When Do Informational Interventions Work? Experimental Evidence from New York City High School Choice*

Sarah R. Cohodes†
University of Michigan and NBER

Sean P. Corcoran
Vanderbilt University

Jennifer L. Jennings
Princeton University

Carolyn Sattin-Bajaj
University of California, Santa Barbara

August 2023

Abstract

Despite evidence that informational interventions can influence K-12 school choices, we know little about the mechanisms through which they work and the factors that produce heterogeneity in student responses. Through a school randomized trial conducted in 473 New York City middle schools serving 115,000 8th graders, we evaluated three counselor-delivered informational interventions that were designed to help students avoid low-graduation high schools, but differed in their level of individual customization and mode of delivery (paper or online). Every intervention reduced likelihood of application to and enrollment in low graduation rate schools (those below the city median of 75 percent). Simplified paper interventions had the largest impacts and produced lower heterogeneity in effects across subgroups than customizable digital formats. A key mechanism by which interventions worked was through new information replacing students' default first choice schools that had low graduation rates and guaranteed admission. We conclude that informational interventions to support school choice can be effectively implemented at scale via school counselors, but that intervention design can lead to differences in who engages, with consequences for inequality.

---

* We are grateful to the research assistants and staff who made this field experiment possible and contributed to the research: Christine Baker-Smith, Stewart Burns Wade, Sherene Alexander, Alexandra Bray, Duwa Alebdy, Andrea Cornejo, Alex Clothier, Florangel DeLeon, Erin Huffer, Shaked Landor, Dhanya Madugalle, Marina Makram, Zach Malter, Katharine Parham Malhotra, Barkat Sikder, Eric Sturm, and Hailey Vogel. Conference participants at the APPAM and AEFP Conferences provided helpful comments. Special thanks go to Alex Eble and Leslie Kern for their feedback. We thank the Smith-Richardson Foundation for financial support. We thank Jim Kemple and the Research Alliance for New York City Schools, which enabled access to NYC administrative data. Additional funding for the NYC High School Admissions Study came from the Heckscher Foundation for Children, the NYU Institute for Human Development and Social Change, the Spencer Foundation, and the William T. Grant Foundation. Staff at the NYCDOE enrollment office provided helpful feedback, in particular Nadiya Chadha, Lianna Wright, and Stewart Burns Wade. This study is registered in the AEA RCT Registry and the identifying number is: AEARCTR-0003736. The study was determined as exempt from human subjects review by the Institutional Review Boards at NYU, Teachers College, and the NYC Department of Education.

† Corresponding author. Gerald R. Ford School of Public Policy, University of Michigan, 735 S. State St. Ann Arbor, MI 48109. E-mail: scohodes@umich.edu.

1 Introduction

"Low-touch" informational interventions in the United States have influenced families' K-12 and college choices (Hastings and Weinstein (2008); Valant (2014); Corcoran et al. (2018); Weixler et al. (2020); Arteaga et al. (2021); Hoxby and Turner (2013); Wiswall and Zafar (2014); Conlon (2019); Dynarski et al. (2021); Bettinger et al. (2012); Page et al. (2020)). In these settings, simplified, salient information reduced the frictions inherent in decision-making and led to better outcomes for students and other choice-makers. Despite initial enthusiasm about their effect sizes, multiple studies have not reproduced the original interventions' benefits, and the factors accounting for these replication failures are unknown (Gurantz et al., 2021; Bergman et al., 2019; Hyman, 2020). Further progress will require a nuanced understanding of the conditions under which low-touch interventions work: the types of decisions for which they are most effective, the relevance of intervention modality (how the intervention is delivered), and the sources of heterogeneity in participants' responses (Saez, 2009; Mrkva et al., 2021; Oreopoulos, 2021).

We advance this literature by designing and implementing multiple informational interventions in a school district in which all students must apply to a high school and where no default options are available. Navigating the high school admissions process in New York City can be a daunting task for 13-year-olds and their families. Despite an application system that is ostensibly in the hands of parents and guardians, both prior research (Sattin-Bajaj, 2014) and our surveys and interviews (Sattin-Bajaj et al., 2018) found that many 8th-graders select high schools with limited parental input. School assignment occurs through a difficult-to-understand deferred acceptance algorithm that accounts for students' choices and priority groups as well as schools' requirements (Abdulkadiroğlu et al., 2005). New York City's wide high school quality distribution means that successfully navigating high school admissions can be a consequential turning point in young New Yorkers' educational careers. Such success is not uniformly distributed. Students with low test scores and from low-income backgrounds enroll in schools, with lower graduation rates (Nathanson et al., 2013; Corcoran et al., 2018)), an outcome largely driven by their application choices. Because of its relatively small geographic area and robust public transportation system, NYC students can reach higher-performing schools in similar travel times, which makes the city an ideal site for an informational intervention.

To investigate the role of information and technology in guiding students to avoid enrolling in high schools with lower graduation rates, we fielded a series of information supports for high school choice in NYC in a school-level randomized controlled trial of 473 middle schools during the 2016-17 and 2017-18 school years. The structure of our interventions makes it possible both to assess whether access to these supports can reduce the likelihood of enrolling in a low graduation rate high school but also what circumstances drive successful intervention and whether such interventions benefit all students similarly. Middle schools were randomly assigned to one of three interventions that differed in their level of customization and mode of delivery (paper or online), or to a control group.

The interventions included a middle school-specific list of recommended nearby high schools selected for having graduation rates above 75 percent (the NYC median graduation rate in 2015) and non-zero probability of admission for past students at that middle school ("Fast Facts"); an online app that generated a list of recommended schools based on student preferences (the "App"); and a publicly available online high school search tool ("School Finder"). We use high school graduation rates as our main measure of school quality in the interventions and when assessing their impact, as opposed to a growth measure, since these are the measures used by the school district. We also show that sample high graduation rate high schools have higher value added than the low graduation rate schools. Middle schools assigned to the Fast Facts treatment arm were also randomized to receive their high school lists in paper or digital formats. School personnel, typically a school counselor, received the intervention tools to distribute along with supplementary materials (lesson plans, video guides, and support from the study office). This method of dissemination approximates how a school district might use these tools in practice with decentralized distribution via counselors, which differs from prior studies with either direct delivery to students by the study team (as in Corcoran et al. (2018)) or to parents (as in Hastings and Weinstein (2008); Valant (2014) and Weixler et al. (2020)). This experiment included almost 80 percent of the middle schools in NYC, with over two-thirds of NYC middle schools receiving some form of treatment, testing the effect of large-scale provision of information.

A related intervention by this research team in the previous school year, 2015-2016, set the stage for our interventions (Corcoran et al., 2018). The prior intervention focused on 165 high-poverty middle schools that were consented and then randomly assigned either to either a control group or to receive a visit from trained research staff provided an earlier version of Fast Facts, which at that point in time was one-page listing of 30 nearby high schools, along with travel time information and the four-year graduation rate, restricted to high schools with a graduation rate of 70% or above as well as a lesson on how to use it. Students in schools that received the treatment selected more schools from the recommended lists, and applied to, matched to, and enrolled in schools that were less likely to have graduation rates below 70%. However, the interventions did not reduce inequality by prior achievement, since, for example, higher-achieving students applied and matched to higher graduation rate schools at a greater rate than lower-achieving students.

The interventions we focus on in this study were designed to go beyond those tested in Corcoran et al. (2018). First, we have a larger sample of schools that represents the full school poverty distribution in New York City and deliver of the materials via school counselors, rather than study staff. This meant we designed interventions that would replicate the policy environment of a school district. Unlike the prior year when we randomized within high-poverty schools that consented to be randomized to an intervention, this iteration included schools at a variety of poverty levels regardless of expressed interest — again, replicating the policy context of district-delivered materials. Second, we fielded a greater variety of interventions, making it possible to investigate the role of technology, personalization, and utilization in information adoption.

In this paper, we first document—using surveys, interviews, and followup calls—that the majority of school counselors who received intervention materials used or planned to use them. We then show that assignment to the utilized tools changed the composition of schools that students listed on their application. In particular, they reduced the likelihood of applying to a guaranteed,[1] low-graduation rate high school — which we define as high schools with graduation rates below 75 percent, the city median at the time — as the first choice. Students substitute higher graduation rate schools on their applications, and, importantly, students assigned some of the treatments shift to schools that are not just higher graduation rate but also have a higher probability of admission. The successful interventions reduce enrollment in low-graduation rate high schools by between 5.1 and 6.1 percentage points, a 13 to 15 percent reduction.

With evidence from subgroup responses, we also show that the shift in high school match and enrollment corresponded to shifts in application behavior, suggesting that those who make greater use of the tools have greater response. We also note that English learners – 12 percent of 8th graders in the district – had the strongest response to all of the interventions.[2] This highlights the need for salient, accessible school choice materials.

Simplified paper interventions had the largest impacts and produced less heterogeneity in effects across subgroups than customizable digital formats. However, putting the same information online as in the paper intervention was not effective. Successful information use requires not only curation, but features that increase engagement. We found that multiple pathways are effective, including physicality and individual customization. We ultimately conclude that the specific design of the intervention is less important than engagement with any intervention, but that the design of materials can led to differences in who engages, with consequences for (in)equality.

These interventions aimed at steering students away from low-graduation high schools led to more students attending high-graduation schools than they would have otherwise. We find no evidence of so-called mismatch (Arcidiacono and Lovenheim, 2016) in that students' subsequent high school performance is similar to that of students in control group schools. We will continue to follow these students and assess impacts on high school graduation as time goes on. The paper proceeds as follows. Section 2 describes the background and context, with more details on the interventions. Section 3 details the data, study design, and estimation methods. Section 4 describes use of the intervention tools. Results are reported in Section 5. We conclude in Section 6.

2 Background and context

2.1 The New York City high school admissions process

In New York City, all 8th graders participate in a high school choice process, through which they submit a rank-ordered list of up to 12 high school choices.[3] School assignments are made centrally by the New York City Department of Education (henceforth, NYCDOE) through the use of a deferred acceptance algorithm (Abdulkadiroğlu et al., 2005, 2009). The algorithm is "strategy-proof," in that not being admitted to a school high on one's personal list does not affect the chances of being admitted to a choice lower on the list. This implies that applicants should list schools based on their true preferences.[4]

In the spring of 7th grade and the fall of 8th grade, school counselors and other school personnel assist in the high school choice process, which can include gathering information about high schools from the NYC High School Directory, open houses and school fairs, and internet sources.[5] Applicants have many choices: NYC has an extensive variety of high school programs including large comprehensive high schools, small, themed schools, and academically-screened schools. Students apply to specific programs rather than high schools. Programs have different admissions methods and students may have priority for different programs, which can include geographic areas, and in some cases, academic and attendance records from 7th grade, all of which influence the chance of getting into a particular program. Some schools also prioritize additional steps, such as attendance at an open house or sitting for a locally-designed exam. This process occurs in parallel to but separate from admission to specialized NYC high schools, where admission is determined by a score on an exam. The district also has schools and programs targeted towards English learners and newcomers to the United States. The city has a comparatively small number of charter high schools with a separate application process.

Applications are due in early December, and matches are released in March or April. In our experimental sample, a plurality of students are matched to their first choice school, and over two thirds are matched to one of their top three choices. Students that are not matched to any school in the first round of the choice process (about 4 percent of applicants) can participate in a second round of the admissions process where the remaining open seats are again allocated by the algorithm. If no match is made at that point, students are administratively assigned to schools, as are 8th or 9th graders who enter the district after the admissions process is complete.[6]

The school choice process in NYC carries a large "administrative burden" (Moynihan et al., 2014). Our interviews with more than 450 students, parents, and counselors demonstrate that students and their adult family members frequently misunderstand key components of this process (Sattin-Bajaj, 2014; Sattin-Bajaj et al., 2018; Jennings et al., 2018; Sattin-Bajaj and Jennings, 2020). Many counselors believe it is not appropriate to give action-guiding advice on high school selection, such as recommending specific schools over others, and counselors also state that they are unaware of all of the available options given the large number of choices (Sattin-Bajaj et al., 2018). Our interviews with students show that students often: 1) believe they will be more likely to get one of their choices if they list fewer options, when the opposite is true; 2) apply to schools for which they do not meet eligibility requirements; or 3) are not aware of rules for "limited unscreened" schools (including many newer small high schools), which give them preference if they attend a school fair or information session (Corcoran et al., 2017). These errors in the application process can lead students to match to lower-quality schools than they might otherwise have and contribute to the inequalities between students with correct information about the process (or with parents or consultants to help navigate the process, as Sattin-Bajaj and Roda (2020) show is the case in NYC) and those with less information about the process. Additionally, even when controlling for academic achievement and borough, substantial gaps remain between subsidized lunch recipients, non-English speaking families, and Black and Hispanic/Latino students and their more advantaged peers in terms of choosing and matching to higher graduation rate schools (see Table 1 in Corcoran et al. (2018)). Misinformation about the admissions process, informational overload, and inequality in school choice outcomes, sets the stage for informational interventions to potentially assist students to make better informed, appropriate choices.

2.2 Informational interventions for school choice and beyond

Prior informational interventions to facilitate K-12 school choice in the United States have changes student attendance patterns.[7] The outcomes of school choice processes are meaningful: where students go to high school matters for their longer-term trajectories (Bloom and Unterman, 2014; Angrist et al., 2016; Deming et al., 2014; Allensworth et al., 2017; Abdulkadiroğlu et al., 2017; Jackson et al., 2020), but access to high-quality high schools is not evenly distributed.

