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WHEN DO INFORMATIONAL INTERVENTIONS WORK? EXPERIMENTAL EVIDENCE FROM NEW YORK CITY HIGH SCHOOL CHOICE

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ABSTRACT

This paper reports the results of a large, school-level randomized controlled trial evaluating a set of three informational interventions for young people choosing high schools in 473 middle schools, serving over 115,000 8th graders. The interventions differed in their level of customization to the student and their mode of delivery (paper or online); all treated schools received identical materials to scaffold the decision-making process. Every intervention reduced likelihood of application to and enrollment in schools with graduation rates below the city median (75 percent). An important channel is their effect on reducing nonoptimal first choice application strategies. Providing a simplified, middle-school specific list of relatively high graduation rate schools had the largest impacts, causing students to enroll in high schools with 1.5-percentage point higher graduation rates. Providing the same information online, however, did not alter students' choices or enrollment. This appears to be due to low utilization. Online interventions with individual customization, including a recommendation tool and search engine, induced students to enroll in high schools with 1-percentage point higher graduation rates, but with more variance in impact. Together, these results show that successful informational interventions must generate engagement with the material, and this is possible through multiple channels.

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A data appendix is available at http://www.nber.org/data-appendix/w29690

1 Introduction

In the last decade, social scientists have implemented interventions to assist individuals in navigating decision-making processes in education, health care, and retirement savings. Studies motivating these interventions concluded that people do not always appreciate the impact of seemingly trivial decisions for their long-term well-being (Thaler and Sunstein, 2003, 2008). The recognition that choice architecture affects the decisions we make independent of *a priori* stated preferences (Tversky and Kahneman, 1985; Tversky and Shafir, 1992; Kahneman and Tversky, 2013; Johnson et al., 2013) launched many "low-touch" informational and decision support interventions. Scholars' enthusiasm about the effect sizes of these early low-cost interventions, which initially targeted discrete, one-time transactions, has evolved into an appreciation that the field needs a more nuanced understanding of under what conditions these interventions work: the types of decisions for which these interventions are most effective, the relevance of intervention modality (how the intervention is delivered), and heterogeneity in participants' responses to these interventions (Saez, 2009; Mrkva et al., 2021; Oreopoulos, 2021).

We contribute to this body of literature by designing and implementing multiple informational interventions in a public education context in which all students must apply to a high school, and no default options are available. Navigating the high school choice process in New York City can be a daunting task for 13-year-olds. Despite an application system that is ostensibly in the hands of parents and guardians, according to prior research (Sattin-Bajaj, 2014) and our surveys and interviews (Sattin-Bajaj et al., 2018), many 8th-graders select high schools to apply to with limited parental input. School assignment then occurs through a difficult-to-understand deferred acceptance algorithm that takes into account students' choices and priority groups as well as schools' requirements (Abdulkadiroğlu et al., 2005). The outcomes of this process 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; Abdulkadiroglu et al., 2017), but access to high-quality high schools is not evenly distributed. In NYC, low-scoring and low-income students apply to, and subsequently enroll in schools, with lower graduation rates (Nathanson et al., 2013; Corcoran et al., 2018). Successfully navigating high school admissions in NYC thus is a consequential turning point in the lives of young New Yorkers, and given disparities in the support and resources students have, there is room for intervention. Providing salient information and introducing technology to increase access to information are strategies to help students and their families manage this complicated process.

To investigate the role of information and technology in school choice, 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 improve school choice outcomes but also *what circumstances* drive successful intervention and whether such interventions benefit all types of students similarly. Middle schools were randomly assigned to one of three interventions or a control group. The interventions included a middle school-specific list of recommended high schools selected for having graduation rates above 75 percent (the NYC median graduation rate in 2015) and some 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 input (the "App"); and an online high school search tool ("School Finder"). 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 guidance counselor, received the intervention tools to distribute along with supplementary materials (lesson plans, video guides, and support from the study office). In contrast to 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 method of dissemination approximates how a school district might use these tools in practice.

We found that the interventions 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 — as the first choice (which we term "nonoptimal first choice strategy") and reducing the likelihood that students applied to any low graduation rate schools. Consequently, all of the interventions succeeded at shifting students out of matching to and enrolling in low graduation rate high schools.

A related intervention by this research team in the previous school year, 2015-2016, sets the stage for our interventions (Corcoran et al., 2018). The prior intervention focused on 165 high-poverty middle schools that were randomly assigned either to receive a visit from trained research staff who provided a list of recommended high schools as well as a lesson on how to use it or to a control group (Corcoran et al., 2018). The interventions were designed to lower the rate of application and match to high schools with low graduation rates (less than 70%). The intervention used an earlier version of Fast Facts, which — at that point in time — was a concise, one-page listing of 30 high schools within a 45-minute commute from that middle school's address along with travel time information and the four-year graduation rate, restricted to high schools with a graduation rate of 70% or above. The sheet was intended to serve as a starting point for high school choosers and prompt their independent research to identify high schools that match their interests and preferences. Students in schools that received the treatments 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, 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 and delivery of the materials via school

[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.

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 schools that consented to be randomized to an intervention, this iteration included schools regardless of whether they expressed interest[2] — 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. However, we note that reported use does not necessarily line up with data on actual take-up of the materials. Using records of internet views, we show that interaction with the Fast Facts Digital tool is about half that of reported use. We then show that assignment to tools (except for Fast Facts Digital, where we know use is low) changes students' application behavior. In the control group, 14 percent of students use a nonoptimal first choice strategy: they list a low graduation rate school that they are guaranteed to be admitted to as their first choice. This means that they have no chance to attend higher graduation rate schools, even if they list them on their application, because the guaranteed probability school ensures a match. The successful interventions reduce this rate by 2.5 to 3.3 percentage points. They also decrease the percentage of high schools that students apply to with graduation rates below 75 percent by 1.5 to 3.1 percentage points. Students substitute higher graduation rate schools on their applications, and, importantly, students assigned to the Fast Facts paper and the App treatments shift to schools that are not just higher graduation rate but also have a higher probability of admission.

These changes in application behavior result in changes in matched and enrolled school, making it less likely that students in successful interventions (Fast Facts paper, the App, and School Finder) match to and enroll in low graduation rate high schools. In the control group, 39 percent of students match to and enroll in schools with low graduation rates.[3] 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. The Fast Facts paper intervention is most effective at shifting the entire distribution of graduation rates, with students matching to high schools that have, on average, graduation rates about 1.5 percentage points higher than the average graduation rate in the control group. However, the digital only version of Fast Facts made little difference in student outcomes. In the cases where the interventions are successful at shifting students into higher graduation rate schools, we find no evidence of so-called mismatch (Arcidiacono and Lovenheim, 2016) in that

[2]Consent was necessary in the first year of the intervention since the study team visited schools. In the second and third years, we solely provided materials, which did not need a site consent process. Voluntary surveys and interviews included a consent process.

[3]Note that low graduation rate high schools account for 50% of high schools, but enrollment in them is under than 50% of students. This is because more students get assigned to these schools administratively after the choice process ends. There is also some slack in the system, especially in co-located schools where classrooms are moved between schools based on enrollment.

students' subsequent high school performance (to date) is similar to that of students in control group schools.

Using our information on tool use, we show descriptive evidence on plausible pathways for the interventions to work, documenting that schools that report greater use of the tools show application behavior that corresponds to the particular intervention they received. These schools also show greater response in terms of reduction in enrollment in low graduation rates schools. We take this as evidence that the tools themselves are a key component of intervention success but also show some evidence for the role of "priming" (activating guidance counselors to participate more in high school admissions) and the supportive materials that we provided alongside the tools.

With evidence from subgroup response, 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 high school enrollment response. We also note that English learners – 12% of 8th graders in the district – had the strongest response to all of the interventions.[4] This highlights the need for salient, accessible school choice materials. We ultimately conclude that the specific design of the intervention is less important than engagement with an intervention, but that the design of materials can led to differences in *who* engages, with consequences for (in)equality.

This paper makes three main contributions. First, we contribute to the growing literature on whether information can influence K-12 school choice in the United States. We are aware of four other experimental informational interventions on this question (Hastings and Weinstein, 2008; Valant, 2014; Weixler et al., 2020; Arteaga et al., 2021) as well as our earlier study in NYC (Corcoran et al., 2018). As more and more school districts adopt universal enrollment systems, adding to the list of districts that include Boston, Denver, New Orleans, and NYC, where all students must participate in a school choice process for school assignment, understanding access to information and how information is used in school choice processes will help maximize the success of such systems. The possibility of embedding informational interventions directly in school choice platforms (as in Arteaga et al. (2021)) may become the norm. Even in U.S. cities without universal enrollment systems, school choice is on the rise, with a growing national charter school sector (Irwin et al., 2021) and increased interest in homeschooling and digital options due to the COVID-19 pandemic. We also highlight that information targeted to students — rather than their parents, as in all of the other interventions we are aware of – can drive choices and eventually school match and enrollment. Our interventions are also the only ones we are aware of to target actors within the school system – school counselors – thus demonstrating the potential for within system changes to guidance and curriculum to influence school choice behaviors.

Second, we contribute to the understanding of *how* and *why* informational interventions work, which is relevant both to the K-12 school choice context, and others. Contrasting treatment years

[4] 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.

and treatment arms shows that there are multiple ways information can reach students and their families. The two largest changes in interventions across study waves were the delivery mechanism (research team staff vs. school counselors) and process scaffolding (tool only vs. suite of supports), which both changed across the study years. Both waves of the study reduced enrollment in low graduation rate schools by similar amounts, suggesting these interventions can be successful in multiple forms, though we note the study schools differed across intervention waves. Disseminating materials through school staff is cheaper and closer to school district practice than interventions conducted by external operators.

We consider three hypotheses for how the interventions generate response. The "tools" hypothesis posits that interaction with the specific high schools highlighted by an intervention generate the response. The "supportive materials" hypothesis suggests that it is use of the supportive materials that changes student outcomes. Finally, the "priming" hypothesis suggests that it is not the tools or materials themselves, but that the interventions induce school counselors to engage more in high school admissions, driving the response in student outcomes. Of course, it is likely the results were produced by a combination of the above channels, and we find evidence of a role for each. We cannot distinguish between student and counselor interaction with the tools and materials, nor do we have a test of information provision with no supportive materials in this study wave.

Within this study, we contrast delivery mode (paper vs. digital interventions), type of information (recommended vs. general), and customization (school-specific vs. person-specific). Fast Facts was provided in paper and digital forms, and the App and School Finder were digital only interventions. Fast Facts and the App recommended schools using a rule that privileged geographically proximate schools and excluded low-graduation rate schools, while School Finder included all district schools. Fast Facts provided middle school-specific school lists whereas the App and School Finder generated person-specific lists (based on student input). Since we found similar overall response to all of the interventions except for Fast Facts Digital, it appears that neither of these design choices were clearly preferable to the alternative. Tailoring the choices available in the intervention to highlight high schools with higher graduation rates pushes students towards such schools, but the difference from a more general intervention that does not limit recommendations in this way is not large.

However, generating engagement with the informational tool and materials is key. We show that engagement can be generated through easy-to-access, salient paper artifacts or through interactive digital tools. In both cases, curation through supportive materials is an important accompaniment to physicality or personalization, and suggestive evidence indicates such materials may account for as much as half of the impacts we observe. We thus conclude that both paper and digital interventions can be successful, but context and presentation matter. Static digital resources like Fast Facts Digital risk going unused due to lack of take-up. Paper resources like the official high school directory can be overwhelming and unguided. Thus providing information alone is not sufficient to ensure recipients engage with the material, at least when those recipients are adolescents.

Finally, our third contribution is to note that design can affect whether the intervention reduces or exacerbates inequality in school choice outcomes. All of the interventions show particular benefits for English learners, demonstrating that there are underserved students in the current system and providing targeted resources to them could increase equality in an unequal system.[5] Both Fast Facts paper and School Finder generated larger benefits for low-scoring students and those without prior test scores (students new to the system). However, the App treatment had larger responses for white students and students not categorized as low-income, underscoring that when tools are less accessible (as a digital app can be) they may exacerbate inequality.

