Economics of Education Review 75 (2020) 101974

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Note

Virtual advising for high-achieving high school students

Oded Gurantz[a,*], Matea Pender[b], Zachary Mabel[b], Cassandra Larson[b], Eric Bettinger[c]

[a] Truman School of Public Affairs, University of Missouri, Columbia, MO
[b] College Board, New York, NY
[c] Stanford University Stanford, CA

ABSTRACT

We examine whether virtual advising – college counseling using technology to communicate remotely – increases postsecondary enrollment in selective colleges. We test this approach using a sample of approximately 16,000 high-achieving, low- and middle-income students identified by the College Board and randomly assigned to receive virtual advising from the College Advising Corps. The offer of virtual advising had no impact on overall college enrollment, but increased enrollment in high graduation rate colleges by 2.7 percentage points (5%), with instrumental variable impacts on treated students of 6.1 percentage points.

1. Introduction

Although postsecondary attendance has increased over the last few decades, large gaps in college attendance and completion between low- and high-income students remain (Bailey & Dynarski, 2011; Bastedo & Jaquette, 2011). One concern is that low-income students are more likely to "undermatch", meaning they are less likely to apply or enroll in academically matched institutions compared to their high-income peers, which can result in lower completion rates (Cohodes & Goodman, 2014; Hoxby & Avery, 2013; Smith, Pender, & Howell, 2013). One explanation for undermatching is the complexity of the college application process, which requires students to: assess the quality of numerous postsecondary institutions, many of which may be geographically distant and unknown to the typical low-income student; understand an opaque financial aid process that leaves the true cost of college unclear; and meet many key consequential deadlines over many months (Klasik, 2012). Failing to follow or optimize among these steps can lead to inferior postsecondary enrollment or completion outcomes (Page & Scott-Clayton, 2016).

College counseling – providing students guidance via human interaction throughout the lengthy college application process – has been generally shown to increase college attendance and persistence, though results vary and do not uniformly lead to positive impacts (Barr & Castleman, 2017; Bettinger & Evans, 2020; Carrell & Sacerdote, 2017; Castleman & Goodman, 2018; Page, Kehoe, Castleman, & Sahadewo, 2017; Phillips & Reber, 2019). Yet there are challenges in providing college counseling at scale. Counseling is often referred to as a "high-touch" intervention, due to the financial costs required to provide students individualized attention. Students with less college-relevant information and most in need of these counseling services may also live in more geographically distant areas, and traveling to reach these all of these students may be cost prohibitive (Hoxby & Avery, 2013). Nonetheless, counseling at the individual-level may be necessary as "light-touch" interventions, which typically rely on providing simplified information about college opportunities and costs through brochures, emails, or texts, have had varying levels of success (Bird et al., 2019; Castleman & Page, 2015; Castleman, Page, & Schooley, 2014; Gurantz et al., 2019; Hoxby & Turner, 2013; Hyman, 2020).

This project examined the impact of "virtual advising": one-on-one college counseling done remotely via computer-assisted face-to-face conversations, along with opportunities for more typical communication via email, phone, or text. Virtual advising enables a single adviser to serve students across a broad geographic region, rather than within a single K-12 institution. In this paper, virtual advisers focused entirely on college planning and application support, whereas school counselors often carry much higher caseloads and are responsible for a number of non-counseling related activities in their schools. Using a pool of PSAT/NMSQT and SAT takers in the high school graduating class of 2018, we identified high-achieving low- and middle-income students who were then randomly assigned to an offer of receiving virtual advising by the College Advising Corps (CAC).[1] Each adviser used text messages, phone, email, and video conferencing capabilities to help their students apply to and enroll in from top colleges. A primary focus of adviser outreach was to promote student enrollment in a select group of "CollegePoint" colleges and universities, defined as institutions with graduation rates above 70%.

As a result of the offer of virtual advising, students sent SAT scores to

* Corresponding author.
E-mail address: gurantzo@missouri.edu (O. Gurantz).