Hastings and Weinstein (2008) provided information about school quality and odds of admission to students participating in the choice process in Charlotte, NC. They found that direct and simplified information about school test scores significantly increased the fraction of families choosing high-performing schools by 5 to 7 percentage points. Building off this work, Valant (2014) gave informational "guides" developed by GreatSchools.org to students and their parents participating in school choice in Milwaukee, Washington, DC, and Philadelphia to determine whether providing additional information about schools and their performance affected the choices made and the roles of adults and children in school choice. Their results varied across grade levels and locales: In Milwaukee and Washington, DC, families choosing middle schools were more likely to select schools identified as higher-performing in the guides while families choosing high schools chose schools with lower academic ratings. Because the study focused on school choice outcomes only, the processes producing variation across cities and levels of schooling are unclear. In New Orleans, Weixler et al. (2020) experimentally provided information about "high-performing" schools (which highlighted new, state-provided letter grades indicating high-growth schools), neighborhood schools (which highlighted nearby schools), and general information about the school choice process (as a control). They found that information about high-performing schools increased the likelihood that a student chose and was placed at such a school, but that impacts were concentrated among high school entrants and students with disabilities.

All of the prior studies and our previous work in NYC (Corcoran et al., 2018) involve paper artifacts shared with students and their families from a study team. Our inventions move beyond this to include digital interventions, framing questions about how intervention design and modality influence their success, as well as providing materials through school counselors, simulating a "real world" test of potential impact. Thus, we also to contribute to a growing literature on *how* the design of school choice platforms and interventions influence choices. Glazerman et al. (2020) use a lab experiment of a hypothetical school choice system to show that small design choices can influence parents' school selections. Ordering choices to promote higher performing schools, summarizing school quality information with icons, and displaying shorter summaries of school information all led to parents selecting higher performing schools. Arteaga et al. (2021) embed interventions directly in school choice platforms in Chile and New Haven. To create these "smart matching platforms," they worked with policymakers to include pop-up or email warnings when choice slates were unlikely to lead to matches and found that this warning led families to select more schools and be more likely to match to a selected school. This intervention targeted application "strategy," with the goal of increasing match but not necessarily school quality.

Beyond K-12 school choice, informational interventions can support decision-making in many other contexts. Within higher education, there are interventions around college and major choice (Hoxby and Turner, 2013; Wiswall and Zafar, 2014; Conlon, 2019), availability and guarantee of college funding (Dynarski et al., 2021), and financial aid completion (Bettinger et al., 2012; Page et al., 2020). These interventions and others in alternative contexts such as insurance and benefit claiming (Abaluck and Gruber, 2016; Johnson et al., 2013; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019), show that the clear presentation of relevant information can improve decision-making and outcomes for choosers. However, as noted previously, not all informational interventions are successful (Gurantz et al., 2021). Another college-access intervention in Michigan (Hyman, 2020), found that a letter with information about college and a link to a website with more details did not increase overall college enrollment, but there were small benefits for low-income students. Similarly, Bergman et al. (2019) find that information about tax benefits for college makes no difference in college enrollment. This highlights that the information context, provider, supportive materials, and targeted population may be important for an informational intervention to succeed. Information is just one piece of a multidimensional problem. Simplifying processes and relieving administrative burdens may be more effective than informational interventions when the main barriers to school access come from the process itself.

2.3 Interventions

To help students navigate the complicated high school choice process described above, we fielded three decision support interventions: a list of recommended schools for each middle school ("Fast Facts"), a personalized list generated by an app (the "App"), and a digital search tool ("School Finder"). The aim of the interventions was to shift students away from schools with low graduation rates. We focused on high school graduation rates as our measure of school quality, as opposed to a measure that takes into account growth, like value-added, for multiple reasons. The App and School Finder were not created by the research team and they each report on high school graduation rates, as does the High School Directory. Our interviews with school counselors revealed that the published graduation rate in the Directory was main point of discussion in their messaging to students. Counselors saw low graduation rates as relevant beyond schools' efficacy in promoting graduation; as both quantitative and qualitative studies have documented (Balfanz and Legters, 2004; Balfanz et al., 2010; Fine, 1991), schools with low graduation rates have higher rates of chronic absenteeism, disorder, and safety concerns. As such, both for consistency across interventions and to increase face validity with students and their families who are used to the familiar graduation rate measure, we used the graduation rate on the intervention we designed (Fast Facts) as well. Additionally, our estimates of high school value-added on high school graduation, shown in Figure 1, indicate that for high schools with graduation rates above 75 percent (the city median, and threshold we used to recommend schools), 78 percent of high schools with above-median graduation rates also had positive value-added. There is much more variation in value-added among low graduation rate high schools. Thus, our easily interpretable measure was also a strong predictor of value-added.

The intervention tools are described in detail below. The interventions were mailed to middle school personnel, typically a school counselor, responsible for shepherding students through high school choice at their school. This is in contrast to our prior intervention (Corcoran et al., 2018), where study team members directly presented the intervention directly to students at schools (supported by the counselor), or alternative designs which could have targeted parents or teachers. We provided each counselor with the intervention tool and a suite of supportive materials (lesson plans, worksheets, video guides, and on-demand assistance from the study office). For some interventions, we provided a printed list of recommended schools (see below for details), for others we provided a postcard with information about how to access the intervention tool online. Fast Facts and School Finder intervention materials were available in both English and Spanish. Materials describing how to access and support the App were available in Spanish, but the App itself was only available in English. We designed an attractive and easy-to-understand suite of materials, with packaging in bright colors to attract attention to the packages in the school mailroom. See Online Appendix F for reproductions of intervention and support materials.

In September 2016, the study team called and emailed all school counselors in treated schools to notify them that we were providing an optional resource to assist in the high school application process as part of a research project and to inquire about the number of Spanish printouts to provide. Shipping of the materials took place in early October, followed by calls to school counselors to ensure that the correct person had access to the materials, as well as to troubleshoot minor issues (e.g., how to access materials on the flash drive, additional copies of materials in Spanish, etc.). The calls took place over October and early November, and the study team remained available to troubleshoot until the high school application was due in early December. School counselors could choose to use the materials, or not, as well as the intensity of use. Based on reports from counselors to our study team, interaction with the materials ranged from no use, to distributing the postcards or school lists with little discussion, to closely reviewing the materials and using our curricular aids to help students use the tools. After high school applications were submitted, the study team fielded a survey of all school counselors in the study, and conducted interviews with a subset of counselors. In the second year of the intervention, we updated the tools and counselor materials, with most of the same experimental structure intact.[8]

All treated schools received the materials and contact with the study team described above. However, the content of the treatments differed. Below, we describe each of our treatments in detail and the randomization design. The interventions differ in whether they were customized at the school or individual level and the degree to which they recommend specific schools. A high-level summary of the randomization is in Online Appendix Table A.1. Further information about the interventions and how they were created is in Section A.2 of Online Appendix A.

School-Customized List of High Schools ("Fast Facts"): This group of 247 middle schools received customized lists of 26 geographically-proximate high schools with graduation rates above

75 percent, along with travel time information, the school's graduation rate, and application information. Schools were only included if past students at that middle school had a history of placing at the high school.[9] Fast Facts lists focused on high schools that counselors and students were likely to be familiar[10] with and omitted schools that had low graduation rates or very low odds of admission. High schools were ordered on Fast Facts by high school graduation rate. Online Appendix Section A.2.1 goes into specifics on the selection process for high schools on the Fast Facts sheets, and sample Fast Facts lists are available in Online Appendix F.[11]

In cross-randomization within this group, students received either a digital only or a paper and digital version of the tool. The digital version was a middle school-specific website, and students received a postcard with instructions on how to access it. Half of schools were assigned to digital only delivery ("Fast Facts Digital") and half to paper and digital delivery ("Fast Facts Paper"). Students in both Fast Facts treatment arms also received access to an additional list of schools for English learners, highlighting schools for newcomers and those learning English, with six-year high school graduation rates above the city median.[12] All materials in the Fast Facts treatments were available in both English and Spanish.

In the second year of randomization for this at-scale study (2017-18), schools previously assigned to Fast Facts continued to receive Fast Facts, using an updated list of recommended high schools. Both digital-only and paper and digital schools received access to the updated Fast Facts website and a digital copy of the printable Fast Facts sheet in English and Spanish, which school counselors could print at their schools to share with students.[13] In order to compare selection of Fast Facts recommended high schools to the control group, as well as across treatment arms (i.e., for schools assigned to the School Finder and App), we generated a Fast Facts list for every school in the study, regardless of their assignment status.

Personalized Recommendations about High Schools from the New York City High School Admissions Guide ("App"): This group of 78 middle schools received a guided introduction to an interactive web and smartphone app designed to help students translate their preferences into a list of school recommendations. The App served as a "virtual school counselor," prompting students to identify their current middle school and their preferences for commute time, academic interests, and extra-curricular interests. It then generated a list of schools, along with performance data, that students could save, share, and explore further. This list was personalized based on the information that the student had entered into the App. The recommendation algorithm was designed to omit low graduation rate schools and to privilege higher graduation rate schools that met a student's criteria, and, if fewer than 20 high schools met those criteria, successively loosen the adherence to students' preferences so that recommended schools continued to have a relatively high graduation rate. Information on other high schools were available on the App through its search function. Detailed descriptions of the App and its algorithm are available in Online Appendix Section A.2.5. In the first year of the study, the App was only available in English. In the second year of randomization, schools continued to receive the App, then available in both English and Spanish.

Personalizable Search Engine of High School Information ("School Finder"): This group of 80 schools received a guided introduction to the NYCDOE School Finder, a search engine for finding high schools that the NYCDOE launched in the 2016-2017 high school admissions cycle and hosted on their main high school admissions website. Since all students had access to this tool (including in the control group), this group allows us to test the effect of a targeted introduction to the tool along with the supportive materials that were offered as part of the intervention. School Finder allowed students to search for specific words (e.g., "soccer" or "performing arts") and included some filters to refine results by admissions methods, location, and school size. However, schools were sortable only by distance and school name, and graduation rate information was only available if a student clicked on a school's name. The information in School Finder was the same as that in the printed directory, but it included active links to school websites and mapping tools to estimate travel time.[14] It was available in English and Spanish. A more detailed description of the School Finder tool is available in Online Appendix Section A.2.6. In the second year of randomization, since School Finder was the main tool being used by the NYCDOE, schools in this treatment arm were reassigned to the App.

Control condition: The 58 schools in the control group did not receive access to any materials designed for the study (the Fast Facts lists and our curricular supports for the interventions). However, students in these schools had access to a number of resources for their high school admissions process: the counselors in their school, their personal networks, online information (including the publicly available School Finder website and the App), school fairs and open houses, and the high school directory. School Finder was widely promoted by NYCDOE at the time of the intervention (so much so, that in the second year we no longer offered it as a separate treatment arm), but the App likely had few users outside the experiment since it was not widely advertised or distributed outside our intervention. Thus, comparisons between treatment and control groups are a test of guided access to our particular suite of materials versus the standard on-the-ground information atmosphere. A "pure" counterfactual where no decision supports are provided is not possible in our context, and while the actual counterfactual students experience is rich with information, the abundance of information and lack of guidance of how to navigate it may contribute to information overload. Thus, our experiment tests the impact of guided access to information as opposed to any access to information. Control group schools remained in the control group in the second year of randomization.

The interventions we focus on in this study were designed to go beyond those tested in Corcoran et al. (2018). First, we have a larger sample of schools. The sample includes both high- and somewhat lower-poverty schools. Specifically, the average poverty rate at middle schools in the 2015-16 year of the intervention was 88 percent; it was 82 percent in the additional sample of the schools we added in the 2016-17 and 2017-18 school years (Online Appendix Table A.1). (The district-wide average poverty rate was 79 percent.) Second, the delivery of the intervention occurred via the school counselor, rather than a trained research team member. This meant that implementation of interventions varied across sites, and that school counselors could choose not to use our materials. At the same time, this design more closely mimics the design of district-based policies, where curriculum options may be provided by district leaders, but individual schools can implement it their own way (see Coburn (2004), Bridwell-Mitchell (2015), and Bridwell-Mitchell and Sherer (2017) for a discussion of how district and state policies are enacted (or not) by school leaders and teachers). School counselors may also have more authority with students than outsiders; alternatively, appealing to external authority may be preferable. Given that the research team could not control the distribution of the tools, we accompanied them with detailed curricular supports, including videos, worksheets, and lesson plans.

Third, we fielded a greater variety of interventions, making it possible to investigate the role of technology, personalization, and utilization in information adoption. Finally, there were several changes in the construction of Fast Facts sheets. We only offered a one-page sheet (the prior intervention included supplements), reduced the number of schools slightly (from 30 to 26), raised the graduation rate floor to 75 percent in order to keep pace with the average graduation rate in NYC, and only included high schools with a successful choice history at that middle school. We summarize these changes in Table 1. For clarity, we call the interventions in 2015-16 the "scale-up" study (Corcoran et al., 2018), and the interventions in 2016-17 and 2017-18 reported here the "at-scale" study.[15] Table 2 highlights the within study differences.

3 Data and research design

3.1 Data and descriptive statistics

NYCDOE provided access to administrative data on students' high school choices, demographics, test scores, and high school placements.[16] We used publicly available information on middle schools for the purposes of randomization blocking, and publicly available information on high schools to generate Fast Facts lists and to describe the high school choices and matches in the student-level data. The main analysis file is formed from the records of the high school admissions process (HSAP) and includes information on students' listed choices, including their priority group (based on geography and other factors) and ranking (by the school) for selective programs. It also includes information on the program to which students were matched, to which we add information on 9th grade high school enrollment. We link students to their demographic information and information about poverty, English learner, and special education status, as well as their seventh grade test scores, which may be used in the admissions process. A full list of student background characteristics is included in Panel A of Table 3. Since the experiment targeted low- and middle-income schools, students in participating schools are more likely to be students of color, to be low-income, and to be English learners than all NYC students.