Regardless of the pathway or the group that responded, we show that many of our interventions nudge students into higher graduation rate schools than they would have attended otherwise. Evidence, so far, shows that these students are not worse off, having been pushed into a higher-performing school. 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, including 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 and discussed in Section 6. We conclude in Section 7.

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.[6] 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

[5] Note that English learners here describes students receiving services to support learning English in the 8th grade, which occurs for 12% of students in NYC. White and Black students are less likely to be English learners (7 and 3% of those populations respectively); Hispanic/Latino and Asian students are more likely to be (18 and 14% of these groups). NYC is a very linguistically diverse city, and 42% of students speak a language other than English at home, but most of these students are not (or are no longer) considered English learners. Additionally, 18% of 8th graders in NYC immigrated to the United States. The majority (59%) of these students do not receive language support services in 8th grade, but immigrant students make up 65% of the English learner group.

[6] 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).

their true preferences.[7]

In the spring of 7th grade and the fall of 8th grade, guidance 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.[8] NYC has an extensive variety of high school programs including large comprehensive high schools, small themed schools that focus on a particular profession, and academically screened schools, and applicants have many choices at which to consider their potential fit. 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 in to 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 selective 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.[9]

As a whole, the school choice process in NYC carries a large "administrative burden" (Moynihan et al., 2014). Our interviews with more than 450 students, parents, and school 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 also believe it is not appropriate to give action-guiding advice on

[7] Of course, it is still possible to make "mistakes" when listing high schools. For example, listing a guaranteed school before other, more preferred, selective schools. In our study years, 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. This may be due to confusion about how the algorithm works. The difficulty of considering large number of school choices may lead to mistakes as well, given psychology evidence that having more choices does not necessarily lead to better choices (Schwartz, 2004).

[8] 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.

[9] 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.

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, which in reality is more likely to lead to a failure to match in the main round; 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, as well as 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

The findings from our multiple studies in NYC add to several papers that show that relatively "low-touch" informational interventions can influence families' K-12 school choices in the United States.[10] 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 after being shown the guides. 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

[10] 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).

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,[11] but that impacts were concentrated among high school entrants and students with disabilities. All of these studies 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, 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 also 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 in the context of a hypothetical choice for their children. Arteaga et al. (2021) embed interventions directly in school choice platforms in Chile and New Haven, Connecticut. 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 eventually be more likely to match to a selected school. This intervention targeted application “strategy,” with the goal of increasing match not necessarily school quality.

Beyond K-12 school choice, informational interventions can support decision-making in many other contexts, both within and outside of education. 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). In these interventions in the higher education sector, and in other realms such as insurance and benefit claiming (Abaluck and Gruber, 2016; Johnson et al., 2013; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019), simplified, salient information often reduces the frictions inherent in decision making and leads to better outcomes for students and other choice-makers.

However, Gurantz et al. (2021), in a replication of a successful experiment to increase college application and matriculation for high-performing, low-income students (Hoxby and Turner, 2013), did not have the same success as the original intervention. A similar 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 a letter with information about tax benefits

[11] They do not report enrollment in school.

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. Given the cost of college, prices, and knowledge of them, may be particularly relevant in higher education.[12] Additionally, 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 access to schools come from the process itself.

2.3 Interventions

To help students navigate the complicated high school choice process described above, we fielded three main 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 intervention tools are described in detail below. The interventions were mailed to middle school personnel, typically a school counselor, who shepherded 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 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 D for reproductions of intervention and support materials.

In September 2016, the study team called and emailed all guidance 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 language printouts to provide. Shipping of the materials took place in early October. This was followed by calls to schools counselors to ensure that the box had arrived and 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—which ranged from no use, to distributing the postcards or school lists with

[12] While there are not monetary costs in K-12 school choice (outside of private schools), students and families must weigh cost like travel time in making their decisions.

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 schools counselors, as well as conducted interviews with a subset of counselors. In the second year of the intervention, counselors were supplied with materials updated with more recent data to serve an additional cohort of students in early Fall 2017 (with initial materials supplied in Spring 2017 to support 7th graders), with most of the same experimental structure intact.[13]

All treated schools received the materials and contact with the study office 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.

1. *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%, along with travel time information, the school's graduation rate, and information about how to apply. Schools were selected if they were nearby, had a high graduation rate, and if past students at that middle school had a history of placing at the high school.[14] Fast Facts lists focused on high schools that counselors and students were likely to be familiar[15] with and omitted schools that had low graduation rates or very low odds of admission.

   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"). 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 C.[16]

[13] More details on differences between the two intervention years are described below and in Online Appendix A.

[14] 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. See Online Appendix Section A.2.1 for details on history of placement. 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.

[15] We included a special provision to allow new, nearby schools on the list, see Online Appendix Section A.2.1 for details.

[16] 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 arm 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 within Fast Facts differences in more detail.

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 as a second language, with six-year high school graduation rates above the city median.[17] All materials in the Fast Facts treatments were available in both English and Spanish.

In the second year of randomization, 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 guidance counselors could print at their schools to share with students.[18] Schools continued to receive the suite of supportive materials, including lesson plans and resources on how to use the tool. 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 interventions), we generated a Fast Facts list for every school in the study, regardless of their assignment status.

2. *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 guidance 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 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, and guidance counselors received the suite of supportive materials.

3. *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

[17] To account for adjustment to the U.S., we used a longer time horizon for high school graduation rates in this supplement.

[18] 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.

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.[19] 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, with both access to the App and App-specific updated supportive materials.

4. *Control condition*: The 58 schools in the control group did not receive access to any materials designed especially 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. Control group schools remained in the control group in the second year of randomization.

As differences between this study and the prior version of the intervention fielded by our team and reported on in Corcoran et al. (2018) may help explain differences in outcomes, we highlight the major changes in our approach. First, this sample is larger and includes both high- and somewhat lower-poverty schools. Specifically, the average poverty rate at middle schools in the first year of the

[19]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.

intervention was 88%; it was 82% in years we focus on here. Second, the delivery of the intervention occurred via the guidance counselor, rather than a trained research team member. This meant that implementation of interventions varied across sites, and that guidance 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 may come down from on high, 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). Guidance 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, these interventions test differences between digital availability and having a paper artifact, as well as introducing interventions that are only accessible online. Technology allows for greater personalization, but it may also be a barrier to access. Finally, there were several changes in the construction of Fast Facts sheets. We only offered a one-page sheet (the prior intervention included supplements that highlighted limited unscreened schools and school themes), reduced the number of schools slightly (from 30 to 26), raised the graduation rate floor to 75% 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 the "at-scale" study.[20] We also highlight the within study differences in Table 2.

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.[21] 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. For instance, by matching a chosen school to the publicly available information on its graduation rate, we can identify whether the chosen school has a graduation rate above or below 75 percent.

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

[20] We also conducted a "pilot" study in 2014-15.

[21] We thank the Research Alliance for New York City Schools, which provided access to deidentified student-level information with the agreement of the NYCDOE.

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 information is included in Panel A of Table 3, which describes the sample of students in the experiment. 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 their 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.[22] 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 schools that closed or consolidated that summer resulted in 592 middle schools as potential participants. Excluding middle schools that primarily enrolled their 8th graders in the same school for 9th grade (e.g. schools serving grades 6-12), as well as relatively low-poverty schools (those with a student body with less than 50 percent low-income students), resulted in 473 schools. These 473 schools then formed our randomization group. 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 guidance counselors remained in the same school across years.

Given our past relationships with guidance 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.[23] We refer to these schools as "Tier 1." Randomization maintained the blocking structure from the prior year and, within blocks, we randomly assigned

[22] We thank Jon Valant for sharing computer code that facilitated our calculation of the deferred acceptance lottery.

[23] Online Appendix Tables C.7 and C.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.

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 guidance counselors were familiar with a previous version from the prior year.

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. Note that the over-representation of Fast Facts treatments is due to their use in Tier 1, when schools received a version of the Fast Facts intervention in the prior school years, as well as the fact that we randomized three versions of the Fast Facts sheet, which we describe in the Appendix and will investigate in future work.

There were some differences in treatment assignment across years. In the second year of the 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.[24] 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. We became aware of a few additional school consolidations and other anomalies 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. 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 thus should not affect our estimates (other than to slightly reduce our sample size, and thus power).[25] 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 guidance counselor. Our intention-to-treat estimation strategy is based on the original treatment assignment, and not these post-randomization updates. For details on balance, school closures, and variations to the original treatment assignment, see Online Appendix Section A.1.2.

[24] Online Appendix Tables C.4 shows impact estimates on key outcomes for each cohort separately, as well as for various definitions of treatment assignment, revealing few differences.

[25] 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 C.7 and C.8.

3.3 Estimation

We estimate the effect of the interventions on an outcome Y_{ij}, for example the percentage of choices with graduation rates above 75%, 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 one of these treatments. Controlling for the randomization block by year, W_b, accounts for our blocked randomization procedures 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. Thus, to generate intent-to-treat effects by experimental arm, we estimate regressions of the following form:

$$Y_{ij} = \beta_1 FF_j + \beta_2 FFDigital_j + \beta_3 App_j + \beta_4 SF_j + \gamma X_i + \alpha S_j + \sum_{b=1}^{97} \alpha_b W_{bj} + \epsilon_{ij}. \tag{1}$$

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.[26] Second, students are assigned to the middle school they were present at in October, but may finalize their school choice process in another school if they transfer after October but before applications are due in December. Finally, our interventions were a suite of materials and support provided to guidance counselors, who were under no obligation to use the materials, and may have chosen not to use the materials for a number of reasons.[27] 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.

[26] 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.

[27] Both formal interviews and informal calls to guidance counselors to check on the delivery status of the materials indicated that a handful of guidance 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. Of the schools that the research team was able to assess use in 2016-17, 85% reported using the intervention materials.

4 Using the Interventions

Before documenting the impact of the interventions we first show that they were adopted by guidance counselors and deployed in schools. We have several sources of information on use of the interventions, including formal surveys and telephone interviews and 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 guidance counselors at 69 schools participated in a followup interview. In both the survey and the interviews, we asked counselors to report if they distributed the tools and/or study materials, and thus show results combined from these samples (there is some overlap).[28] About half of schools had at least one participant in either the survey or the interview, with participation rates higher for schools with interventions than the control group, as shown in Figure 2, Panel F.

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% 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% of counselors (Panel B). Many 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 an online application as it is in the present. About 70% of the non-School Finder respondents, including those in the control group, shared School Finder. Being assigned to the School Finder treatment increased this by over 20 percentage points. Overall, the survey and interview group shows high reported use of the tools and materials, which is perhaps not surprising in a respondent sample. However, since response rates were fairly high, this still reflects at least half of the interventions schools reporting use of at least some aspect of the study interventions, and we have additional measure of use, as described below.

We add to the survey/interview sample responses from informal calls from the research team in Fall 2016, as displayed in Figure 3. Research team members called school staff to ensure receipt of the box of study materials and troubleshoot access to any materials as needed. Additionally, at this point in time, they asked counselors 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% of the treated schools, as shown in Panel C of Figure 3. 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 do not have this data for non-respondents.

Panel A of Figure 3 shows the rate at which school counselors reported using or planning to use the study materials, where an affirmative to any channel is counted as a "yes." In this case,

[28] 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.

80-91% percent of respondents report using or planning to use the intervention. Panel B shows a similar combination, except survey/interview responses are allowed to supersede the informal call. So, if a counselor 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%.

