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EMERGEing Educational Opportunities: The Effects of Social Capital and Nudging on Selective College Outcomes

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Abstract
There is a growing debate in social science and education policy research on how to improve college access for high-performing students from low-income or first-generation backgrounds. While some studies suggest that providing information to students impacts college access, other studies do not and suggest that students may need more support in the college search and choice processes. Using a regression discontinuity research design with a layered randomized controlled trial, this study examines how information and personal assistance impact SAT scores, college application behaviors, and college enrollment decisions among low-income and first-generation high school students in a large urban school district. The results show that an intensive, multi-year college access program has large, positive effects on applying to a selective college, the number of applications submitted to selective colleges, and enrollment in a selective college. In contrast, a low-touch, general information packet intervention shows null effects on these outcomes. Implications for future nudge interventions and scaling up social capital interventions are discussed.

Keywords
college access, college selectivity, social capital, nudging, low-income students, first-generation college students, regression discontinuity, randomized controlled trial

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Author Contributions
Brian Holzman designed the study, cleaned and analyzed the data, and wrote the paper. Irina Chukhray assisted with the literature review and Courtney Thrash assisted with the analyses.

Introduction

Educational attainment is associated with a variety of social and economic outcomes, including employment, earnings, healthy lifestyles, civic engagement, and life satisfaction (Carnevale et al., 2016; Grodsky & Posselt, 2019; Hout, 2012; Irwin et al., 2023; Ma et al., 2019; Perna, 2005). While educational attainment is often perceived as attending and graduating from college, research has increasingly shown that the type of postsecondary institution students attend (e.g., whether it is highly selective) can enhance life outcomes. For example, scholars have found that enrolling in more selective institutions can increase graduation rates and yield positive labor market outcomes, particularly for Black and Hispanic students and students from low-income backgrounds (D. A. Black & Smith, 2004; Brand & Halaby, 2006; Brand & Xie, 2010; Dale & Krueger, 2002, 2014; Hoekstra, 2009; Long, 2008).

Despite the benefits, students from historically marginalized backgrounds are less likely to apply to and enroll in selective institutions. Pallais and Turner found that "[w]hile 23.5 percent of families with 17-year-old children in the U.S. live in families earning less than $30,000…only 8.2 percent of students in top-ranked private universities and about nine percent of students in flagship state universities are from low-income families" (2006, p. 359). More recently, Hoxby and Avery (2013) showed that only 53% of low-income high-achievers[1] applied to a college with an average achievement similar to their own (i.e., the college's median college entrance exam score lay within 15 percentiles of their own score).

Because low-income high-achievers students are less likely to apply to selective institutions, they may suffer from *academic undermatch*, which is when students attend colleges and universities less competitive than their academic qualifications may allow. While academic

[1] The authors defined low-income high-achievers as students who came from families in the bottom quartile of the income distribution and who scored at or above the 90th percentile on the ACT or SAT.

undermatch is relatively common (Belasco & Trivette, 2015; Smith et al., 2013), it is more pronounced among students from lower socioeconomic backgrounds (Smith et al., 2013) and Black and Hispanic students (S. E. Black et al., 2015).[2] This may be problematic since highly selective institutions spend more per capita on instruction and support services than less selective institutions (Hoxby, 2009). Selective institutions also have more resources and can cover higher shares of students' tuition costs through gifts (Hoxby, 2009), which actually may make it cheaper for students from low-income families to attend a highly selective institution than a less selective institution (Hoxby & Turner, 2013b).

Since highly selective institutions provide more resources to students and because they can result in higher graduation rates and labor market returns, particularly for students from historically marginalized populations, scholars have sought to identify interventions that can address inequalities in college preparation and improve selective college access. For nearly two decades, social scientists have investigated how social capital and nudging interventions can impact college application behaviors and enrollment. Social capital approaches to the college opportunity gap, sometimes called personal assistance interventions, usually involve in-person meetings or sustained support from guidance counselors or other professionals. These approaches largely find positive effects on student outcomes (e.g., Avery, 2013; Stephan & Rosenbaum, 2013), yet they are often costly and difficult to scale up due to capacity constraints, which may be particularly profound in school districts serving low-income and first-generation populations.

Nudge interventions, which convey bits of information to students and families, are an alternative approach to addressing the college opportunity gaps. Although the content, level of

[2] Smith, Pender, and Howell (2013) find that racial and ethnic minority students are less likely to undermatch than White students, but explain that this pattern may be partly mechanical (e.g., minority students have less access to selective colleges based on their academic qualifications).

customization, and delivery system can vary, they may share information and tips on the college application process or provide students and families with concrete information on their financial aid package using mailed information packets or email or text message reminders (e.g., Dynarski et al., 2021; Hoxby & Turner, 2013a). While these approaches are much less intense and sustained than social capital interventions, nudging is much cheaper and easier to scale up, which may appeal to schools and districts in economically marginalized contexts.

In this study, we simultaneously compare the impact of social capital and nudging approaches to education on student outcomes through a regression discontinuity design with a layered randomized controlled trial. Specifically, we examine the causal effects of the EMERGE Fellowship, a college advising program that functions as an intensive social capital intervention and serves high-performing students from low-income and first-generation backgrounds. Leveraging a regression discontinuity design, we compare SAT scores, college application behaviors, and college enrollment rates between students selected to receive intensive guidance in the form of personal assistance from EMERGE program staff to their peers who were close to the cutoff but not selected for EMERGE. For students who fell below the cutoff (i.e., were not admitted to EMERGE), we randomly assigned half to an information packet intervention. These students received two packets of general information on preparing for and taking the SAT and information and tips on the college application process.[3] Therefore, at the cutoff, we can estimate the effects of a social capital intervention (EMERGE) and a nudging intervention (general information packets) on student outcomes, relative to the regular supports the school district is able to provide.

[3] The information packet intervention was separate from EMERGE and designed to evaluate less expensive alternatives to supporting students. It was inspired by Hoxby and Turner's Expanding College Opportunities experiment (2013a), but was very different in the content and level of customization. Information packets were designed by research staff at the Houston Education Research Consortium with input from staff at the Houston Independent School District and EMERGE, as well as Rachel Baker, an associate professor at the University of Pennsylvania.

We find that students admitted to EMERGE were significantly more likely to apply to selective institutions, submit more applications to selective institutions, and enroll in selective institutions. In contrast, the general information packets showed null effects on student outcomes. These findings may inform school and district interventions designed to help high-performing students from low-income and first-generation backgrounds to prepare for, apply to, and enroll in selective colleges and universities. We also hope this study can contribute to the conversation around social capital and nudging in education and provide guidance to researchers, policymakers, and practitioners developing interventions that can address inequities in educational opportunities.

Inequalities in College Preparation and Information

Hossler and Gallagher's seminal college choice model (1987) proposes three phases leading to college enrollment: *predisposition* (the first phase during which the student establishes an interest in going to college, or an aspiration), *search* (the phase stage during which the student gathers information about colleges and develops a choice set), and *choice* (the final phase during which the student decides which college to attend). While the predisposition phase has been studied extensively since the 1960s (Schneider & Saw, 2016), the search and choice stages have received less attention despite increasing college enrollment and completion gaps between students from low- and high-income families (Jackson & Holzman, 2020). The search and choice phases are quite important to the college choice process and encompass a series of steps students should or must complete to prepare for and apply to college that range from achieving the academic qualifications for college (e.g., a high grade point average and a college preparatory curriculum) to searching for information about colleges (e.g., visiting websites and meeting with a high school counselor). Students who complete more of these intermediate steps are more likely to enroll in a four-year college or university (Klasik, 2012). However, racial, ethnic, and family income gaps in step completion remain (Holzman et al., 2020). Further, Holzman et al. (2020) find that inequalities in step completion, specifically achieving the academic qualifications for college and taking a college entrance exam, predict inequalities in selective college enrollment. In a study focused on the role of socioeconomic status (SES) in selective college enrollment and the mediating role of academic preparation, Bastedo and Jaquette (2011) show that while low-SES students are more prepared for college than they were in the past, high-SES students are still even more prepared, allowing high-SES students to maintain their competitive advantage in selective college enrollment over time.

Inequalities in college preparation may be tied to inequalities in what students know about college—not just facts about different institutions, but also how to navigate the college application gauntlet, the series of steps students must complete for admission (Roderick et al., 2009). Grodsky and Jones (2007) find racial, ethnic, and socioeconomic inequalities in the ability of parents to provide an estimate of college tuition. Using a sample of high school students, Nienhusser and Oshio (2017) find racial and ethnic gaps in the accuracy of four-year public and private college tuition estimates. Informational inequalities may affect decision-making like applying to selective colleges. For example, students, particularly those from low-income backgrounds, may lack adequate guidance or an adequate understanding of the difference between sticker price and net cost, leading them to decide that they cannot afford college or forcing them to choose a less selective institution where they may end up paying more (Hoxby & Turner, 2013b).

Students' college knowledge and preparation may affect how they engage in the search and choice phases. Bleemer and Zafar (2018) show that beliefs about college costs and the returns to education predict whether household heads expect their child to attend college. Through an experiment, they find that providing information on the returns to college positively impacts household heads' expectations that their child will attend college; these impacts are more pronounced among low-income and non-college-educated individuals. In a descriptive study of four-year college enrollment, Perna (2000) shows that measures of social and cultural capital—defined as information and resources like parental involvement and help from school staff—are as important as academic performance measures at predicting enrollment decisions for Black and Hispanic students. Therefore, interventions that focus on the search and choice phases, especially those that emphasize institutional type, may be important to develop and study if we aim to increase selective college choice and reduce academic undermatch.

Social Capital and Nudging in College Access

For decades, scholars have examined different strategies to provide historically marginalized populations with information and resources that can support their college enrollment. To address inequities, much of the focus has been on social capital interventions, which aim to supplement the support that students may receive at home or from school. These interventions provide students information and resources through "social interactions and which are distinct from academic or financial resources (e.g., college knowledge, assistance, and social support)" (Stephan & Rosenbaum, 2013, p. 200). Social capital interventions programs can take a variety of forms and be delivered at school or through an external provider like a nonprofit organization. Despite their differences, they tend to provide personalized support to students, in small groups or one-on-one, guiding them through the college search, application, and financial aid processes.

A number of studies have examined coaching models which aim to improve college access. Stephan and Rosenbaum (2013) clarify that coaches are distinct from high school guidance counselors in that they do not necessarily have formal training in school counseling or psychology and that their duties are focused on advising and supporting students in the college search and choice processes. They also employ novel strategies to connect with and serve students, including proactive outreach and group activities. In a difference-in-differences study of a coaching program implemented in 12 high schools in Chicago, Stephan and Rosenbaum (2013) find that students in schools with a coach were more likely to apply to three or more colleges, complete the FAFSA, and plan to attend a specific college after graduating. They also they found some evidence of direct effects on college enrollment, particularly on less selective four-year college enrollment relative to two-year college enrollment among students from marginalized populations or schools. Avery (2013) evaluated a program similar in structure to EMERGE called College Possible, a two-year after-school college coaching program serving low-income high school students with a grade point average (GPA) of 2.0 or higher. Through a randomized controlled trial, he found that students in College Possible were more likely to apply to and enroll in four-year and competitive colleges; there were no effects on ACT scores. Other college coaching or advising studies have found positive effects on college application, enrollment, or choice. These studies have examined the impact of individualized college advising during high school (Avery, 2010; Barr & Castleman, 2021; Carrell & Sacerdote, 2017; Castleman et al., 2020; Castleman & Goodman, 2018), receiving personal assistance in filling out the FAFSA (Bettinger et al., 2012), in-class lessons focused on searching for and applying to college (Hyman, 2023; Oreopoulos & Ford, 2019), providing support to college-intending students during the summer after high school graduation (Castleman et al., 2012, 2015;

Castleman & Page, 2014, 2015), meeting with a high school guidance counselor to discuss college (Belasco, 2013), attending a high school with an advisor or a center decided to assisting students with the college application process (Bettinger & Evans, 2019; Cunha et al., 2018), and attending a high school with a low school-guidance counselor ratio (Hurwitz & Howell, 2014). While many prior studies focus on socioeconomically disadvantaged students, it remains unclear how social capital interventions like college coaching or advising affect higher-achieving students and their admissions to highly selective institutions.

