed. Opt., Screened
Urban Assembly School for Law and Justice	Brooklyn	88	17	333	Lim. unscreened
Harvest Collegiate High School	Manhattan	*new	17	389	Lim. unscreened
Urban Assembly Academy of Government and Law	Manhattan	87	11	474	Lim. unscreened
High School of Economics and Finance	Manhattan	82	19	409	Ed. Opt.
New Design High School	Manhattan	82	11	447	Ed. Opt.
University Neighborhood High School	Manhattan	80	13	472	Ed. Opt., Screened, Screened: Lang.
George Westinghouse Career and Technical Education High School	Brooklyn	79	18	254	Ed. Opt., Screened
City Polytechnic High School of Engineering, Architecture, and Technology	Brooklyn	78	18	224	Lim. unscreened
Urban Assembly School of Business for Young Women	Manhattan	77	17	484	Lim. unscreened
Urban Assembly School for Emergency Management	Manhattan	*new	18	479	Lim. unscreened
Cobble Hill School of American Studies	Brooklyn	73	18	227	Ed. Opt.
Lower Manhattan Arts Academy	Manhattan	72	11	424	Lim. unscreened
Essex Street Academy	Manhattan	71	11	375	Lim. unscreened
Richard R. Green High School of Teaching	Manhattan	71	17	459	Ed. Opt.
Brooklyn Frontiers High School	Brooklyn	*new	17	197	Lim. unscreened
The Clinton School for Writers and Artists	Manhattan	*new	19	363	Screened
Manhattan Early College School for Advertising	Manhattan	*new	17	432	Lim. unscreened
Urban Assembly Maker Academy	Manhattan	*new	15	477	Lim. unscreened

NYC HIGH SCHOOL ADMISSIONS STUDY

12

You may list up to 12 schools on your High School Application form. Fill out **ALL 12 CHOICES** *to increase your chances of getting a match!*

For more details about these schools and programs, and other schools and programs not on this list, see the New York City High School Directory and http://schools.nyc.gov/ChoicesEnrollment/High/Resources/default.htm

ADMISSIONS METHODS

MOST SELECTIVE → LEAST SELECTIVE

METHOD	DESCRIPTION	WHAT DO I NEED TO DO?
AUDITION	Students are ranked by the school based on an audition (for example, art, design, and performing arts programs) and a review of their academic record.	• Compare your grades and test scores to those used by the school to rank students. • Schedule an audition! • Find out what else is required to apply, if anything (essay, interview, etc.).
SCREENED	Students are ranked by the school based on their 7th grade math and ELA test scores, grades, and/or attendance. Schools may have additional criteria like an interview or essay.	• Compare your grades and test scores to those used by the school to rank students. • Find out what else is required to apply, if anything (essay, interview, etc.).
EDUCATIONAL OPTION	Seats are reserved for students with low, middle, and high 7th grade ELA test scores. Half of students are admitted randomly and half are ranked by the school.	• Find out whether students in **your** borough or school district receive priority admission.
SCREENED: LANGUAGE	These programs specialize in serving students who are learning English.	• Be sure that you meet the language requirements to attend.
LIMITED UNSCREENED	Priority is given to students who attend a school information session or open house event, or visit the school's table at a High School Fair. Students must **sign in** at these events in order to receive admissions priority.	• Find out how the school awards admissions priority (like attending an info session or open house). • Be sure to sign in at an event! • Find out whether students in **your** borough or school district receive priority admission.
ZONED	Students living within a specific geographic area are given priority.	• Find out whether students in **your** borough or school district receive priority admission.
UNSCREENED	Students are admitted randomly.	• Find out whether students in **your** borough or school district receive priority admission.

Source: New York City Department of Education

NYC HIGH SCHOOL ADMISSIONS STUDY

Figure B.1: Sample Fast Facts (front and back)

WHY OPEN HOUSES ARE IMPORTANT

"Limited unscreened" high school programs **don't** review your grades and test scores, but **do** give admissions priority to students who attend **and sign in** at an open house or high school fair. This means you have a **better chance** of getting into this type of school if you visit and sign in. You and your parent or guardian can keep track of open houses using this calendar, and our **text message reminders**. We will text a short message letting you know when and where these open houses will take place. When we text you or your parent an open house date, **add it to your calendar**! Some limited unscreened schools have regular visiting hours, or let you make an appointment to visit the school. **Call the high schools you are interested in to find out!**

Parents can **text 251 to 917-426-0188** to start receiving weekly reminder texts right away! (Participants must be 13 or older to receive text messages).

Don't forget to sign in at an Open House or High School Fair!

These are the limited unscreened schools from your Fast Facts list:			
School/Program Name	**Borough**	**Program Code**	**Directory Page #**
South Bronx Prep: A College Board School	Bronx	X11X	146
Knowledge & Power Prep Academy Int'l HS	Bronx	Y32A	116
HERO High (Health, Education, & Research Occupations HS)	Bronx	X37A	95
East Bronx Academy for the Future	Bronx	X83X	85
Bronx HS for Law & Community Service: Law Enforcement	Bronx	X53A	49
Global Learning Collaborative	Manhattan	A17A	385
Urban Assembly School for Careers in Sports	Bronx	X31A	156
Urban Assembly School for Global Commerce	Manhattan	M48A	480
High School for Excellence and Innovation	Manhattan	A24A	396
Inwood Early College for Health & Information Tech	Manhattan	A16A	418
Murray Hill Academy	Manhattan	A19A	440
Here are a few more limited unscreened programs to consider:			
Academy of Finance and Enterprise	Queens	Q08R	496
Stephen T. Mather Building Arts & Craftsmanship HS	Manhattan	M18A	464
Urban Assembly Gateway School for Tech	Manhattan	A23A	475
HS for Arts, Imagination & Inquiry	Manhattan	M11R	392
HS for Teaching & the Professions	Bronx	X34A	103
Bronx Theatre HS: Theatre Design & Tech	Bronx	X38A	67
Bronx Theatre HS: Performance & Production	Bronx	X38B	67
High School of Applied Communication	Queens	Q07R	543
School for Tourism and Hospitality	Bronx	Y59A	144