[1] This approach necessarily results in an analysis that relies more heavily on regions that take the PSAT and SAT exam (rather than say the ACT), but does encompass students in all 50 states and Washington DC. Appendix Figure 1 shows the percent of our sample that comes from each state. Having fewer students in a state is a reflection predominately of state size and PSAT/SAT participation rates, and is not intended to reflect the average academic achievement of a state.

https://doi.org/10.1016/j.econedurev.2020.101974
Received 6 September 2019; Received in revised form 10 February 2020; Accepted 14 February 2020
0272-7757/

0.32 (7%) more CollegePoint colleges and were 2.7 percentage points (5.4%) more likely to attend these schools. Students were 1.7 percentage points (2.8%) more likely to attend institutions in the top three Barron's ratings, exhibiting small and sometimes statistically significant increases in measures intended to identify college selectivity or "quality", such as institutional graduation rates.[2] We also find evidence for homophily effects, where non-white students randomly assigned to an adviser of non-white ethnicity exhibit larger treatment impacts, but find no such evidence for assignment by adviser gender.

2. Experimental background

Beginning in 2017, with support from Bloomberg Philanthropies, College Board (CB) and College Advising Corps (CAC) partnered to test the impact of connecting students directly with a virtual adviser. For this experiment, College Board identified 16,256 high-achieving, low- and moderate-income students from the class of 2018.[3] High-achieving was defined as being in the top 10% of the national PSAT or SAT distribution for students who took the exam in their 11th grade year. We identified low- and middle-income status through a combination of SAT fee waiver usage, PSAT and SAT questionnaire responses, and a methodology that predicted income using geographic data (e.g., census tract, high school) and survey responses on the SAT's student data questionnaire.[4]

From this sample, three-fourths of the students were randomly assigned to receive an offer to participate in virtual advising (12,215 treatment and 4041 control students). Each treated student was also randomly assigned to one of 23 advisers. As students were randomly assigned to treatment we expected that some students would not engage with their adviser, thus necessitating a larger split for treatment (three-fourth of the sample) than control (one-fourth of the sample). Engagement rates were 44%, resulting in an average adviser caseloads of approximately 235 students each.

Students assigned to the treatment group received outreach from College Board notifying them of their selection to the program and introducing their CAC college adviser. Once a student responded to the outreach and their identity was verified, the student was considered part of the adviser's caseload. An initial virtual advising meeting was then scheduled with the student.[5]

CB and CAC executed two recruitment campaigns to engage assigned treatment students. Each campaign included direct mail, email, text, and phone calls. The first campaign launched in early May 2017 to 6822 eligible students with letters and emails. The second campaign launched in August 2017 to 5422 students and followed a similar letter/email sequence, with some minor adjustments based on observations from the first campaign. Advisers in the first wave of outreach reached caseload capacity in October 2017. In order to meet CAC caseload goals for wave 2 advisers, 3686 non-responsive students in the first campaign were reassigned to advisers in the second campaign and received another round of text messages and phone calls. Wave 2 advisers reached caseload capacity in January 2018. 44.6% of the 12,244 students who received an offer were placed in an adviser caseload by January 2018 (5460 students). These students actively and passively engaged with adviser outreach through August 2018. Outreach to students in the treatment group not assigned to an adviser caseload stopped in January 2018.

During this time period, students in the treated group were also enrolled in a separate College Board initiative known as Reach Your College Potential, a "light-touch" intervention that, for example, provided students brochures with information about selective colleges. We do not believe that the simultaneous enrollment in this initiative is likely to impact our results as prior research found precisely estimated null effects of this program on college enrollment decisions (Gurantz et al., 2019).

3. Methodology and outcome measures

The empirical strategy based on our experimental design is represented by Eq. (1):

$$Y_{igt} = \beta_0 + \beta_1 * VirtualAdvising_{igt} + \theta_{gt} + \varepsilon_{igt} \tag{1}$$

Y_{igt} represents an outcome of interest for high-achieving individual i in income group g (i.e., low- versus middle-income) at time t (Spring or Fall outreach). We control for students' income status and the timing of randomization (θ_{gt}). Students who were randomly assigned in Spring 2017 and did not respond were then randomly re-assigned to a second adviser in the Fall; for these students we still "assign" them to their initial randomization pool and adviser to ensure accurate analysis. $VirtualAdvising_{igt}$ is equal to one for individuals assigned to a treatment condition.

Our primary outcome measures are College Board data on SAT "score sends" and National Student Clearinghouse (NSC) data on postsecondary enrollment. We cannot observe college application data, but SAT score sends serve as a rough proxy for application patterns to four-year colleges (Smith, 2018). NSC data identify students' initial postsecondary enrollment. We use IPEDS data to create metrics of the selectivity of the college attended, using both SAT percentiles (the rank order of median SAT scores for four-year colleges) and the college's six-year (150% time) graduation rates. We also examine whether enrollment shifted students to a college that is likely to be less expensive for them, as measured by IPEDS data on net costs for students from low-income families (i.e., incomes of $48,000 and below).