We generate a number of outcome variables from the high school application data by using the high schools students listed, including students' first choice, their first through third choices, all choices, and their matched and enrolled schools. Outcomes include: high school choices' presence on the Fast Facts list, high school characteristics like graduation rate and admissions method, and process outcomes like an indicator for match to the first choice school. For outcomes that relate to the probability of getting into a particular high school, we simulate the high school admissions process 1,000 times, and calculate the empirical probability of matching to a particular program.[17] Means of the main graduation rate outcomes are listed in Table 3, Panel B.

3.2 Research design and randomization

The randomization pool of potential candidates for the experiment began with all 603 middle schools reported as operating in New York City in Summer 2016, including charter schools. Eliminating a handful of middle schools that closed or consolidated that summer resulted in 592 potential schools. Excluding middle schools that primarily enrolled their 8th graders in the same school for 9th grade (e.g., schools serving grades 6 through 12), as well as relatively low-poverty schools (those with a student body where less than 50 percent are low-income students), resulted in 473 schools. Experimental status was randomly assigned among those 473 middle schools. A high-level summary of the randomization process is below; details are in Online Appendix Section A.1.1. We preserved the initial randomization structure in the 2017-18 school year with minor updates since most school counselors remained in the same school across years.

Given our past relationships with counselors from our interventions in the 2015-16 scale-up study (Corcoran et al., 2018), we guaranteed that all 161 still-open middle schools that participated in the prior year's experiment would receive a treatment (and none were assigned to control). This means that these schools contribute to estimating contrasts across treatments, but not with comparisons to the control group.[18] We refer to these schools as "Tier 1." Randomization maintained the blocking structure from the prior year and, within blocks, we randomly assigned schools to the Fast Facts (digital or paper), App, or School Finder treatment. We emphasized the Fast Facts treatment in this Tier 1 group since school counselors were familiar with a previous version from the prior year, which results in an overrepresentation of Fast Facts in the experiment.

The remaining 312 schools ("Tier 2") were randomly assigned to the Fast Facts treatment (paper or digital), the App, School Finder, or a control group. Random assignment occurred within blocks of matched schools to increase precision (Bruhn and McKenzie, 2009). The randomization as designed is shown in Online Appendix Figure A.1. Fast Facts was assigned to 136 schools, split evenly between digital only and digital and paper delivery. The App and School Finder were each assigned to 58 schools, and 60 schools served as controls.

There were some differences in treatment assignment across years. In the second year of this at-scale intervention, all middle schools previously assigned to School Finder were assigned to the App, since School Finder was widely in use across NYCDOE schools in the 2017-18 year. As all schools assigned to Fast Facts received a version that they could easily print and distribute to their students in the second year, none of the Fast Facts treatments are considered digital only.[19] We discuss how these assignments contribute to estimation below in Section 3.3.

In both tiers, school characteristics were balanced across treatments, as shown in Online Appendix Tables A.3 and A.5. A few school consolidations and other anomalies occured after random assignment, and thus these schools could not participate in the intervention (as they no longer served students or 8th graders). Additionally, a few campuses closed between the first and second year of the intervention (for details on balance see Online Appendix Section A.1.2). School characteristics remained balanced even after these schools dropped out, as shown in Online Appendix Tables A.4 and A.6. As these school changes were unrelated to treatment assignment, they do not affect the random nature of treatment assignment and should not affect our estimates (other than to slightly reduce our sample size, and thus power).[20] A few additional changes occurred due to treatment assignment, typically due to the research team choosing to assign the same treatment to schools that shared a school counselor. Our intention-to-treat estimation strategy is based on the original treatment assignment, and not these post-randomization updates.

3.3 Estimation

We estimate the effect of the interventions on an outcome Y_{ij}, for example enrolling in a low graduation rate high school, for a student *i*, in middle school *j*, as a function of assignment to one of the treatment arms. The school-level treatments are represented by FF_j for the paper version of Fast Facts, $FFDigital_j$ for the digital version, App_j for the NYC High School Application Guide, and SF_j for School Finder, each with a corresponding coefficient that measures the causal impact of assignment to each treatment. Controlling for the randomization block by year, W_b, accounts for blocked randomization and increases power. We also control for vectors of student and school demographic characteristics measured prior to the intervention (X_i and S_j) to increase our precision. The standard errors are adjusted for clustering at the middle school level. To generate intent-to-treat effects by experimental arm, we estimate regressions of the following form:

$$Y_{ij} = \beta_1 FF_j + B_2 FFDigital_j + \beta_3 App_j + \beta_4 SF_j + \gamma X_i + \propto S_j + \sum_{b=1}^{97} \propto_b W_{bj} + \epsilon_{ij}. \quad (1)$$

The estimating procedure generates an intent-to-treat estimate in several senses. First, we assign schools to their original treatment status, even if exigencies in the field required us to deviate from the original treatment plan.[21] Second, students are assigned to the middle school they were enrolled in as of October 1, but may finalize their school choice process in another school if they transfer after October 1 but before applications are due in December. Finally, our interventions were a suite of materials and support provided to school counselors, who were under no obligation to use the materials, and may have chosen not to use the materials for a number of reasons.[22] For all of these reasons, our estimates represent the impact of assignment and access to the intervention, not the use of the intervention. Note, however, that this may be the policy-relevant estimate, as it is consistent with the way the NYCDOE has approached school-based dissemination of information on high school admissions. The DOE can provide materials and encourage use, but they do not enforce or have oversight over a particular approach or curriculum.

4 Using the Interventions

We first show that the interventions were adopted by school counselors and deployed in schools via formal surveys and telephone interviews and informal follow-up phone calls inquiring about use. All school staff responsible for high school admissions at the middle schools were invited to participate in a survey about the admissions process in January 2017 (after high school applications were submitted) and school counselors at 69 schools participated in a follow-up interview. In both the survey and the interviews, we asked counselors to report if they distributed the tools and/or study materials, and show results combined from these samples (there is some overlap).[23] About half of schools had at least one participant in either the survey or the interview, with participation rates higher for schools with interventions (56 to 68 percent) than the control group (43 percent), as shown in Figure 2, Panel D.

Figure 2 reports the rate at which counselors report sharing either the tool or supportive materials with students or parents. In the survey/interview sample, 87-97 percent of respondents report sharing their assigned tool with students or parents (Panel A), with a lower rate reporting sharing the materials (worksheets, practice application, etc.) at 64 to 83 percent of counselors (Panel B). About 70 percent of counselors, including those in the control group, reported sharing School Finder with students (Panel C). Recall School Finder was a new tool announced and publicly available at the time of our interventions, but not integrated into the online application as it was in later years. Being assigned to the School Finder treatment increased this by more than 20 percentage points. Overall, the survey and interview groups show high reported use of the tools and materials—not necessarily surprising in a respondent sample. However, since overall response rates were high, this still reflects at least half of the treated schools reporting use of at least some aspect of the study interventions.

We add to the survey/interview sample responses the informal calls from the research team in Fall 2016 soon after the materials arrived, as displayed in Panels E through G. Research team members called school staff to ensure receipt of the box of study materials and to troubleshoot access to any materials as needed. Additionally, counselors were asked if they had used the materials or if they planned to use them. Combining the survey/interview sample with the call sample reached about 85 percent of the treated schools, as shown in Panel G of Figure 2. In our measure of "use," we supersede responses to follow-up calls with responses to the survey or interview, to reflect the difference between actual usage and planned usage, but unfortunately, we do not have this data for non-respondents.

Panel E shows the rate at which school counselors reported using or planning to use the study materials, where any affirmative is counted as a "yes." In this case, 80-91 percent of respondents report using or planning to use the intervention. Panel F shows a similar combination, except survey/interview responses supersede the informal call. For example, if a counselor initially reported plans to use the tool in the call but later said they did not in the survey, the survey would trump the call. Use rates are slightly lower with this definition, ranging from 80-90 percent.

We also augment reported use with a direct measure of Fast Facts website use observed through a web tracker (Panel H). The Fast Facts website was available to both the paper and digital recipients of the intervention and created only for schools in that treatment arm. We count "use" if there were at least 5 unique views of a school-specific website. We consider a lower number of views likely indicative of a staff member checking out the website but not necessarily sharing it with students. The distribution of digital Fast Facts use is reported in Online Appendix Table D.1 and ranges between 0 and 310 views per school. Only a small number of Fast Facts paper schools had digital use, with 14 percent having at least five views. For the digital version of Fast Facts that increased to 42 percent, despite a high rate at which counselors reported distributing the tool. This is an important reminder that tool distribution does not necessarily mean that students use the tools. We do not have access to equivalent data from the App and School Finder.

Overall, all measures of intervention use indicate that most responding school counselors used the tools. However, our only tool with a direct measure of student use—Fast Facts Digital—shows a lower rate of utilization, indicating that staff-reported use is not a sufficient measure of engagement. Staff may report planning to use the interventions and then not follow through, or may distribute the tool but leave it to students to interact with the contents. Another way to measure use is to determine if assignment to the interventions changed high school applications, as we do in the next section. In Online Appendix C, we split the sample by reported use.

5 Results

In this section, we detail the impacts of the interventions on the various stages of the high school choice process. First, we document how the experiment impacted students' choices, then describe the impacts on matched and enrolled high schools. Throughout, we focus on three key outcomes. The first outcome is the percentage of the top three choices on a student's high school application that have graduation rates below 75 percent (the NYC median). Both the Fast Facts interventions and the App were designed to not include high schools below this floor; we thus consider a reduction in percentage of choices below this floor a key indicator of tool use. The second outcome we term "guaranteed low graduation first choice." This is an indicator for listing as a first choice a high school with a graduation rate below 75 percent and a student-specific guaranteed admissions probability. If this type of school was listed first on an application, it would guarantee that student matched to a low graduation rate school. While this is just one measure of how admissions probabilities interact with choices to form matches, we consider it a concise measure of whether our interventions influence application strategy. Finally, we consider whether a student actually enrolled in a school with a graduation rate below 75 percent to assess whether changes in application behavior converted to changes in high school matriculation.

Since admissions probability is a component of one of our key outcomes, we define and describe how we simulate it here. We conceptualize admissions probability as the probability of matching to a school given one's ranked preferences and those of other students. This probability is a function not only of ranked choices, but also admissions priorities and schools' rankings of students in the case of screened programs. To estimate admissions probability, we run the deferred acceptance algorithm on the high school choices in each students' application, using a random lottery number, one thousand times. The simulated admissions probability is the share of the thousand cases that a student is assigned via the algorithm to a school. If a student always matches to a single school on their application, they have guaranteed probability at that school; if they never match, they have no admissions probability at that school. Since this is an empirical exercise, we can only calculate this probability for schools to which a student applies. Furthermore, priority and ranking information is only available for schools on a student's application.

Admissions priorities are bifurcated, as shown in Online Appendix Figure D.2, which shows the simulated probabilities of admission at first choice schools, not differentiated by study arm. Almost half of students apply to schools that they have no probability of attending as their first choice; 37 percent apply to a first-choice school at which they have guaranteed admission. The remaining 16 percent apply to a school at which they have some chance of admission.

Estimating admissions probabilities for choices beyond the first choice is difficult to conceptualize, since a student can have a zero chance of admission at a later choice school both because of their priority and school ranking or because they matched to a prior choice. Therefore, for choices after the first choice, we use the cumulative admissions probability, which is the sum of admissions probabilities of all choices up to and including the choice of focus.

5.1 Do the interventions change students' high school choices?

Most of the interventions improve high school application quality in terms of reducing application to low graduation rate schools and improving application strategy. In Panel A of Figure 3, we show our key measure of impact on high school choices, the percentage of the first three high schools listed on the high school application with graduation rates below 75 percent, the city median graduation rate. In the control group, 21.1 percent of students' top three high school choices have graduation rates below 75 percent. Assignment to any of the treatment arms reduces this percentage. Fast Facts paper reduces the percentage of low graduation rate schools by 3.1 percentage points to 18.0 percent; for the digital only version of the intervention there is a small, not significant decline of 1.2 percentage points. For the App, there is a 2.6 percentage point reduction percent of school choices that are low graduation rate, and for School Finder, a small reduction of 1.5 percentage points. The sharpest declines are for the interventions that did not allow low graduation rate schools to appear on the tool (Fast Facts and the App), except in the case where we have evidence of low utilization (Fast Facts digital). This gives some credence to the idea that it is engagement with the school lists provided by the tools that generates changes in application behavior, rather than the supportive materials or greater attention to the high school choice process.

We supplement this figure with detailed results in Table 4. The estimates for percentage of high schools with low graduation rates (below 75 percent) which correspond to Figure 3 are in

Panel B. Panel A shows impacts on graduation rates directly, and Panels B and C also combine graduation rate indicators with information on odds of admission.

Panel A of Table 4 shows that Fast Facts paper and App treatment assignment generally increase the average graduation rate of high schools listed on the application. Fast Facts Digital and School Finder have few differences in average graduation rate. The only statistically significant effects are for Fast Facts, where treatment increases the average graduation rate of the top three choices by about 0.8 percentage points off of a base of 85.5 percent. Treatment effects are more apparent when the outcome is the percentage of the top three high school choices with graduation rates below 75 percent, as discussed with regard Figure 3 above.