10 | OCTOBER

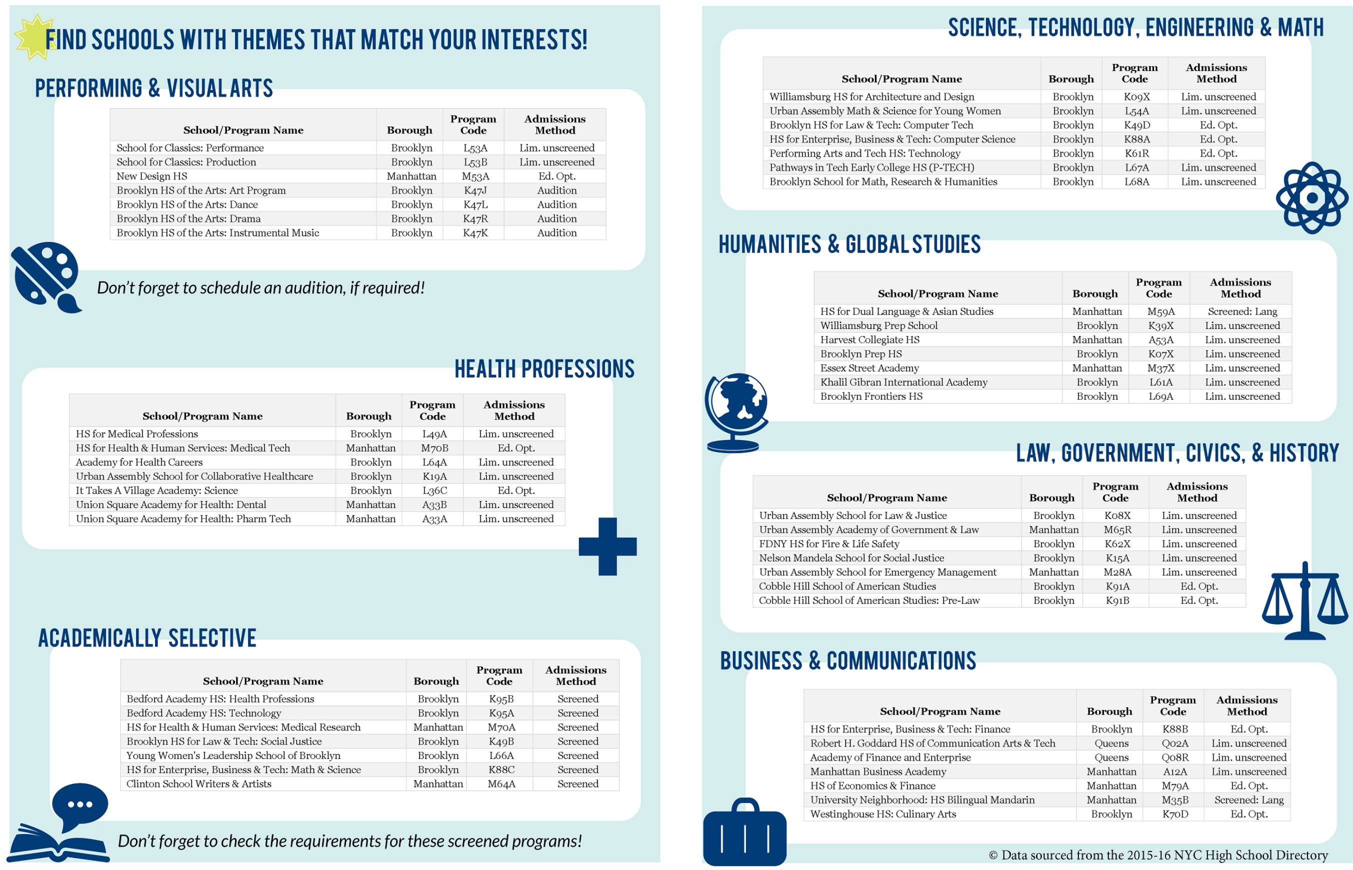
S	M	T	W	T	F	S
				1	2	3
4	5	6	7	8	9	10
11	12	13	14	15	16	Borough High School Fairs 17
Borough High School Fairs 18	19	20	21	22	23	24
25	26	27	28	29	30	

11 | NOVEMBER

S	M	T	W	T	F	S
1	2	3	4	5	6	7
8	9	10	11	12	13	14
15	16	17	18	19	20	21
22	23	24	25	26	27	28
29	30	HS Apps Due! 1				

When we text an Open House date,
ADD IT TO YOUR CALENDAR!

Figure B.2: Sample academically non-selective school supplement



FIND SCHOOLS WITH THEMES THAT MATCH YOUR INTERESTS!

PERFORMING & VISUAL ARTS

School/Program Name	Borough	Program Code	Admissions Method
School for Classics: Performance	Brooklyn	L53A	Lim. unscreened
School for Classics: Production	Brooklyn	L53B	Lim. unscreened
New Design HS	Manhattan	M53A	Ed. Opt.
Brooklyn HS of the Arts: Art Program	Brooklyn	K47J	Audition
Brooklyn HS of the Arts: Dance	Brooklyn	K47L	Audition
Brooklyn HS of the Arts: Drama	Brooklyn	K47R	Audition
Brooklyn HS of the Arts: Instrumental Music	Brooklyn	K47K	Audition

Don't forget to schedule an audition, if required!

HEALTH PROFESSIONS

School/Program Name	Borough	Program Code	Admissions Method
HS for Medical Professions	Brooklyn	L49A	Lim. unscreened
HS for Health & Human Services: Medical Tech	Manhattan	M70B	Ed. Opt.
Academy for Health Careers	Brooklyn	L64A	Lim. unscreened
Urban Assembly School for Collaborative Healthcare	Brooklyn	K19A	Lim. unscreened
It Takes A Village Academy: Science	Brooklyn	L36C	Ed. Opt.
Union Square Academy for Health: Dental	Manhattan	A33B	Lim. unscreened
Union Square Academy for Health: Pharm Tech	Manhattan	A33A	Lim. unscreened

ACADEMICALLY SELECTIVE

School/Program Name	Borough	Program Code	Admissions Method
Bedford Academy HS: Health Professions	Brooklyn	K95B	Screened
Bedford Academy HS: Technology	Brooklyn	K95A	Screened
HS for Health & Human Services: Medical Research	Manhattan	M70A	Screened
Brooklyn HS for Law & Tech: Social Justice	Brooklyn	K49B	Screened
Young Women's Leadership School of Brooklyn	Brooklyn	L66A	Screened
HS for Enterprise, Business & Tech: Math & Science	Brooklyn	K88C	Screened
Clinton School Writers & Artists	Manhattan	M64A	Screened

Don't forget to check the requirements for these screened programs!

SCIENCE, TECHNOLOGY, ENGINEERING & MATH

School/Program Name	Borough	Program Code	Admissions Method
Williamsburg HS for Architecture and Design	Brooklyn	K09X	Lim. unscreened
Urban Assembly Math & Science for Young Women	Brooklyn	L54A	Lim. unscreened
Brooklyn HS for Law & Tech: Computer Tech	Brooklyn	K49D	Ed. Opt.
HS for Enterprise, Business & Tech: Computer Science	Brooklyn	K88A	Ed. Opt.
Performing Arts and Tech HS: Technology	Brooklyn	K61R	Ed. Opt.
Pathways in Tech Early College HS (P-TECH)	Brooklyn	L67A	Lim. unscreened
Brooklyn School for Math, Research & Humanities	Brooklyn	L68A	Lim. unscreened

HUMANITIES & GLOBAL STUDIES

School/Program Name	Borough	Program Code	Admissions Method
HS for Dual Language & Asian Studies	Manhattan	M59A	Screened: Lang
Williamsburg Prep School	Brooklyn	K39X	Lim. unscreened
Harvest Collegiate HS	Manhattan	A53A	Lim. unscreened
Brooklyn Prep HS	Brooklyn	K07X	Lim. unscreened
Essex Street Academy	Manhattan	M37X	Lim. unscreened
Khalil Gibran International Academy	Brooklyn	L61A	Lim. unscreened
Brooklyn Frontiers HS	Brooklyn	L69A	Lim. unscreened

LAW, GOVERNMENT, CIVICS, & HISTORY

School/Program Name	Borough	Program Code	Admissions Method
Urban Assembly School for Law & Justice	Brooklyn	K08X	Lim. unscreened
Urban Assembly Academy of Government & Law	Manhattan	M65R	Lim. unscreened
FDNY HS for Fire & Life Safety	Brooklyn	K62X	Lim. unscreened
Nelson Mandela School for Social Justice	Brooklyn	K15A	Lim. unscreened
Urban Assembly School for Emergency Management	Manhattan	M28A	Lim. unscreened
Cobble Hill School of American Studies	Brooklyn	K91A	Ed. Opt.
Cobble Hill School of American Studies: Pre-Law	Brooklyn	K91B	Ed. Opt.