We also augment reported use with a measure of use of the Fast Facts website (Panel D), indicating that a school used the intervention if we observed use of the website through a web tracker tool. The Fast Facts website was available to both the paper and digital versions of the intervention, and we count "use" by an indicator of schools that had at least 5 unique views of their 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 C.1 and ranges between 0 and 310 views per school.[29] Only a small number of Fast Facts paper schools had digital use at this level, with 14% having at least five unique views. For the digital version of Fast Facts that increased to 42%, which is higher, but indicates that less than half of Fast Facts schools had 5 or more hits to the website, despite a high rate at which counselors reported distributing the tool, which perhaps is due to the fact that students needed to go to a website rather than being able to immediately interact with the tool. It is also an important reminder that tool distribution does not necessarily mean that students use the tools. Unfortunately, data from the App were impossible to connect to individual middle schools as students did not need to enter a middle school into their profile, and data from School Finder was not made available to the research team. Thus we do not have use records for these interventions; it was impossible for them to use the Fast Facts website as we did not create a school-specific site for schools in these interventions.

As a whole, all of our measures of intervention use show a high level of use or reported use by guidance counselors. However, our only tool with a direct measure of use by students – Fast Facts Digital – shows a lower rate of utilization, indicating that school staff report of use is not sufficient for student engagement. Staff may report planning to use the interventions and then not follow through, or they may distribute the tool but leave it to students to interact (or not) with the contents. Another way to measure use is to determine if assignment to the interventions changed high school applications, which we do in the next section. In later analyses, we split the sample by reported use (Section 5.3).

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

[29]We removed out views from NYU ISPs, the research team site.

the impacts on matched and enrolled high schools. Throughout, we rely on a few key outcomes. Our first key indicator is the percentage of the top three choices on a student's high school application that have graduation rates below 75% (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 and impact on application. The second key indicator we term "nonoptimal first choice strategy." This is an indicator for listing as a first choice a school that has a graduation rate below 75% *and* has a student-specific guaranteed admissions probability. If a school like this was listed first on an application, it would guarantee that a 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 clear and concise measure of whether our interventions influence "application strategy" and thus use it as one of our main outcome measures. Finally, we consider whether a student actually enrolled in a school with a graduation rate below 75% to assess whether changes in application behavior converted to changes in high school matriculation. Impacts on these three indicators are summarized in Figure 3, and we go into greater detail below.

Since admissions probability is a component of one of our key outcomes, and discussed throughout our results, we define and describe how we simulate it. We conceptualize admissions probability here 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 a simulated admissions probability, we run the deferred acceptance algorithm on the high school choices in each students' application, using a random lottery number, a thousand times. Thus the simulated admissions probability is the share of the thousand cases that a student is assigned via the algorithm to a particular school on their application. 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 that a student applies to. Furthermore, priority and ranking information is only available for schools on a student's application. Admissions priorities are quite bifurcated, as shown in Online Appendix Figure C.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% 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 admissions. Estimating admissions probabilities for choices beyond the first choice is a little more complicated, since a student can have a zero chance of admission at a 2nd (or later) choice school both because of their priority and school ranking *or* because they matched to a prior choice. Thus for choices after the first choice, we use the cumulative admissions probability for that choice and prior choices.

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%, the city median graduation rate. In the control group, 21.1% of students' top three high school choices have graduation rates below 75%. 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%; 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. Interestingly, we see the sharpest declines for the interventions that did not allow low-graduation rate schools to appear on the tool (Fast Facts and the App, not School Finder), except in the case where we have evidence of low utilization (Fast Facts Digital, see Figure 2, Panel D). 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 just 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%) which correspond to Figure 3 are in Panel B, along with a similar estimate for a lower threshold (70%). Panel A shows impacts on graduation rates directly, and Panels C and D 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 70 percent or 75 percent, as discussed with regard Figure 3 above. The impacts we saw at the 70% threshold are similar to those at the 75% threshold.

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 higher graduation rate schools. Thus Figure 3, Figure 4 and Panels C and D 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. As a whole, we consider these indicators of "application strategy."[30]

[30] Impacts on measures of application probability alone, not combined with graduation rates, are in Online Appendix

Our key measure of application strategy is presented in Panel B of Figure 3, the likelihood that a student applies to a low graduation rate school as their first choice *and* they are guaranteed admission at that school. With such a first choice, a student would be automatically matched to a school with a low graduation rate. Despite this being what we term a "nonoptimal first choice strategy," 14.4 percent of control group students choose such a school as their first choice. This means that they have no opportunity to match to a higher graduation rate school, even if they list one later on their application. We see that treatment assignment always reduces the likelihood of a nonoptimal first choice. For Fast Facts paper, it is reduced to 11.1%, the digital version reduces this to 12.6%, the App to 11.2%, and School Finder to 11.9%. The corresponding estimates are 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 of a nonoptimal first choice that would lock a student into such a 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 D of Table 4. In both cases, these measures combine the likelihood of listing schools with graduation rates *above* 75% 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 to a later choice. These outcomes jointly indicate selection of relatively high graduation rate schools *and* the likelihood of getting in to (or not getting in to) such a school. Figure 4 shows that assignment to treatment does not change much the likelihood that a student applies to high graduation rate schools for all three of their top choices (Panel A). About 56% of control group students apply to all high graduation rate schools. Treatment assignment increases this for Fast Facts paper and the App, by about 2.5 percentage points, though the differences are not statistically significant. Fast Facts digital and School Finder may decrease the likelihood of applying to high graduation rate schools. However, more meaningful change is revealed in Panel B, which separates out 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 Fast Facts paper and the App are coming at high schools with some or guaranteed probability of admission, meaning that students in these treatments are upgrading their application strategies.

Panel D of Table 4 confirms the above, and also makes clear, 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 but no probability of admission schools, meaning that this treatment also shifts students away from "wasted" applications at higher graduation rate schools at which a student has no chance of actually being accepted.[31] The App also increases application to high graduation,

Table C.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.

[31]While application to these schools does not affect admission to lower ranked schools with the deferred acceptance

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 seem to 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 schools. This sets the stage for students avoid 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.[32] First, applications to high-quality schools with low probability of admission ("wasted" applications) would not translate to meaningful enrollment changes, if few students have a chance of actually 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 Table 5. As highlighted in Figure 3, almost 39 percent of students in the control enroll in high schools with graduation rates below 75%. Treatment assignment generally reduces this. 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%. 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 5 shows the impact estimates behind Figure 3, and more impacts on match and enrolled schools.[33] The interventions increase average graduation rates of the enrolled school by 1.5% for

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.

[32] 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.

[33] Note that sample sizes decrease from choice outcomes, to match outcomes, to enrolled school outcomes. This is

Fast Facts paper, 1.2% for the App, and 1.1% for School Finder. The reduction in low graduation rate enrollment is preceded by similar magnitude reductions in match to low graduation rate schools, and that the reduction is sharpest for Fast Facts paper and the App at the 75% threshold, but School Finder also shows a large reduction in matching to and enrolling in schools with graduation rates below 70%.[34]

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 Figure 5, which plots the impact estimate at each potential graduation threshold. For example, the point at 60% shows the impact of treatment assignment to enrollment in a high school with a graduation rate below 60%: if the gray confidence interval overlaps with the line at zero, then the impact does not reduce enrollment in high schools with graduation rates below this threshold, if the confidence interval does not overlap with zero than the treatment decreases enrollment to schools beneath that threshold.

The impact of Fast Facts is in Panel A. This treatment reduces the likelihood of enrolling in a low graduation rate high school at just about any threshold for "low," however the impact is largest around the 75% threshold targeted by the treatment. Unsurprisingly, as we observed little impact of Fast Facts Digital elsewhere, there is no difference at basically any point in the graduation rate threshold distribution. The App, as shown in Panel C, also has the largest reduction at the 75% threshold, though, unlike Fast Facts paper, it appears to have little impact at other points in the distribution. In contrast, the School Finder intervention (Panel D) has the greatest reduction when in enrollment in low graduation high schools when the threshold is at 69.5%, which explains why their is a statistically significant decline at the 70% threshold but not the 75%. Notably, these patterns correspond to our understanding of how each of the tools functioned: Fast Facts and the App explicitly did not list schools underneath the 75% threshold and thus the decline implies that students directly engaged with listed schools. We have reports of 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.

Figure 6 shows that the changes in choices and their admissions probabilities drives the changes in enrolled graduation rates. This figure shows, within each block, the treatment minus control

not due to student dropout. Instead, this is due to students matching to and enrolling in new(er) school which does not have a graduation rate yet. They choose these schools less frequently. We display various imputed graduation rates in Online Appendix Table C.3, and our conclusions remain the same with and without imputed graduation rates for newer schools.

[34]We also 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 C.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. This implies that there is enough slack in the system to handle additional applications to relatively higher graduation rate schools, though students in treated schools are very slightly (half a percentage point) less likely to match to a school in the first round of high school admissions.

difference in nonoptimal first choice strategy and low graduation rate choices, each plotted against the treatment control difference in enrolled school graduation rates. Across all treatments, we see that within block school contrasts line up: both a reduction in nonoptimal strategy *and* a reduction in low graduation rate choices correspond to a reduction in rates of enrolling in low graduation rate schools.

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 into higher quality 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 C.5. Here, we show a resounding lack of impact (either positive or negative) on GPA or credits failed. The real test of match quality will come when we follow these students to high school graduation and determine if they are more likely to graduate.

5.3 Why did (most of) the interventions work?

There are three potential channels through which the interventions may have influenced the choices and thus the matched and enrolled schools of students. First, the schools recommended by or found in searches using the tools themselves may have influenced their choices. For this to be true, guidance counselors must have distributed the tools and students must have interacted with them. This is the "tools" hypothesis. Second, the supportive materials might have been used by guidance counselors and shared with students, influencing their choices and knowledge of the high school choice process, regardless of a specific tool. We refer to this as the "supportive materials" hypothesis. Finally, guidance counselors may not have used the tools or materials, but they may have been prompted by the receipt of study materials to deploy their own curriculum and guidance around choice. We call this the "priming" hypothesis.

We present three types of analyses to determine which channels play a role in response to the interventions. In all cases, these analyses rely, at least in part, on reported use of tools by guidance counselors in our survey, interview, or call groups. Thus, since use of the tool itself is a response to treatment, these findings cannot be considered causal estimates, and it is impossible to distinguish between impact of a tool and selection into using a tool. Rather, we consider these analyses suggestive, descriptive findings. However, we note in Online Appendix Table C.9 that there are generally few systematic differences in school characteristics between schools with counselors reporting using the tool, those reporting no use, those that did not respond, and the control group. Results here are limited to the 2016-17 year, since this is the year in which we collected extensive use information. Thus estimates can be compared to the 2016-17 only estimates in Online Appendix Table C.4.

For the tools hypothesis, we determine if students in schools where counselors reported using a particular tool list choices on their high school application that correspond to that particular tool. To test the priming hypothesis, we examine whether there are beneficial outcomes in schools that either reported not using the tool or did not respond to our outreach. For the test of the supportive materials hypothesis, we compare schools in the control group that reporting using School Finder with those that did not (also in the control group). This gives an estimate of the tool impact on its own. Subtracting this estimate from our standard School Finder estimate then gives a suggestive estimate of what the supportive materials do on top of the tool itself.

5.3.1 The "tools" hypothesis

To examine use of the tools, we focus on the particular high schools that were emphasized by each tool in turn, hypothesizing that if students are using a tool they are more likely to list high schools highlighted by the tool on their application. Thus we focus on whether a particular type of school appears in the top three choices for each intervention, in schools that report using that particular intervention. We note, however, a tool could still be influential without necessarily affecting these particular margins, by changing the order of schools listed, the admissions probability within a school type, or other aspects of the high school application.