Changes in graduation rates of choices have the potential to influence high school match and enrollment, but it is also possible that such changes are "wasted" if students have no chance of admission at the higher graduation rate schools. Thus Figure 3, Figure 4 and Panels B and C of Table 4 combine measures of school quality with respect to high school graduation rates with admissions probability—the likelihood that a student will match to that high school if they apply. We consider these indicators of "application strategy."[24]

Our key measure of application strategy is presented in Panel B of Figure 3: the likelihood of application to a low graduation rate school with guaranteed admission to as first choice. Such choices may be students' true preferences; alternatively, it could represent a misunderstanding about the admissions process; 14.4 percent of control group students choose such a school as their first choice. These choices block the opportunity to match to a higher graduation rate school, even if one is listed later on their application. We see that treatment assignment always reduces the likelihood of a guaranteed low graduation first choice by 2 to 3 percentage points. For Fast Facts paper, it is reduced to 11.1 percent, the digital version reduces this to 12.6 percent, the App to 11.2

percent, and School Finder to 11.9 percent (detailed estimates in Panel C of Table 4). Thus, we see that treatment assignment not only reduces the likelihood of applying to low graduation rate high schools, but it also reduces the likelihood applying to a school that would lock a student into a low graduation school with no chance of matching to a higher graduation rate school.

We show some alternative measures of application strategy in Figure 4 and Panel B of Table 4. In both cases, these measures combine the likelihood of listing schools with graduation rates above 75 percent in the top three spots on the high school application with admissions probability for those schools. Recall that when we examine admissions probability beyond the first choice school, we use cumulative probability of a match to account for the fact that if a match occurs to an early choice, by definition it cannot at a later choice. These outcomes jointly indicate selection of relatively high graduation rate schools and the likelihood of getting in to such a school. About 56 percent of control group students apply to all high graduation rate schools, and assignment to treatment does not induce large changes in the likelihood that a student applies to high graduation rate schools for all of their top three choices (Figure 3, Panel A). Assignment to Fast Facts paper or the App increases this by about 2.5 percentage points; Fast Facts digital and School Finder may decrease the likelihood of applying to high graduation rate schools. More meaningful change is revealed in Panel B, which separates application to high graduation rate schools by probability of admission. Here it becomes clear that the increases in application to high graduation rate schools for students assigned to Fast Facts paper and the App are at high schools with some or guaranteed probability of admission, meaning that students in these treatments are implementing more successful application strategies.

Panel B of Table 4 confirms the above and also shows, for Fast Facts paper, the increase in application to high probability, high graduation rate schools is paralleled by a decrease in application to higher graduation rate but no probability of admission schools: this treatment shifts students away from applications at higher graduation rate schools at which they would have no chance of being accepted.[25] The App also increases application to high graduation, high probability schools, though there is not a parallel decrease in high graduation, low probability applications. Both Fast Facts Digital and School Finder do not affect these outcomes.

As a whole, the Fast Facts paper intervention and the App both decrease the likelihood that students list below median graduation-rate schools on their high school applications, and School Finder also reduces this possibility (though the difference is not statistically significant). Exposure to some of the interventions also improves application strategy by several measures. Experimental treatment thus shifts application behavior in two important ways: shifting the likelihood of applying to any low-graduation rate school and reducing the probability of getting "stuck" in a low-graduation rate school due to listing a guaranteed low graduation rate school first. This sets the stage for students avoiding matching to and enrolling in low graduation rate schools.

5.2 Do the interventions improve the quality of matched and enrolled high schools?

Changing students' choices is the first step to changing the schools that students match to and enroll in. However, choices may not translate into match at and enrollment in higher graduation rate schools, for two reasons.[26] First, applications to high-quality schools with low probability of admission would not translate to meaningful enrollment changes if few students have a chance of getting into chosen schools. For example, if our interventions induced students to apply to screened schools for which students did not meet admissions criteria, we would expect a change in choices, but not match and enrollment. Impacts on match and enrollment may also be dampened if there are not sufficient seats at higher graduation rate schools.

We show that these possibilities do not hold, and that students' choices of improved high school quality and application strategy, as described in Section 5.1, indeed translate into higher quality at matched and enrolled high schools in Panel C of Figure 3 and Panels D and E of Table 4. As highlighted in Figure 3, almost 39 percent of students in the control group enroll in high schools with graduation rates below 75 percent. Students in the Fast Facts paper group reduce their likelihood of enrolling in a low graduation rate high school by 6.1 percentage points, reducing the enrollment rate to 33 percent. There is a small, not statistically significant reduction for those assigned to the Fast Facts Digital group. The App has a reduction of the same magnitude as Fast Facts paper, with School Finder a little behind with a reduction of 5.1 percentage points. Outside of the Fast Facts Digital, all of the treatments reduce enrollment in low graduation rate schools.

Table 4 shows the impact estimates behind Figure 3, and more impacts on match and enrolled schools.[27] The interventions increase average graduation rates of the enrolled school by 1.5 percent for Fast Facts paper, 1.2 percent for the App, and 1.1 percent for School Finder. The reduction in enrollment in low graduation rate schools is preceded by similar magnitude reductions in match to such schools, and the reduction is sharpest for Fast Facts paper and the App at the 75 percent threshold—the cutoff for high school inclusion in both tools, but School Finder also shows a large reduction in matching to and enrolling in schools with graduation rates below 70 percent.[28]

We note that this improvement in match and enrollment quality do not come at expense of satisfaction with the choice process. As shown in Online Appendix Table D.2, students in treated schools are slightly more likely to match to their first choice (or top three choices), likely due to applications to schools with better admissions probability, though students in treated schools are very slightly less likely to match to a school in the first round of high school admissions.

Figure 5 shows that the changes in choices and their admissions probabilities drive the changes in enrolled school graduation rates. This figure shows, within each block, the treatment minus control difference in guaranteed low graduation and low graduation rate choices, each plotted against the treatment control difference in enrolled school graduation rates and weighted by number of observations in each block. Across all treatments, we see that within-block contrasts line up: both a reduction in guaranteed low graduation and a reduction in low graduation rate choices correspond to a reduction in rates of enrolling in low graduation rate schools. While there are a range of outcomes due to sampling variation and failure to take up the intervention, the majority of data points are in the lower left quadrant, indicating an advantageous reduction in both outcomes. We consider exactly what aspect of the tools drives this finding in Online Appendix C.

All of the interventions except for Fast Facts Digital result in students matching to and enrolling in higher graduation rate schools, demonstrating that the experiment was effective at its goal of placing students in higher quality schools. By inducing students away from lower graduation rate schools, it may be the case that the intervention pushed students into high school settings for which they were unprepared. We can investigate this "overmatch" concern by following these students into 9th and 10th grade, which we do in Online Appendix Table D.5. Here, we show a resounding lack of impact (either positive or negative) on GPA or credits failed. Even when we examine impacts on a summary measure of academic progress, the "on-track" indicator, by students from different academic preparations, we do not see any consistent evidence of overmatch. For example, students with the lowest or missing test scores are not more likely to have a reduction in on track rate. The real test of match quality will come in future work, as we follow these students to high school graduation and determine if they are more likely to graduate.

5.3 Heterogeneity by student background

An investigation of impacts by student background uncovers two main findings. First, the tools are not effective for all students, and different students benefit from different interventions, though there are universal benefits for English learners.[29] Second, we find the greatest decrease in enrollment in low-graduation rate high schools for the subgroups who respond to the interventions with the greatest reductions in guaranteed low-graduation first choices and percent of low-graduation chosen schools. This implies that enrollment effects manifest for the groups of students who make the greatest use of the tools. We summarize these important relationships in Figure 6 and Online Appendix Figure D.6, displaying impact estimates by subgroups on the proportion of students enrolled in high schools with graduation rates below 75 percent plotted against the subgroup-specific impact estimates on guaranteed low graduation first choices (Figure 6) or the percent of top three choices with graduation rates below 75 percent (Online Appendix Figure D.6). We are testing multiple relationships and have not formally tested for differences between subgroups, so subgroup level findings should be considered suggestive.

The subgroup analysis has some consistent patterns across all the treatments. There are few differences by gender. Students with top-tercile scores on their 7th grade standardized tests tend not to respond to any of the interventions, perhaps because these students are already likely to have school choice plans that aim for admission to exam and screened schools.[30] All of the treatments appear to be particularly effective for English learners, even Fast Facts Digital, with a reduction of enrollment in low-graduation rate high schools of 6.2 to 12.3 percentage points. Consistent with this, the evidence suggests large impacts for students who speak Spanish at home, though the App is particularly effective for students whose home language is neither English nor Spanish.[31]

While NYCDOE provides school choice information in 11 different languages, translated materials are not always immediately available or easy to access. The Fast Facts and School Finder interventions were available in Spanish, and we provided information in Spanish on how to access the App (though the App itself was not available in Spanish at first). Treatment impacts for both Fast Facts paper and School Finder suggest sizeable impacts for students from Spanish-speaking families, which highlights that inducing engagement with tools by removing language barriers facilitates response. These findings underscore how important it is to provide easy to access school choice materials in students' home languages, and to go beyond Spanish in school districts with large numbers of non-Spanish speaking students (Sattin-Bajaj, 2014).

For the Fast Facts paper intervention, Hispanic/Latino students have the largest response of any race/ethnicity category, with a reduction of 7.9 percentage points in likelihood of enrolling in a low graduation rate school and corresponding reductions in guaranteed low graduation first choices and listing low graduation rate high schools among school choice options. The Fast Facts paper intervention suggests greater effectiveness for lower-scoring students, including those with low scores (bottom tercile on 7th grade standardized exams), medium scores (middle tercile), and those missing 7th grade scores. Impacts are larger for low-income students, though these students make up the overwhelming majority of the sample. As discussed above, impacts are particularly large for English learners and students who speak Spanish at home. This pattern of results is suggestive of Fast benefiting historically excluded students the most. This is an interesting contrast to our intervention in the prior year (Corcoran et al., 2018) where treatment effects were larger for comparatively advantaged students. This may reflect differences in school context or random variation in the data across years. The prior year intervention targeted the highest-poverty schools, whereas the scale-up intervention was carried out in a more economically diverse set of schools. The digital version of the intervention generally has few impacts, though some benefits remain for English learners and Spanish speakers.

In contrast to the Fast Facts paper intervention, in some cases, the App treatment suggests greater effectiveness for more historically advantaged groups. Impacts on application strategy, percent of top three choices, and proportion of students enrolled in schools with low graduation rates are larger for white students compared to other students. Asian students also had a bigger response than Black or Hispanic/Latino students. Students who were not low-income also had a slightly larger response than low-income peers. However, English learners and those who do not speak English at home also saw large impacts, as do those with medium, low, and missing test scores. Groups with the largest response for enrolled schools (white students, other language speakers, and English learners) also had the largest impacts on guaranteed low graduation first choices and percentages of listed schools with low graduation rates. The School Finder intervention tends to benefit similar student groups as the Fast Facts paper intervention. Impacts are similar for all race/ethnicity groups, and impacts are largest for those with low or missing math scores and English learners. Again, impacts across the three key outcomes generally align for these subgroups.

The subgroup results underscore a two main points. The first is that groups that use the tools more, as measured by application changes, tend to have the biggest impacts on enrolled high school graduation outcomes. Second, English learners and those whose home language was not English seem to benefit the most from the interventions, pointing to the need for targeted help and materials in home languages for families navigating the school choice process.

6 Conclusion

This paper reports the result of a large, school-level randomized controlled trial of decision supports for young people navigating a complicated high school choice process in NYC. The goal of the interventions, presented in a manner replicating the dissemination of curricular materials from a school district, was to discourage students from enrolling in low-graduation rate high schools, given the known harms for students of attending such schools. We show evidence that most treated schools used the intervention materials, though in the case of Fast Facts Digital, data on internet hits indicates that reports of use do not necessarily convert to changes in application behavior. Response to intervention is greatest at schools that report using the tools and materials.

Most of the interventions ultimately decreased match to and enrollment in schools with graduation rates below the city median (75 percent). Fast Facts paper, a printed list of recommended, relatively high graduation rate schools elicited a strong response, with students in the schools that received Fast Facts treatments ultimately reducing their enrollment in low-graduation rate high schools by 6.1 percentage points. The digital version of the Fast Facts treatment did not alter students' choices, matches, or enrollment. The App and School Finder treatments, each interactive digital search tools, also reduced the proportion of students enrolling in low graduation rate high schools, by 6.1 and 5.1 percentage points, respectively. Fast Facts and the App limited recommended schools to those with relatively higher graduation rates; School Finder was a search engine that did not allow students to search or sort by graduation rate, yet both types of treatment reduced enrollment in low graduation rate schools. Impacts of all the interventions were particularly large for English learners and students who did not speak English at home, perhaps due to the accessible integration of translation.[32]

We interpret the pattern of responses to the interventions to mean that successful informational interventions in a highly-complex context like school choice must spark interaction with the intervention materials in order to generate a response, but that such engagement can be generated through multiple pathways. Providing information alone is not sufficient to generate engagement, especially with an audience of young people. Engagement can come in multiple forms: using the tools themselves, engaging with supportive materials, or being prompted by the tools to engage in more support for the high school choice process. And engagement may come from different sources. It could be driven by the school counselors—the direct recipients of intervention materials in our case—or it could be driven by students and their families. In the first year of our interventions (Corcoran et al., 2018), the presence of a study team member delivering the intervention obliged some level of engagement.