Although social capital interventions which provide students some form of personal assistance are largely helpful, they may be challenging to implement broadly. Social capital interventions may require additional staff and specialized training, which, in turn, demands more funding that many schools and districts in historically marginalized communities lack access to. Even if schools and districts do have funding, providing support to students to attend college or, specifically, highly selective ones, may not be a priority if educators are more concerned with state accountability metrics like test scores and graduation rates. For these reasons, researchers have started to explore less costly and time-consuming alternatives to providing students with guidance in the college application process. Instead of personalized, small group or one-on-one attention, these interventions are called nudges and may be defined as “any aspect of the choice architecture that alters people’s behavior in a predictable way without forbidding any options or significantly changing their economic incentives” (Thaler & Sunstein, 2009, p. 6). Nudges are meant to be cheap and easy to implement and, in education, often provide students, and sometimes their families, with bits of information or reminders to complete a given task.

Results from nudge interventions providing college information are somewhat mixed. Hoxby and Turner (2013a) studied a sample of low-income high-achieving students and randomly assigned them to receive detailed and customized information packets on the college application process. Their intervention, which they estimated to cost six dollars per student, found positive effects on college application, acceptance, and enrollment, particularly at selective institutions. In a follow-up study, the authors showed that treatment group students were more knowledgeable about college, specifically about financial aid (Hoxby & Turner, 2015). Nudge interventions that sent students information about free tuition options at a selective four-year institution (Dynarski et al., 2021), provided information, messages, and limited support with the college application process (Linkow, Parsad, et al., 2021; Martinez et al., 2018; Phillips & Reber, 2022), sent text message reminders to high school students about steps they needed to complete before college application and enrollment (Avery et al., 2021; Castleman & Page, 2015; Page & Gehlbach, 2017), provided information on the returns to schooling (Jensen, 2010), and encouraged students to visit a college information website (Hyman, 2020) have found some positive effects on college application, enrollment, or choice for all students or for students from historically marginalized populations.

However, other nudge interventions designed to impact college application and enrollment have found null effects (Avery et al., 2021; Bergman et al., 2019; Bird et al., 2021; Gurantz et al., 2021; Linkow, Miller, et al., 2021). For example, Gurantz et al. (2021) evaluated a number of College Board experiments that aimed to increase selective college enrollment. In the most comprehensive treatment arm, students received mailers which included customized information on potential colleges to apply to and information and guidance on how to apply to college and for financial aid. Some students were also offered small financial incentives and support like text message reminders to assist with their college applications. The authors, however, found that the College Board's nudge campaigns had no positive effects on any college enrollment outcome. Although the College Board study was inspired by Hoxby and Turner (2013a), it differed in ways like the branding of materials and the sampling frame, which excluded ACT-takers.

Using a sample of high-performing students from low-SES backgrounds in an urban school district, we build on the existing literature by determining whether personal assistance provided by a high school college access program or information provided by general information packets can impact SAT scores and selective college application and enrollment. This study aims to contribute to the literature in three ways. First, it appears to be sole study to focus on high-performing low-SES students and simultaneously compare a social capital intervention to a nudge alternative.[4] Second, the nudge this study evaluates may be considered general, in contrast to the nudges provided in the Hoxby and Turner (2013a) and Gurantz et al. (2021) studies. While customization may be ideal and more effective[5], it can be hard to implement in some contexts, such as schools and districts which lack sophisticated data management systems or staff capacity to develop personalized materials. More general materials may be what these entities are capable of producing. Third, the interventions in this study started early, before the college search and application processes. Moreover, the EMERGE program provided support through the end of high school. These features can help us understand how early and long-lasting interventions can impact selective college choice among high-performing low-SES students. Finally, while this study is not nationally-representative, it does take place in a majority-minority urban school district with a large low-income student population. The wide majority of EMERGE applicants are economically disadvantaged, first-generation college-goers,

[4] Bettinger et al.(2012) and Carrell and Sacerdote (2017) compared social capital interventions to nudge alternatives. In both studies, the social capital approach had positive effects on college outcomes, while the nudge approach had null effects. Neither study focused on high-achieving low-SES students nor selective college outcomes.

[5] Hyman (2020) and Jensen (2010) may be considered examples of nudge interventions that provided general information and found positive effects.

and non-White. This is the type of context large-scale studies are trying to target; however, this study takes place in a school district, perhaps shining a light into how practitioners on the ground may be able to implement college access interventions.

The Interventions

The EMERGE Fellowship (Social Capital Intervention)

The EMERGE Fellowship is a nonprofit organization that aims to empower and prepare talented students from underserved communities, including economically disadvantaged and first-generation college students, to successfully attend and graduate from the nation's top colleges and universities. EMERGE is a personalized approach to college coaching and counseling which provides high school sophomores, juniors, and seniors with school-based, small-group and one-on-one academic year and summer programming. The program is relatively small, which facilitates individualized support; during the 2016-2017 school year, the program served approximately two percent of high school students in the Houston Independent School District and had a student-to-counselor ratio of 7-to-1. During the academic year, EMERGE Fellows participate in biweekly after-school and occasional weekend workshops, while during the summer, students have free opportunities to visit colleges and universities, use vouchers to take a standardized test preparation course[6], and receive college and financial aid application advising. The program is particularly interested in addressing academic undermatch, in which students attend colleges and universities that are less competitive than their academic qualifications may allow.

[6] Due to budget constraints, EMERGE no longer provides students vouchers to take a test preparation course.

EMERGE is a selective and competitive program that requires students to apply for participation in the program in the fall semester of their sophomore year of high school.[7] In the 2016-17 academic year, the application process had two phases. In Phase I, students submitted an application that included their demographic and socioeconomic background, participation and leadership in extracurricular activities, and a short essay response. Administrative staff gathered information on applicants' academic performance, including their grade point average (GPA) and PSAT score. Two groups underrepresented in EMERGE, male and Black students, both received extra points on their applications. Each component of the Phase I application was scored as follows:

[Insert Table 1 Here]

Each school in the district was assigned a specified number of students that they could select for advancement to Phase II of the application process. The number of slots allotted to each school was based on the gradient score, a district-designed metric based on GPA and PSAT scores from the previous 10th grade cohort that was used to estimate the number of students who might apply to EMERGE. Within each school, students were sorted and ranked by their Phase I scores and the top students (based on the number of slots allotted to the school) advanced to Phase II.

Students who advanced to Phase II of the application process participated in in-person interviews with EMERGE staff, HISD staff, and volunteers. Interviews were short, lasting 10-15 minutes, and required students to respond to questions about the classes they enjoyed, something they were passionate about, their family, a time in which they faced a difficult situation, and their

[7] The application process has since shifted to the spring semester of the sophomore year, shortening the program duration from 2.5 to 2 years.

goals. A rubric was used to score the interview on a 30-point scale based on each applicant's demonstrated resilience, investment, and passion.

Each applicant received a total score which was a sum of their Phase I and Phase II scores, with a maximum possible score of 125 points. Each school in the district was assigned a specified number of slots for EMERGE students. Students within each school were once again sorted and ranked by their total scores, and the students with the top scores (based on the number of slots allotted) were selected for the EMERGE Fellowship.

General Information Packets on the SAT and College Application Process (Nudge Alternative)

EMERGE applicants not accepted into the program were randomly assigned into two groups. One group of students served as the control group and received no additional intervention (business-as-usual group). The business-as-usual group continued to have access to regular school support like College Success Advisors (CSAs), a whole-school postsecondary initiative in the district. The second group received a series of packets with general information on the SAT and the college application process (information packet group). We worked collaboratively with the EMERGE nonprofit organization and HISD staff to determine the content to be included in the information packets and to design and review them. Each packet included a non-personalized cover sheet that provided a short description of the information within. Unlike previous studies that personalized packets for each student, our packets contained profiles of five colleges and universities to provide a sense of how institutions might differ with respect to factors likes the average SAT scores of students, net costs, and application deadlines. The packets were delivered through an online portal used by the district (Naviance), email, and mail. The six-page SAT information packet was distributed in the fall semester of students' junior year (fall 2017), while the 11-page college application process information packet was distributed in the spring semester (spring 2018). The information packet group also continued to have access to regular school support like the CSAs. By splitting the non-EMERGE students in half, we can simultaneously compare the impact of EMERGE to two treatment conditions and potentially demonstrate how the program is not only more effective than a true control condition, but also a lower-touch, general information alternative.

Research Questions

Given the focus of EMERGE and the general information packets on steps in the college search and choice phases, as well as EMERGE's stated goal of helping talented but underserved youth attend the nation's top colleges, we address the following research question:

- What are the effects of a social capital intervention (EMERGE) and a nudge alternative (general information packets) on low-SES high-performing students' SAT scores, selective college application behaviors, and selective college enrollment?

Data

Sample

Our study focuses on EMERGE applicants in the Houston Independent School District (HISD), which provided us with administrative records and information collected by EMERGE staff during the application and selection process. The sample consists of students who applied to the EMERGE Fellowship as sophomores in fall 2016. Students applied early in the fall semester of 2016 and were notified of their acceptance in late fall 2016. In spring 2017, students began participating in EMERGE programming, which continued through their high school graduation in spring 2019. The total applicant pool includes 1,078 students. However, some students are missing outcome data, so the sample size varies by outcome: 1,017 for SAT outcomes, 1,026 for college application outcomes, and 1,030 for college enrollment outcomes.[8]

As discussed earlier, HISD staff rated EMERGE applicants on a 125-point scale and schools sorted students by their total score, resulting in a rank. Each school was allocated a specific number of slots for EMERGE, which was determined at the district-level and tied to school size and the pool of high-performing low-SES students. If a school was assigned 12 slots for EMERGE, the 12 top-ranked students were offered admission. It must be noted that the top-ranked students at one school might have different total scores than the top-ranked students at another school. HISD decided against setting a district-wide cutoff for the program because they wanted EMERGE to serve all schools in the district. Given patterns of residential segregation and the relationship between socioeconomic status and academic performance, if the district had set a single, district-wide cutoff for admission, then all the EMERGE applicants would be clustered in the most-advantaged schools, whereas the least-advantaged schools would have few to no applicants. Using the school-based rank approach, it is possible that students above the cutoff at a more disadvantaged school might have fallen below the cutoff at less advantaged school. Therefore, students admitted to EMERGE might be considered high-performing *relative to* applicants at their school. We consider this a virtue because students above their school-based cutoff may represent different points in the academic performance distribution. Therefore, the school-specific cutoffs make any effects we detect more likely to be attributed to the interventions rather than pre-treatment characteristics like test scores and grades. Because of the school-based rank approach, all analyses center student ranks around each school's cutoff and all

[8] As a robustness check, we estimated treatment effects by imputing missing outcome data (see Appendix Table 5). Specifically, we estimated models in which we assign students missing data to the 25th or the 75th percentile (for continuous outcomes) or to the 0 or 1 category (for binary outcomes). These results are similar to those discussed, although the magnitude and significance of effects are attenuated. When assigning missing Top 1 Barron's college enrollment data to 1, the effect on enrollment of EMERGE is no longer statistically significant but remains 0.11 points; this is the only case when the imputed estimate is rendered insignificant.

models include school site fixed-effects. Students at or above their school's centered cutoff were admitted to EMERGE (centered rank ≥ 0), whereas students below their school's centered cutoff were not admitted to EMERGE (centered rank < 0).

Students who fell below their school's cutoff were randomly assigned to the information packet intervention or the business-as-usual condition through blocked, within-school randomization. Students assigned to the information packet group were scheduled to receive information packets on the SAT in fall 2017 and the college application process in spring 2018. These packets were sent to students by mail, by email, and through Naviance, a college and career readiness software provider HISD used.

EMERGE staff reserved the right to move students in and out of the program if they failed to meet eligibility requirements for participation. Some students declined to participate in the program or moved out of the district. Although EMERGE did not restrict any HISD student from applying, after the admission process, they decided that they would not serve students who were neither economically disadvantaged nor a first-generation college-goer. Students who fell above their school's cutoff and were not low-SES were not formally offered admission. Schools then went down their waitlist (i.e., further down the ranked total score) to select eligible students. EMERGE was also dissatisfied with the lack of representation of Black male students in their program. They decided to admit a special pool of Black males who fell just below their schools' cutoffs into the program. Obviously, this post-assignment movement is not ideal for a research study. However, the compliance rate across all treatments through September 1, 2017 was high, about 92 percent.[9] Unfortunately, the research team was not able to acquire program participation or other forms of post-assignment movement past this date from HISD. We do know that EMERGE had requirements for continued participation in the program, such as maintaining high grades and attending after-school advising sessions, and might remove students from the program if they failed to meet their standards. There were also minor challenges with the distribution of mailed information packets on the college application process, and we also got a fair amount of email bouncebacks and return-to-sender envelopes from both of the packet distributions. The research team initially intended to examine local average treatment effects (LATE) through the end of high school (e.g., initial assignment as an instrument for complete EMERGE participation or months in EMERGE), but incomplete compliance data past fall 2017 prohibits us from doing that. As a robustness check, we estimate LATE with a fuzzy regression discontinuity model and the fall 2017 partial compliance data. These findings are consistent with our main intent-to-treat estimates from the sharp regression discontinuity and are available in the Appendix.