For emphasized schools for the Fast Facts treatment group, we look at schools from the Fast Fact lists.[35] For the App, we look at screened schools. Since the App listed search findings in order of graduation rate and did not limit the number of screened schools listed (unlike Fast Facts, where we had a ceiling on number of screened schools). Finally, for School Finder, which allowed sorting by alphabet and by distance, we examine whether students listed schools at the top of the alphabet (school name begins with a A, B, or C) as well as travel time.[36]

We present results from this exercise in Table 6. Here, we have separated out groups not just by treatment status, but also by reported use, as described in Section 4. Thus the estimates in this table are not causal impacts but are descriptively interesting since they point to how response to treatment aligns (or does not align) with use. Columns 1 through 4 of Table 6 report estimates for students assigned to schools in a given treatment arm that reported use of a tool in a survey, interview, or check-in call. Note that reports of use from the check-in calls include "plans" to use the tool. Column 5 reports estimates from *all* treatments where guidance counselors reported that they did *not* use or plan to use the tools. Column 6 reports estimates from guidance counselors who did not respond to any of our outreach or with whom we could not connect for a follow-up

[35] Recall that Fast Facts lists were created for all middle schools, including App, School Finder, and control schools. Thus, we can contrast the percentage of listed Fast Facts high schools, even for middle schools not assigned to Fast Facts.

[36] Note that 23 percent of high school names begin with A, B, or C, indicating that those with power to name schools are likely savvy with regard to alphabetic listing of schools.High schools at the top of the alphabet have a high school graduation rate of 73.4%; schools in the rest of the alphabet have an average graduation rate of 75.8%. The difference is not statistically significant.

call. We have no information about what to expect from these schools. It could be that they were unlikely to use the materials, given they did not respond to us; however that could have been a coincidence.[37]

Panel A of Table 6 shows the indicators of tool use for the groups described above. Our first indicator is percentage of top three choices listing Fast Fact high schools. Here, there are no statistically significant impacts on listing Fast Facts schools, but the pattern of results is interesting regardless. As shown by the control group mean of 58%, most students list Fast Facts schools as part of their top choices. This may not be surprising, since we selected Fast Facts schools to be nearby high schools, and ones that students at a given middle school had at least some history of matching to.[38] This rate is higher for schools that reported using Fast Facts paper. Fast Facts Digital – even these schools that reported use – had no difference when it compared to the control group. We take this as evidence that the lower rates of use when using internet views were more accurate than counselor support, as well as that a hit to a website is not sufficient to ensure take-up.[39] While the App treatment did not specifically target Fast Fact schools, it had a similar graduation rate cutoff for suggested schools as part of its algorithm, and also suggested nearby schools. There was little difference in this metric for those that used School Finder or those that reported not using the tools. However, those that did not respond to the survey seemed less likely to list Fast Facts schools, perhaps indicating unfocused high school searches for this group.

As predicted, students in schools that reported use of the App were more likely to list at least one screened school in their top three choices. While these schools tend to have high graduation rates, listing screened schools is not necessarily optimal strategy, as admissions probability at these schools tends to be low, and impossible for students without appropriate academic credentials. Most of the other groups do not show a difference in listing screened schools, though there is a not-significant increase for Fast Facts Digital.

When it comes to listing schools near the top of the alphabet, we see that all groups that reported use are more likely to list at least one of these schools in their top three choices, with statistically significant estimates for Fast Facts and School Finder. We take this an indicator that when counselors report use of tools or materials, these are the counselors and students that are participating more in any part of the choice process, and thus, as active choosers, may be more likely to encounter materials with schools listed in alphabetical order. This is an important reminder that there is no such thing as a "neutral" default (Carroll et al., 2009; Levav et al., 2010; Chen et al., 2014; Feenberg et al., 2017; Beshears et al., 2019). Listing schools in alphabetical order may seem like it is neutral, but instead, it preferences schools near the beginning of the alphabet. Students in

[37]It would be interesting to separate both the did not use and did not respond groups by specific treatment arm, but sample sizes are very small when we do this.

[38]At least one student a middle school had to have matched to a school in the past six years for it to be added to the Fast Facts list.

[39]Online Appendix Table C.11 shows the estimates with Fast Facts Digital use recoded to follow online views of the website, and still shows few differences for this group.

schools where guidance counselors did not respond to outreach also had increased levels of listing early alphabet schools, perhaps indicating that these schools used alternative, non-targeted school choice curricula.

As a whole, we take the general trend of correspondence between in high school listing indicators for specific tools with schools reporting use of that as confirmatory evidence that at least some of the impact of the interventions comes from directly listing the recommended high schools of that particular tool (the "tools" hypothesis). We see in turn in Panels B and C of Table 6 that the actively used tools (Fast Facts paper, the App, and School Finder) in turn are more likely to have reductions in nonoptimal first choice strategy and listing low graduation rate schools, and eventually are less likely to enroll in low graduation rate schools.

5.3.2 The "priming" hypothesis

Interestingly, we see a drop, though not a statistically significant one, in likelihood of matching to a low graduation rate school for Fast Facts digital, non-using schools, and non-respondent schools. This ranges between -1.4 and -3.2 percentage points — smaller than the statistically significant -4.4 to -6.5 found for the other treatments reporting use — but is a difference from untreated control schools. These groups also appear to have a lower rate of nonoptimal first choice strategy (though again, not to the extent of Fast Facts, the App, or School Finder). This is suggestive, though not conclusive evidence of a small "priming" channel. Despite low rates of actual use vis-a-vis internet views and a lack of utilization of Fast Facts schools (Fast Facts Digital), reporting no use (Did Not Use), and unknown status, all three of these groups received intervention materials thus guidance counselors may have been prompted to engage more (or more intensively) in the high school choice process than control group schools, as we do observe small reductions in poor choice and enrollment outcomes, though these differences are not statistically significant. It is impossible to distinguish between a priming effect and selection out of treatment by counselors who already have impacts on the outcomes we focus on, though again, there are few differences among the low- or no-use groups in terms of school characteristics (Online Appendix Table C.9). One notable difference is that guidance counselors who do not opt in or respond to our outreach are slightly more likely to be charter schools, though this difference is not statistically significant.

5.3.3 The "supportive materials" hypothesis

Finally, to assess the "supportive materials" hypothesis, we turn to a different group: control group schools. In Table 7, we compare between control group schools that report using School Finder and those that either report not using School Finder (about a quarter of control schools that respond to the survey) or do not respond to the survey request. To estimate differences by School Finder in this group, we must remove block fixed effects since blocks only have one control group school. We thus substitute borough fixed effects, which means we compare between using and non-using/nonresponding schools within the same borough. Again, this difference should not be considered a causal estimate, and may be a mix of School Finder impacts and selection into use.[40]

To test the supportive materials hypothesis, we compare the School Finder "effect" in the control group to the impact in the experiment. Control group schools which report School Finder use, as shown in Panel B of Table 7, reduce nonoptimal first choice strategy and percentage of top three choices that are low graduation rate, and in turn, are 3.2 percentage points less likely to enroll in low graduation rate schools.[41] This is about three-fifths of the School Finder intervention effect of -5.1 percentage points (Table 5. If we take the control group use estimate as a suggestive estimate of tool use, this leaves about two fifths of the School Finder impact to be explained by the supportive materials we provided with the tool. However, the School Finder "effect" in the control group may be biased upward, if counselors that report use are generally better counselors. If that is the case, there is even more scope for the supportive materials to contribute to the intervention impact. Again, we cannot draw firm conclusions here, but these findings show at least some benefit of wraparound materials, beyond individual tools.

5.3.4 Conclusions from the mechanisms analysis

We cannot definitively pin down the channel through which the interventions are effective, finding suggestive evidence for all three channels: tools, priming, and materials, or — most likely — some interaction between these channels. We note here, and discuss more deeply in our consideration of subgroup effects in Section 5.4 that intervention impacts are strongest where we see the greatest changes in application strategy (application to higher probability schools, application to higher graduation rate schools) and that for interventions to make a difference in match and enrollment, they likely need to push these levers.

5.4 Heterogeneity by student background

We next investigate impacts by student background, which reveals two main findings. First, the treatments are not effective for all students, and different students benefit from different interventions, though one universal is benefit for English learners in all treatments. Second, we generally find the greatest decrease in enrollment in low graduation rate high schools within the subgroups who respond to intervention with the greatest changes in nonoptimal first choice strategy and graduation rates of chosen schools, implying that enrollment effects manifest for the groups of students who make the greatest use of the tools. Figures 7 through 10 summarize the treatment effects for student subgroups, displaying impact estimates for students by gender, race/ethnicity,

[40] We observe few difference in school characteristics in terms of student demographics and tests scores between using and other schools in Online Appendix Table C.10. However, schools that report using School Finder in the control group are much more likely to be charter schools.

[41] They also increase application to screened schools, which perhaps contributes to a smaller response to School Finder here than for intervention schools, despite big changes in application behavior.

prior math scores, home language, English learner status, free/reduced price lunch status, and special education status. The figures display the impact estimates on nonoptimal first choice strategy, the percent of top three choices with graduation rates above 75%, and the proportion of students enrolled in high schools with graduation rates below 75%.[42]

There are some consistent patterns across all the treatments. There are few differences by gender. High-scoring students tend not to respond to any of the interventions, perhaps because these students are those most likely to already have school choice plans and to aim for admission to exam and screened schools.[43] *All* of the treatments are particularly effective for English learners, even Fast Facts Digital, with a reduction of enrollment in a high school with a graduation rate below 75% of by 6.2 to 12.3 percentage points. Consistent with this, impacts tend to be largest for students who speak Spanish at home, though the App is particularly effective for students whose home language is neither English or Spanish.[44]

For the Fast Facts paper intervention (Figure 7), 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 nonoptimal first choice strategy and listing low graduation rate high schools. The Fast Facts paper intervention is most effective for the lower scoring students, including those with low scores, medium scores, 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. In general, Fast Facts tends to benefit 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 and may reflect differences in school context. 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 (Figure 8) 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 was more effective for more historically advantaged groups (Figure 9). Impacts on 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

[42] Results including additional outcomes and standard errors are in tables in Online Appendix D.

[43] 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.

[44] 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.

or Hispanic/Latino students. Not low-income students also had a slightly larger response than low-income students. However, English learners and those who do not speak English at home also see the largest 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 nonoptimal first choice strategy and percentages of listed schools with low graduation rates.

As with Fast Facts paper, the School Finder intervention tends to benefit more historically disadvantaged students (Figure 10). 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 outcomes generally align for these subgroups.

As a whole, the subgroup results underscore a few main points. The first is that groups that use the tools more (as measured by changes in application behavior) tend to have the biggest impacts on graduation outcomes for the enrolled school. The second is that Fast Facts paper and School Finder generally benefited historically disadvantaged groups, while the App had greater response from comparatively more advantaged groups. Finally, in all cases, English learners and those that did not speak English at home benefited the most from the interventions, pointing to the need for targeted help and materials in home languages for families navigating both the school choice process and an unfamiliar language.

6 Engagement Matters

Considering all of the possible intervention channels and responses, one theme becomes clear: engagement matters. 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 — the primary choosers in New York City. 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. We show here that interventions can generate a similar improvement in graduation rates for the enrolled high school even when they are disseminated through school counselors, as long as interaction with the tool occurs, though note that the interventions took place with somewhat different school populations.

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.

In this experiment, the path to each of the digital tools was the same: a postcard and supporting lessons and materials for guidance counselors. But the material at the destination was different. Fast Facts Digital was a static resource, but both the App and School Finder had interactive components. It could have been the case that guidance counselors made the choice not to provide Fast Facts Digital after reviewing its non-interactivity, or it could have been the case that students went to the resource but did not find it engaging. Either way, engaging materials (either a salient paper artifact, or an interactive digital tool) seem to be part of the foundation for information to work.

However, tools and their interactivity may not be sufficient. In the case of the School Finder tool, students could already access the publicly available tool. Our analysis within the control group shows suggestive evidence that more than half of the School Finder impact was due to the tool, leaving a role for supportive materials and an active counselor as well. Thus we found the full potential of the School Finder intervention was realized with the curated engagement with it, which we supported through lesson plans, instructional materials, and responses to guidance counselors about how to use the tools. Materials use is a form of engagement with the tool, as it pushes students to engage with concepts like the admissions probabilities and graduation rates of listed high schools.

There is likely a similar story around the large response we found for English learners. 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 were particularly large for students from Spanish-speaking families, again highlighting that inducing engagement with tools by removing language barriers facilitates response. Even Fast Facts Digital showed a beneficial response for English learners (though differences are not statistically significant). This also underscores 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.