BUSINESS & COMMUNICATIONS

School/Program Name	Borough	Program Code	Admissions Method
HS for Enterprise, Business & Tech: Finance	Brooklyn	K88B	Ed. Opt.
Robert H. Goddard HS of Communication Arts & Tech	Queens	Q02A	Lim. unscreened
Academy of Finance and Enterprise	Queens	Q08R	Lim. unscreened
Manhattan Business Academy	Manhattan	A12A	Lim. unscreened
HS of Economics & Finance	Manhattan	M79A	Ed. Opt.
University Neighborhood: HS Bilingual Mandarin	Manhattan	M35B	Screened: Lang
Westinghouse HS: Culinary Arts	Brooklyn	K70D	Ed. Opt.

© Data sourced from the 2015-16 NYC High School Directory

Figure B.3: Sample academic interest area supplement

Figure B.4: Screened language insert

FAST FACTS || SCREENED: LANGUAGE

Below is a list of New York City high school programs especially designed for students who are learning English. These Screened: Language programs admit students who are learning English and may also require students to be living in the United States for 4 years or fewer. Some of these programs (marked with a *) have course grade and other academic requirements as well. Go to the program's page in the Directory of New York City High Schools to find out about any additional requirements the program may have. At the end of this flyer you will find explanations of some of the additional requirements.

	SCHOOL NAME	PROGRAM NAME	CODE	PAGE	LANGUAGE
BRONX	Academy for Language and Technology	Media Communications	Y31B	21	Spanish
		Institute for Computer Technology	Y31C	21	Spanish
		Computer Networking	Y31D	21	Spanish
	Alfred E. Smith Career and Technical Education HS	Bilingual Spanish NATEF Automotive Technology	X69D	24	Spanish
	Bronx Aerospace HS	Dual Language Spanish Program	X30B	35	Spanish
	Bronx Bridges HS	Bronx Bridges HS	Y47A	37	Spanish, Bengali, Arabic, French
	Bronx Int'l HS	Bronx Int'l HS	X36A	58	Any
	Claremont Int'l HS	Claremont Int'l HS	X69D	74	Any
	Crotona Int'l HS	Digital Media Entertainment Technology	Y56A	79	Spanish
	HS of Language and Innovation	High School of Language and Innovation	Y52A	108	Many; See directory
	International School for Liberal Arts	International School for Liberal Arts	Y24A	114	Spanish
	M.S. 223 Lab School of Finance and Technology	Dual Language Spanish Program	Y72A	118	Spanish
	Marble Hill HS for International Studies	International Academy	X43B	120	Any
	New World HS	New World HS	X87R	134	Any
	Pan American Int'l HS at Monroe	Pan American Int'l HS at Monroe	Y26A	136	Spanish
	World View HS	Spanish Transitional Bilingual	X89B	165	Spanish
BROOKLYN	Brooklyn Int'l High School	Brooklyn Int'l High School	K53A	204	Any
	Clara Barton HS	Bilingual Haitian Creole Program	K50B	226	Haitian Creole
		Dual Language Russian Program	K50C	226	Russian
	Edward R. Murrow HS	Bilingual Mandarin Communication Arts	K57B	239	Mandarin Chinese
		Bilingual Spanish Communication Arts	K57C	239	Spanish
	Green School: Acad. for Environmental Careers	Transitional Bilingual	L27C	257	Spanish
	Int'l HS at Prospect Heights	Int'l High School at Prospect Heights	K98X	273	Any
	John Dewey HS	Bilingual Mandarin College Prep Program	K56B	278	Mandarin Chinese
	Midwood HS	Bilingual Haitian Creole Institute*	K56B	291	Haitian Creole
MANHATTAN	A. Philip Randolph Campus High School	Dual Language Spanish Program	M19L	346	Spanish
	Esperanza Preparatory Academy	Esperanza Preparatory Academy	A37A	388	Spanish
	Gregorio Luperon HS for Science and Math	Gregorio Luperon HS for Science and Math*	M62A	388	Spanish
	High School for Dual Language and Asian Studies	HS for Dual Language and Asian Studies*	M59A	393	Mandarin Chinese
	Queens High School for Language Studies	Queens HS for Language Studies	Q62A	581	Mandarin Chinese
	Manhattan Bridges HS	Bilingual Spanish IT and Computer Science*	M57B	427	Spanish
		Bilingual Spanish Pre-Engineering*	M57B	427	Spanish
		Dual Lang. Spanish Pre-Engineering*	M57D	427	Spanish
		Dual Lang. Spanish IT and Comp. Science	M57E	427	Spanish
	Manhattan Center for Science and Mathematics	Bilingual Spanish Science and Math*	M16K	430	Spanish
	Manhattan Int'l HS	Humanities and Interdisciplinary	M93A	434	Any
	University Neighborhood HS	Bilingual Mandarin	M35B	472	Mandarin Chinese
QUEENS	Flushing Int'l HS	Flushing Int'l HS	Q25X	525	Any
	Int'l HS at LaGuardia Community College	Int'l HS at LaGuardia Community College	Q27J	552	Any
	Int'l HS for Health Sciences	Int'l HS for Health Sciences	Q63A	554	Any
	Newcomers HS	Newcomers HS	Q98A	569	Any
	Queens HS for Language Studies	Queens HS for Language Studies	Q62A	581	Mandarin Chinese

INFORMATION ABOUT PROGRAM REQUIREMENTS

Some Screened: Language programs have additional requirements for admission. If you are interested in a program, go to the page in the Directory of New York City High Schools and look under Admissions Priorities and Eligibility to determine if there are specific requirements such as:

Home Language Spanish: The language spoken most often in your home is Spanish.

New York City Residents Who Have Lived in the United States 4 Years or Fewer: You arrived in the United States from another country within the last four years.

English Language Learner or Limited English Proficiency: Your school has told you and/or your parents that you qualify for additional help learning English.