One of the clearest findings from our interventions is that simply providing the same content in digital format, as in Fast Facts paper versus Fast Facts Digital, does not produce the same results. In all of our analyses, assignment to Fast Facts Digital barely influenced choices or matches, likely because of low rates of use of the tool itself, as shown by internet hits to the Fast Facts website. However, it is not that digital interventions themselves are not useful: both the App and School Finder came in digital format and showed success at reducing student enrollment in low graduation rate high schools. The contrast here was that these latter interventions were interactive and personalized, which meant that students needed to interact with the digital materials to a greater extent. Similarly, the interventions provided the same information about high schools that was in the high school directory, but in a salient and/or interactive format. Supportive materials can also provide a pathway to engagement: we found suggestive evidence that the full potential of the School Finder intervention was realized with the curated engagement, via lesson plans, instructional materials, and responses from school counselors about how to use the tools.

An important caveat is that the person-specific App intervention induced the biggest response from comparatively more advantaged students, meaning that personalized, digital interventions may not reduce inequality if students are not equally likely or well-prepared to take advantage of the material. Digital platforms have the potential to reduce costs for providers and allow for personalization based on user input, but internet access is still an issue. While we do not have direct evidence on internet use in our context, but an estimate using 2013 data concluded that over a quarter of NYC households had no broadband internet (Office of the NYC Comptroller, 2014). A more recent analysis highlighted that the gap persisted (though smaller) in 2018, and that many households relied on cell phone service for internet access (Citizen's Committee for Children, 2020). As noted earlier, the NYCDOE has moved much of the high school directory online, supplying only a shortened guide to the process as a booklet, meaning that gaps in access and the existence of select paper tools continue to be relevant.

All of these narratives are consistent with a finding from the information intervention literature: information without curation is often not enough. For interventions to be successful, typically some form of assistance must come with that information (Bettinger et al., 2012; Finkelstein and Notowidigdo, 2019; Carrell and Sacerdote, 2017; Hoxby and Turner, 2013). In the case of the interventions we fielded, take-up via the counselor sets the stage for an effective intervention and both "assistance" in the form of supportive materials and either physicality or personalization seem to contribute to intervention success. Therefore, to enhance the efficacy of informational interventions, policymakers seeking to employ information as a tool to improve student outcomes will want to consider whether and how students and their counselors interact with the materials, and how materials and their presentation can be designed to elicit use.

This may involve embedding tools directly into required materials for the school choice process, as the DOE has with the School Finder tool. Arteaga et al. (2021) show that this can go further, with explicit recommendations connected to application tools. Even then, Arteaga et al. (2021) made recommendations that improved match but not school quality—another potential lever that could be embedded within application systems. However, counselor-distributed or embedded tools may not be effective if such tools limit direct comparison of schools on factors like graduation rate; as traditional districts have not embraced tools or messaging that suggests picking some of their schools over others. For that reason, third parties (i.e., non-profits) may be needed if a goal is comparison on quality metrics. We also note that there is a ceiling to the extent to which informational interventions can improve student outcomes when there is a limited supply of higher graduation rate schools (see, for example, Lincove et al. (2018)).

Due to these interventions, many students now attend higher graduation rate high schools than they would have in absence of the randomized controlled trial. Having been nudged away from low-graduation rate schools, students may in turn be more likely to succeed and graduate themselves. Alternatively, it could be the case that student trajectories are not impacted by high school attendance, as shown for NYC exam schools (Abdulkadiroğlu et al., 2014), and that students are no more likely to graduate from high school than they would have been in absence of the interventions. If there is a "mismatch" between student skills and high school curricula, a push away from a lower-graduation rate school may make some students worse off. Current evidence on high school progress shows little difference for treated students. Future research will track these students over time to determine which of these potential paths matches students' experiences.

Salient and engaging information can change students' choices, matches, and school enrollment. Adapting information content and delivery to different audiences, given different language needs and technology access, may be a key component of intervention success, as may be offering procedural guidance alongside direct information about schools. We caution, however, that even the best information cannot ensure a school match for every student when administrative barriers remain in school choice systems or when there is an undersupply of successful schools.

References

Abaluck, J. and J. Gruber (2016). Improving the quality of choices in health insurance markets. National Bureau of Economic Research Working Paper 22917.

Abdulkadiroğlu, A., J. Angrist, and P. Pathak (2014). The elite illusion: Achievement effects at Boston and New York exam schools. *Econometrica 82*(1), 137–196.

Abdulkadiroğlu, A., J. D. Angrist, Y. Narita, and P. A. Pathak (2017, December). Impact evaluation in matching markets with general tie-breaking. National Bureau of Economic Research Working Paper 24172.

Abdulkadiroğlu, A., P. A. Pathak, and A. E. Roth (2005). The New York City high school match. *American Economic Review 95*(2), 364–367.

Abdulkadiroğlu, A., P. A. Pathak, and A. E. Roth (2009). Strategy-proofness versus efficiency in matching with indifferences: Redesigning the NYC high school match. *American Economic Review 99*(5), 1954–1978.

Ainsworth, R., R. Dehejia, C. Pop-Eleches, and M. Urquiola (2020). Information, preferences, and household demand for school value added. National Bureau of Economic Research Working Paper 28267.

Ajayi, K. F., W. H. Friedman, and A. M. Lucas (2020, October). When information is not enough: Evidence from a centralized school choice system. National Bureau of Economic Research Working Paper 27887.

Allensworth, E. M., P. T. Moore, L. Sartain, and M. de la Torre (2017). The educational benefits of attending higher performing schools: Evidence from Chicago high schools. *Educational Evaluation and Policy Analysis 39*(2), 175–197.

Andrabi, T., J. Das, and A. I. Khwaja (2017). Report cards: The impact of providing school and child test scores on educational markets. *American Economic Review 107*(6), 1535–63.

Angrist, J. D., S. R. Cohodes, S. M. Dynarski, P. A. Pathak, and C. R. Walters (2016). Stand and deliver: Effects of Boston's charter high schools on college preparation, entry, and choice. *Journal of Labor Economics 34*(2), 275–318.

Arcidiacono, P. and M. Lovenheim (2016). Affirmative action and the quality-fit trade-off. *Journal of Economic Literature 54*(1), 3–51.

Arteaga, F., A. J. Kapor, C. A. Neilson, and S. D. Zimmerman (2021). Smart matching platforms and heterogeneous beliefs in centralized school choice. National Bureau of Economic Research Working Paper 28946.

Balfanz, R., J. M. Bridgeland, L. A. Moore, and J. H. Fox (2010). Building a grad nation: Progress and challenge in ending the high school dropout epidemic. Civic Enterprises.

Balfanz, R. and N. Legters (2004). Locating the dropout crisis: Which high schools produce the nation's dropouts? Where are they located? Who attends them? Report 70. Center for Research on the Education of Students Placed at Risk (CRESPAR).

Bergman, P., J. T. Denning, and D. Manoli (2019). Is information enough? The effect of information about education tax benefits on student outcomes. *Journal of Policy Analysis and Management 38*(3), 706–731.

Beshears, J., J. J. Choi, D. Laibson, and B. C. Madrian (2019). Active choice, implicit defaults, and the incentive to choose. *Organizational Behavior and Human Decision Processes.*

Bettinger, E. P., B. T. Long, P. Oreopoulos, and L. Sanbonmatsu (2012). The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment. *The Quarterly Journal of Economics 127*(3), 1205–1242.

Bhargava, S. and D. Manoli (2015). Psychological frictions and the incomplete take-up of social benefits: Evidence from an IRS field experiment. *American Economic Review 105*(11), 3489–3529.

Bloom, H. S. and R. Unterman (2014). Can small high schools of choice improve educational prospects for disadvantaged students? *Journal of Policy Analysis and Management 33*(2), 290–319.

Bobba, M. and V. Frisancho (2016-11). Learning about oneself: The effects of performance feedback on school choice. Social Science Research Network Scholarly Paper 2956702.

Bridwell-Mitchell, E. N. (2015). Theorizing teacher agency and reform: How institutionalized instructional practices change and persist. *Sociology of education 88*(2), 140–159.

Bridwell-Mitchell, E. N. and D. G. Sherer (2017). Institutional complexity and policy implementation: How underlying logics drive teacher interpretations of reform. *Educational Evaluation and Policy Analysis 39*(2), 223–247.

Bruhn, M. and D. McKenzie (2009). In pursuit of balance: Randomization in practice in development field experiments. *American Economic Journal: Applied Economics 1*(4), 200–232.

Carrell, S. and B. Sacerdote (2017, July). Why do college-going interventions work? *American Economic Journal: Applied Economics 9*(3), 124–51.

Carroll, G. D., J. J. Choi, D. Laibson, B. C. Madrian, and A. Metrick (2009). Optimal defaults and active decisions. *The Quarterly Journal of Economics 124*(4), 1639–1674.

Chen, E., G. Simonovits, J. A. Krosnick, and J. Pasek (2014). The impact of candidate name order on election outcomes in North Dakota. *Electoral Studies 35*, 115–122.

Citizen's Committee for Children (2020). New York City's digital divide: 500,000 NYC households have no internet access when it is more important than ever before. Citizen's Committee for Children of New York City.

Coburn, C. E. (2004). Beyond decoupling: Rethinking the relationship between the institutional environment and the classroom. *Sociology of Education 77*(3), 211–244.

Conlon, J. J. (2019). Major malfunction: A field experiment correcting undergraduates' beliefs about salaries. *Journal of Human Resources*, 0317–8599R2.

Corcoran, S., S. Jennings, S. R. Cohodes, and C. Sattin-Bajaj (2017). Administrative complexity as a barrier to school choice: Evidence from New York City. Unpublished working paper, New York University.

Corcoran, S. P., J. L. Jennings, S. R. Cohodes, and C. Sattin-Bajaj (2018). Leveling the playing field for high school choice: Results from a field experiment of informational interventions. National Bureau of Economic Research Working Paper 24471.

Deming, D. J., J. S. Hastings, T. J. Kane, and D. O. Staiger (2014). School choice, school quality, and postsecondary attainment. *American Economic Review 104*(3), 991–1013.

Dynarski, S., C. Libassi, K. Michelmore, and S. Owen (2021). Closing the gap: The effect of reducing complexity and uncertainty in college pricing on the choices of low-income students. *American Economic Review 111*(6), 1721–56.

Feenberg, D., I. Ganguli, P. Gaule, and J. Gruber (2017). It's good to be first: Order bias in reading and citing NBER working papers. *Review of Economics and Statistics 99*(1), 32–39.

Fine, M. (1991). *Framing dropouts: Notes on the politics of an urban high school.* Suny Press.

Finkelstein, A. and M. J. Notowidigdo (2019). Take-up and targeting: Experimental evidence from SNAP. *The Quarterly Journal of Economics 134*(3), 1505–1556.

Glazerman, S., I. Nichols-Barrer, J. Valant, J. Chandler, and A. Burnett (2020). The choice architecture of school choice websites. *Journal of Research on Educational Effectiveness 13*(2), 322–350.

Gurantz, O., J. Howell, M. Hurwitz, C. Larson, M. Pender, and B. White (2021). A national-level informational experiment to promote enrollment in selective colleges. *Journal of Policy Analysis and Management 40*(2), 453–479.

Hastings, J. S. and J. M. Weinstein (2008). Information, school choice, and academic achievement: Evidence from two experiments. *The Quarterly Journal of Economics 123*(4), 1373–1414.

Hoxby, C. and S. Turner (2013). Expanding college opportunities for high-achieving, low-income students. Stanford Institute for Economic Policy Research Discussion Paper 12014.

Hyman, J. (2020). Can light-touch college-going interventions make a difference? Evidence from a statewide experiment in Michigan. *Journal of Policy Analysis and Management 39*(1), 159–190.

Jackson, C. K., S. C. Porter, J. Q. Easton, A. Blanchard, and S. Kiguel (2020). School effects on socioemotional development, school-based arrests, and educational attainment. *American Economic Review: Insights 2(*4), 491–508.

Jackson, C. K., S. C. Porter, J. Q. Easton, and S. Kiguel (2020, December). Who benefits from attending effective schools? Examining heterogeneity in high school impacts. Technical Report 336, Annenberg Institute at Brown University.

Jennings, J., C. Sattin-Bajaj, S. Burns, and A. Bray (2018, March). The how, what and why of school choice informational interventions: Evidence from interview data. Association for Education Finance and Policy Annual Conference.

Jensen, R. (2010). The (perceived) returns to education and the demand for schooling. *The Quarterly Journal of Economics 125*(2), 515–548.

Johnson, E., R. Hassin, T. Baker, A. Bajger, and G. Treuer (2013). Can consumers make affordable care affordable? The value of choice architecture. *PLoS ONE 8*(12), e81521.

King, G., E. Gakidou, N. Ravishankar, R. Moore, J. Lakin, M. Vargas, M. M. T´ellez-Rojo, J. E. H. Ávila, M. H. Ávila, and H. H. Llamas (2007). A “politically robust” experimental design for public policy evaluation, with application to the Mexican universal health insurance program. *Journal of Policy Analysis and Management 26*, 479–506.

Levav, J., M. Heitmann, A. Herrmann, and S. S. Iyengar (2010). Order in product customization *decisions: Evidence from field experiments. Journal of Political Economy 118*(2), 274–299.

Lincove, J. A., J. Valant, and J. M. Cowen (2018). You can’t always get what you want: Capacity constraints in a choice-based school system. *Economics of Education Review 67*, 94–109.

Mizala, A. and M. Urquiola (2013). School markets: The impact of information approximating schools’ effectiveness. *Journal of Development Economics 103*, 313–335.