An important caveat here is that the person-specific App intervention in particular 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. While we do not have direct evidence on this question, differential responses to digital interventions may be due to access to a smartphone or other personal means of accessing the internet (and thus the tools). Digital platforms have the potential to reduce costs for providers and allow for personalization based on user input, but access is still an issue. 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). During the COVID-19 pandemic there has been a continuing need to get NYC students access to internet and devices, indicating that the problem persists in the city.[45] As noted earlier, the NYCDOE has moved much of the high school directory online, supplying a shortened guide to the process as a booklet with material about schools only available online, meaning that gaps in access and the existence of select paper tools are even more relevant now.

Finally, we note that the interventions with the largest response included "nudges" in the form of excluding other schools from lists of schools and schools presented in descending order by graduation rate (Fast Facts paper, the App). However, the School Finder intervention was a search tool, and did not explicitly recommend specific schools, nor was it searchable by school performance. While impacts of School Finder on enrollment in low graduation rate schools were somewhat smaller than those of other interventions (a reduction of 5.1 percentage points versus a reduction of 6.1 percentage points for Fast Facts Paper and the App,), it still made a meaningful difference in the type of enrolled school. We thus interpret the success of the School Finder intervention as evidence that *any* coherent high school admissions curriculum can help students: the important thing is to do something that draws students into the process, more so than the specific tool used. This idea is bolstered by our suggestive finding that part of the School Finder effect is due to interaction with the tool, and part of it due to engagement with supportive materials.

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.

7 Conclusion

This paper reports the result of a large, school-level randomized controlled trial of decisions supports for young people navigating a complicated high school choice process in NYC in a context that

[45]See, for example, many articles in Chalkbeat NYC, including one from September 22, 2020, "NYC schools scramble to help students who lack devices as online learning ramps up again," available: `https://ny.chalkbeat.org/2020/9/22/21451613/nyc-schools-device-access-remote-learning`.

replicated the dissemination of curricular materials through a school district. 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 to not necessarily convert to interaction with the tools. We also show that response to intervention is greatest at schools that report using the tools are materials.

Most of the interventions ultimately increased match to and enrollment in schools with graduation rates above the city median (75%). 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 enrolling in high schools with graduation rates 1.5 percentage points higher than those attended by the control group, and reducing the likelihood of enrolling in a low graduation rate high school 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. Impacts of all the interventions were particularly large for English learners and students who did not speak English at home.

Our interpretation of the pattern of responses to the interventions is 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. 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. We also note that there is a ceiling to the extent that 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, students now attend higher graduation rate high schools than they would have in absence of the randomized controlled trial. Having been nudged into a higher attainment school, students may in turn be more likely to succeed and graduate from high school. Or, it could be the case that student trajectories are not impacted by high school attendance, as in evidence shows for NYC exam schools (Abdulkadiroğlu et al., 2014), and that the students are no more likely to graduate from high school than they would have been in absence of the interventions. Finally, if there is a "mismatch" between student skills and high school curricula, a push into a higher 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 intervention cannot ensure a school match for every student when administrative barriers remain in school choice systems (as in Corcoran et al. (2017)), or when there is an undersupply of successful schools.

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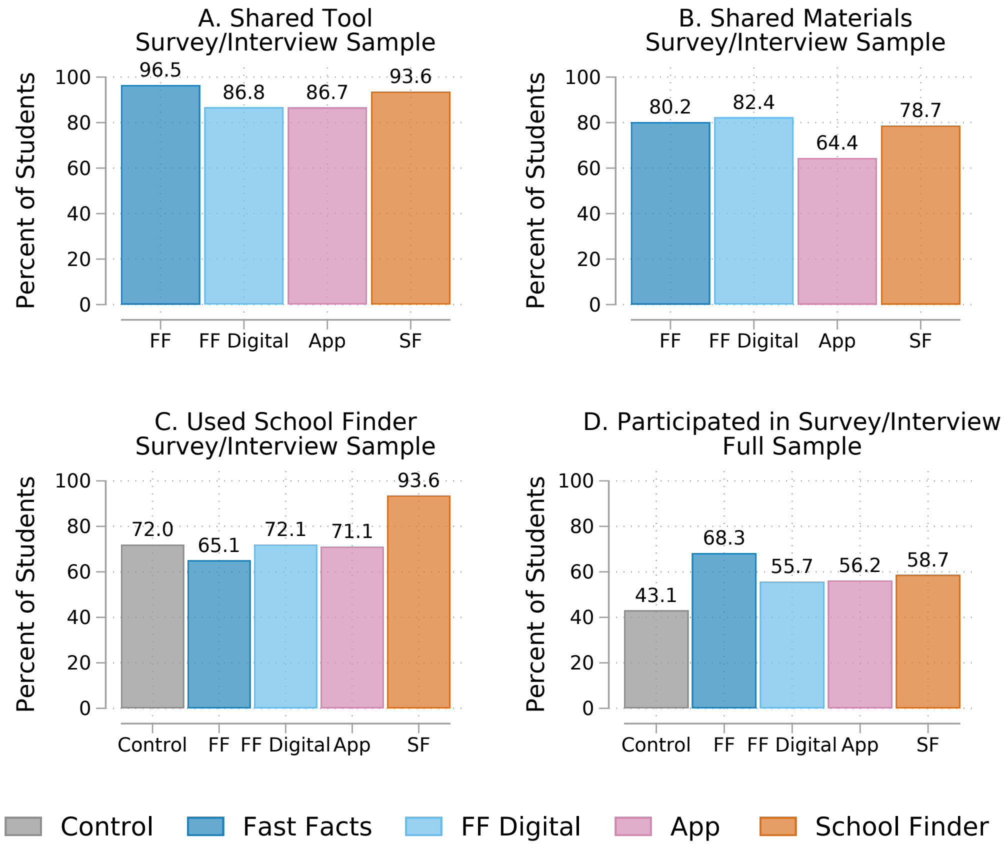
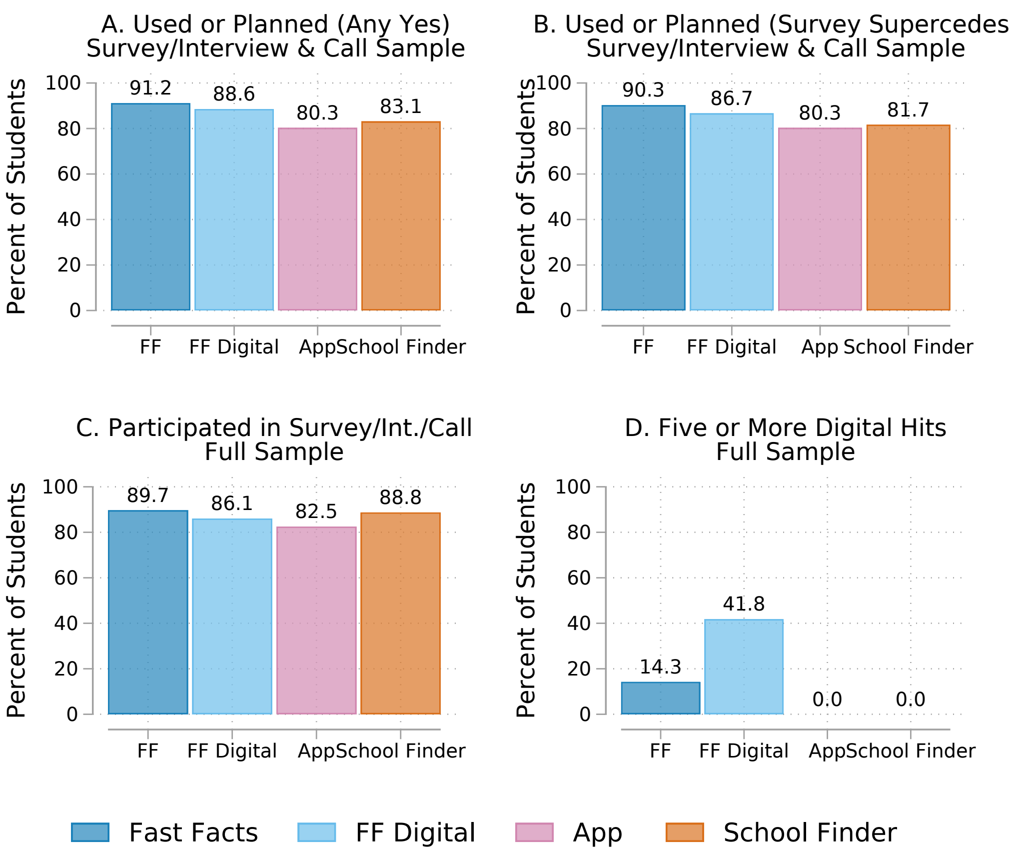
Figure 1: Counselor Reports of Tool and Material Use (Survey/Interview Sample)



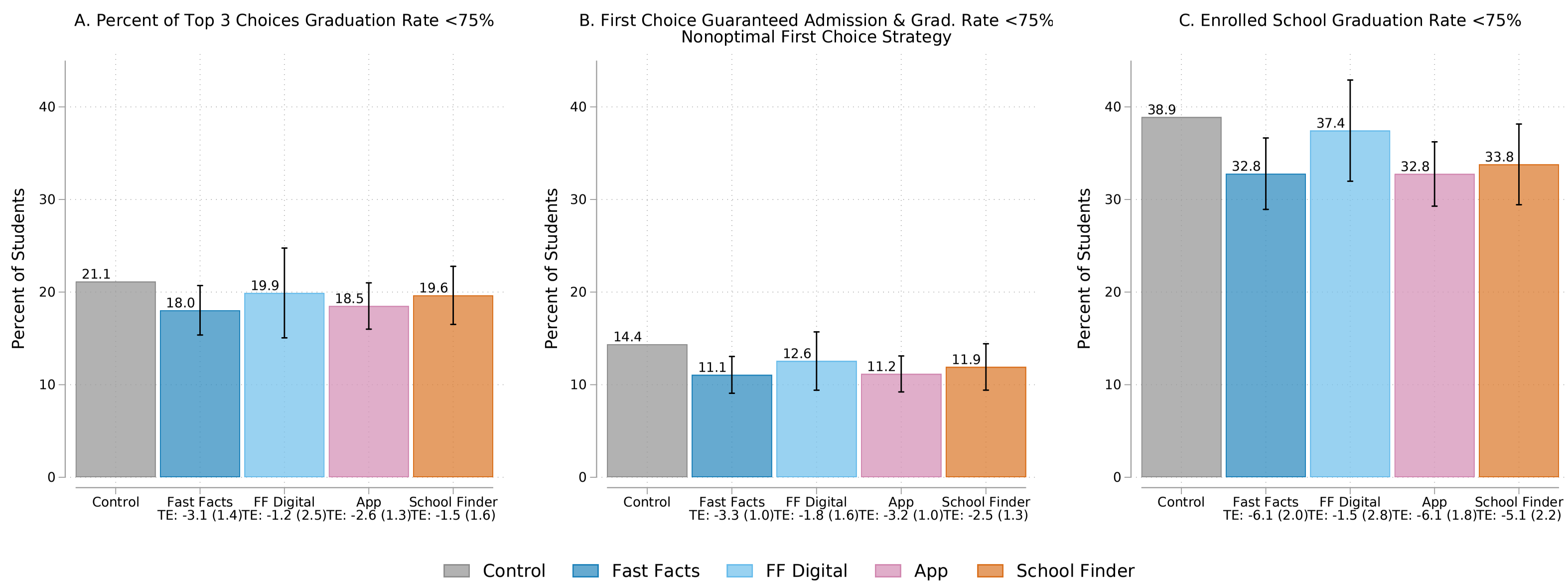
Notes: This figure shows reported use of the intervention tools and materials, for the group of school counselors that responded to the study team's survey and/or interview request. Use is counted if any school staff at a given school reported use. Response rates to the survey/interview are in Panel D. Control group schools are only included in panels where the control group had an opportunity to participate.