Dependent Variables

In the analyses, we examined outcomes related to three areas of interest: SAT scores, selective college application behaviors, and selective college enrollment. First, math, verbal, and composite SAT scores were examined to determine if EMERGE, which provided a voucher so students could take an SAT preparation course, or the SAT information packet increased test

[9] September 2017 compliance rates by initial assignment were 94% for business-as-usual students, 94% for information packet students, and 86% for EMERGE students. The compliance rate within eight points of the cutoff was 86%; by initial assignment, these rates were 84% for business-as-usual students, 89% for information packet students, and 86% for EMERGE students. The reason why compliance rates might have been lower near the cutoff is that most of the post-assignment treatment shifting done by EMERGE staff occurred around the cutoff. Although this was not ideal from a researcher's perspective, EMERGE staff shifted low-SES students into EMERGE, likely biasing treatment effects downward.

scores. Math scores ranged from 310 to 800, verbal scores ranged from 310 to 780, and composite scores ranged from 690 to 1,580.

Second, we analyzed application behavior to Top 1 and Top 2 Barron's colleges and universities. Top 1 Barron's colleges and universities include those that are defined as "most competitive" according to the Barron's College Admissions Selector.[10] These colleges and universities typically require a high school rank in the top 10 to 20 percent, average grades of an A to a B+, SAT scores between 655 and 800, and ACT scores of 29 or above, as well as admit fewer than a third of all applicants. Top 2 Barron's colleges and universities include those that are defined as "highly competitive" according to Barron's. These colleges and universities typically require a high school rank in the top 20 to 35 percent, average grades of a B+ to a B, SAT scores between 620 and 654, ACT scores between 27 and 28, as well as admit between one-third and a one-half of all applicants. Using college application data provided by the district[11] and our Barron's rating data, we generated four measures of selective college application. Two were binary: whether students applied to any Top 1 Barron's college or any Top 1 or Top 2 Barron's college, with the latter being a slightly more expansive definition of *selective*. We also examined the number of selective college applications submitted: the number submitted to Top 1 Barron's colleges and the number submitted to Top 1 or Top 2 Barron's colleges. These four outcomes allow us to determine whether EMERGE or the college application process information packet increased the likelihood of selective college application and the total number

[10] We hand-coded the College Admissions Selector from Profiles of American Colleges 2015. This was published near the time of high school entry for this cohort of students and was available to the research team at the launch of the study. Barron's ratings do not change much over time, particularly over short periods, so it is unlikely that the use of the 2015 ratings affect the analyses.

[11] The district provided the research team data from ApplyTX, a portal where students can apply to public or private institutions in Texas, as well as data from the Common Application, which can capture additional institutions across the country.

of selective applications submitted; the latter might even be considered a proxy of how determined a student was to attend a selective institution.

Third, we examined whether students enrolled in a Top 1 Barron's college and or a Top 1 or Top 2 Barron's college during the fall semester following high school graduation. These data came through the district and were originally from the National Student Clearinghouse. These outcomes allowed us to determine whether EMERGE or the information packets increased enrollment in selective institutions.

Independent Variables

The primary independent variable of interest was categorical and measured each student's initial treatment assignment: business-as-usual, general information packets, and EMERGE. Initial assignment was first based on each student's rank within their school, which we centered in analyses. Students with a rank of 0 or above were assigned to EMERGE (N = 262). The remaining 816 students with ranks below 0 were randomly assigned to the business-as-usual group (N = 407) or the general information packets group (N = 409).

The control variables in all analyses included age, female, and race/ethnicity, as well as whether the student was foreign-born, an English learner, in special education, economically disadvantaged, or first-generation. We also controlled for whether a student had a sibling who previously participated in EMERGE. While the rank variable comprised a number of EMERGE application components, including GPA and PSAT scores, we did control for two additional academic measures: the number of advanced courses a student took and whether they had declared a STEM endorsement. Advanced courses included pre-Advanced Placement (AP), pre-International Baccalaureate (IB), AP, IB, and academic dual credit courses.[12] Endorsements are

[12] Academic dual credit courses are dual credit courses that are more academic in nature (i.e., not considered a Career and Technical Education course).

part of the state's high school graduation requirements and are, more or less, a high school concentration or major. There are five endorsement options, but STEM (science, technology, engineering, and math) is the most aligned to selective college admissions (Holzman & Lewis, 2020).

Table 1 presents summary statistics for the applicant pool. The average composite SAT score for EMERGE applicants was 1,109. Nearly two-fifths applied to at least one Top 1 Barron's college and the average number of Top 1 Barron's college applications submitted was about 1.6. Nine percent of applicants ended up enrolling in a Top 1 Barron's college. About one-quarter of applicants were admitted to EMERGE (had a centered rank of 0 or higher). The remaining three-quarters had a centered rank below zero and were evenly divided into the business-as-usual (38 percent) and information packets (38 percent) groups. The average centered rank was about 11 spots below the centered cutoff; the skew is related to the fact that the majority of applicants were not admitted to EMERGE.

[Insert Table 2 Here]

The applicant pool was majority female (67 percent). Most students were Hispanic (63 percent), although one-in-five were Black and 8 percent were Asian. Seventy-seven percent of applicants were economically disadvantaged and 72 percent were first-generation college goers. Economically disadvantaged students were eligible for free or reduced-price lunch or participated in other federal anti-poverty programs, while first-generation was defined as students whose parents did not hold a four-year college degree from a U.S. institution. About three-fifths of applicants were both economically disadvantaged and first-generation and only one-tenth were neither; this socioeconomic composition reflects the goals of the EMERGE program, as well as the school district context, where 78 percent of students were economically disadvantaged in the 2016-2017 school year.

Identification Strategy

We used a regression discontinuity (RD) design with a layered randomized control trial to compare the outcomes of applicants initially assigned to EMERGE, the general information packet intervention, and the business-as-usual condition. Specifically, the RD design compared the outcomes of applicants who were just eligible for admission to EMERGE (i.e., a centered rank at or below 0) to the outcomes of EMERGE applicants just ineligible for admission to EMERGE (i.e., a centered rank below 0). RD designs have strong causal warrant: by focusing on students near an arbitrarily-set cutoff used for admission, we can minimize differences between students admitted to EMERGE and those who were not and can rigorously and accurately measure treatment effects. After accounting for students' centered ranks, if there is a significant jump in the cutoff for EMERGE admission, then we can say with confidence that there is a positive effect of EMERGE on student outcomes. Moreover, because students ineligible for admission were randomly assigned to business-as-usual or information packet groups, we can compare the outcomes of students admitted to EMERGE to both these conditions, as well as compare the outcomes of the information packet group to the business-as-usual group.

We used a sharp RD design where we examine the intent-to-treat among EMERGE applicants, instead of actual participation in the program. As mentioned earlier, this is because we only had partial compliance data through September 1, 2017. Among applicants not offered admission to EMERGE, we randomly assigned students within schools to the business-as-usual and information packets groups. Because of random assignment, we can compare EMERGE admission to both these non-EMERGE conditions, as well as compare these two conditions to one another.

Because each school had its own ranking system and number of slots available to EMERGE students, each of the 42 school sites had its own cutoff for inclusion in the program. As discussed earlier, from HISD's perspective, a single cutoff for inclusion in or exclusion from the program would not ensure equitable representation across the district. To account for the separate process that occurred at each school site, student ranks were centered by school and all models included school site fixed-effects. The analytic model used in the analyses is the following[13]:

$$Y_{ij} = \beta_0 + \beta_1 INFOPACK_{ij} + \beta_2 \, EMERGE_{ij} + \beta_3 \, RANK_{ij} + \theta_{ij} + \Psi_j + \varepsilon_{ij} \qquad (1)$$

where Y_{ij} is a student outcome (e.g., Top 1 college enrollment) for student i in school j. The variables $INFOPACK_{ij}$ and $EMERGE_{ij}$ measure the impact of being assigned to the information packet and EMERGE groups, relative to the business-as-usual group (reference category). $EMERGE_{ij}$ is basically a dummy variable signifying whether a student had a centered rank at or above 0. All students in the information packet and business-as-usual groups had a centered rank below 0, so the $INFOPACK_{ij}$ dummy measures whether a student was randomly assigned to the information packet group. $RANK_{ij}$ is a continuous measure of each student's rank, centered by school. The variable θ_{ij} is a vector of student-level covariates, such as economically

[13] In addition to the regression model shown here, we estimated ITT effects and LATE using a local polynomial model and an 8-point bandwidth (we used the `rdrobust` command in Stata; see Appendix Table 6). While these models could not incorporate the multiple treatment conditions (we had to dichotomize as EMERGE vs. non-EMERGE), we did find similar patterns, although the magnitude of effects was slightly smaller.

disadvantaged and first-generation, while Ψ_j represents school-fixed effects. All models use linear regression and cluster standard errors by school site.

Most analyses focus on an 8-point bandwidth from each school's cutoff (centered at 0). We conducted tests to determine the optimal bandwidth using methods developed by Ludwig and Miller (2007) and Imbens and Kalyanaraman (2012). These tests appeared to suggest an ±8-point bandwidth fit best. We do, however, present results at other bandwidths using graphs that plot point estimates and standard errors for all bandwidths from ±2 to ±59 (all the data) from the cutoff. Using the preferred bandwidth, we tested different functional forms of centered rank, the forcing variable that determined admission to EMERGE: 1) a linear model with the same slope on both sides of the cutoff, 2) a linear model with different slopes on each side of the cutoff, 3) a quadratic model with the same slope on both sides, and 4) a quadratic model with different slopes on each side. These tests consistently showed that the simplest model, linear with the same slope on both sides of the cutoff (shown in Equation 1), fit the data best.

The primary assumption of the RD design is that students with similar centered ranks who are just below the cutoff for EMERGE admission are similar and comparable to students just above the cutoff; lying on one side of the cutoff versus the other is quasi-random and cannot be gamed. For example, if schools purposefully chose slightly higher-performing students for EMERGE such that the students one point above the cutoff had much higher PSAT scores than students one point below the cutoff, then we might worry that the effect of EMERGE would actually be tied to the higher test scores of EMERGE students rather than the utility of the program itself. There are two ways we addressed this concern. First, to examine whether students were manipulated by themselves or others to be on one side of the cutoff, we examined histograms and conducted a density test (McCrary, 2008). These steps could show us whether heaping occurred around the cutoff (e.g., a sizeable share of students receiving scores that placed them just above each site's cutoff for EMERGE admission). Given that the district allocated slots and ranked students into those slots, heaping might be less of a concern. Indeed, in Figure 1A, which shows the distribution of ranks within 8 points of the cutoff, we see that there is no meaningful change at the cutoff (centered at 0 and denoted with a dashed line). Figure 1B plots a McCrary density test and confirms this pattern. The confidence intervals around the cutoff show that we cannot reject the null hypothesis that there is no change in the density of the rank distribution around the cutoff.

[Insert Figure 1 Here]

Next, we examined pre-treatment covariate balance by initial assignment: business-as-usual, information packets, and EMERGE. If a covariate significantly changes at the cutoff (i.e., EMERGE compared to non-EMERGE groups), then that may suggest the presence of a confounder, specifically that something else is changing at the cutoff that may explain subsequent treatment effects. If covariates are similar below and above the cutoff, then we can be more confident that the quasi-random assumption holds and that there is not an unobserved confounder driving treatment assignment and, ultimately, the outcomes of interest. Table 3 presents results from regression discontinuity models in which each pre-treatment covariate serves as the dependent variable. The sample was limited to observations within an 8-point bandwidth and each model controlled for treatment condition, centered rank, and school site fixed-effects. Because there are three treatment conditions, we present results from three comparisons: business-as-usual (ref.) vs. information packets, business-as-usual (ref.) vs. EMERGE, and information packets (ref.) vs. EMERGE. There are no statistically significant coefficients in pre-treatment covariates in the first comparison; this is not surprising since treatment assignment to the business-as-usual or information packets group was determined through randomization. When comparing EMERGE to business-as-usual and EMERGE to information packets, there are marginally significant differences in foreign-born status (i.e., EMERGE students were more likely to be immigrants). Given multiple testing and the potential for spurious correlations, we also employed seemingly unrelated regression as an omnibus test of group differences. The F-statistics from our seemingly unrelated regressions are available in the final row of the table and show that we cannot reject the null hypothesis of no group differences.