NYC HIGH SCHOOL ADMISSIONS STUDY

Table B.1: Mean characteristics of schools on Fast Facts

	All study schools	Treatment groups: FF1	FF2	FF3	Control	p-value
N	165	39	39	40	47	
Number of schools on FF1	30	30	30	30	30	
Total # of seats	4036.3	4146.8	4002.1	4066.0	3947.9	0.639
Graduation rate	81.5	81.6	81.4	81.4	81.7	0.423
Imputed gradrate	0.176	0.185	0.180	0.163	0.176	0.244
Graduation rate ≥70%	0.985	0.979	0.977	0.989	0.994	0.322
Apps per seat	9.4	9.6	9.4	9.1	9.4	0.529
Same borough	0.789	0.812	0.789	0.805	0.757	0.124
Travel time (mins.)	25.3	24.7	26.7	25.7	24.2	0.077
Audition	0.077	0.083	0.068	0.078	0.077	0.519
Ed Option	0.152	0.148	0.148	0.163	0.151	0.499
Limited Unscreened	0.574	0.573	0.599	0.572	0.557	0.162
Screened	0.251	0.248	0.246	0.254	0.255	0.738
Screened: Language	0.096	0.093	0.088	0.091	0.108	0.170
Zoned	0.014	0.016	0.018	0.015	0.009	0.465
Screened pgms only	0.233	0.237	0.216	0.234	0.243	0.192
Bronx	0.355	0.352	0.376	0.336	0.357	0.063
Brooklyn	0.330	0.332	0.323	0.353	0.313	0.910
Manhattan	0.254	0.259	0.233	0.242	0.279	0.248
Queens	0.061	0.056	0.068	0.069	0.052	0.380
New school	0.263	0.256	0.265	0.257	0.272	0.589
SD gradrate (with imp)	9.0	9.0	9.0	9.0	8.9	0.996

Notes: for this table we first calculated a mean for each middle school characterizing the 30 high schools on its Fast Facts list. This table reports the means of those quantities, over all study schools (N=165) and separately by treatment group. Recall that Fast Facts lists were generated for all schools in the study, regardless of treatment group. The p-value reported in the rightmost column is from a regression of the listed high school characteristic on a set of treatment group indicators and randomization block fixed effects. The null hypothesis tested is that the coefficients on the three treatment indicators are jointly zero. The graduation rate and graduation rate ≥70% outcomes are conditional on being non-missing. Total seat counts do not include zoned guarantee programs, which do not have a maximum seat count.

Table B.2: Mean characteristics of high schools listed on combined Fast Facts and academically non-selective school supplement

	All study schools	Treatment groups: FF1	FF2	FF3	Control	p-value
N	165	39	39	40	47	
Number of schools on FF2	32.4	32.5	32.1	32.3	32.8	0.347
Total # of seats	4310.9	4441.3	4237.2	4325.9	4251.1	0.607
Graduation rate	81.2	81.3	81.1	81.1	81.4	0.418
Imputed gradrate	0.190	0.200	0.196	0.172	0.191	0.093
Graduation rate ≥70%	0.981	0.975	0.971	0.987	0.989	0.299
Apps per seat	9.3	9.5	9.3	9.1	9.3	0.675
Same borough	0.767	0.781	0.765	0.789	0.737	0.158
Travel time (mins.)	26.1	25.6	27.2	26.5	25.3	0.242
Audition	0.069	0.073	0.062	0.070	0.069	0.623
Ed Option	0.140	0.135	0.136	0.151	0.137	0.409
Limited Unscreened	0.612	0.612	0.631	0.607	0.601	0.232
Screened	0.230	0.227	0.228	0.235	0.231	0.866
Screened: Language	0.089	0.086	0.082	0.085	0.099	0.270
Zoned	0.013	0.015	0.017	0.015	0.008	0.459
Screened pgms only	0.212	0.214	0.199	0.213	0.220	0.335
Bronx	0.363	0.360	0.388	0.344	0.360	0.026*
Brooklyn	0.325	0.332	0.315	0.349	0.308	0.813
Manhattan	0.251	0.255	0.228	0.238	0.279	0.183
Queens	0.061	0.053	0.069	0.069	0.053	0.279
New school	0.271	0.267	0.275	0.261	0.281	0.487
SD gradrate (with imp)	8.9	9.0	9.0	8.9	8.9	0.922

Notes: for this table, we first calculated a mean for each middle school characterizing the 30+ high schools on its combined Fast Facts and non-selective school supplement. This table reports the means of those quantities, over all study schools (N=165) and separately by treatment group. Recall that these lists were generated for all schools in the study, regardless of treatment group. The p-value reported in the rightmost column is from a regression of the reported school characteristics on a set of treatment group indicators and randomization block fixed effects. The null hypothesis tested is that the coefficients on the three treatment indicators are jointly zero. The graduation rate and graduation rate ≥70% outcomes are conditional on being non-missing. Total seat counts do not include zoned guarantee programs, which do not have a maximum seat count.

Table B.3: Mean characteristics of high schools listed on combined Fast Facts and academic interest area supplement

	All study schools	Treatment groups: FF1	FF2	FF3	Control	p-value
N	165	39	39	40	47	
Number of schools on FF3	42.9	43.0	43.2	42.3	43.0	0.512
Total # of seats	6278.3	6473.2	6230.4	6208.1	6216.0	0.206
Graduation rate	82.6	82.7	82.6	82.3	82.7	0.448
Imputed gradrate	0.162	0.168	0.161	0.153	0.163	0.472
Graduation rate ≥70%	0.989	0.987	0.984	0.988	0.997	0.318
Apps per seat	9.4	9.6	9.5	9.2	9.4	0.438
Same borough	0.653	0.669	0.648	0.674	0.625	0.113
Travel time (mins.)	31.6	31.4	33.1	31.7	30.5	0.096
Audition	0.069	0.077	0.065	0.070	0.067	0.161
Ed Option	0.223	0.215	0.221	0.231	0.225	0.191
Limited Unscreened	0.570	0.572	0.586	0.567	0.560	0.211
Screened	0.252	0.250	0.250	0.251	0.254	0.883
Screened: Language	0.092	0.093	0.086	0.091	0.099	0.161
Zoned	0.019	0.021	0.021	0.020	0.014	0.510
Screened pgms only	0.177	0.181	0.168	0.177	0.180	0.335
Bronx	0.280	0.274	0.287	0.271	0.286	0.366
Brooklyn	0.310	0.314	0.304	0.332	0.294	0.877
Manhattan	0.316	0.322	0.303	0.307	0.331	0.485
Queens	0.093	0.090	0.106	0.090	0.088	0.330
New school	0.224	0.219	0.222	0.221	0.231	0.696
SD gradrate (with imp)	8.804	8.846	8.859	8.792	8.733	0.782

Notes: for this table, we first calculated a mean for each middle school characterizing the 30+ high schools on its combined Fast Facts and academic interest area supplement. This table reports the means of those quantities, over all study schools (N=165) and separately by treatment group. Recall that these lists were generated for all schools in the study, regardless of treatment group. The p-value reported in the rightmost column is from a regression of the reported school characteristics on a set of treatment group indicators and randomization block fixed effects. The null hypothesis tested is that the coefficients on the three treatment indicators are jointly zero. The graduation rate and graduation rate ≥70% outcomes are conditional on being non-missing. Total seat counts do not include zoned guarantee programs, which do not have a maximum seat count.