Figure 2: Counselor Reports of Tool and Material Use and Planned Use (Survey/Interview/Call Sample)



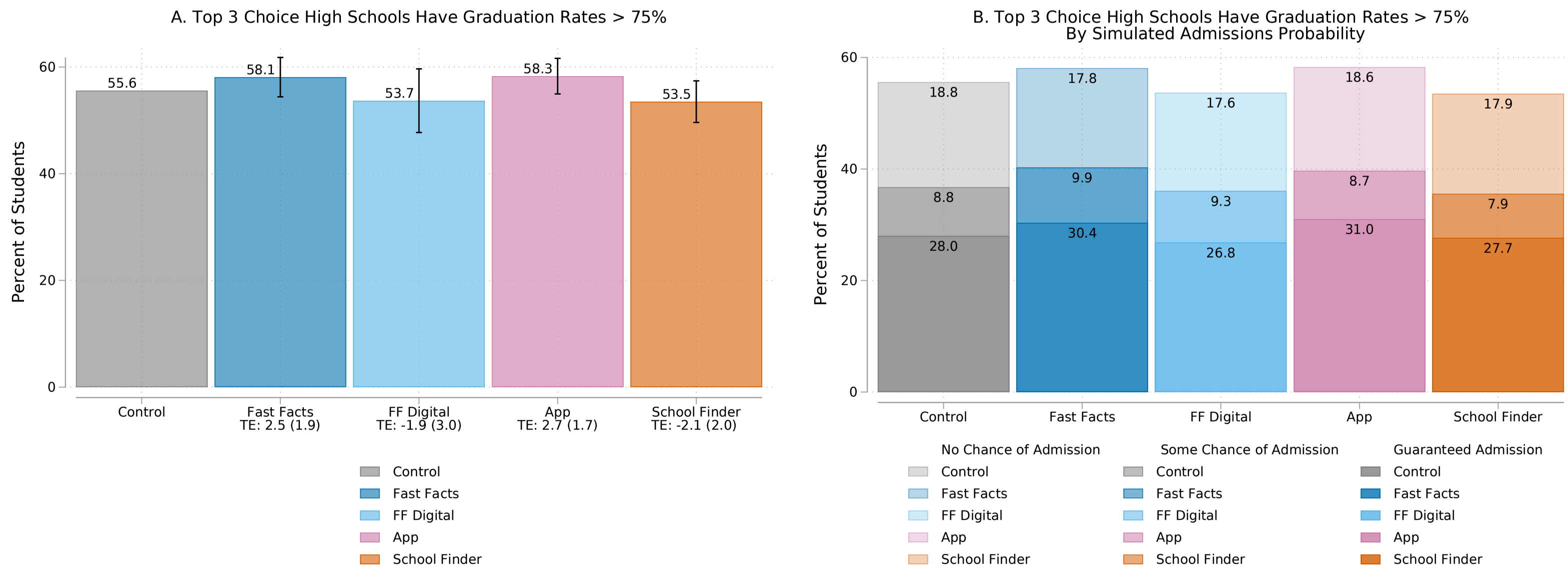
Notes: This figure shows reported use of the intervention tools and materials, for the group of school counselors that responded to the study team's survey and/or interview request or who responded to a call from the study team to confirm receipt of the intervention materials. Use is counted if any school 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 followup call could include intentions. Response rates are in Panel C. Panel D 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



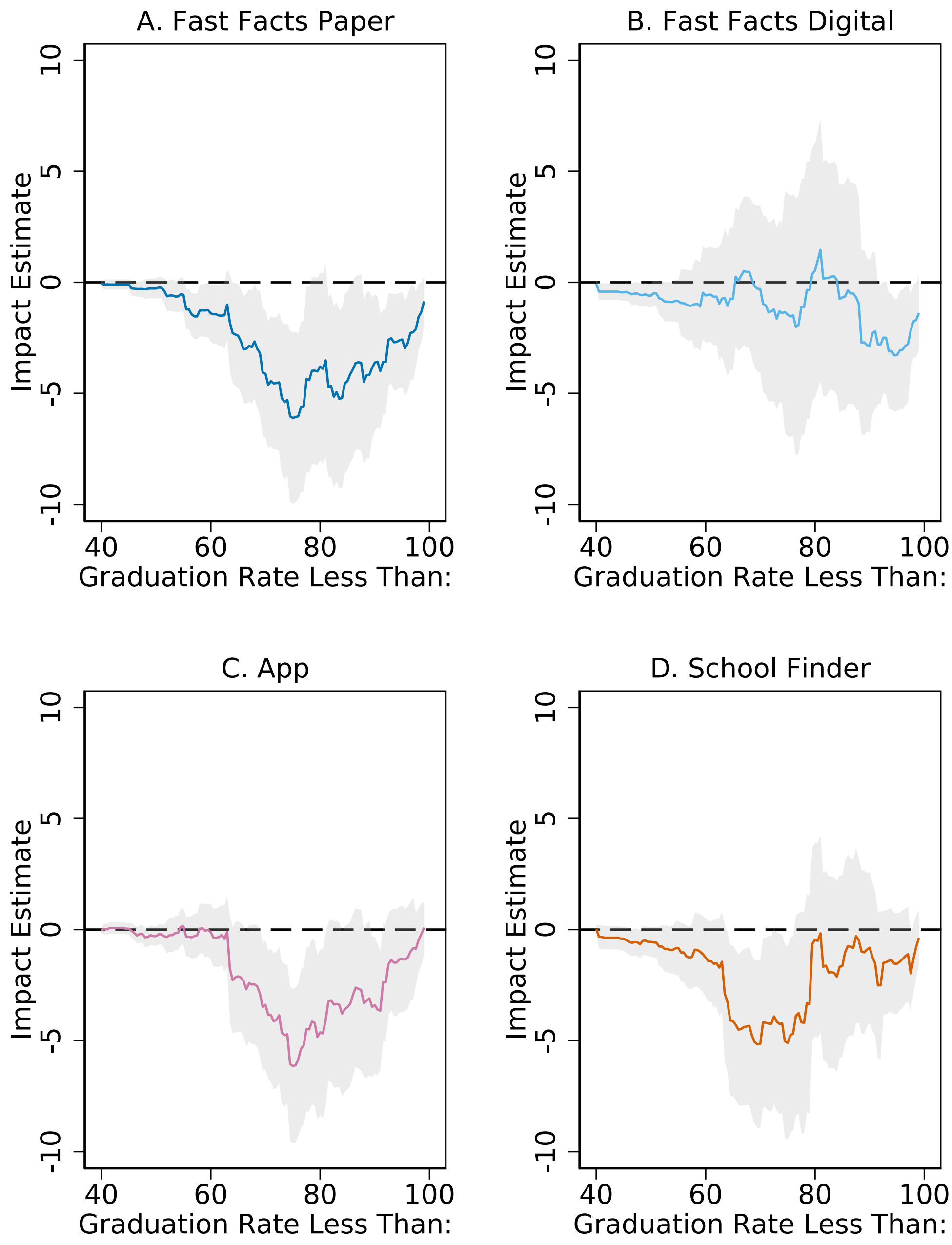
Notes: This figure show treatment-control contrasts for thee key outcomes: the percent of top 3 choices on the high school choice application with graduation rates below 75%; the likelihood of having a first choice school with guaranteed admission and a low graduation rate ("nonoptimal first choice strategy");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