Moynihan, D., P. Herd, and H. Harvey (2014, 02). Administrative Burden: Learning, Psychological, and Compliance Costs in Citizen-State Interactions. *Journal of Public Administration Research and Theory 25*(1), 43–69.

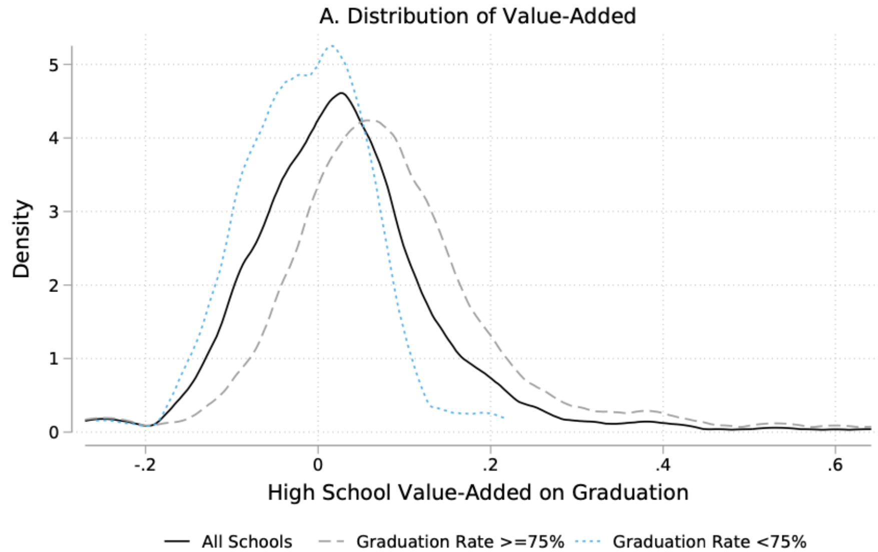
Mrkva, K., N. A. Posner, C. Reeck, and E. J. Johnson (2021). Do nudges reduce disparities? Choice architecture compensates for low consumer knowledge. *Journal of Marketing*, 0022242921993186.

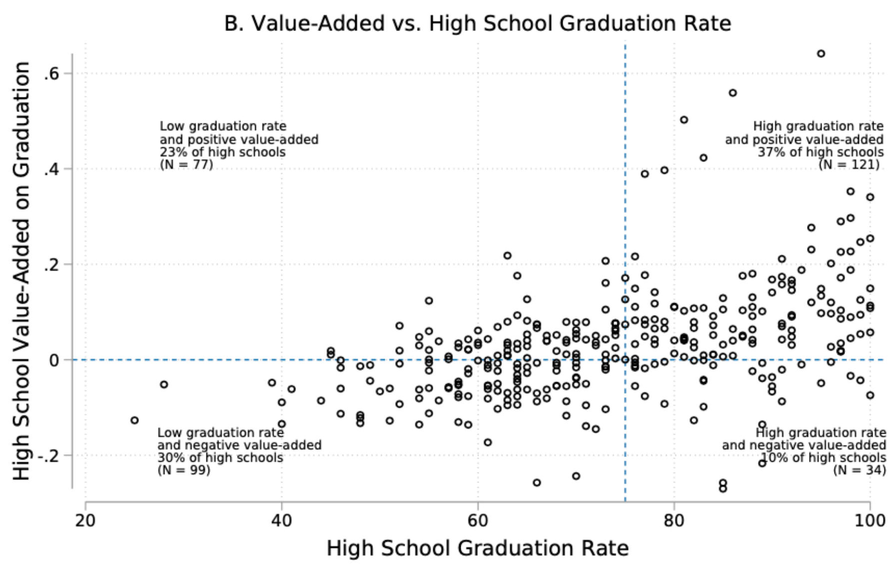
Nathanson, L., S. Corcoran, and C. Baker-Smith (2013). High school choice in New York City: A report on the school choices and placements. Research Alliance for New York City Schools.

Neilson, C., C. Allende, and F. Gallego (2019). Approximating the equilibrium effects of informed school choice. Working paper.

Nguyen, T. (2013). Information, role models and perceived returns to education experimental evidence from Madagascar. Technical Report, The World Bank.
Office of the NYC Comptroller (2014). Internet inequality: Broadband access in New York City.
Office of the New York City Comptroller, Bureau of Policy and Research, Office of the New York City Comptroller, Bureau of Policy and Research.
Oreopoulos, P. (2021). Nudging and shoving students toward success: What the research shows about the promise and limitations of behavioral science in education. *Education Next 21*(2), 8–16.
Page, L. C., B. L. Castleman, and K. Meyer (2020). Customized nudging to improve FAFSA completion and income verification. *Educational Evaluation and Policy Analysis 42*(1), 3–21.
Saez, E. (2009). Details matter: The impact of presentation and information on the take-up of financial incentives for retirement saving. *American Economic Journal: Economic Policy 1*(1), 204–28.
Sattin-Bajaj, C. (2014). *Unaccompanied Minors: Immigrant Youth, School Choice, and the Pursuit of Equity.* Harvard Education Press.
Sattin-Bajaj, C. and J. L. Jennings (2020). School counselors' assessment of the legitimacy of high school choice policy. Educational Policy 34 (1), 21–42.
Sattin-Bajaj, C., J. L. Jennings, S. P. Corcoran, E. C. Baker-Smith, and C. Hailey (2018). Surviving at the street level: How counselors' implementation of school choice policy shapes students' high school destinations. *Sociology of Education 91*(1), 46–71.
Sattin-Bajaj, C. and A. Roda (2020). Opportunity hoarding in school choice contexts: The role of policy design in promoting middle-class parents' exclusionary behaviors. *Educational Policy 34*(7), 992–1035.
Schwartz, B. (2004). *The paradox of choice: Why more is less.* Ecco New York.
Valant, J. (2014). Better Data, Better Decisions: Informing School Choosers to Improve Education Markets. American Enterprise Institute for Public Policy Research.
Weixler, L., J. Valant, D. Bassok, J. B. Doromal, and A. Gerry (2020). Helping parents navigate the early childhood education enrollment process: Experimental evidence from New Orleans. *Educational Evaluation and Policy Analysis 42*(3), 307–330.
Wiswall, M. and B. Zafar (2014). Determinants of college major choice: Identification using an information experiment. *The Review of Economic Studies 82*(2), 791–824.

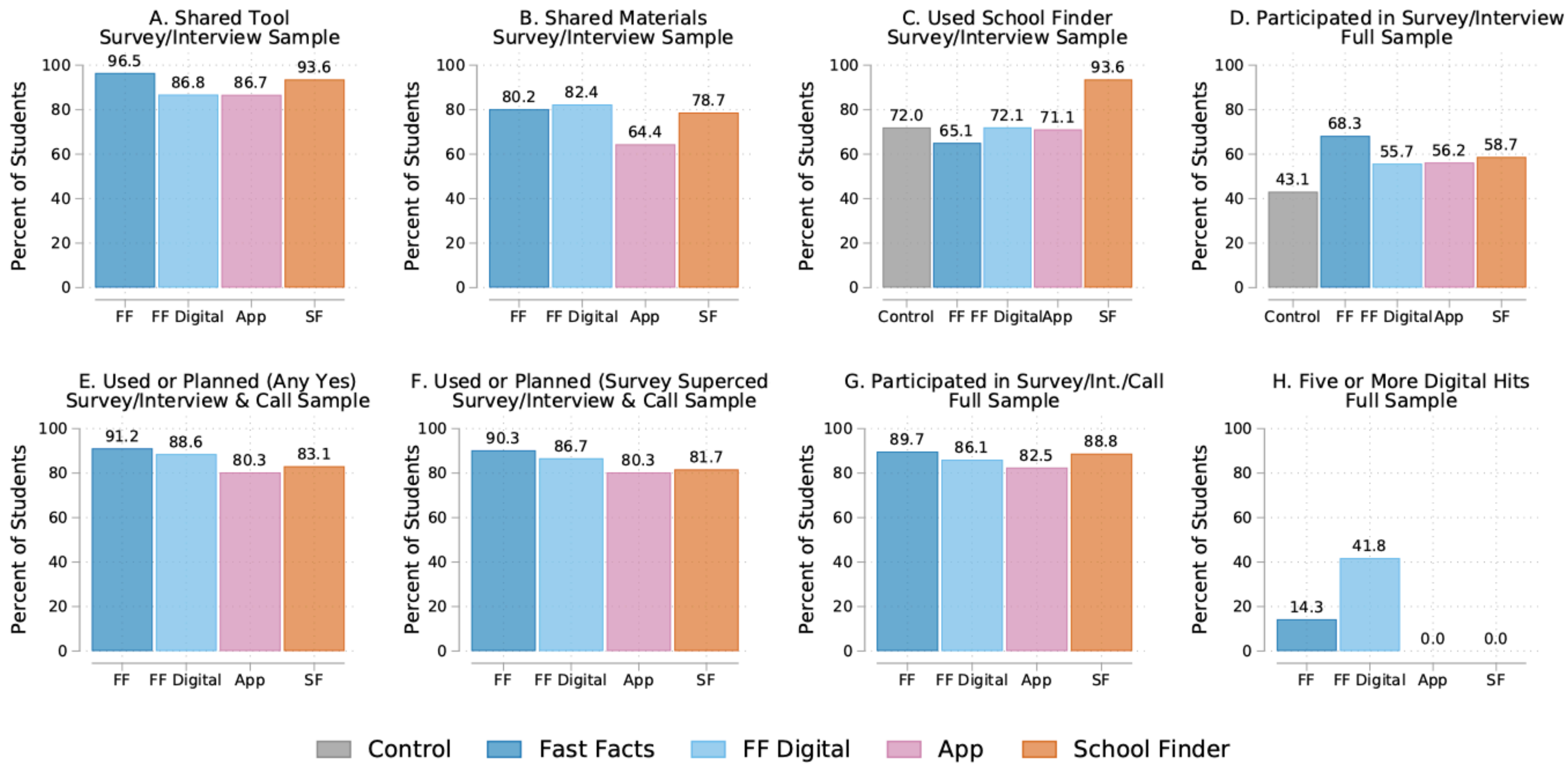
Figure 1. High School Value-Added on Graduation





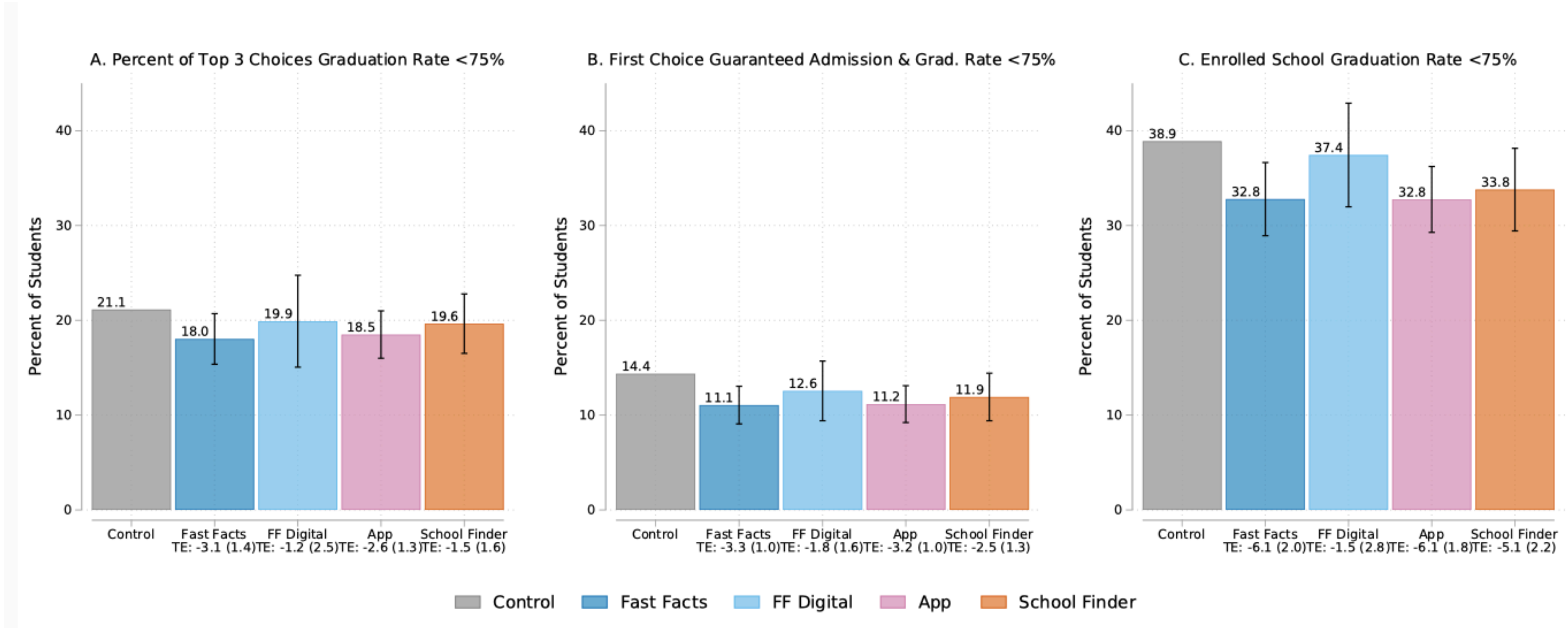
Notes: Panel A of this figure displays the distribution of value-added of high school impacts on four-year graduation for all schools, schools with graduation rates at or above the city median (75 percent), and schools below the city median. Panel B plots these school value-added measures against school graduation rates (from 2014). In both panels, the sample excludes schools that admit students outside the main applications process (charter schools, specialized high schools, D75/special education schools, and D79/alternative schools), and schools which mostly accept returning students in 9th grade (N = 331). 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.