[Insert Table 3 Here]

Results

Main Results

Table 4 presents the main regression discontinuity results estimating the effect of general information packets and EMERGE admission, relative to business-as-usual, on SAT scores, selective college application behaviors, and selective college enrollment. In terms of SAT scores (composite, math, verbal; in Panel A), we find that neither the information packet nor EMERGE improved outcomes for students, relative to business-as-usual. Although the coefficients are positive for both treatment conditions, they are relatively small in magnitude and fail to reach statistical significance at conventional levels.

[Insert Table 4 Here]

The next series of results in Panel B focused on college application behaviors. First, we find that EMERGE students were more likely to submit an application to a selective institution. Compared to business-as-usual students, EMERGE increased the likelihood of applying to a Top 1 Barron's college by 19 percentage points (Cohen's *d* effect size of 0.40 SD) and the likelihood of applying to a Top 1 or Top 2 Barron's college by 18 percentage points (Cohen's *d* effect size of 0.44 SD). The effect of the general information packet intervention, while positive, was smaller and failed to reach statistical significance. Next in Panel C, we examined the number of selective college applications submitted. In both models, we find that EMERGE had a positive impact on the number of applications submitted; specifically, EMERGE students applied to 1.6 more Top 1 Barron's colleges (Cohen's *d* effect size of 0.47 SD) and 2.7 more Top 1 or Top 2 Barron's colleges (Cohen's *d* effect size of 0.61 SD). Again, the effect of the information packet intervention was closer to zero and failed to reach significance.

Finally, Panel D shows the effects of general information packets and EMERGE on selective college enrollment the fall following high school. In both cases, we see strong and positive effects of EMERGE admission: a 15-percentage point effect on enrollment in a Top 1 Barron's college (Cohen's *d* effect size of 0.43 SD) and a 19-percentage point effect on enrollment in a Top 1 or Top 2 Barron's college (Cohen's *d* effect size of 0.44 SD). We found no effect of the information packet on either outcome.

Figure 2 plots RD estimates for selected SAT score, college application behavior, and college enrollment outcomes. In each figure, the x-axis shows the centered rank. The dotted vertical line is the rank variable, centered at zero, which is the cutoff for EMERGE admission. Observations are binned in groups of two centered rank points, with business-as-usual observations denoted by an open-circle (o), information packet observations detonated by an x (×), and EMERGE observations denoted by a plus-sign (+). In Figure 2A, it is clear that there is no jump in SAT composite scores between students in the two groups below the EMERGE cutoff and students above the cutoff; figures are similar for SAT math and verbal scores and available from the authors upon request. In contrast, Figures 2B, 2C, and 2D show clear discontinuities at the cutoff, indicating that EMERGE had a positive impact on Top 1 Barron's college application, the number of Top 1 Barron's college applications submitted, and Top 1 Barron's college enrollment; figures are similar for Top 1 or Top 2 Barron's college outcomes and available from the author upon request. To provide some intuition, we generated linear predictions of the outcomes from the models. These predictions show that 47 percent of business-as-usual and 48 percent of information packet students applied to a Top 1 Barron's college; in contrast, 66 percent of EMERGE students did so. EMERGE students also submitted nearly double the number of applications to Top 1 Barron's colleges (3.2) than business-as-usual (1.7) and information packet (1.6) students. Finally, while eight and nine percent of business-as-usual and information packet students, respectively, enrolled in a Top 1 Barron's college, EMERGE students nearly tripled those selective enrollment rates with 24 percent choosing to attend a Top 1 institution.

Post-Hoc Subgroup Analyses

In addition to the main effects, we examined whether the impacts of being assigned to EMERGE or the general information packet treatment conditions varied by subgroup. Specifically, we examined gender (male and female students), race/ethnicity (Black and Hispanic students), and high school context. In terms of race/ethnicity, there was not sufficient sample size to examine subgroup results for other racial and ethnic groups. In terms of high school context, we created a dichotomous measure of the school-level four-year college enrollment rate distribution, with low college-going schools defined as the bottom three quintiles of the distribution (9-44 percent of graduates attending a four-year institution) and high college-going schools defined as the top two quintiles (44-85 percent of graduates attending a four-year institution) This measure might proxy college-going culture, so the benefits of social capital and nudging might matter more or less in different types of schools. The subgroup analyses are estimated using separate models (e.g., estimating effects among males and females separately).

We want to note that these post-hoc analyses reduce the sample size, so readers should interpret the findings with caution. While the tables note *p*-values less than 0.20, in the interpretation below, we focus on broad patterns rather than specific statistical tests. In terms of SAT scores, we did not find any notable subgroup results. Like the main results discussed earlier, there appeared to be no effect of EMERGE or the information packets on SAT scores for any of the subgroups examined. Tables 5 and 6 present the subgroup results for college application behaviors and enrollment.

[Insert Table 5 Here]

[Insert Table 6 Here]

In terms of gender, we found that the effect of EMERGE on college application and enrollment behaviors appeared concentrated among female students. Female students admitted to EMERGE were more likely to apply to and eventually enroll in selective colleges and universities, relative to female students not admitted to EMERGE. In contrast, the patterns were so pronounced among male students, particularly for application outcomes. Today, female students are more likely to enroll in and complete college than male students, a pattern that reversed among birth cohorts born in the mid-20th century (Jackson & Holzman, 2020). Even within HISD, female students were more likely to apply to more colleges, enroll in college, and persist through the first semester of college than male students (Thrash et al., 2020). There is some research that shows how social capital interventions affect male and female students before and during college in different ways (Angrist et al., 2009; Carrell & Sacerdote, 2017; Ellis & Gershenson, 2020; Gentry et al., 2023). For example, Carrell and Sacerdote (2017) evaluate an intensive mentoring program for high school seniors at risk of not applying to college and find strong effects on any college and four-year college enrollment for women but not men. One potential reason why they find heterogenous treatment effects is that "[h]igh school educated men are receiving signals from the labor market that they will have strong earnings even without a college degree" (Carrell & Sacerdote, 2017, p. 142).

For Black and Hispanic students, we found positive effects of EMERGE on applying to selective colleges and universities. Analyses also suggested Black and Hispanic EMERGE students submitted more applications to selective institutions. However, the effects on enrollment differed by race/ethnicity. While the effect of EMERGE on selective college enrollment for Hispanic students remained positive, the effect for Black students was negligible. For Black students, a desire to learn about Black culture and history and to connect with the Black community may lead many to consider attending Historically Black Colleges and Universities (HBCUs) (Freeman, 1999). Currently, no HBCUs fall in the Top 1 or 2 Barron's categories. To explore whether HBCU enrollment might explain the negligible effect of EMERGE on selective college enrollment among Black students, we took a close look at students who satisfied three conditions: 1) was a Black student admitted to EMERGE, 2) applied to at least one Top 1 or 2 Barron's college, and 3) did not enroll in a Top 1 or 2 Barron's college. Among this population of students, 46 percent ended up enrolling in an HBCU. Therefore, HBCU enrollment might explain why EMERGE's effects on enrollment differed by race/ethnicity.

Finally, we found some evidence that the effect of EMERGE on applications were stronger for students in high schools with lower college-going rates. EMERGE students who attended schools with lower college-going rates were more likely to apply to selective colleges and universities, relative to business-as-usual students at these schools. In contrast, these patterns did not appear for EMERGE students who attended schools with higher college-going rates. To some extent, the effect of EMERGE on the number of college applications submitted was concentrated among students who attended schools with lower college-going rates, as well. The effects on enrollment were similar at both types of school, however. Still, these findings suggest that EMERGE may be more impactful at schools with a less-pronounced college-going culture. In the absence of EMERGE, high-performing students at high schools with a less-pronounced college going-culture may not have as much exposure to information or access to support that can enable them to apply to selective institutions; EMERGE may fill in the gap for students at these high schools.

Robustness Checks

The main results presented use an 8-point bandwidth around the cutoff. We estimated effects for additional bandwidths ranging from 2 points from the cutoff to the entire sample (no bandwidth restriction); these effects are plotted in Figure 3.[14] In the figures, the effects are positive for the application and enrollment outcomes at all bandwidths (the box notes the treatment effect at our preferred bandwidth). However, the confidence intervals at narrower bandwidths are noisier and do not reach statistical significance at a 95 percent confidence interval. We investigated this further through power analyses, which can be found in Figure 4. This power analyses used the PowerUp! tool (Dong & Maynard, 2013) and helped us identify the minimum detectable effect size for the sample. Basically, we found that the minimum detectable effect size at the narrowest bandwidth approached 0.44 standard deviations. Even though the main effects with our preferred specification were quite large, the power analyses suggest that it might not be surprising that the effects did not reach statistical significance at very narrow

[14] Plot-over-bandwidth graphs are available for SAT math and verbal scores, Top 1 or Top 2 Barron's outcomes, as well as for the general information packet treatment group upon request.

bandwidths from the cutoff. Despite sample size limitations and the low power at narrow bandwidths, the graphs which plot effects over different bandwidths show fairly positive and consistent effects of EMERGE on college application and enrollment.

[Insert Figure 3 Here]

[Insert Figure 4 Here]

As mentioned earlier, we received compliance data for the first six months of the program. For the remaining two years of the program, the school district did not provide us with additional compliance information. We did, however, find that within the first six months of the program, 92 percent of students complied with their initial treatment assignment. We also speculate that most of the movement between treatment conditions occurred during the first six months (e.g., students choosing not to accept EMERGE's offer of admission and replacement students chosen from the waitlist.). Even though our compliance data are incomplete, as a robustness check, we use a fuzzy RD design to determine the effect of the treatment among students who fully complied with their assignment to the EMERGE, information packets, and business-as-usual groups. Results for the sharp RD design and the fuzzy RD design are consistent (see Appendix Table 1) and reinforce our confidence that admission to the EMERGE program yielded positive effects on college application behaviors and enrollment.

HISD implemented a number of college access initiatives during the study period, including hiring College Success Advisors (CSAs), part- or full-time counselors at each high school who would provide students, primarily 12th graders, with assistance during the college application process. One may be concerned that the effects we detect are a result of these initiatives rather than EMERGE. To test this proposition, we estimated models that predicted any college enrollment, any four-year college enrollment, and enrollment in Top 1, 2, or 3 Barron's institution. Since EMERGE is uniquely focused on encouraging students to enroll in the nation's most selective institutions, we may expect there to be null to smaller effects on these broader, less selective categories. This is because EMERGE students are not at risk of non-enrollment or enrollment at a community college—they are high-performing and very likely to enroll—but at risk of enrolling in at a four-year institution of low to moderate selectivity. These results are shown in Appendix Table 2. With one exception (Top 1, 2, or 3 Barron's college application), we found no effects of the information packet intervention on these less selective college outcomes. The effects of EMERGE on less selective college application show negative relationships with marginal or no statistical significance. There do appear to be positive effects on the number of applications submitted to all three categories of less selective institutions. The coefficients are similar in magnitude to the Top 1 or 2 coefficient shown in Table 4, so it is possible that these effects are driven by applications to more selective institutions. Another possibility is that as students submit more applications to more selective colleges, they also submit more applications to less selective colleges, especially if they want safety schools to choose from. That said, we find no effects of EMERGE on less selective college enrollment. Overall, these patterns suggest that EMERGE is providing students with a unique and additional benefit; specifically, it helps students apply to and enroll in selective colleges over and above what their schools provide through business-as-usual services like the CSAs.

In another check, we estimated models that placed the admission cutoff at placebo thresholds. For example, instead of setting treatment assignment at a centered rank of 0, we shifted treatment assignment backward and forward in the rank distribution: -4, -2, -2, -1, +1, and +2 points relative to the cutoff. Effects estimated with these new thresholds, which hold less relevance to treatment assignment in real life, should be null or small. Appendix Table 3 shows these results. For the most part, effects estimated at placebo thresholds are null, marginally significant, or, if significant, smaller in magnitude. It is possible that effects estimated when we shifted the placebo threshold back to -1 or -2 ranks reflected the fact that a number of students from the waitlist were shifted into the program following initial assignment. In terms of enrollment, we see positive effects when we shifted the placebo threshold +2 points, but interestingly not when we shift it +1 points. The effect at +2 points may reflect small sample size.