As part of the initiative, College Advising Corps was interested in encouraging students to attend a set of colleges called "CollegePoint" colleges. This included a set of approximately 290 colleges with graduation rates above 70%.[6] We examine changes in the number of score sends and enrollment in CollegePoint colleges specifically, as they were

[2] There is an open question as to how researchers should measures shifts in college enrollment, with many using changes in institutional graduation rates, average freshmen standardized test scores, per student expenditures, or other measures to indicate changes in college quality or selectivity. These measures matter to the extent they causally improve student outcomes, rather than simply reflect inputs into the institution. Although there is no clear answer, some papers have found evidence that these changes appear causally related to student outcomes, at least in part (Cohodes & Goodman, 2014; Goodman, Hurwitz, & Smith, 2017).

[3] College Board identified 32,528 high achieving, low- and moderate-income students from the class of 2018. The partnership with CAC was structured to allow a two-way data exchange between CAC and CB to support students throughout the advising cycle, enabled through a detailed data privacy and security audit, and establishing clear use of data guidelines. Half of the identified students had their information provided to CollegePoint, another college counseling organization, with these students included in the experiment described in a paper by Sullivan, Castleman, and Bettinger (2019), henceforth "SCB". Appendix Table 1 shows some primary differences between the virtual advising models: (1) treatment assignment in this paper was an intent-to-treat effect as all students were offered advising, whereas SCB asked students to participate before randomizing on a smaller sample of students who expressed interest; (2) this paper relied only on College Board identified students assigned to one counseling provider (CAC), compared to multiple counseling providers utilized in SCB.

[4] The College Board developed a methodology to identify income status through an algorithm that includes student self-reported data on the SAT's student data questionnaire (SDQ), high school attended, and census tract. Low-income students were identified then by either receipt of an SAT fee waiver or an estimated annual income below approximately $58,000; moderate-income students were identified based on incomes below approximately $77,000 per year, but above the low-income threshold. These cutoffs were selected as in earlier years students were tagged as low- and middle-income by Dr. Caroline Hoxby using higher quality income data, and the College Board methodology identified those specific income levels as best matching her income tagging.

[5] Conversations between students and advisers did not suggest that internet access was an issue, but it may be that some students assigned to treatment ultimately did not participate due to potential technological challenges. Thanks to the reviewers for raising this issue.

[6] Full list of CollegePoint colleges: https://ogurantz.github.io/website/Gurantz_2019_VirtualAdvising_Colleges.pdf

a focus of adviser outreach. College Board generated a list of the closest CollegePoint colleges for each student, to enable them to provide advice and suggestions to augment each student's college application portfolio.

4. Results

4.1. Background characteristics

Table 1 shows the characteristics of the high-achieving sample, and provides evidence that the randomization resulted in no differences in the average characteristics of treatment and control students. Students in this experiment were roughly evenly split between female and male (47% to 53%) and came from families with strong academic backgrounds (40% reported having at least one parent with a bachelor's degree). The students predominately identified as white (38%) and Asian (33%), with 18% and 5% of students identifying as Hispanic or African-American, respectively. By design, the students were very high-achieving, with an average SAT score of 1357 (out of 1600) on their first attempt. On all these characteristics, we find no evidence that the average treatment student differed from the average control group student, confirming that the randomization process generated balanced samples.

4.2. Impacts of treatment assignment on college attendance

Given the high achievement of these students, an overwhelming majority already intended to attend college. Over 87% of the control group students attended some type of college, and we find that the offer of College Advising Corps virtual advising (VA) has no effect on college-attendance overall. However, the intent of the program was to reduce “mismatch” by encouraging enrollment at CollegePoint colleges – institutions with graduation rates stronger than they might otherwise consider. The program increased attendance at CollegePoint colleges, with positive and statistically significant impacts on some commonly used measures of college selectivity.

Table 2 shows intent to treat estimates from the VA offer on SAT score sending behavior, a proxy for changes in college application patterns. Students in the treatment group exhibited no change in SAT scores to non-CollegePoint colleges (column 1), but increased their SAT score sends by 0.3 (5%) toward CollegePoint colleges (column 2). Score send impacts were concentrated among low-income (row 4), rather than middle-income (row 5), students. This likely occurred as low-income students were offered 4 additional free SAT score sends as part of the treatment condition, whereas middle-income students were not offered additional free score sends. The VA offer did not significantly increase college-going, either overall (column 3) or at four-year institutions (column 4), but this is in large part because most of these high-achieving students attended college (87%) or attended four-year colleges (83%) at baseline.