Figure 2. Counselor Reports of Tool and Material Use



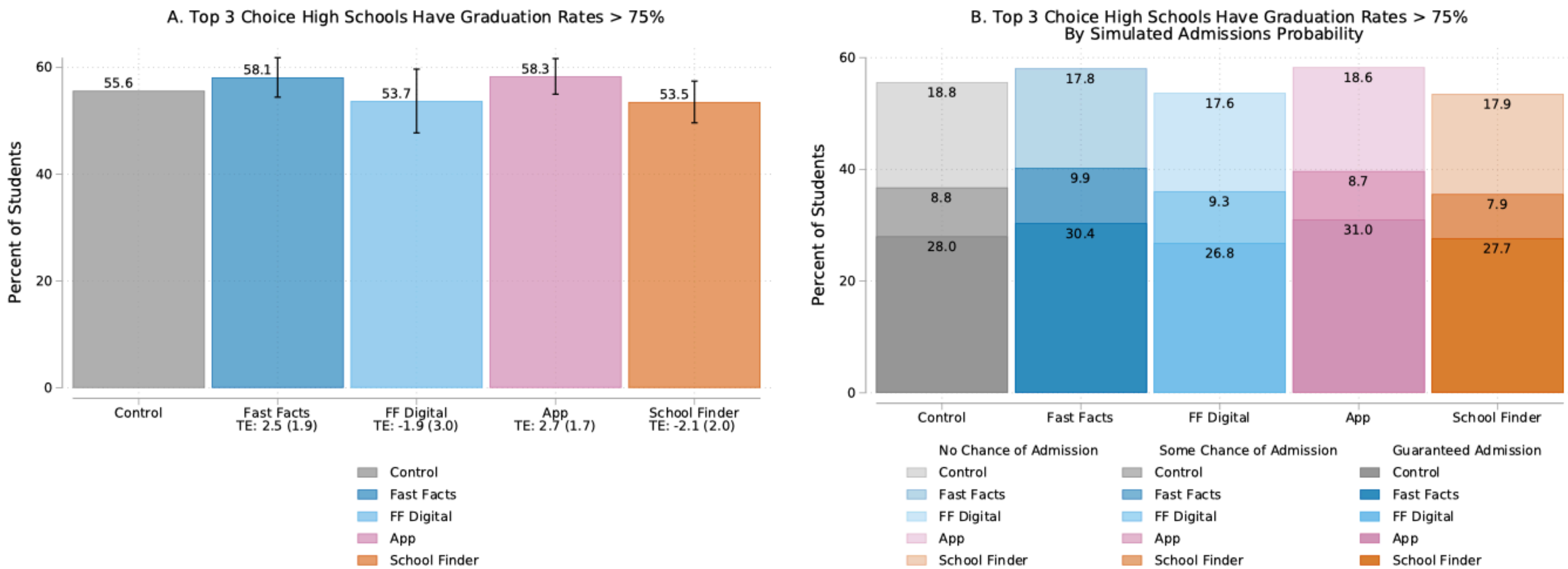
Notes: This figure shows reported use of the intervention tools and materials. Panels A-C are for the group of school counselors that responded to the study team's survey and/or interview request. Use is counted if any school staff at a given school reported use. Response rates to the survey/interview are in Panel D. Control group schools are only included in panels where the control group had an opportunity to participate. Panels E and F for the group of school counselors that responded to the study team's survey and/or interview request or who responded to a call from the study team to confirm receipt of the intervention materials. Use is counted if any school staff at a given school reported use, and in the case for calls from the study team, use is counted for reported use or "plans" to use. Survey/interview supersedes the call since they occurred after completion of the intervention and the follow-up call could include intentions. Response rates are in Panel G. Panel H shows an indicator for a school have 5 or more unique visits to the FF Digital website.

Figure 3. Summary of Impacts on Key Outcomes



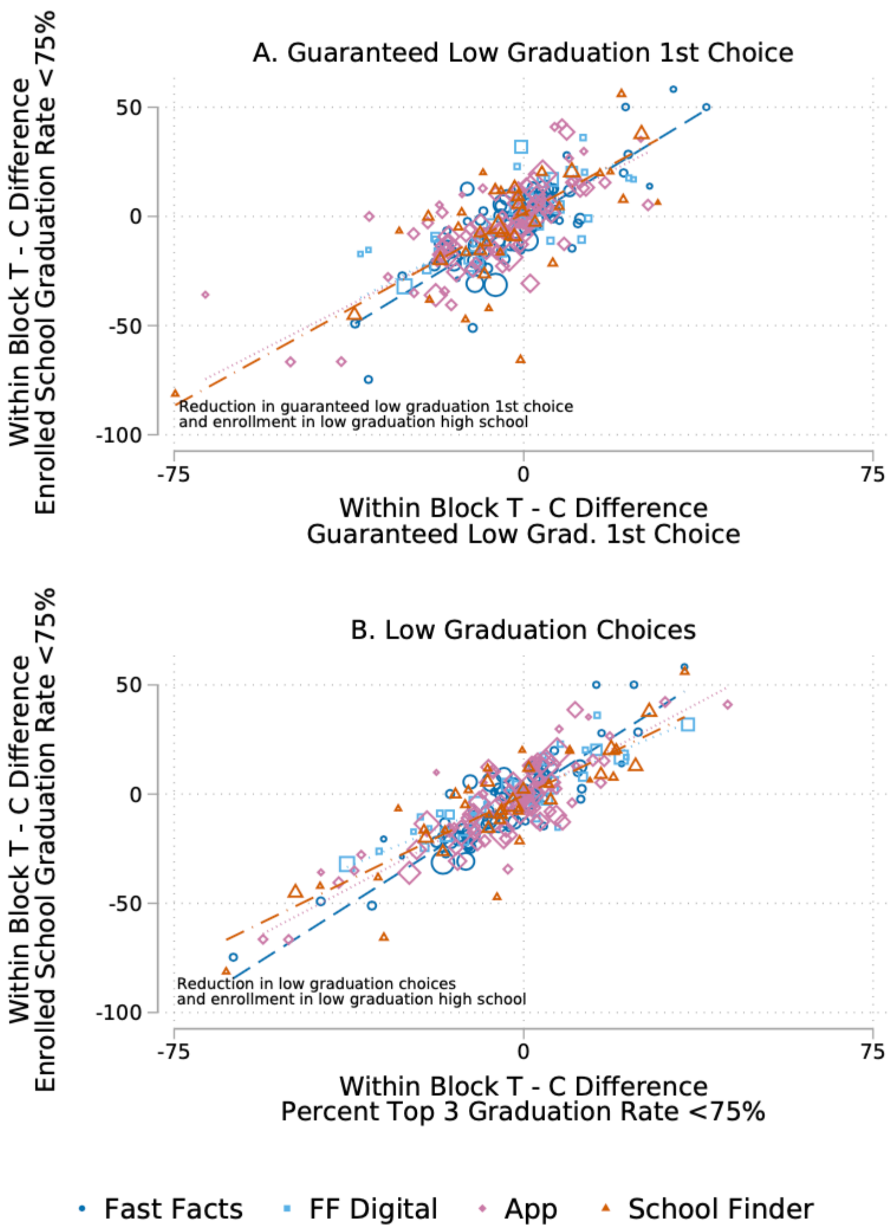
Notes: This figure show treatment-control contrasts for thee key outcomes: the percent of top 3 choices on the high school choice application with graduation rates below 75 percent; the likelihood of having a first choice school with guaranteed admission and a low graduation rate ("guaranteed low graduation"); and the likelihood of enrolling in a high school with a graduation rate below 75 percent. The treatment effect is reported beneath each bar as "TE."

Figure 4. Impact on Admissions Strategy



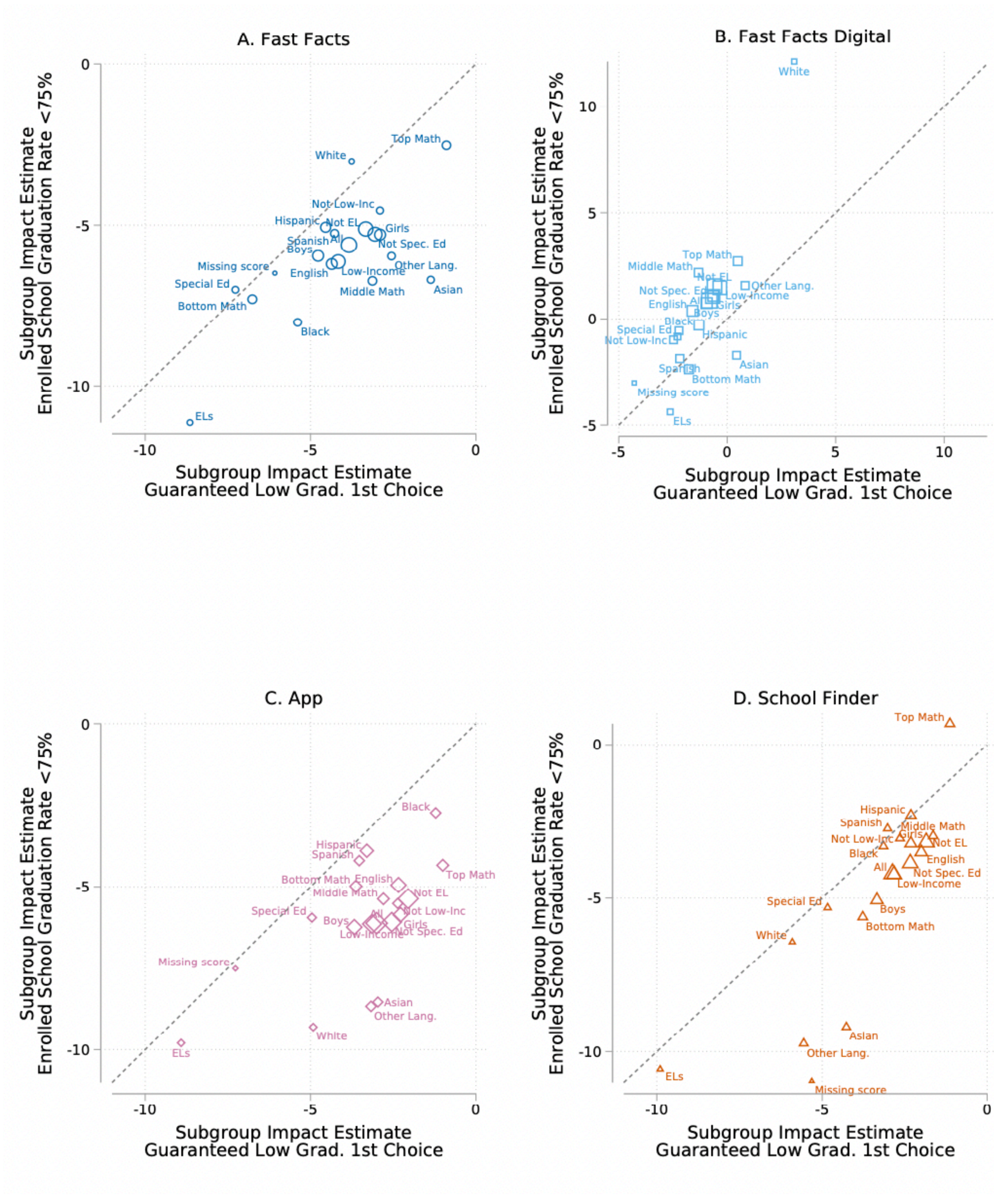
Notes: This figure show treatment-control contrasts for an indicator that all three top application choices have a graduation rate above 75 percent (Panel A) and the same indicator split by the simulated admissions probabilities of being offered admission to at least one of the three choices. The treatment effect is reported beneath each bar as "TE" in Panel A. In Panel B, no simulated admissions probability is the lightest shading, some admissions probability is the medium shading, and guaranteed admissions probability is the darkest shading.

Figure 5. Within Block Treatment-Control Differences for Key Outcomes



Notes: This figure plots within block regression adjusted treatment control comparisons of impacts on enrolled school graduation rates versus guaranteed low graduation first choice school (Panels A) or the percent of low graduation top three choices (Panel B), weighted by number of observations. The estimates are generated using the same estimation strategy as for the main estimates, limited to a sole treatment and comparison school within a single block. At least 20 students must be present in the treatment and control groups for the comparison to be included in the plots.

Figure 6. Subgroup Impacts on Low Graduation First Choice vs. Enrollment in Low Graduation High Schools



Notes: This figure plots subgroup-specific impacts on low graduation enrolled school rates versus subgroup-specific impacts on guaranteed low graduation first choice school, weighted by number of observations in each subgroup. The estimates are generated using the same estimation strategy as for the main estimates, limited to subgroup members. Note that panels are on different scales. The dashed line is a 45 degree reference line. Precise estimates and standard errors are available in Appendix E.

Table 1. Summary of Differences across Intervention Years

Channel (1)	Scale-Up Study 2015-16 (2)	At-Scale Study 2016-17 and 2017-18 (3)
Study staff vs. school counselors	Study staff	School counselors
Paper vs. digital	Paper	Both
School-specific vs. person-specific	School-specific	Both
Recommended vs. general	Recommended	Both
Tool only vs. suite of supports	Tool only	Suite of supports

Notes: This table summarizes the major differences in intervention format across years of the intervention. The scale-up study is the focus of Corcoran et al. (2018) and the at-scale study is the focus of this paper.