Finally, we estimated effects using inverse probability of treatment (IPT) weights among individuals within 8 points of the cutoff (see Appendix Table 4). First, we used multinomial logistic regression models to predict treatment assignment, controlling for GPA, PSAT scores, other covariates, and site fixed-effects; we did not control for the rank because of collinearity with treatment assignment. Next, we generated inverse probabilities, truncating weights at the 90th percentile to avoid having outlier observations drive results. Finally, we estimated linear regression models of treatment effects, which were doubly-robust—they were weighted by our IPT weight and included control variables to reduce residual bias and increase precision. The effects of EMERGE using IPT weighting were similar in magnitude, or larger, than those produced by the regression discontinuity models. Following each regression, we used the KonFound-It! command to estimate the percent of bias needed to invalidate the results (Frank, 2000; Frank et al., 2013). For enrollment outcomes, we would have to replace a little more than half the cases with an effect of 0 to invalidate the results. For application outcomes, there was a little more variation: to nullify results, we would have to replace 21 percent of cases at the low end (any Top 1 or 2 Barron's college application) to 57 percent of cases at the high end (number of applications to Top 1 or 2 Barron's colleges). Overall, this alternative identification strategy seems to support the results derived from the regression discontinuity.[15]

Discussion

The results presented in this study indicate that applying and being accepted into EMERGE, a district-run program for high-performing low-SES students, led to statistically significant improvements in applying to and eventually enrolling in selective colleges and universities. Students involved in EMERGE were more likely to apply to selective colleges and universities, submitted a greater number of applications to selective colleges and universities, and were more likely to enroll at selective colleges and universities than their non-EMERGE counterparts in the business-as-usual and general information packet groups. These results are encouraging and suggest that social capital, specifically intensive, personalized assistance programs, can help students from historically underrepresented backgrounds navigate the college application gauntlet. Moreover, these findings are in line with prior studies that have found that social capital interventions which provide personal assistance with the college application or financial aid processes improve college outcomes (e.g., Avery, 2013; Stephan & Rosenbaum, 2013).

Despite the positive effect of EMERGE on applying to selective colleges, the number of selective college applications submitted, and selective college enrollment, we did not find any effects on SAT math, verbal, and composite test scores. During the study period, students who participated in EMERGE received vouchers to take an SAT preparation course at a local test preparation company. According to EMERGE staff in HISD, nearly all students used the

[15] In addition to the robustness checks described, we 1) estimated effects using pooled school site fixed-effects (a very slight alteration of school site fixed-effects that was necessary for information packet treatment randomization), 2) estimated effects among interviewed students only (this dropped fewer than 30 cases from the main results), 3) estimated effects using a binary indicator of EMERGE assignment (i.e., combining the business-as-usual and information packet categories), and 4) estimated effects using the centered total score as the running variable instead of centered rank. These results were largely similar to those from our preferred specification.

vouchers and enrolled in a course. However, since EMERGE effectively outsourced this training to an external entity, there are many unknowns about students' participation in the test preparation course, as well as the quality of the provider. We did not have access to information on how often students attended the course or whether they completed assignments or prepared for exams. We also did not acquire details on how comprehensive or rigorous the test preparation course students took was or whether the instructors were well-trained and implemented effective pedagogical strategies. Fundamentally, any support provided by the test preparation company was likely narrower and less personal than the multi-year, comprehensive services that the EMERGE program provided. Moreover, given that applying to EMERGE may reflect an interest in selective college enrollment, EMERGE applicants who are ultimately not admitted may find alternative ways to prepare themselves for the selective college application process, including enrolling in a test preparation course or related service. Our data cannot determine whether students in the comparison groups completed a test preparation course or related service on their own. Regardless, despite not finding significant effects of EMERGE on SAT scores, it should be noted that students in all three treatment groups were high-performing and scored higher than the national average (College Board, 2022).

Given potential concerns with the cost and scalability of intensive, personalized social capital interventions, we sought to test whether a low-cost nudge in the form of two general information packets might yield positive effects on student outcomes. The information packet component in this study, however, did not find positive effects on any outcome, ranging from SAT scores to selective enrollment. This result is in line with the Bergman et al. (2019) study of a general, information-only intervention, but stands in contrast to other general information approaches that do find positive effects (Hyman, 2020; Jensen, 2010).

There are a multitude of reasons why the information packets used in this study were ineffective. First, the information our packets provided was general and less comprehensive than packets used in other nudge interventions. For example, according to Hoxby and Turner, their packets were semi-customized: using their "rich database infrastructure and intervention framework, [they] can ensure that students see information about colleges that are local, colleges at which they will pay in-state rates, financial aid for which they would qualify, and the like" (2013a, p. 7). Due to data limitations and time constraints, the packets developed were not customized for each student. Instead, scattered throughout, they provided side-by-side comparisons of key characteristics (e.g., SAT score range, four-year college graduation rates) for five institutions: University of Houston, University of Texas at Austin, Rice University, Harvard University, and Williams College. These institutions were chosen due to location, familiarity, and breadth of selectivity. Relatedly, our packet intervention was less comprehensive than the Hoxby and Turner intervention (2013a). The two packets we developed focused on the SAT and the college application process[16] but did not provide students a copy of the Common Application, a detailed discussion of financial aid, lists of merit and need-based aid opportunities, or application fee waivers—all features that the Hoxby and Turner study (2013a) included. Despite our intentions to highlight differences among institutions, the lack of customization for each student, as well as narrower scope of information provided, might have hindered our packets' effectiveness in helping students understand the variation among colleges and universities and the importance of applying to selective institutions in particular.

Aside from customization and scope, our packets might have failed, in part, because they did not provide students with concrete information. In an experiment by Dynarski et al. (2021),

[16] A third packet focused on college costs and financial aid was planned but later canceled due to a change in district priorities. The college application packet briefly discussed cost and compared the five institutions on sticker price, net price by family income bracket, and average percent of need met.

they found that students who received a letter guaranteeing them a full-tuition scholarship were more likely to enroll at the University of Michigan. This guarantee might have reduced students' uncertainty about college enrollment, specifically financial aid. The packets we designed did not provide students any promises that might be seen as reducing the burden of applying to college. Aside from the guarantee, the Dynarski et al. study (2021) is notable because the packets they sent students came directly from the University of Michigan and students might see it as a trusted source.[17] Their packets were also aesthetically appealing: "glossy" and styled in the University of Michigan's colors (Dynarski et al., 2021, p. 1732). Our packets were not produced by graphic designers and were fairly text-heavy; these characteristics might not have drawn students' attention or made it hard for them to digest the content quickly and easily.

Third, context might matter as well, as according to Hoxby and Avery (2013), places like the Houston metropolitan area have a critical mass of high-performing students. Applying to selective colleges might not be a rare event in HISD and students might have some familiarity with what those institutions offer. Therefore, the information packets might not have conveyed anything new. Students, however, might not have had the heuristic knowledge to navigate the complicated college application process, which could be why EMERGE's support demonstrated such strong effects. Regardless, there is reason to believe other types of low-touch, information-sharing nudges have value, even if there was not a result observed in this study. More work ought to be done to understand for whom, when, what, and how to maximize this specific support for students in the face of limited resources for college advising. With dedicated staff, resources, and time, information packets can be relatively cheap and easy to create compared to

[17] The packets in this study included introduction letters with the HISD logo and were written by the research team on behalf of HISD. HISD distributed the packets to students directly by email and through Naviance. HISD asked the research team to distribute the mailed version. While that version included the HISD introduction letter, it was sent in a Rice University envelope.

more intensive interventions and may be able to supplement the supports students receive from other school and district programs.

While the very positive effects of EMERGE participations are encouraging, there is a question as to how a small, district program characterized by personalized attention, advising, and curriculum can be scaled up to reach even more students. EMERGE is a sustained, time-intensive, and costly program with multiple layers of support that many school districts do not have the personnel or financial resources to implement. A program of this nature requires a committed group of individuals who are devoted to educating students about the college application, enrollment, and financial aid processes and who are available to help students with the transition from high school to college. In addition, it requires financial resources to help students prepare for the SAT and tour colleges and universities outside of their home state.

According to Dr. Rick Cruz, HISD's former Deputy Superintendent and founder of EMERGE, the annual cost per student to operate EMERGE is around $2,500 (Hess, 2018). This figure, no doubt, may raise concerns about scalability, particularly in school districts that are already struggling with budgetary resources or who serve high concentrations of low-income and first-generation students. However, in order to provide equitable college opportunities for some students from historically marginalized populations, this amount of investment may simply be necessary. EMERGE certainly requires more financial investment than an information packet, but the investment it requires may pale in comparison to the resources that middle- and upper-class children have since birth. Parents of children from middle- and upper-income backgrounds are able to spend a significantly greater amount of money on their children (Kornrich, 2016), which may give these students a competitive edge when preparing for college (Buchmann et al., 2010). In a detailed analysis of parental spending on children, Kornrich (2016) found that the wealthiest 10% of American families spent over $7,000 per child in a three-month period. In contrast, families in the bottom 25% of the income distribution spent less than $1,000 per child. If we stretch that $6,000 gap from three months to a year, that amounts to a $24,000 annual gap, which, in turn, is almost 10 times more than the amount EMERGE's founder says the program spends per student per year. The level of investment and resources that EMERGE requires to operate, while they may appear large, may be considered a bargain and be exactly what many less advantaged students need in order to apply to and enroll in selective institutions.

Finally, although we consider them exploratory, the post-hoc subgroup analyses suggests that some groups may benefit from intense, personalized support more than others. For example, the findings showed that EMERGE's benefits were concentrated among male students and did not translate into selective enrollment for Black students. While Black student enrollment patterns may be partially explained by HBCU enrollment, the lack of an effect for males remains a puzzle. In future studies, researchers ought to explore gender differences in the effects of social capital intervention further, perhaps by conducting focus groups or interviews to better understand the choices males make regarding college application and enrollment.

Conclusion

Given inequalities in college enrollment and the benefits of selective college enrollment, researchers, policymakers, and practitioners are persistently searching for new and innovative ways to support low-income and first-generation college students in the application and enrollment processes. The supports that programs like EMERGE provide are unique and may address the information barriers and unfamiliarity navigating the complex college application process that socioeconomically marginalized students and families face. The results shown in this study are in line with findings from related social capital interventions (e.g., Avery, 2013;

Stephan & Rosenbaum, 2013). Continuing and expanding similar in-depth, prolonged, and personalized college coaching efforts to help more college-aspiring students apply to and enroll in selective institutions is clearly one way to address systemic inequities affecting marginalized populations and expand their educational opportunities.

Yet even if is a worthwhile, long-term investment, schools and districts may not have or be willing to spend that amount of money on student supports for college. Therefore, continuing to develop and evaluate low-cost alternatives that can support historically marginalized populations in the pathway to college is imperative. Although the nudge intervention in this study was not effective, other research has shown that low-cost, information-sharing nudges can help students navigate the college application process and enroll in college after graduating from high school (e.g., Dynarski et al., 2021; Hoxby & Turner, 2013a; Hyman, 2020; Jensen, 2010). While our design could not determine which features of our nudge were ineffective, researchers might consider paying close attention to customization, comprehensiveness, providing guarantees, and aesthetic design in future information interventions. Additional research may pinpoint how to best share information with a wider number and variety of students, as well as shed light on what information should be shared and who to share that information with.

In an ideal world, social capital interventions like EMERGE should be made available to all students who aspire to attend college, not just the top two percent. In many contexts, implementing or expanding these programs is likely infeasible due to cost, but it is also unclear whether the findings would replicate in a lower-performing population. Therefore, it is important to pilot these types of programs in alternative settings with diverse populations or to test specific components separately (e.g., after-school advising, college visits). We also encourage more scholars to test social capital and nudge interventions simultaneously since that may help determine which students can get by on information alone and which ones need extra support like personal assistance. Such studies may shed light on what can be translated to a broader population of students in an effort to expand educational opportunity.