Table B.4: Text message reminders and participants by week

Week number	Date of message (Sunday)	Text message 1: General	Openhse	Text message 2: General	Openhse	Cumulative Treatment 2 School visits	Number of unique text participants	Number of unique text schools
1	20-Sep-15	9	30	39	0	12	93	19
2	27-Sep-15	3	36	22	17	17	339	27
3	4-Oct-15	2	37	10	29	24	591	32
4	11-Oct-15	0	39	1	38	33	868	38
5	18-Oct-15	0	39	0	39	34	1194	38
6	25-Oct-15	0	39	1	38	36	1585	38
7	1-Nov-15	0	39	0	39	37	1665	38
8	8-Nov-15	1	38	10	29	38	1729	39
9	15-Nov-15	0	39	2	37	39	1787	39
10	22-Nov-15	0	39	14	25	39	1881	39
11	29-Nov-15	1	38	22	17	39	1881	39
	Total	16	413	121	308			

Notes: authors' calculations.

C Estimating text messaging effects

Estimating the marginal effect of the text messaging component of FF2 on students' choices and other process outcomes, including open house priority status, is complicated by several factors. First, FF2 students were not randomly assigned to receive text messages. All 8th graders in FF2 treatment schools were given printed materials *and* the opportunity to receive text message reminders. Thus, it is conceptually difficult to separate the two treatment components. Second, participation was voluntary and take-up rates varied (see Online Appendix B). Many participants did not opt in until the last 4-5 weeks of the study. Third, text messages were sent for only a select subset of limited unscreened high schools. If enrolled for the full study period of 11 weeks, a family would receive at most 22 open house reminders. In practice, due to repeat messages, the average treatment school was notified about 19.4 unique high schools and 17.4 unique high schools holding open houses. Families who signed up late received even fewer.

In sum, the causal effect of text messaging alone is not identified by our design, and the ITT effect of text messages alone is likely to be small unless the treatment-on-treated effect is large. That said, we can look descriptively at students' propensity to apply to schools that were the subject of our text messages. The first row of Table C.1 reports the results of a regression in which the outcome Y_{ij} is defined as the percent of top three choices for which student i's middle school j was sent a text message. (Recall we have both the actual messages sent and counterfactual messages that would have been sent to the other treatment arms and control group had they been eligible to receive them). The effect is small, but positive and statistically significant for students in the FF2 treatment. Whether these effects are due to the text messages themselves is less clear. As Table 7 showed, students in FF2 schools were induced to apply to more limited unscreened schools in general. The open house reminders pertained to limited unscreened schools, and it is possible the effect shown in Table C.1 is driven by this increase.

In an attempt to address this, we estimated impacts on students' propensity to apply to high schools for which their middle school was sent a text message, but the high school did *not* appear on their Fast Facts list or non-selective school supplement. More than half of all text messages to a middle school pertained to high schools that did not appear on their printed list, since the non-selective school supplement was insufficient to populate text messages every week. These results are reported in the last two rows of Table C.1. We observe no effect of FF2 or any treatment on this outcome. It is important to keep in mind that this outcome represents applications to a rather small subset of schools that—for whatever reason—were chosen for a text message but did not appear on the school's non-selective supplement. Only 2.7 percent of the control group ranked one of these schools as their first choice, suggesting these schools are mostly outside the typical choice set.

Finally, we find a marginally statistically significant effect ($p < 0.10$) of FF2 on the number of student choices that were the subject of text messages and for which the student had open house priority (second row of Table C.1). The point estimate is small (0.12), but meaningful relative to the control group standard deviation (1.1). However, the coefficient is similar to that estimated for FF1, which did not participate in text messaging, so it is unlikely to be due to the messaging component.

Taken together, while our experiment was not designed to test the separate effects of FF2 materials and text messaging, the data do not suggest a strong response to the specific text messages that were sent. This may be due to low take-up, or the fact that the messages focused on a relatively narrow set of schools. Of course, the texting component may have affected students' choices and behavior in other ways, by generally underscoring the importance of open houses. Or, students may have attended the open house and decided not to apply to the school, an outcome we cannot observe.

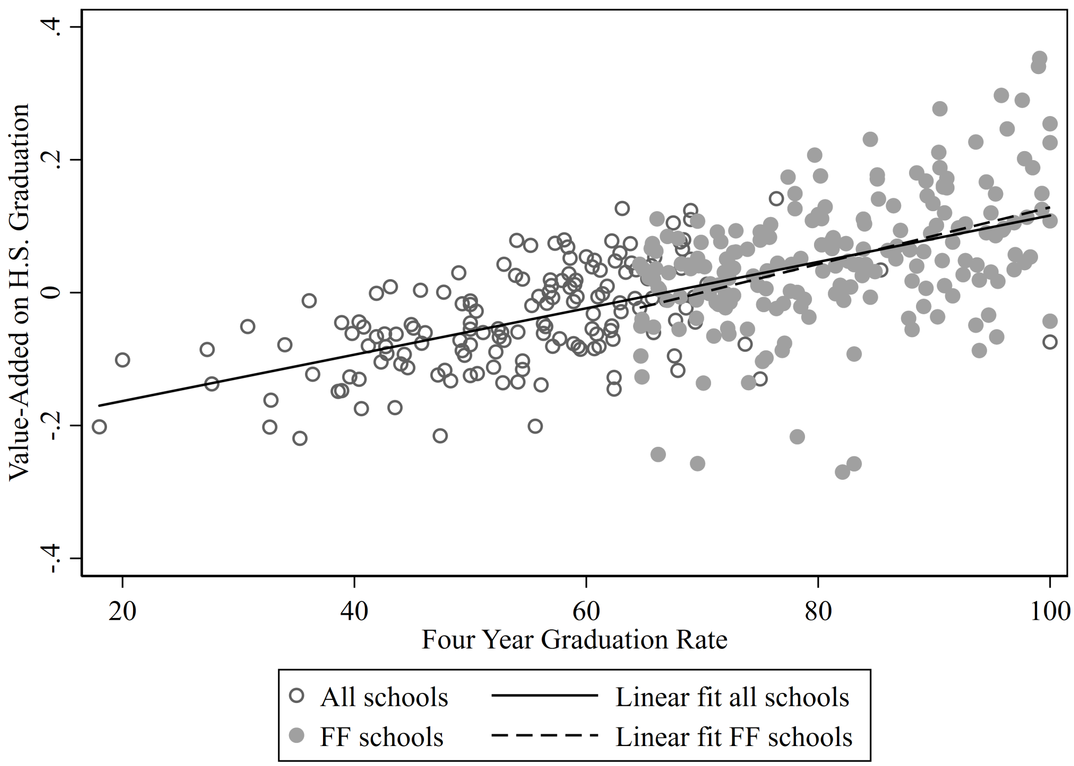
Table C.1: Applications to schools for which a text message was sent