Figure 4: Impact on Admissions Strategy



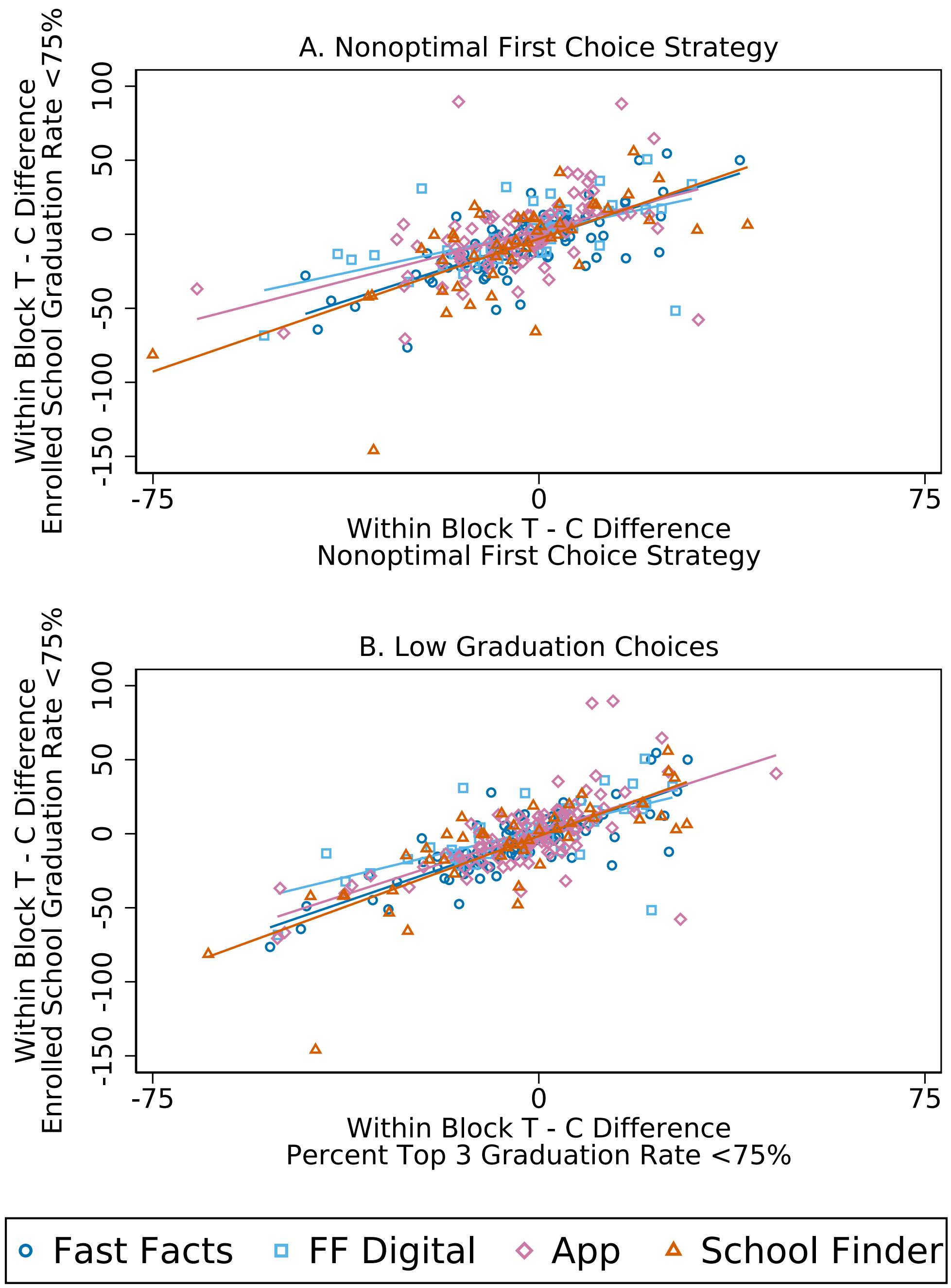
Notes: UPDATE NOTES

Figure 5: Impact on enrolled school graduation rate thresholds



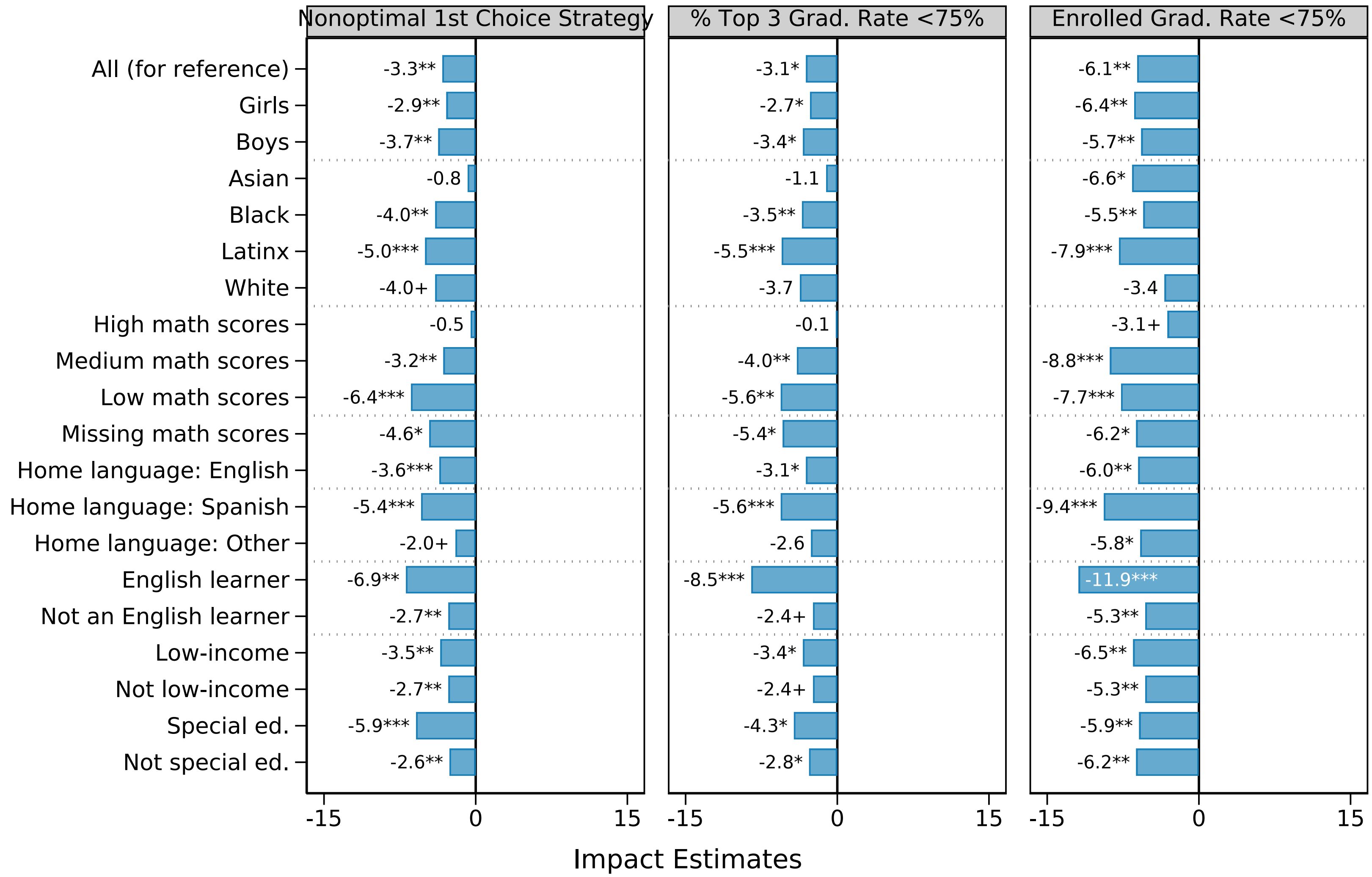
Notes: This figure plots the treatment effects from estimates of intervention impacts on a series of indicator variables for the enrolled school graduation rate being less than a threshold from 40% to 99%. The 95% confidence intervals are indicated in grey.

Figure 6: Within block treatment control differences in average graduation rate of enrolled school by percent of top 3 choices on the Fast Facts list



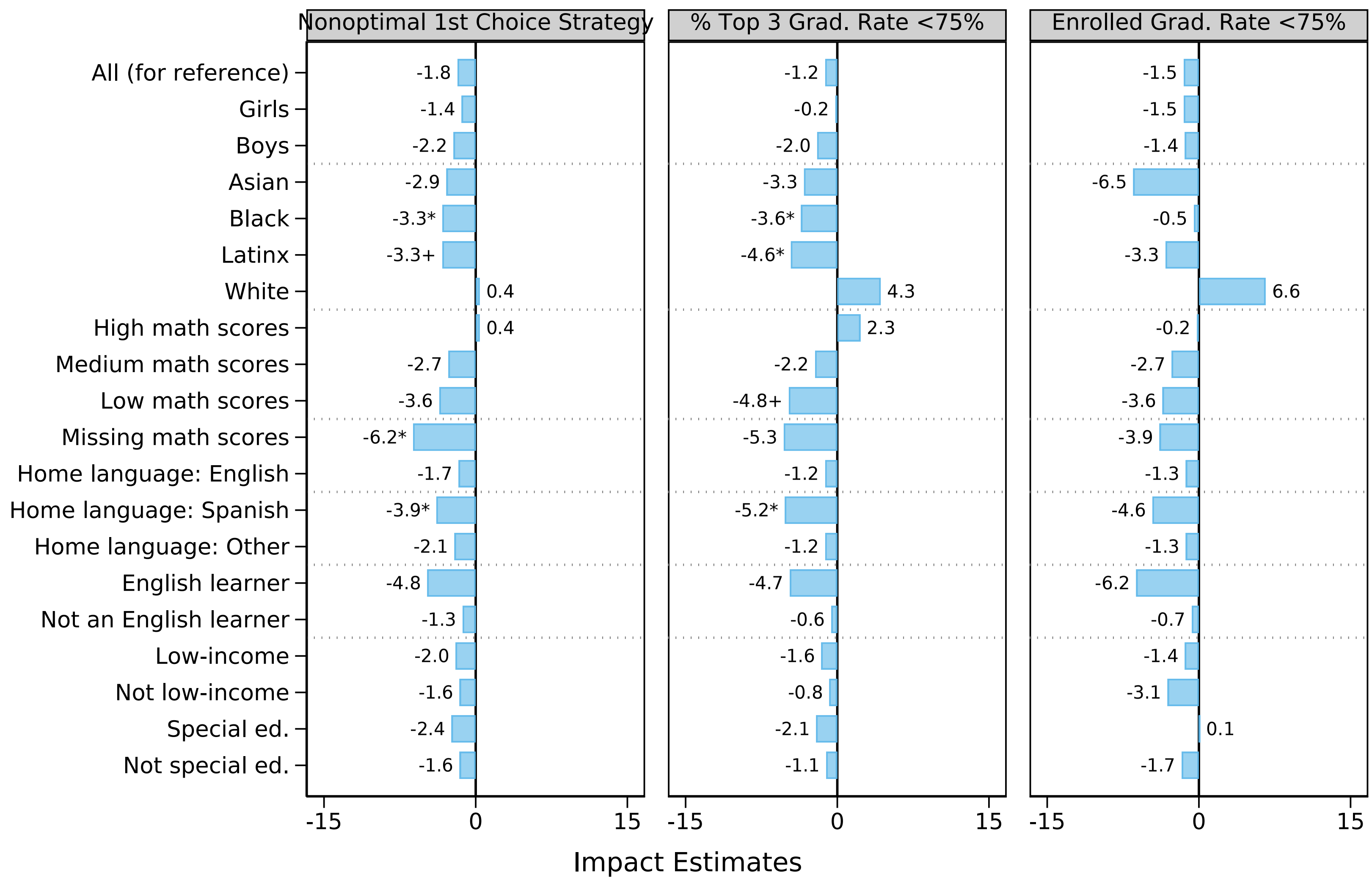
Notes: This figure plots within block regression adjusted treatment control comparisons of impacts on enrolled school graduation rates versus the percent of top three choices from the Fast Facts lists. The estimates are generated using the same estimation strategy as for the main estimates, limited to 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. Open markers in Panel A indicate digital only treatment.

Figure 7: Treatment effects by subgroups: Fast Facts



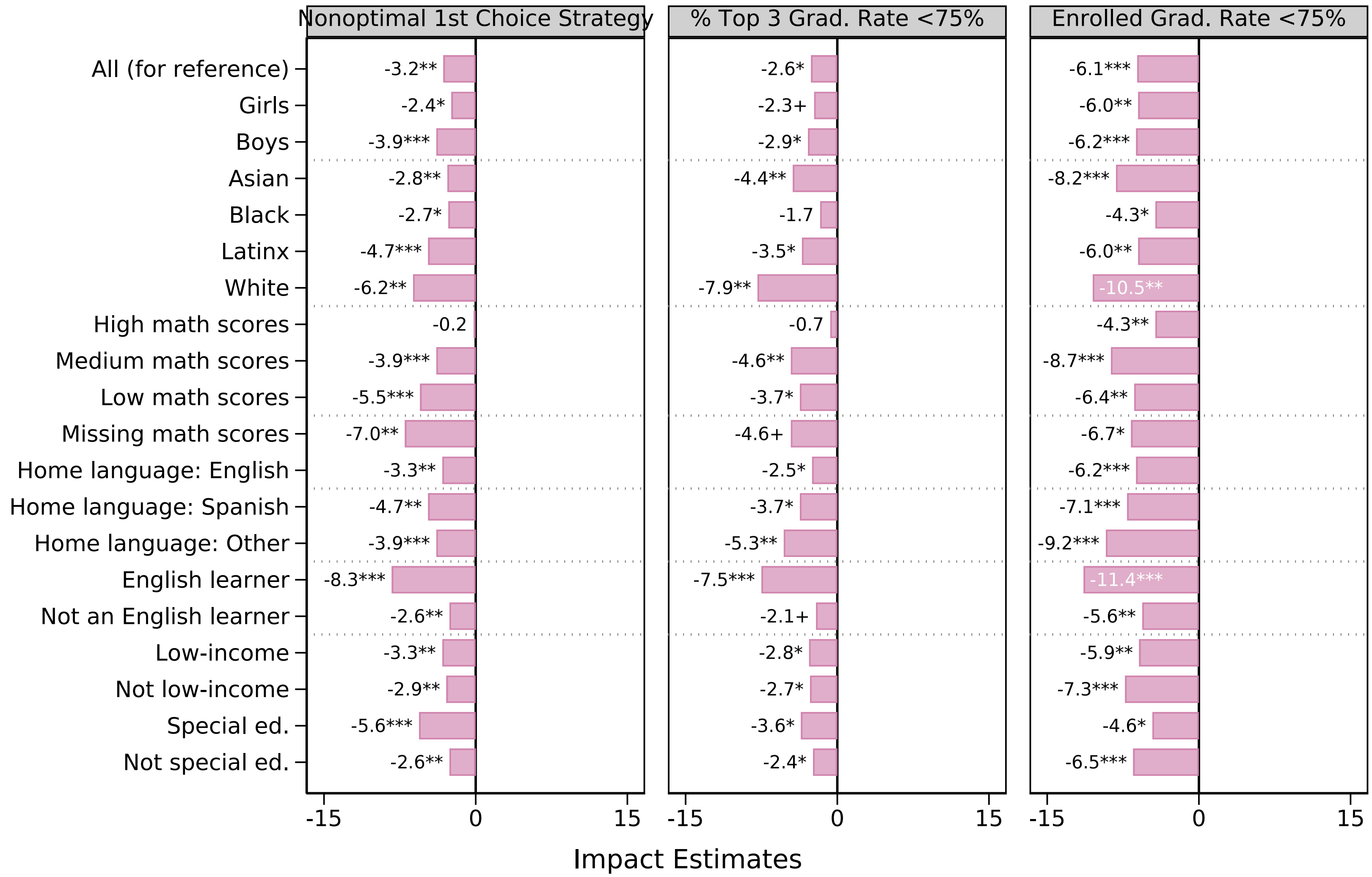
Notes: This figure summarizes student subgroup Fast Facts impact estimates for several outcomes. Estimates come from regressions like those in the main specification, limited to the students who are members of the indicated subgroup. Tables with more complete information, including standard errors and additional outcomes, are in Online Appendix D.