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Figures

Figure 1. Distribution of Rank Centered at Each School's Cutoff

Figure 1A. Frequency Histogram within an 8-Point Bandwidth

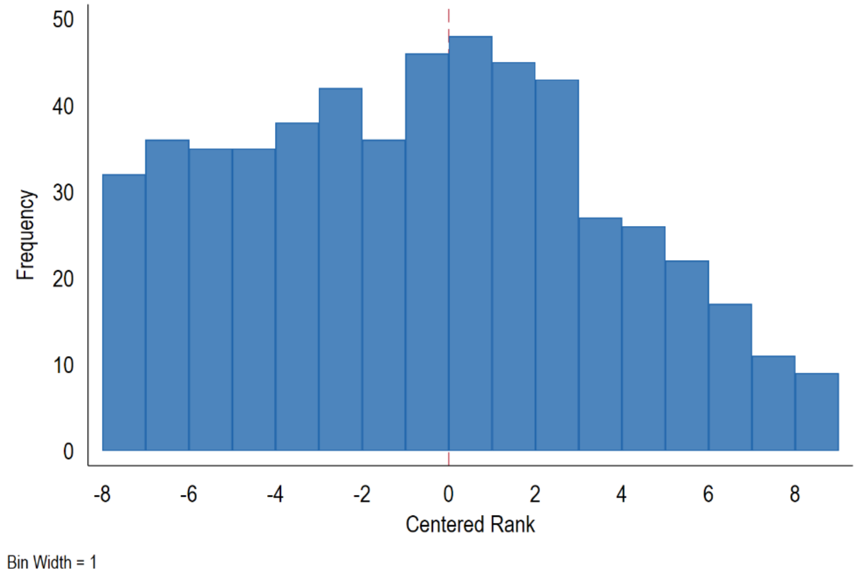


Figure 1B. Density Test (McCrary, 2008) within an 8-Point Bandwidth

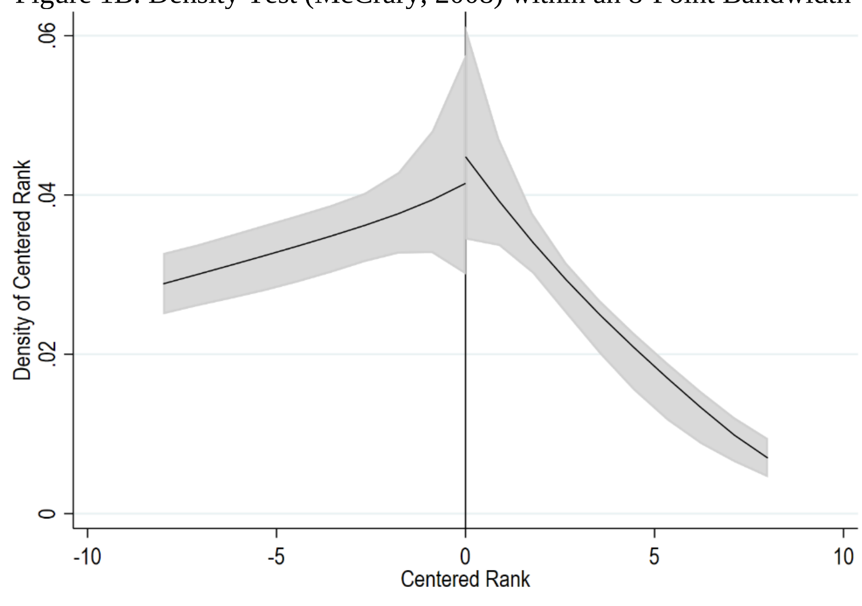


Figure 2. Graphs of RD Estimates within an 8-Point Bandwidth

Figure 2A. SAT Composite Scores

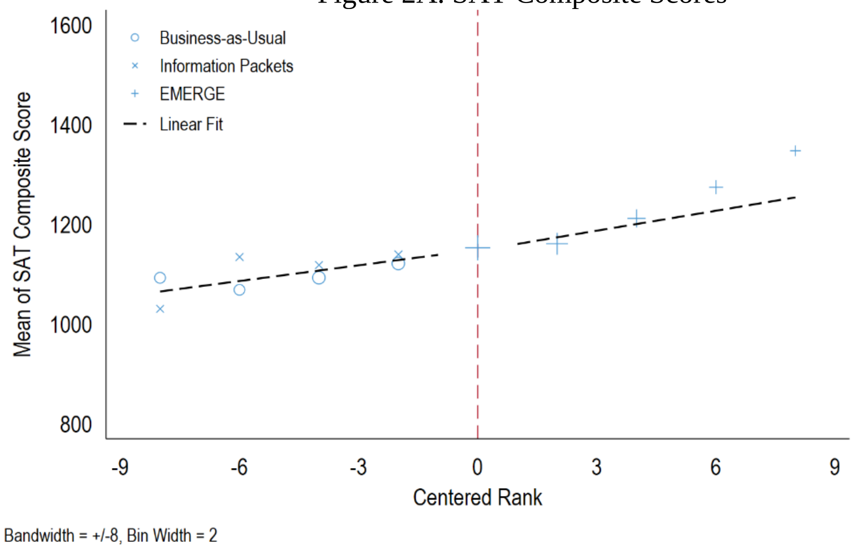


Figure 2B. Any Application to a Top 1 Barron's College

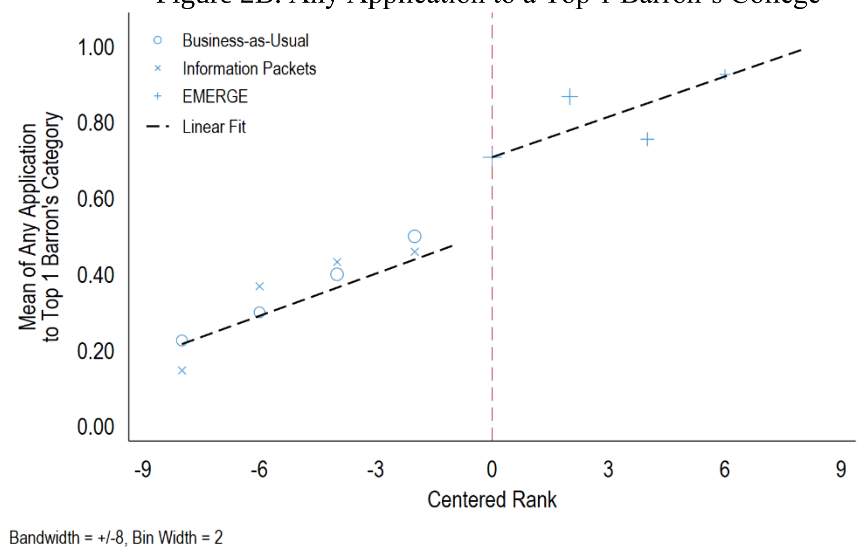


Figure 2C. Number of Applications Submitted to Top 1 Barron's Colleges

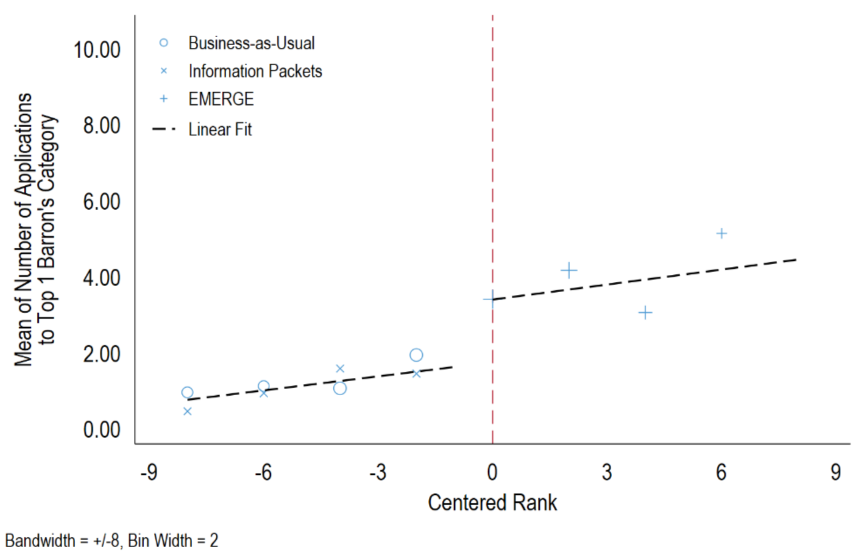


Figure 2D. Enrollment at a Top 1 Barron's College

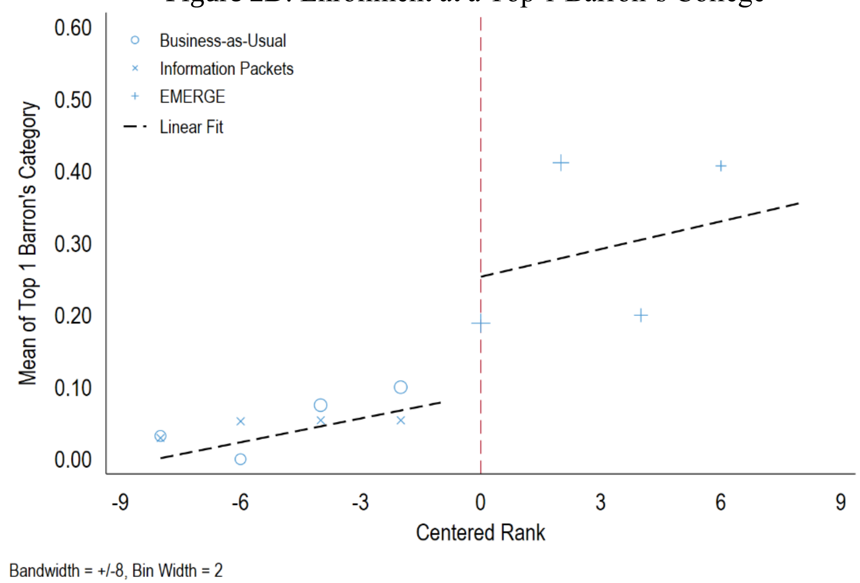


Figure 3. Plot-over-Bandwidth RD Estimates for EMERGE

Figure 3A. SAT Composite Scores

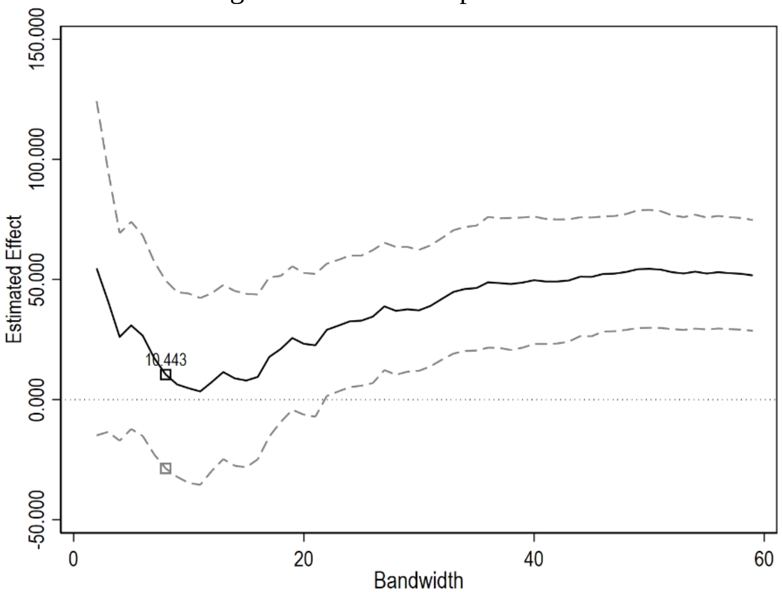


Figure 3B. Any Application to a Top 1 Barron's College

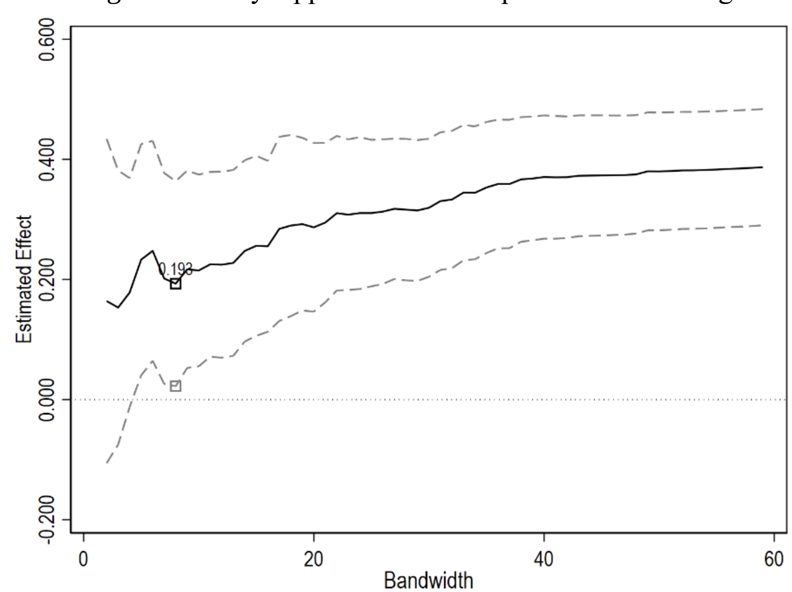


Figure 3C. Number of Applications Submitted to Top 1 Barron's Colleges

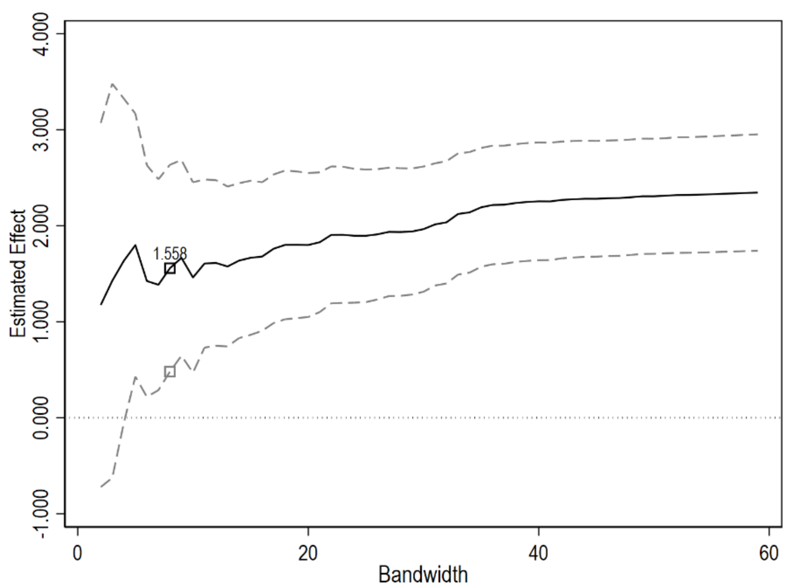


Figure 3D. Enrollment at a Top 1 Barron's College

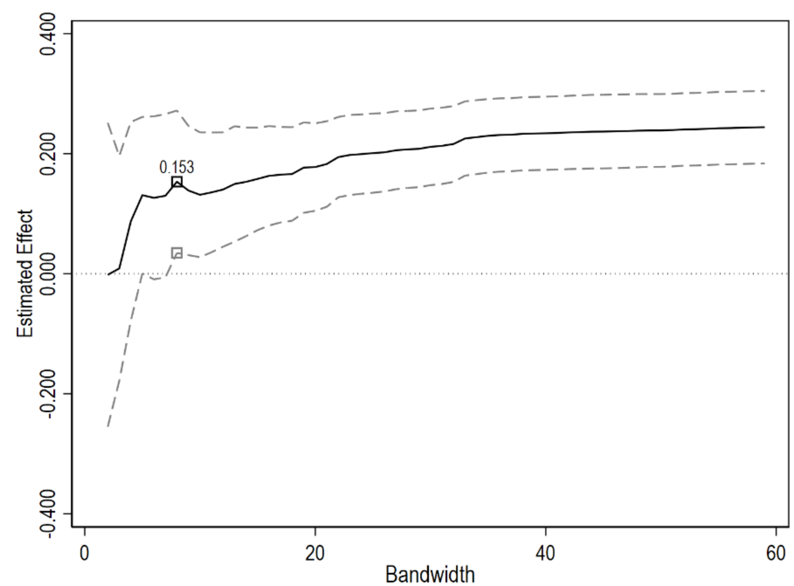
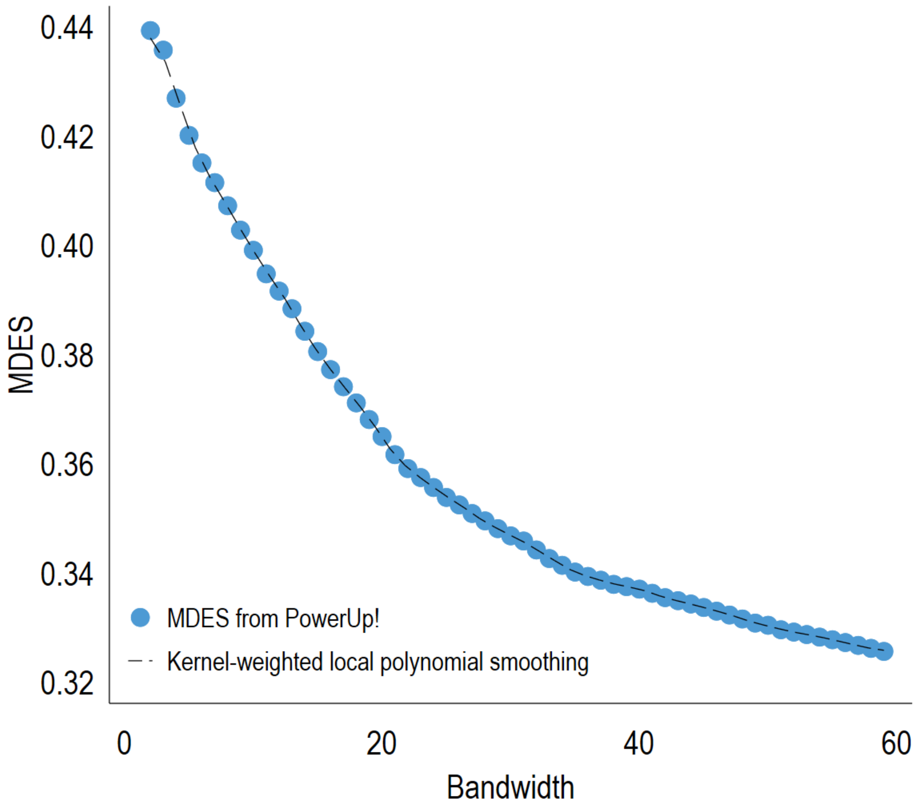


Figure 4. Minimum Detectable Effect Size by Bandwidth



Tables

Table 1. Phase I Scoring	
Component	Maximum Points
GPA	35
PSAT Score	25
Extracurricular Activities	16
Essay	16
Male	2
Black	1
Total Phase I Score	95

Table 2. Summary Statistics

Dependent Variables		
Variable	Mean	*SD*
SAT Scores		
Verbal Score	559.16	(85.45)
Math Score	549.93	(91.02)
Composite Score	1,109.10	(164.01)
Selective College Application		
Applied to a Top 1 Barron's College	0.39	(0.49)
Applied to a Top 1 or 2 Barron's College	0.62	(0.49)
No. of Selective College Applications Submitted		
No. Submitted to Top 1 Barron's Colleges	1.57	(2.88)
No. Submitted to Top 1 or 2 Barron's Colleges	3.01	(4.14)
Selective College Enrollment		
Enrolled in a Top 1 Barron's College	0.09	(0.29)
Enrolled in a Top 1 or 2 Barron's College	0.17	(0.38)
Independent Variables		
Variable	Mean	SD
Initial Assignment (ref. = Business-as-Usual)		
Information Packet	0.38	(0.49)
EMERGE	0.24	(0.43)
Centered Rank	-10.60	(12.85)
Age	15.19	(0.46)
Female	0.67	(0.47)
Race/Ethnicity (ref. = Hispanic)		
Black	0.22	(0.42)
Asian	0.08	(0.27)
Other	0.07	(0.26)
Foreign-Born	0.15	(0.36)
English Learner	0.04	(0.20)
Special Education	0.04	(0.18)
Economically Disadvantaged	0.77	(0.42)
First-Generation	0.72	(0.45)
Sibling Participated in EMERGE	0.05	(0.21)
No. Advanced Courses Taken	3.56	(1.48)
STEM Endorsement	0.31	(0.46)

Note. N = 1,078 EMERGE applicants from the Houston Independent School District in fall 2016. Some students were missing data on the outcomes of interest: the number of students with outcome data was 1,030 for SAT scores, 1,017 for selective college application, and 1,026 for selective college enrollment.

Table 3. Covariate Balance within an 8-Point Bandwidth

Independent Variable	All Students		Business-as-Usual		Information Packets		EMERGE		Information Packets vs. Business-as-Usual			EMERGE vs. Business-as-Usual			EMERGE vs. Information Packets		