	Treatment groups			Control group	
	FF1	FF2	FF3	Mean	SD
Percent text message schools, 1st-3rd choices	1.435	3.411*	-0.079	12.8	21.8
	(1.415)	(1.356)	(1.419)		
Number of choices that were text message schools and student was in open house priority group	0.114	0.119+	0.0293		
	(0.0754)	(0.0656)	(0.0721)	0.7	1.1
Any 1st-3rd choice school was text message school not on list	0.112	0.616	0.222	8.1	27.4
	(1.000)	(1.030)	(0.956)		
Percent of all choices that were text message school not on list	-0.051	-0.078	0.049	3.2	6.8
	(0.357)	(0.375)	(0.359)		

Notes: Each row represents estimates from a separate regression. Sample sizes 19,109 in each case. All regression models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-score and other covariates, and an indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

D Supplemental figures and tables

Figure D.1: Value-added and 4-year graduation rates for NYC high schools



Notes: N=352 high schools with students graduating in 2012, 2013, or 2014. Excludes schools that admit students outside the main applications process (charter schools, the nine specialized high schools, D75/special education schools, and D79/alternative schools), as well as schools in Staten Island, and schools which mostly accept returning students in 9th grade. Schools shown as solid circles appeared on at least one Fast Facts or supplemental list; schools shown as hollow circles did not appear on any list. Four-year graduation rate on the horizontal axis is the one used to generate Fast Facts (data from 2013-14, as reported in the 2015-16 high school directory).

Value-added estimates come from a random effects model that regresses students' four-year graduation status on their characteristics and prior achievement, their peers' average characteristics and achievement, and a cohort fixed effect. School value-added is the BLUP random effect from this model. The estimation pools data from three cohorts of 9th graders entering in 2008-09, 2009-10, and 2010-11, with four-year graduation status observed in 2012, 2013, and 2014. Student and peer characteristics include gender, race/ethnicity, immigrant status, special education and EL status, and eligibility for free or reduced-price lunch. Achievement measures include math and ELA scores standardized by year, with missing values imputed as zero. (A dummy indicator for missing values is also included in the regression).

Table D.1: Percent of students with information session priority, 2014-15

	1st choice	1st-5th choices	All LUS choices
All students	40.8	36	34.8
Free lunch	37.9	34.3	33.4
Reduced price lunch	49.3	41.5	39
Not free or reduced	53.7	45.4	43
EL	33.3	30.4	29.7
Not EL	41.9	36.9	35.6
Special education	35.6	31.7	30.2
Not special education	42.4	37.4	36.1
Black	40.3	37	36
Hispanic	37.1	32.4	31.2
Not Black or Hispanic	54.1	46.4	44.5
Female	42.3	37.1	36
Male	39.5	35.1	33.7
Bottom two ELA quartiles	36.2	32.5	31.5
Top two ELA quartiles	50.3	43.3	41.4
N	18,379	87,446	149,038

Notes: authors' analysis using data from the 2014-15 high school admissions process. Only public school applicants to limited unscreened (LUS) programs that gave open house or information session priority are included. Students given priority for other reasons—such as returning 8th graders—are excluded from these calculations. Column (1) includes the 18,379 students who ranked a LUS program as their 1st choice. In columns (2) and (3), the unit of observation is a student-choice. For example, if a student ranked three LUS programs and received information session priority for two, they would be counted twice among those with priority and once among those without priority. These columns can be interpreted as the probability a student with a given characteristic—having ranked a LUS school—received information session priority for that school. "Not free or reduced" also excludes students enrolled in a universal free meals school.

Table D.2: Impact of informational interventions on graduation rate of choices and matches, with missings imputed

	Treatment groups				Control group	
	FF1	FF2	FF3	Pooled	Mean	SD
Graduation rate:						
1st-3rd choices (mean)	0.219	-0.376	-1.026+	-0.384	80.9	10.6
(with imputed)	(0.451)	(0.606)	(0.534)	(0.424)		
Final matched school	1.633**	0.565	-0.0775	0.743	73.6	13.2
(with imputed)	(0.537)	(0.618)	(0.567)	(0.455)		
9th grade enrolled school	1.056+	0.396	-0.156	0.450	74.4	13.7
(with imputed)	(0.538)	(0.615)	(0.585)	(0.459)		
Graduation rate below 70%:						
1st-3rd choices (mean)	-3.186*	-1.116	0.474	-1.330	21.9	29.5
(with imputed)	(1.592)	(2.004)	(1.977)	(1.576)		
Final matched school	-7.235**	-5.522*	-3.113	-5.311*	40.3	49.0
(with imputed)	(2.284)	(2.781)	(2.771)	(2.185)		
9th grade enrolled school	-5.869**	-4.319	-2.827	-4.376*	38.4	48.6
(with imputed)	(2.238)	(2.677)	(2.735)	(2.143)		

Notes: each *row* reports estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Graduation rates were imputed for high schools that had not yet had a graduating cohort (see Online Appendix B for details). Sample sizes vary from 18,058 (9th grade enrolled school) to 19,107 (1st-3rd choices). All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.3: Impact of informational interventions: excluding pilot study schools

	Treatment groups			Control group	
	FF1	FF2	FF3	Mean	SD
% of 1st-3rd choices from intervention-specific list	9.619***	5.479*	6.714**	37.2	32.6
	(2.239)	(2.254)	(2.005)		
Matched to 1st choice	2.378	2.572	2.625	44.6	49.7
	(1.615)	(1.844)	(1.603)		
Graduation rate:					
1st-3rd choices (mean)	0.444	-0.149	-0.578	80.9	11.2
	(0.487)	(0.702)	(0.552)		
Final matched school	1.633**	0.342	0.366	73.4	13.7
	(0.590)	(0.718)	(0.616)		
Graduation rate <70%:					
1st-3rd choices (mean)	-3.140+	-2.032	-0.631	23.1	31.6
	(1.828)	(2.310)	(2.116)		
Final matched school	-6.702**	-5.261+	-4.806	42.9	49.5
	(2.474)	(3.093)	(2.940)		

Notes: each *row* reports estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Sample sizes vary from 14,705 (graduation rate at final matched school) to 17,083 (matched to 1st choices). All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.4: Impact of informational interventions: excluding charter schools

	Treatment groups			Control group	
	FF1	FF2	FF3	Mean	SD
% of 1st-3rd choices from intervention-specific list	10.66***	5.130*	5.630**	37.5	32.6
	(2.163)	(2.148)	(2.047)		
Matched to 1st choice	3.090+	3.037	3.248+	44.9	49.7
	(1.785)	(1.922)	(1.720)		
Graduation rate:					
1st-3rd choices (mean)	0.397	-0.237	-1.054+	80.8	11.2
	(0.509)	(0.711)	(0.562)		
Final matched school	1.545**	0.204	-0.147	73.4	13.7
	(0.583)	(0.708)	(0.627)		
Graduation rate <70%:					
1st-3rd choices (mean)	-3.112+	-0.957	0.560	23.3	31.7
	(1.845)	(2.404)	(2.169)		
Final matched school	-6.387*	-4.578	-2.963	42.8	49.5
	(2.554)	(3.204)	(3.020)		