Table 2. Summary of Differences within the At-Scale Intervention

Channel (1)	Fast Facts Paper (2)	Fast Facts Digital (3)	App (4)	School Finder (5)
Study staff vs. school counselors	School counselors (all)			
Paper vs. digital	Both	Digital	Digital	Digital
School-specific vs. person-specific	School-specific	School-specific	Person-specific	Person-specific
Recommended vs. general	Recommended	Recommended	Recommended	General
Tool only vs. suite of supports	Suite of supports (all)			

Notes: This table summarizes the major differences in potential channels within the at-scale interventions (school years: 2016-17 and 2017-18)

Table 3. Student characteristics and outcomes

	All schools (1)	Study schools (2)	Tier 1 (3)	Tier 2 (4)
(A) Student Characteristics				
Female	0.494	0.487	0.483	0.489
Asian	0.168	0.173	0.101	0.206
Black	0.250	0.270	0.270	0.269
Hispanic/Latino	0.400	0.432	0.552	0.377
Other race	0.025	0.017	0.013	0.019
White	0.156	0.106	0.062	0.126
Students with disabilities	0.193	0.192	0.220	0.179
EL	0.115	0.131	0.167	0.115
Low income	0.723	0.782	0.826	0.762
7th math score	0.019	-0.091	-0.327	0.018
7th ELA score	0.021	-0.099	-0.309	-0.003
(B) Outcomes				
% 1st choices from FF	54.0	55.4	47.0	59.3
% 1st-3rd choices from FF	51.3	52.4	44.1	56.2
% all choices from FF	46.4	47.1	39.1	50.8
Graduation rate, 1st-3rd choices	85.4	84.6	83.1	85.4
Graduation rate, matched school	80.1	78.9	76.7	80.0
Graduation rate, enrolled school	80.3	79.1	76.7	80.2
Grad. rate <70%, 1st-3rd choices	12.2	13.5	17.2	11.7
Grad. rate <70%, matched school	23.7	25.9	31.4	23.3
Grad. rate <70%, enrolled school	23.4	25.7	31.6	23.0
Grad. rate <75%, 1st-3rd choices	20.6	22.6	27.3	20.4
Grad. rate <75%, matched school	36.7	40.0	46.5	37.1
Grad. rate <75% enrolled school	36.3	39.6	46.4	36.5
N	154,238	115,126	36,384	78,742

Notes: This table reports means of baseline student-level characteristics for each group listed in the column heading. Tier 1 indicates middle schools that participated in the 2015-2016 experiment; Tier 2 middle schools new to the experiment in 2016-2017. The sample includes all students present in October of their 8th grade years in the 2016-2017 and 2017-2018 school years who attended randomization sample schools.

Table 4. Impact of Informational Interventions on Choices, Matched School, and Enrolled School

	Fast Facts (1)	FF Digital (2)	App (3)	School Finder (4)	Control Mean (5)	N (6)
(A) Average Graduation Rate of Choices						
1^{st} Choice	0.788+	0.050	0.519	0.046	86.8	109,733
	(0.440)	(0.598)	(0.404)	(0.488)	[12.2]	
1^{st}-3^{rd} Choices	0.837*	0.391	0.521	0.172	85.5	114,696
	(0.419)	(0.578)	(0.383)	(0.458)	[10.2]	
All Choices	0.692	0.328	0.492	0.285	83.9	114,791
	(0.450)	(0.623)	(0.398)	(0.476)	[8.8]	
Range of Grad. Rates	-0.755	-0.208	-0.719	-1.010	22.9	114,791
	(0.707)	(0.970)	(0.638)	(0.766)	[13.9]	
(B) % of 1^{st}-3^{rd} Choices						
Grad Rate <70%	-2.856***	-1.593	-1.745*	-1.740	12.3	114,696
	(0.850)	(1.159)	(0.885)	(1.185)	[25.5]	
Grad Rate <75%	-3.100*	-1.230	-2.640*	-1.491	21.1	114,696
	(1.358)	(2.468)	(1.272)	(1.595)	[32.6]	
High Grad + Chance Admission	3.523*	-0.697	2.936*	-1.172	36.8	115,126
	(1.584)	(2.050)	(1.493)	(1.575)	[48.2]	
High Grad No Chance Admission	-1.023	-1.220	-0.250	-0.929	18.8	115,126
	(1.087)	(1.564)	(1.118)	(1.217)	[39.1]	
(C) 1^{st} Choice Application Strategy						
% 1^{st} choices from FF	-3.304**	-1.809	-3.205**	-2.451+	14.4	109,733
	(1.010)	(1.602)	(0.988)	(1.275)	[35.1]	
(D) Matched School						
Graduation Rate	1.440**	0.534	1.047*	1.004*	79.9	106,628
	(0.452)	(0.566)	(0.410)	(0.505)	[13.7]	
Grad Rate <70%	-4.148**	-0.205	-2.924*	-4.461*	24.4	106,628
	(1.421)	(1.823)	(1.405)	(1.816)	[42.9]	
Grad Rate <75%	-5.815**	-1.095	-5.476**	-4.461*	39.1	106,628
	(1.933)	(2.677)	(1.722)	(2.123)	[48.8]	
(E) Enrolled School						
Graduation Rate	1.514**	0.574	1.157**	1.118*	80.0	98,455
	(0.466)	(0.590)	(0.432)	(0.523)	[13.7]	
Grad Rate <70%	-4.114***	-0.299	-3.388*	-5.148*	24.3	98,455
	(1.462)	(1.905)	(1.481)	(1.924)	[42.9]	
Grad Rate <75%	-6.110**	-1.459	-6.146***	-5.106*	38.9	98,455
	(1.962)	(2.780)	(1.767)	(2.218)	[48.8]	

Notes: This table reports regression coefficients representing assignment to an informational intervention middle school on the graduation rates of choices, matched and enrolled schools. All regressions include controls for the variables listed in Table 1, as well as for randomization block by year fixed effects. The estimation sample includes all students present in October of their 8th grade years in the 2016-2017 and 2017-2018 school years who attended randomization sample schools and participated in the Round 1 high school choice process. Robust standard errors clustered by middle school are in parentheses (+ p<.10 * p<.05 ** p<.01).

[1] We estimate admissions probability by simulating the school choice lottery, see Section 5 for details on these simulations. A student has guaranteed admission at a high school if in all simulations they match to that high school.
[2] Fast Facts and School Finder were available in both English and Spanish. In the first year of the intervention, the App was only available in English, but we provided supportive materials about App access in English and Spanish. It was available in Spanish in the second year.
[3] To be precise, students apply to high school programs, not schools themselves, as schools can host multiple programs (for example, one academically selective program and one not).
[4] We do observe application behavior that may indicate a misunderstanding of the algorithm. For example, students listing guaranteed schools before other, potentially more preferred, selective schools. In the present study, 2.4 percent of students list an unscreened or zoned school as their first choice, and a more selective school as their second choice. If this is their true preference, then it is not a mistake. But in the vast majority of these cases, students' applications to the second school will never be considered, since they will match to the non-selective school first. Additionally, more choices do not necessarily lead to better choices (Schwartz, 2004).
[5] Our interventions took place prior to the COVID-19 pandemic, which changed both the requirements of many high schools as well as the process for considering schools. It remains to be determined which of these policy changes will persist beyond that time period.
[6] There is a second opportunity for a match in 10th grade, where a similar process takes place for open seats, though there are few open seats at this point in time, making it difficult to make a change after initial assignment in 9th grade.
[7] There are also a number of informational interventions around school choice and informing families about the returns to education outside the U.S., including Jensen (2010), Mizala and Urquiola (2013), Nguyen (2013), Bobba and Frisancho (6 11), Andrabi et al. (2017), Neilson et al. (2019), Ainsworth et al. (2020), and Ajayi et al. (2020).
[8] More details on differences between the two intervention years are described below and in Online Appendix A.
[9] We defined success at past matching if one student in that middle school had successfully applied to and matched to a high school in the past six years. The goal of focusing on high schools where there was a history of past match was to highlight higher graduation rate schools that students still had a chance of getting into and to avoid schools where students had no or very low chance of getting in, due to high selectivity or geographic priorities.
[10] We included a special provision to allow new, nearby schools on the list.
[11] A second cross-randomization generated two additional versions of Fast Facts, where the final two schools on the Fast Facts list (those with the relatively lowest graduation rates) were omitted and replaced with two additional schools, with text that discourage application to these schools. The additional schools in one of these treatment arms made salient the fact that some schools have very low admissions rates (the Fast Facts "low odds" treatment). A second supplemental school treatment arm discouraged application to low graduation rate high schools (the Fast Facts "low graduation" treatment). See Online Appendix Section A.2.2 for more details on the selection of these supplemental schools. In this paper, we do not distinguish between Fast Facts types in order to focus on the larger scale variations across treatment arms. In later work, we will report on Fast Facts differences in more detail.
[12] To account for adjustment to the U.S., we used a longer time horizon for high school graduation rates in this supplement.
[13] Since all schools in the second year of the intervention had access to a printable version of Fast Facts, we count them as being assigned to Fast Facts paper.
[14] The NYCDOE has increasingly moved to digital resources. While during the time of our interventions they supplied a printed high school directory to each student, in 2019 they have shifted to an abridged guide, primarily relying on the School Finder tool (now called "MySchools"), which was updated to be embedded in the online application portal, to substitute for the directory.
[15] We also conducted a "pilot" study in 2014-15.
[16] We thank the Research Alliance for New York City Schools, which provided access to deidentified student-level information with the agreement of the NYCDOE.
[17] We thank Jon Valant for sharing computer code that facilitated our calculation of the deferred acceptance lottery.
[18] Online Appendix Tables D.7 and D.8 reproduce the main estimates excluding these Tier 1 schools, and, as predicted, the main difference between results with Tier 2 schools only is slightly less precision. It is not possible to estimate treatment effects on Tier 1 only, as there are no control schools in this group.
[19] Online Appendix Tables D.4 shows impact estimates on key outcomes for each cohort separately, as well as for various definitions of treatment assignment, revealing few differences.
[20] To make sure this is the case, we include a robustness check where randomization blocks with any closed schools are excluded. Results remain very similar, see Online Appendix Tables D.7 and D.8.

[21] See Online Appendix Section A.1.2 for details on the four cases where treatments deviated from the assigned status. It is not possible to estimate an effect for cases where schools closed or consolidated, but these occasions are orthogonal to treatment assignment. In one case, a new control school was randomly drawn from non-participating schools.

[22] Both formal interviews and informal calls to school counselors to check on the delivery status of the materials indicated that a handful of counselors did not use the materials because they already had their own system and materials for high school admissions, and that some did not use the materials because they had already done most of their related programming. Among the schools that the research team was able to assess the level of materials use in 2016-17, 85 percent reported using the intervention materials.

[23] If multiple school staff participated in the survey or interviews, we considered that the tool and/or materials were shared if any of the personnel reported distributing.

[24] Impacts on measures of application probability alone, not combined with graduation rates, are in Online Appendix Table D.1. Generally, assignment to Fast Facts paper or the App seem to in increase application to "some probability" schools and reduce application to "no probability" schools.

[25] While application to these schools does not affect admission to lower ranked schools with the deferred acceptance algorithm, if students and their families have limited space in their mental accounts for the school choice process, they may in practice reduce the number of viable schools a student applies to.

[26] Information and process supports alone cannot ensure every student enrolls in a high school they will be successful in. Ajayi et al. (2020) find this is the case in a school choice system in Ghana. Despite inducing students to apply and be admitted to higher-quality schools, informational interventions did not increase enrollment in such schools.

[27] Note that sample sizes decrease from choice outcomes, to match outcomes, to enrolled school outcomes. This is not due to student dropout, but to students matching to and enrolling in new(er) schools which do not yet have a graduation rate. Students choose these schools less frequently. We display various imputed graduation rates in Online Appendix Table D.3, and our conclusions remain the same with and without imputed graduation rates for newer schools.

[28] The variation in impact at different graduation rate thresholds invites the question of where in the distribution of high school graduation rates each intervention makes the greatest difference. This is shown in Online Appendix Figure D.3, which plots the impact estimate at each potential graduation threshold. The patterns in this Figure correspond to our understanding of how each of the tools functioned: Fast Facts and the App explicitly did not list schools underneath the 75 percent threshold and thus the decline in enrolled school graduation rate focused at 75 percent implies that students directly engaged with listed schools. There was low interaction with Fast Facts Digital and there are few impacts anywhere in the distribution. School Finder did not target specific graduation rates but our materials may have encouraged students to research schools more deeply and shift away from relatively low graduation rate schools.

[29] Online Appendix Figures E.1 through E.4 summarize the treatment effects for student subgroups. Results including additional outcomes and standard errors are in tables in Online Appendix E.

[30] This is in contrast to our prior year's intervention (Corcoran et al., 2018), which showed larger response from higher scoring students, perhaps due to the context which focused on the highest-poverty schools. The interventions studied here were primarily fielded in medium-high and medium-poverty schools where it is possible high-scoring students already had access to high school application supports.

[31] The most common non-English, non-Spanish language in NYC is Chinese (including Mandarin and Cantonese) followed by Russian, French Creole, and Bengali (see https://www1.nyc.gov/assets/planning/download/pdf/data-maps/nyc-population/acs/top_lang_2015pums5yr_nyc.pdf for details). We had supportive materials in non- Spanish languages, but were only able to provide Spanish translation for Fast Facts, and later, the App, meaning that other language groups may not have had the same access.

[32] The DOE does provide translated materials, but counselors complain they are difficult to access.