Figure 8: Treatment effects by subgroups: Fast Facts Digital



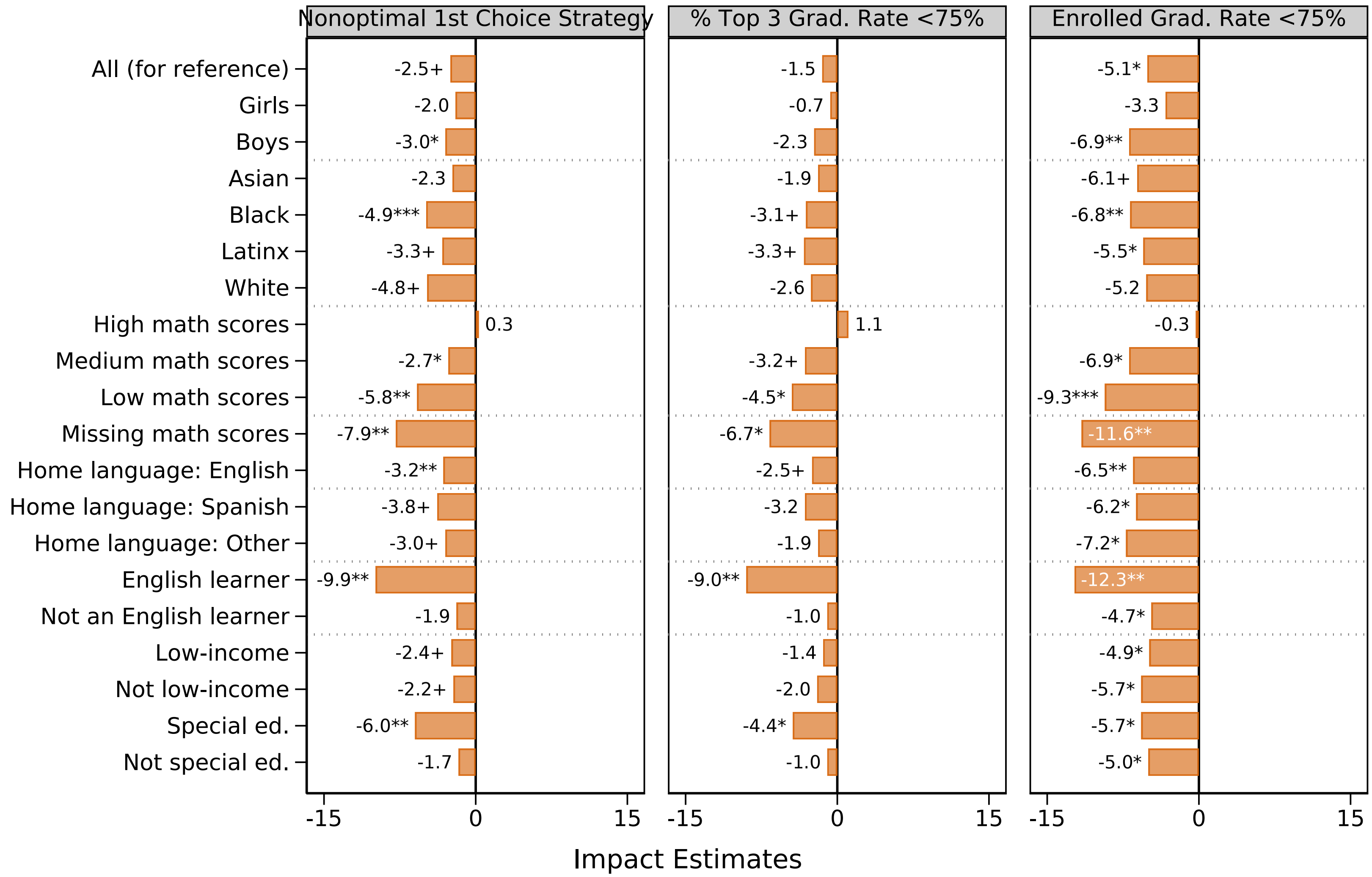
Notes: This figure summarizes student subgroup Fast Facts digital impact estimates for several outcomes. Estimates come from regressions like those in the main specification, limited to the students who are members of the indicated subgroup. Tables with more complete information, including standard errors and additional outcomes, are in Online Appendix D.

Figure 9: Treatment effects by subgroups: App



Notes: This figure summarizes student subgroup App impact estimates for several outcomes. Estimates come from regressions like those in the main specification, limited to the students who are members of the indicated subgroup. Tables with more complete information, including standard errors and additional outcomes, are in Online Appendix D.

Figure 10: Treatment effects by subgroups: School Finder



Notes: This figure summarizes student subgroup School Finder impact estimates for several outcomes. Estimates come from regressions like those in the main specification, limited to the students who are members of the indicated subgroup. Tables with more complete information, including standard errors and additional outcomes, are in Online Appendix D.

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- 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 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
8th math score	0.019	-0.091	-0.327	0.018
8th 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	154238	115126	36384	78742

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 Graduation Rates of Choices

	Fast Facts (1)	FF Digital (2)	App (3)	School Finder (4)	Control Mean (5)	N (6)
(A) Average Graduation Rate						
1st Choice	0.788+	0.050	0.519	0.046	86.8	109,733
	(0.440)	(0.598)	(0.404)	(0.488)	[12.2]	
1st-3rd 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 1st-3rd 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]	
(C) 1st Choice Grad. Rate <75% and...						
Nonoptimal first choice strategy	-3.304**	-1.809	-3.205**	-2.451+	14.4	109,733
	(1.010)	(1.602)	(0.988)	(1.275)	[35.1]	
(D) 1st-3rd Grad. Rate $\geq$ 75% and...						
Non-Zero Chance Admission	3.523*	-0.697	2.936*	-1.172	36.8	115,126
	(1.584)	(2.050)	(1.493)	(1.575)	[48.2]	
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]	

Notes: This table reports regression coefficients representing assignment to an informational intervention middle school on the graduation rates of high school choices. 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).

Table 5: Impact of Informational Interventions on Graduation Rates of Matched and Enrolled School

	Fast Facts (1)	FF Digital (2)	App (3)	School Finder (4)	Control Mean (5)	N (6)
(A) 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]	
(B) 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 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).

Table 6: Impact of Informational Interventions by Take-Up (2016-17)

	Used							
	Fast Facts (1)	FF Digital (2)	App (3)	School Finder (4)	Did Not Use (5)	No Response (6)	Control Mean (7)	N (8)
(A) Use Indicators								
% of Top 3 Choices from FF List	1.736	-0.201	3.571	0.542	-1.910	-4.087	57.7	58,141
	(2.369)	(3.155)	(2.445)	(2.182)	(2.595)	(2.724)	[37.2]	
Any of Top 3 Choices Screened	-0.040	2.822	3.620+	-1.672	-0.600	-0.073	71.1	58,141
	(1.883)	(2.209)	(1.860)	(1.924)	(2.113)	(2.171)	[45.3]	
Any of Top 3 Choices Early Alphabet	5.591*	3.609	3.758	5.205*	-0.493	3.542	44.6	58,141
	(2.486)	(2.887)	(2.572)	(2.538)	(2.817)	(2.716)	[49.7]	
Avg. Travel Time Top 3 Choices	-0.380	-0.176	-1.945	-1.459	-1.494	0.461	34.0	58,134
	(1.369)	(1.586)	(1.719)	(1.385)	(1.884)	(1.425)	[16.1]	
(B) Choices								
Nonoptimal First Choice Strategy	-3.583*	-2.231	-3.250*	-2.544+	-1.433	-1.652	14.7	54,926
	(1.455)	(1.747)	(1.559)	(1.447)	(1.887)	(1.804)	[35.4]	
% of Top 3 Choices Grad Rate <75%	-3.109	-1.816	-3.769+	-1.979	0.349	-0.971	22.9	57,871
	(1.904)	(2.768)	(2.090)	(1.761)	(2.480)	(2.296)	[33.8]	
(C) Enrolled School								
Graduation Rate	1.726**	0.921	1.023+	1.188*	0.571	0.612	79.3	49,119
	(0.566)	(0.604)	(0.554)	(0.536)	(0.787)	(0.584)	[14.3]	
Grad Rate <75%	-6.736**	-1.699	-6.709*	-5.200*	-2.617	-3.511	40.2	49,119
	(2.373)	(2.953)	(2.618)	(2.342)	(3.323)	(2.720)	[49.0]	
(D) Other Choice Outcomes								
Matched to 1st Choice	-0.234	1.055	1.094	0.779	3.679+	0.414	42.0	58,141
	(1.533)	(1.714)	(1.559)	(1.503)	(1.974)	(1.835)	[49.4]	
Matched in R1	-0.776	-0.293	-1.306	-0.814	-0.106	-1.812+	92.7	58,141
	(0.845)	(0.948)	(0.973)	(0.965)	(1.013)	(0.936)	[26.0]	
Enroll in Matched School	0.100	-0.212	-0.520	1.515	0.993	-1.062	88.1	57,489
	(1.177)	(1.196)	(1.411)	(1.393)	(1.269)	(1.464)	[32.4]	
Number of Schools (2016-17)	99	83	53	58	55	60	58	

Notes: This table reports regression coefficients representing assignment to an informational intervention middle school on key outcomes, separated by use. 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 school year who attended randomization sample schools and participated in the Round 1 high school choice process. Use was determined by an affirmative response for indicating sharing the intervention with students or parents in any of the following: a follow-up call with the research team to confirm receipt of the materials, a survey distributed to all guidance counselors, or an interview with a sample of guidance counselors. Some schools have multiple personnel responsible for high school admissions; an affirmative response from any of these staff members was considered as use for that school. Schools without a response to any of the above are included as non-responders, and all control schools are used. The sample is limited to the first cohort, as this is the group with detailed use information, and some treatments were changed in the second year of the intervention. Robust standard errors clustered by middle school are in parentheses (+ p<.10 * p<.05 ** p<.01).

Table 7: Differences within Control Group Schools by School Finder Use (2016-17)

	Used School Finder (1)	Did Not Use SF or No Response (2)	N (3)
(A) Use Indicators			
% of Top 3 Choices from FF List	8.9+	56.0	8,564
	(5.2)	[37.3]	
Any of Top 3 Choices Screened	8.1**	69.0	8,564
	(2.9)	[46.3]	
Any of Top 3 Choices Early Alphabet	-8.6	45.5	8,564
	(5.2)	[49.8]	
Avg. Travel Time Top 3 Choices	-7.8**	34.9	8,564
	(2.6)	[15.7]	
(B) Choices			
Nonoptimal First Choice Strategy	-6.5*	16.6	8,181
	(2.7)	[37.2]	
% of Top 3 Choices Grad Rate <75%	-5.4	25.2	8,548
	(4.4)	[34.8]	
(C) Enrolled School			
Graduation Rate	0.5	79.0	7,314
	(1.3)	[14.2]	
Grad Rate <75%	-3.2	40.9	7,314
	(5.8)	[49.2]	
(D) Other Choice Outcomes			
Matched to 1st Choice	-4.2	45.2	8,564
	(2.7)	[49.8]	
Matched in R1	-1.5	93.9	8,564
	(1.6)	[24.0]	
Enroll in Matched School	1.0	88.7	8,471
	(1.7)	[31.7]	
Number of Schools (2016-17)	18	40	

Notes: This table reports regression coefficients representing use of School Finder, within the control group. All regressions include controls for the variables listed in Table 1, and, in lieu of randomization block by year fixed effects, a borough fixed effect. The estimation sample includes all students present in October of their 8th grade years in the 2016-2017 school year who attended control group schools and participated in the Round 1 high school choice process. School Finder use was determined by an affirmative response from a survey distributed to all guidance counselors or an interview with a sample of guidance counselors. Some schools have multiple personnel responsible for high school admissions; an affirmative response from any of these staff members was considered as use for that school. Schools without a response to any of the above are included as non-responders, and all control schools are used. The sample is limited to the first cohort, as this is the group with detailed use information, and some treatments were changed in the second year of the intervention. Robust standard errors clustered by middle school are in parentheses (+ p<.10 * p<.05 ** p<.01).