Notes: each *row* reports estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Sample sizes vary from 15,766 (graduation rate at final matched school) to 18,301 (matched to 1st choices). All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.5: Impact of informational interventions on other measures of HS quality

	Treatment groups				Control group	
	FF1	FF2	FF3	Pooled	Mean	SD
HS 9th grade % on track:						
1st-3rd choices (mean)	-0.0840	-0.386	-1.176**	-0.557+	85.6	7.4
	(0.320)	(0.414)	(0.353)	(0.296)		
Final matched school	0.752+	0.204	-0.738+	0.0750	81.3	10.2
	(0.382)	(0.443)	(0.407)	(0.314)		
College readiness %:						
1st-3rd choices (mean)	0.196	-0.902	-1.596*	-0.734	63.1	14.9
	(0.607)	(0.831)	(0.758)	(0.554)		
Final matched school	1.649*	-0.239	-1.148+	0.152	53.9	16.4
	(0.645)	(0.803)	(0.655)	(0.545)		
% of students who feel safe:						
1st-3rd choices (mean)	-0.504	-0.835*	-0.918**	-0.739*	83.4	6.8
	(0.337)	(0.396)	(0.332)	(0.288)		
Final matched school	-0.259	-0.544	-0.737+	-0.505	80.0	9.5
	(0.362)	(0.421)	(0.410)	(0.318)		

Notes: each *row* reports estimates from two regressions. The first includes indicator variables for the separate treatment groups (FF1-FF3). The second pools the three treatment groups into one indicator variable. Sample sizes vary from 15,961 (college readiness at final matched school) to 19,107 (on-track percent at 1st-3rd choices). All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.6: Pooled impact estimates by subgroup, part 1

	FRPL	Not FRPL	Spanish	Other lang	English	Math Q1	Math Q4	New to dist
% of 1st-3rd	7.314***	7.638***	9.622***	11.34***	4.211**	6.154***	10.80***	6.256+
choices from FF1	(1.747)	(2.004)	(1.601)	(2.652)	(1.570)	(1.727)	(2.677)	(3.203)
Matched to	2.905*	5.286**	1.297	1.287	3.558*	3.255+	5.445+	9.504*
1st choice	(1.454)	(1.770)	(1.823)	(3.355)	(1.472)	(1.829)	(3.161)	(4.501)
Matched to	2.225+	6.124***	3.328*	-2.965	2.888*	4.712**	3.692	6.246
1st-3rd choice	(1.264)	(1.754)	(1.571)	(2.901)	(1.286)	(1.451)	(2.553)	(4.152)
Graduation rate:	-0.205	-0.881+	-0.0863	0.387	-0.568	-0.692	-0.581	-0.774
1st-3rd choices	(0.463)	(0.528)	(0.547)	(0.877)	(0.490)	(0.539)	(0.603)	(1.156)
Graduation rate:	0.832	0.652	0.453	0.193	0.686	0.779	1.338	3.141+
matched school	(0.521)	(0.562)	(0.568)	(0.934)	(0.556)	(0.593)	(1.147)	(1.679)
% below 70%	-1.240	-0.388	-1.839	-5.315+	0.322	-0.101	-2.852	-1.827
1st-3rd choices	(1.756)	(1.783)	(1.775)	(2.739)	(1.564)	(1.832)	(1.981)	(3.439)
Graduation rate	-5.040*	-5.063+	-4.688*	-7.535+	-3.393	-4.552*	-9.962*	-7.807
below 70% (match)	(2.432)	(2.599)	(2.226)	(3.924)	(2.186)	(2.092)	(4.521)	(5.791)

Notes: Student and school covariates and block effects included (as in earlier tables). Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.7: Pooled impact estimates by subgroup, part 2

	Girls	Boys	White	Black	Hispanic	Asian	Immigrant	Born in US
% of 1st-3rd choices from FF1	6.835***	7.825***	15.60***	2.707+	8.581***	12.48***	10.54***	7.025***
	(1.812)	(1.825)	(3.018)	(1.469)	(1.597)	(3.288)	(1.943)	(1.760)
Matched to 1st choice	5.307***	1.104	6.800+	4.575*	0.895	7.115+	3.821	3.223*
	(1.532)	(1.640)	(4.068)	(1.848)	(1.595)	(4.070)	(2.462)	(1.354)
Matched to 1st-3rd choice	4.375**	1.081	-11.22***	5.240**	3.029*	2.403	3.972+	2.476*
	(1.497)	(1.294)	(3.257)	(1.602)	(1.431)	(3.101)	(2.241)	(1.201)
Graduation rate: 1st-3rd choices	-0.420	-0.322	2.075+	-0.992+	-0.127	0.364	-0.636	-0.293
	(0.445)	(0.511)	(1.131)	(0.590)	(0.481)	(1.273)	(0.712)	(0.453)
Graduation rate: matched school	0.731	0.610	0.722	0.414	0.974*	1.795	-0.007	0.843+
	(0.470)	(0.579)	(1.403)	(0.671)	(0.459)	(1.688)	(0.769)	(0.473)
% below 70% 1st-3rd choices	-0.621	-1.565	-14.79***	1.813	-1.354	-5.896+	-0.457	-1.275
	(1.521)	(1.969)	(3.531)	(1.671)	(1.570)	(3.454)	(2.408)	(1.676)
Graduation rate below 70% (match)	-4.634+	-4.696+	-14.40*	-2.244	-5.597**	-15.10*	-5.604+	-4.717*
	(2.348)	(2.511)	(5.608)	(2.267)	(2.035)	(5.894)	(3.125)	(2.284)

Notes: Student and school covariates and block effects included (as in earlier tables). Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.8: Other subgroups: usage and match rates

	Usage: % of 1st-3rd choices from intervention-specific list			Matched to 1st choice			N
	FF1	FF2	FF3	FF1	FF2	FF3	
Full study sample	10.43***	5.503**	5.482**	3.104+	3.530+	3.539*	19109
	(2.112)	(2.051)	(1.957)	(1.651)	(1.794)	(1.655)	
Girls	10.07***	5.275*	4.509*	5.985**	4.449*	5.200**	9371
	(2.122)	(2.192)	(2.068)	(1.864)	(2.053)	(1.939)	
Boys	10.71***	5.655**	5.915**	-0.0630	2.203	1.580	9738
	(2.308)	(2.135)	(2.041)	(1.963)	(2.223)	(1.942)	
Foreign born	14.30***	9.663***	7.917***	3.386	4.187	4.020	3042
	(2.503)	(2.815)	(2.198)	(3.062)	(3.379)	(3.068)	
Born in US	9.921***	5.030*	5.173*	3.066+	3.279+	3.353*	16067
	(2.116)	(2.042)	(2.010)	(1.627)	(1.823)	(1.631)	
EL	13.11***	6.169*	6.837*	1.022	-0.131	6.241*	3064
	(2.795)	(3.041)	(2.878)	(2.277)	(3.423)	(3.026)	
Not EL	9.788***	5.192*	4.974*	3.397*	3.538*	2.511	16045
	(2.166)	(2.059)	(1.962)	(1.720)	(1.755)	(1.673)	
Special education	8.779***	4.388+	5.744*	0.324	-0.114	3.478	4141
	(2.498)	(2.299)	(2.315)	(2.572)	(2.600)	(2.446)	
Not special education	10.84***	5.888**	5.527**	3.706*	4.545*	3.289+	14968
	(2.118)	(2.159)	(2.004)	(1.681)	(1.832)	(1.686)	
Girls - Q1 math	8.491***	6.760**	7.755***	3.949	0.00185	5.366+	2821
	(2.076)	(2.020)	(1.970)	(2.851)	(3.214)	(3.048)	
Girls - Q4 math	13.15***	7.616+	1.046	7.288	-6.345	10.31*	1098
	(3.719)	(4.077)	(4.657)	(4.668)	(5.343)	(5.050)	
Boys - Q1 math	5.702*	3.808	4.334*	-1.310	3.548	4.554	3197
	(2.562)	(2.655)	(2.186)	(2.766)	(3.124)	(2.791)	
Boys - Q4 math	19.77***	7.553*	12.86***	7.885	5.591	8.042	1030
	(3.895)	(3.724)	(3.270)	(5.703)	(5.696)	(5.225)	