Table 2, column 5 shows that treated students were 2.7 percentage points (5%) more likely to attend CollegePoint colleges. Impacts were slightly larger in wave two, which occurred in the Fall of 12th grade rather than Spring of 11th grade, though impacts were equivalent for low- and middle-income students. The shift into CollegePoint colleges was accompanied by some evidence of corresponding increases in college selectivity. Students were 1.7 percentage points (3%) more likely to attend a school ranked in the Barron's top three categories (column 6), as a result of shifting out of less selective four-year colleges (column 7).[7] Column 9 shows that overall shifts led students to attend colleges with IPEDS-measured graduation rates that were 0.8 percentage points (1%) higher. There were no other

Table 1
Descriptive statistics and covariate balance.

	Control group mean	Test for statistical difference
Individual characteristics		
Female	47.4%	−0.006 (0.009)
Parent has bachelor's degree	39.5%	0.003 (0.009)
White	38.4%	−0.004 (0.009)
Hispanic	17.6%	0.005 (0.007)
African-American	5.2%	−0.003 (0.004)
Asian	32.7%	−0.001 (0.008)
SAT score	1357	−0.709 (1.573)
School characteristics		
School size	1822	−27.325 (21.266)
City	36.8%	−0.001 (0.009)
Suburb	37.0%	−0.009 (0.009)
Town	5.8%	−0.003 (0.004)
Rural	9.6%	0.006 (0.005)
Percent free and reduced price lunch	42.0%	0.004 (0.004)

Notes: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. Regression based on sample of 16,256 students.

observed changes in characteristics of colleges attended, with impacts on average freshmen SAT and net price (based on low-income students) statistically insignificant. Other values, such as net price for other income levels or expenditures per FTE were also unchanged.

Although most treatment effects on institutional values, such as the average graduation rate of the college attended, are small or statistically insignificant, they are in line with what we should expect based on the enrollment outcomes. Fig. 1 shows the full distribution of college graduation rates among control group and treatment students enrolled in four-year colleges, with most of the shifting coming from a decline in attendance at colleges having graduation rates between 50% and 70% towards institutions with graduation rates between 70% and 90%. If we hypothesize that the 2.7 percentage point increase in CollegePoint attendance moves students from the 25th percentile to the 75th percentile of the graduation rate distribution in Fig. 1, the resulting treatment effect would be a 0.7 percentage point increase in average graduation rate, roughly similar to the positive effect of 0.8 found in Table 2.[8]

As anticipated, not all students who were offered virtual advising ultimately participated in the program. We find that 44% of students chose to participate, as measured by College Advising Corps adviser tracking using their student information management system. Figure 2 examines the distribution of who engaged their adviser, based on their predicted probability of attending a CollegePoint college.[9] There is some evidence of positive

[7] Barron's top three ratings are “most competitive”, “highly competitive plus”, and “highly competitive”, with ranking four being “very competitive plus”. Few students in our sample attended four-year colleges with lower Barron's ratings, and these students are included with category four. Appendix Table 4 disaggregates by each Barron's ranking and also finds some additional evidence of shifting, with some students exiting non-CollegePoint “highly competitive plus” colleges to shift into similarly ranked CollegePoint colleges.

[8] Additional histograms for SAT percentile and net price are shown in Appendix Figures 2 and 3. The SAT percentile figure shows some similarity to the graduation rate figure, though net price shows some suggestive evidence of a compression towards the middle, with some decline in colleges with both very low and very high net price for students in families earning $30,000 to $48,000.

[9] Predicted probabilities derive from a logistic regression using control group students that controlled for: student ethnicity; gender; parental education; school urbanicity; whether they took the SAT zero, one, or two or more times; a cubic of initial SAT math and verbal scores, school-level free and reduced price lunch, and school size. Results based on predicted probability of attending a four-year college produce results with a similar interpretation.

Table 2
Impacts of virtual advising, Intent-to-treat estimates.