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Beta	SE	Sig.	Beta	SE	Sig.	Beta	SE	Sig.
Age	15.18	(0.45)	15.18	(0.48)	15.19	(0.44)	15.17	(0.44)	0.02	(0.05)		0.02	(0.09)		0.00	(0.08)	
Female	0.68	(0.47)	0.72	(0.45)	0.68	(0.47)	0.66	(0.48)	-0.05	(0.05)		-0.06	(0.09)		-0.02	(0.09)	
Race/Ethnicity (ref. = Hispanic)																	
Black	0.24	(0.43)	0.30	(0.46)	0.25	(0.43)	0.21	(0.40)	-0.04	(0.05)		0.01	(0.07)		0.06	(0.07)	
Asian	0.08	(0.28)	0.05	(0.23)	0.06	(0.24)	0.12	(0.32)	0.02	(0.03)		0.05	(0.03)		0.03	(0.03)	
Other	0.06	(0.24)	0.05	(0.23)	0.04	(0.20)	0.08	(0.28)	0.00	(0.02)		0.02	(0.04)		0.02	(0.04)	
Foreign-Born	0.15	(0.36)	0.12	(0.33)	0.12	(0.33)	0.18	(0.39)	0.01	(0.03)		0.12	(0.06)	+	0.11	(0.06)	+
English Learner	0.03	(0.17)	0.04	(0.20)	0.03	(0.18)	0.02	(0.15)	-0.01	(0.01)		0.05	(0.03)		0.05	(0.04)	
Special Education	0.02	(0.15)	0.02	(0.14)	0.03	(0.16)	0.02	(0.15)	0.01	(0.02)		0.00	(0.03)		-0.01	(0.02)	
Economically Disadvantaged	0.77	(0.42)	0.75	(0.43)	0.77	(0.42)	0.77	(0.42)	0.00	(0.06)		0.04	(0.09)		0.03	(0.08)	
First-Generation	0.74	(0.44)	0.70	(0.46)	0.70	(0.46)	0.79	(0.40)	-0.02	(0.05)		0.05	(0.09)		0.07	(0.09)	
Sibling Participated in EMERGE	0.05	(0.21)	0.03	(0.16)	0.05	(0.21)	0.06	(0.23)	0.01	(0.02)		-0.01	(0.03)		-0.02	(0.04)	
No. Advanced Courses Taken	3.66	(1.43)	3.31	(1.58)	3.47	(1.54)	3.99	(1.18)	0.14	(0.12)		0.15	(0.15)		0.01	(0.17)	
STEM Endorsement	0.34	(0.48)	0.34	(0.47)	0.33	(0.47)	0.36	(0.48)	-0.03	(0.03)		0.05	(0.06)		0.00	(0.00)	
N	548		149		151		248										

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. The final columns of the table assess covariate balance from separate regression discontinuity models in which each covariate is set as the dependent variable. Models control for rank (centered at each school site's cutoff) and school site fixed-effects. Standard errors are clustered by school site.

Table 4. Regression Discontinuity Estimates within an 8-Point Bandwidth

	Panel A. SAT Outcomes								
	Composite Score			Math Score			Verbal Score		
Information Packet	15.764	(11.0967)		9.5841	(5.8530)		6.1800	(7.3461)	
EMERGE	10.443	(19.3405)		5.5880	(10.9094)		4.8552	(10.7734)	
R^2	0.28			0.29			0.21		
N	519			519			519		
	Panel B. Any Selective College Application								
	Top 1 Barron's College			Top 1 or Top 2 Barron's College					
Information Packet	0.0177	(0.0513)		0.0880	(0.0593)				
EMERGE	0.1931	(0.0845)	*	0.1819	(0.0779)	*			
R^2	0.23			0.14					
N	525			525					
	Panel C. Number of Selective College Applications Submitted								
	Top 1 Barron's College			Top 1 or Top 2 Barron's College					
Information Packet	-0.0597	(0.2567)		0.0389	(0.3768)				
EMERGE	1.5581	(0.5331)	**	2.7363	(0.7548)	***			
R^2	0.19			0.22					
N	525			525					
	Panel D. Selective College Enrollment								
	Top 1 Barron's College			Top 1 or Top 2 Barron's College					
Information Packet	0.0072	(0.0324)		0.0238	(0.0369)				
EMERGE	0.1534	(0.0587)	*	0.1903	(0.0703)	**			
R^2	0.11			0.13					
N	526			526					

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from sharp regression discontinuity models with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.

+ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ (two-tailed tests)

Table 5. Regression Discontinuity Estimates, Application Outcomes by Subgroup

Panel A. Any Application to a Top 1 Barron's College

	Female	Male	Black	Hispanic	Low College-Going	High College-Going
Information Packet	0.0633	-0.1380	0.0757	0.0192	-0.0088	0.0444
	(0.0562)	(0.1098)	(0.0869)	(0.0652)	(0.0566)	(0.0899)
EMERGE	0.2774 **	-0.2333 &	0.4109 **	0.1522 &	0.3167 **	0.0089
	(0.0898)	(0.1521)	(0.1368)	(0.1104)	(0.1091)	(0.1314)
R^2	0.26	0.24	0.23	0.34	0.15	0.36
N	356	169	122	325	287	238

Panel B. Any Application to a Top 1 or Top 2 Barron's College

	Female	Male	Black	Hispanic	Low College-Going	High College-Going
Information Packet	0.1088 +	-0.0874	0.2261 +	0.0737	0.0721	0.0871 &
	(0.0608)	(0.0935)	(0.1126)	(0.0667)	(0.0925)	(0.0634)
EMERGE	0.2006 +	-0.1227	0.2334 &	0.1720 &	0.3589 **	-0.0761
	(0.1018)	(0.1151)	(0.1653)	(0.1056)	(0.1092)	(0.0902)
R^2	0.26	0.24	0.23	0.34	0.15	0.36
N	356	169	122	325	287	238

Panel C. Number of Applications Submitted to Top 1 Barron's Colleges

	Female	Male	Black	Hispanic	Low College-Going	High College-Going
Information Packet	0.2228	-0.9035	0.4923 &	-0.2360	-0.4849 &	0.4239
	(0.3088)	(0.7912)	(0.3427)	(0.3585)	(0.3069)	(0.4641)
EMERGE	1.7244 **	0.1864	1.6029 *	1.9579 *	1.3559 +	1.4857 &
	(0.6244)	(0.9441)	(0.6572)	(0.7524)	(0.6619)	(0.9360)
R^2	0.19	0.15	0.16	0.20	0.14	0.23
N	356	169	122	325	287	238

Panel D. Number of Applications Submitted to Top 1 or Top 2 Barron's Colleges

	Female	Male	Black	Hispanic	Low College-Going	High College-Going
Information Packet	0.6345 &	-1.7911 &	1.1023 &	-0.0349	-0.5043	0.7184
	(0.4224)	(1.1125)	(0.7100)	(0.5292)	(0.5073)	(0.6720)
EMERGE	3.1517 **	-0.1054	3.5955 **	3.1283 **	3.3932 **	1.5742
	(0.8986)	(1.5945)	(1.2464)	(1.1178)	(0.9458)	(1.2429)
R^2	0.26	0.18	0.24	0.18	0.16	0.28
N	356	169	122	325	287	238

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from sharp regression discontinuity models with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.

& p<0.20, + p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)

Table 6. Regression Discontinuity Estimates, Enrollment Outcomes by Subgroup

Panel A. Enrollment at a Top 1 Barron's College

	Female		Male		Black		Hispanic		Low College-Going		High College-Going	
Information Packet	0.0141		0.0298		0.0507		0.0036		0.0307		-0.0136	
	(0.0415)		(0.0620)		(0.0689)		(0.0399)		(0.0242)		(0.0659)	
EMERGE	0.1727	+	0.1501		0.0393		0.1386	+	0.1335	+	0.1247	
	(0.0872)		(0.1302)		(0.0796)		(0.0729)		(0.0724)		(0.1040)	
R^2	0.12		0.07		0.15		0.15		0.14		0.14	
N	356		170		122		325		288		238	

Panel B. Enrollment at a Top 1 or Top 2 Barron's College

	Female		Male		Black		Hispanic		Low College-Going		High College-Going	
Information Packet	0.0244		0.0488		0.1110		-0.0073		0.0455		0.0084	
	(0.0435)		(0.0880)		(0.0990)		(0.0375)		(0.0432)		(0.0599)	
EMERGE	0.1826	+	0.2031	&	0.0773		0.1642	+	0.1661	*	0.2206	&
	(0.0944)		(0.1230)		(0.0928)		(0.0968)		(0.0790)		(0.1402)	
R^2	0.13		0.17		0.16		0.15		0.12		0.18	
N	356		170		122		325		288		238	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from sharp regression discontinuity models with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.
& p<0.20, + p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)

Appendix

Appendix Table 1. Fuzzy Regression Discontinuity Estimates

	Top 1 Barron's College		Top 1 or Top 2 Barron's College	
Panel A. Any Application				
Information Packet	0.0385		0.1171	+
	(0.0569)		(0.0702)	
EMERGE	0.3680	*	0.3337	*
	(0.1544)		(0.1384)	
First-Stage F-Tests				
Information Packet	597.06			
EMERGE	19.87			
R^2	0.47		0.30	
N	525		525	
Panel B. Number of Applications Submitted				
Information Packet	0.0816		0.3061	
	(0.3335)		(0.4184)	
EMERGE	3.0066	*	5.2541	***
	(1.1708)		(1.4685)	
First-Stage F-Tests				
Information Packet	597.06			
EMERGE	19.87			
R^2	0.35		0.39	
N	525		525	
Panel C. Enrollment				
Information Packet	0.0198		0.0389	
	(0.0383)		(0.0423)	
EMERGE	0.2953	*	0.3583	**
	(0.1153)		(0.1389)	
First-Stage F-Tests				
Information Packet	595.59			
EMERGE	18.91			
R^2	0.13		0.16	
N	526		526	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from fuzzy regression discontinuity models (using partial compliance data) with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site. First-stage results are available upon request.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)

Appendix Table 2. Regression Discontinuity Estimates, Less Selective College Enrollment

Panel A. Any Application

	Any College		Any Four-Year College		Top 1, 2, or 3 Barron's College	
Information Packet	0.0024		0.0169		0.0741	*
	(0.0199)		(0.0220)		(0.0332)	
EMERGE	-0.0553	+	-0.0513		-0.0034	
	(0.0304)		(0.0320)		(0.0534)	
R^2	0.03		0.02		0.05	
N	525		525		525	

Panel B. Number of Applications Submitted

	Any College		Any Four-Year College		Top 1, 2, or 3 Barron's College	
Information Packet	0.3996		0.3875		0.0730	
	(0.6330)		(0.6345)		(0.5026)	
EMERGE	2.2962	*	2.4260	*	2.8399	**
	(0.9146)		(0.9494)		(0.9415)	
R^2	0.11		0.12		0.20	
N	525		525		525	

Panel C. Enrollment

	Any College		Any Four-Year College		Top 1, 2, or 3 Barron's College	
Information Packet	-0.0047		0.0115		0.0051	
	(0.0522)		(0.0571)		(0.0558)	
EMERGE	0.0263		0.0455		0.0837	
	(0.0728)		(0.0755)		(0.0863)	
R^2	0.03		0.02		0.13	
N	526		526		526	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from sharp regression discontinuity models with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.

+ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$ (two-tailed tests)

Appendix Table 3. Regression Discontinuity Estimates of EMERGE, Placebo and Actual Discontinuity Thresholds

	Any Application				Number of Applications Submitted				Enrollment			
Threshold	Top 1 Barron's College		Top 1 or Top 2 Barron's College		Top 1 Barron's College		Top 1 or Top 2 Barron's College		Top 1 Barron's College		Top 1 or Top 2 Barron's College	
Rank ≥ -4	0.0102		0.0365		-0.1156		-0.1316		-0.0254		-0.0658	
	(0.0778)		(0.0710)		(0.3543)		(0.5176)		(0.0477)		(0.0462)	
Rank ≥ -3	0.1005		0.1064		0.5808		0.9109		-0.0103		0.0205	
	(0.0612)		(0.0636)		(0.3797)		(0.5423)		(0.0484)		(0.0541)	
Rank ≥ -2	0.0868		0.0967		0.6773		1.3793	*	0.0303		0.0557	
	(0.0790)		(0.0779)		(0.4134)		(0.5863)		(0.0447)		(0.0617)	
Rank ≥ -1	0.1475	+	0.0412		1.4127	**	2.3545	**	0.0891		0.1655	**
	(0.0806)		(0.0736)		(0.5019)		(0.7218)		(0.0568)		(0.0579)	
Rank ≥ 0	0.1849	*	0.1411	+	1.5857	**	2.7183	**	0.1500	**	0.1793	*
	(0.0812)		(0.0714)		(0.5502)		(0.7805)		(0.0541)		(0.0671)	
Rank ≥ 1	0.0203		0.0569		0.4649		0.8434		0.0758		0.0525	
	(0.0582)		(0.0502)		(0.4755)		(0.6292)		(0.0643)		(0.0796)	
Rank ≥ 2	0.0431		0.0165		0.2555		0.2356		0.1668	*	0.1593	*
	(0.0547)		(0.0582)		(0.4752)		(0.6980)		(0.0638)		(0.0749)	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from sharp regression discontinuity models with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)

Appendix Table 4. Inverse Probability of Treatment Weighting Estimates

Panel A. Any Application

	Top 1 Barron's College		Top 1 or Top 2 Barron's College	
Information Packet	0.0052		0.0824	
	(0.0567)		(0.0635)	
EMERGE	0.2786	***	0.1809	*
	(0.0752)		(0.0730)	
KonFound-It! Threshold to Invalidate EMERGE Effect	46.98% cases replaced		20.72% cases replaced	
R^2	0.29		0.23	
N	525		525	

Panel B. Number of Applications Submitted

	Top 1 Barron's College		Top 1 or Top 2 Barron's College	
Information Packet	-0.2102		-0.0812	
	(0.2906)		(0.4461)	
EMERGE	1.5171	**	2.8097	***
	(0.4376)		(0.6092)	
KonFound-It! Threshold to Invalidate EMERGE Effect	43.31% cases replaced		57.40% cases replaced	
R^2	0.24		0.20	
N	525		525	

Panel C. Enrollment

	Top 1 Barron's College		Top 1 or Top 2 Barron's College	
Information Packet	-0.0048		0.0230	
	(0.0418)		(0.0423)	
EMERGE	0.1802	***	0.2598	***
	(0.0418)		(0.0647)	
KonFound-It! Threshold to Invalidate EMERGE Effect	54.43% cases replaced		51.07% cases replaced	
R^2	0.13		0.21	
N	526		526	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from doubly-robust inverse probability of treatment weighting models. Models are weighted and control for pre-treatment variables, including GPA and PSAT score, and school site fixed-effects. Standard errors are clustered by school site. Models predicting the propensity score are available upon request.

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)

Appendix Table 5. Regression Discontinuity Estimates, Imputed Missing Outcomes Data

Panel A. Any Application

	Top 1 Barron's College				Top 1 or Top 2 Barron's College			
	Missing → 0		Missing → 1		Missing → 0		Missing → 1	
Information Packet	0.0162		-0.0071		0.0897		0.0664	
	(0.0522)		(0.0511)		(0.0599)		(0.0578)	
EMERGE	0.1941	*	0.1634	+	0.1897	*	0.1590	*
	(0.0835)		(0.0828)		(0.0779)		(0.0755)	
R^2	0.25		0.25		0.11		0.11	
N	548		548		548		548	

Panel B. Number of Applications Submitted

	Top 1 Barron's College				Top 1 or Top 2 Barron's College			
	Missing → 25%tile		Missing → 75%tile		Missing → 25%tile		Missing → 75%tile	
Information Packet	-0.1197		-0.1664		-0.0166		-0.1334	
	(0.2602)		(0.2531)		(0.3810)		(0.3672)	
EMERGE	1.5032	**	1.4419	**	2.6393	**	2.4860	**
	(0.5387)		(0.5281)		(0.7815)		(0.7475)	
R^2	0.16		0.16		0.20		0.20	
N	548		548		548		548	

Panel C. Enrollment

	Top 1 Barron's College				Top 1 or Top 2 Barron's College			
	Missing → 0		Missing → 1		Missing → 0		Missing → 1	
Information Packet	0.0048		-0.0177		0.0195		-0.0030	
	(0.0317)		(0.0355)		(0.0357)		(0.0376)	
EMERGE	0.1470	*	0.1054		0.1843	*	0.1427	+
	(0.0569)		(0.0634)		(0.0683)		(0.0707)	
R^2	0.10		0.10		0.12		0.11	
N	548		548		548		548	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Students missing outcomes data had their data imputed: for binary outcomes, missing outcomes were assigned to a 0 or 1, while for continuous outcomes, missing outcomes were assigned to the 25th or 75th percentiles. Estimates come from sharp regression discontinuity models with a linear slope on both sides of the cutoff. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)

Appendix Table 6. Regression Discontinuity Estimates of EMERGE, Non-Parametric Local Linear Specifications

	Top 1 Barron's College		Top 1 or Top 2 Barron's College	
Panel A. Any Application				
Shard RD	0.1766	**	0.1370	*
	(0.0642)		(0.0628)	
Fuzzy RD	0.4158	**	0.3224	*
	(0.1390)		(0.1434)	
N	525		525	
Panel B. Number of Applications Submitted				
Shard RD	1.2917	**	2.1752	**
	(0.4298)		(0.6295)	
Fuzzy RD	3.0409	**	5.1209	***
	(1.0543)		(1.3857)	
N	525		525	
Panel C. Enrollment				
Shard RD	0.1158	*	0.1230	*
	(0.0485)		(0.0586)	
Fuzzy RD	0.2805	*	0.2981	*
	(0.1237)		(0.1514)	
N	526		526	

Note: Sample is limited to EMERGE applicants from the Houston Independent School District in fall 2016 who were within an 8-point bandwidth of their school site's cutoff. Estimates come from non-parametric local polynomial regression discontinuity models. Models control for rank (centered at each school site's cutoff), pre-treatment variables, and school site fixed-effects. Standard errors are clustered by school site.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001 (two-tailed tests)