Notes: Each row and column set (FF1-FF3) represents estimates from a separate regression for the indicated subgroup. Student and school covariates and block effects included (as in earlier tables). Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.9: Other subgroups: graduation rates of choices and matches

	Graduation rate matched school			Graduation rate: below 70% matched school			N
	FF1	FF2	FF3	FF1	FF2	FF3	
Full study sample	1.664**	0.526	-0.066	-6.274*	-5.147+	-3.346	16657
	(0.571)	(0.662)	(0.596)	(2.418)	(2.959)	(2.865)	
Girls	1.439**	0.677	0.0382	-5.795*	-5.597+	-2.820	8272
	(0.546)	(0.690)	(0.589)	(2.450)	(3.104)	(2.945)	
Boys	1.754*	0.290	-0.372	-6.280*	-4.291	-3.245	8385
	(0.679)	(0.755)	(0.709)	(2.726)	(3.216)	(3.043)	
Foreign born	0.316	0.283	-0.442	-4.967	-8.233*	-4.825	2651
	(0.960)	(1.007)	(0.952)	(3.699)	(4.062)	(3.833)	
Born in US	1.850***	0.579	-0.0417	-6.410**	-4.747	-2.896	14006
	(0.533)	(0.644)	(0.574)	(2.310)	(2.908)	(2.840)	
EL	2.151*	0.0174	-2.495*	-12.75***	-5.345	-0.399	2707
	(1.049)	(1.086)	(1.137)	(3.435)	(3.590)	(3.704)	
Not EL	1.570**	0.606	0.341	-4.949*	-5.053	-3.558	13950
	(0.569)	(0.681)	(0.591)	(2.476)	(3.117)	(2.907)	
Special education	0.969	-0.420	-1.228+	-0.875	-1.510	1.913	3662
	(0.760)	(0.870)	(0.698)	(2.943)	(3.583)	(3.215)	
Not special education	1.741**	0.631	0.0661	-7.390**	-5.749+	-4.322	12995
	(0.600)	(0.688)	(0.637)	(2.580)	(3.068)	(3.018)	
Girls - Q1 math	1.636*	1.601	-0.429	-6.346*	-11.02**	1.245	2505
	(0.754)	(0.990)	(0.806)	(3.079)	(3.903)	(3.397)	
Girls - Q4 math	0.418	-0.267	1.563	-6.815	-6.491	-11.40*	908
	(1.381)	(1.500)	(1.678)	(4.956)	(5.213)	(5.455)	
Boys - Q1 math	1.633+	0.605	-0.552	-5.613+	-5.693+	-2.206	2808
	(0.868)	(0.838)	(0.854)	(2.999)	(3.342)	(3.155)	
Boys - Q4 math	3.822*	0.570	0.275	-14.58*	-2.764	-9.329	785
	(1.599)	(2.069)	(1.731)	(6.220)	(7.835)	(6.433)	

Each row and column set (FF1-FF3) represents estimates from a separate regression for the indicated subgroup. Student and school covariates and block effects included (as in earlier tables). Standard errors in parentheses, adjusted for clustering at the school level. + $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table D.10: Impact of informational interventions on other choice outcomes

	Treatment groups				Control group	
	FF1	FF2	FF3	Pooled	Mean	SD
Nonselective and screened language supplement:						
Percent from nonselective supplement, 1st-3rd choices	3.263*	6.748***	3.049+	4.077**	14.5	23.3
	(1.455)	(1.588)	(1.570)	(1.235)		
On screened language supplement, % of all choices	0.485	-0.309	0.167	0.166	3.4	11.6
	(0.439)	(0.386)	(0.427)	(0.359)		
Any choice from screened language supplement	-0.740	-0.551	-0.957	-0.770	15.3	36.0
	(1.351)	(1.529)	(1.280)	(1.119)		
Characteristics of choices:						
Percent new schools, 1st-3rd choices	-0.0982	1.082	-1.056	-0.143	9.0	17.9
	(0.735)	(0.870)	(0.760)	(0.628)		
All choices in the same borough	9.009**	3.551	3.982	5.792*	51.8	–
	(2.817)	(3.474)	(2.771)	(2.308)		
Top 3 choices in the same borough	9.370***	2.346	3.950	5.609**	64.9	–
	(2.564)	(2.965)	(2.526)	(2.063)		
Graduation rate of choices 1-3 in descending order	2.075*	2.995**	1.362	2.052*	34.2	47.4
	(1.010)	(0.953)	(0.997)	(0.791)		
Percent of all choices within 45 minutes	-1.103	-3.850+	-3.141+	-2.544	80.0	22.6
	(2.254)	(2.132)	(1.836)	(1.661)		
Other outcomes:						
Took SPHS exam	0.519	-2.347	-1.109	-0.804	27.3	44.5
	(1.601)	(1.903)	(1.789)	(1.498)		
Offered a SPHS seat	-0.0594	0.157	-0.311	-0.0950	2.1	14.3
	(0.243)	(0.287)	(0.236)	(0.212)		

Notes: each *row* represents estimates from a separate regression. Sample sizes vary from 19,013 (graduation rates in descending order) to 19,109 (all others). All models include the following controls: school randomization block, student race/ethnicity, female, free lunch eligible, reduced-price lunch eligible, special education, EL, foreign born, quadratic in 7th grade ELA and mathematics z-scores, missing indicators for z-scores and other covariates, and indicator for students in schools that received a treatment in our 2014-15 pilot study. School-level controls include a charter indicator, 8th grade enrollment, percent female, percent by race/ethnicity, percent with disabilities, percent EL, and mean 8th grade math and ELA scores. All school controls are measured in the year prior to treatment. Standard errors in parentheses, adjusted for clustering at the school level. $+\ p < 0.10$ $*\ p < 0.05$ $**\ p < 0.01$ $***\ p < 0.001$.