Group	(1) SAT score sends Non-CollegePoint colleges	(2) CollegePoint colleges	(3) Attendance (NSC data) Any	(4) Four-year	(5) CollegePoint college	(6) Barron's Ranking Top 3	(7) 4 or higher	(8) College characteristics SAT percentile	(9) Graduation rate	(10) Net price: $30-48K
All students	0.001	0.312**	0.003	0.010	0.026**	0.017^{+}	−0.013^{+}	0.193	0.008*	−30.854
	(0.034)	(0.086)	(0.006)	(0.007)	(0.009)	(0.009)	(0.008)	(0.227)	(0.004)	(122.316)
Control mean	6.0	4.3	87.3%	82.7%	50.0%	60.9%	26.1%	88.5	72.1%	$12,391
N	16256	16256	16256	16256	16256	16256	16256	12654	14023	14134
Wave 1	0.003	0.223^{+}	−0.002	0.005	0.014	0.015	−0.018	0.092	0.005	−159.447
	(0.046)	(0.119)	(0.009)	(0.010)	(0.013)	(0.013)	(0.011)	(0.327)	(0.005)	(168.521)
Control mean	5.5	4.0	87.1%	82.3%	48.5%	59.6%	27.4%	88.5	71.8%	$12,136
N	8203	8203	8203	8203	8203	8203	8203	6374	7032	7092
Wave 2	−0.002	0.401**	0.008	0.014	0.037**	0.019	−0.009	0.304	0.011*	93.688
	(0.049)	(0.124)	(0.008)	(0.009)	(0.013)	(0.012)	(0.011)	(0.316)	(0.005)	(177.291)
Control mean	6.4	4.6	87.5%	83.0%	51.5%	62.2%	24.9%	88.4	72.4%	$12,638
N	8053	8053	8053	8053	8053	8053	8053	6280	6991	7042
Low-income	0.085^{+}	0.510**	0.005	0.013	0.025*	0.013	−0.007	0.179	0.006	130.440
	(0.045)	(0.118)	(0.008)	(0.009)	(0.012)	(0.012)	(0.010)	(0.302)	(0.005)	(162.945)
Control mean	6.3	4.6	86.9%	82.4%	52.1%	62.4%	24.1%	89.1	73.5%	$12,047
N	9397	9397	9397	9397	9397	9397	9397	7355	8078	8137
Middle-income	−0.113*	0.048	0.000	0.005	0.027*	0.022	−0.022^{+}	0.223	0.011^{+}	−246.228
	(0.049)	(0.125)	(0.009)	(0.010)	(0.014)	(0.014)	(0.012)	(0.345)	(0.006)	(185.054)
Control mean	5.5	3.9	87.7%	83.0%	47.2%	58.9%	28.8%	87.6	70.2%	$12,839
N	6859	6859	6859	6859	6859	6859	6859	5299	5945	5997

Notes: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. All estimates compare the randomized offer of virtual advising to control group students not offered virtual advising.

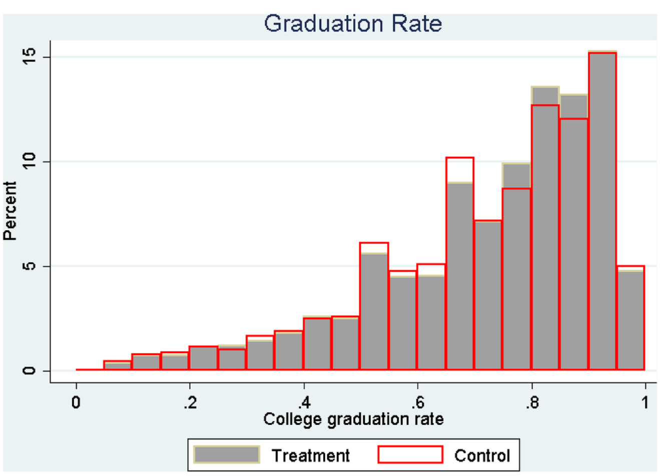


Fig. 1. Comparison of college graduation rate for treatment and control groups, four-year enrollees only.
Notes. Results from a histogram comparing the full distribution of treatment and control groups for all college attendees only. Bins are 5 percentage points.

selection into engagement, with higher engagement rates among students with stronger propensity to attend a CollegePoint college. Overall, the average propensity was 52.5% among engaged students relative to 48.7% among treated students who did not engage, though there is considerable overlap across the distribution of propensity scores.

Under the assumption that positive impacts could only have come through actual program participation, Appendix Table 3 shows complementary but alternative treatment-on-the-treated estimates based on actual program participation. The first-stage treatment effect is based on how many students ultimately contacted their adviser, which as stated above was approximately 44%. Thus results in Appendix Table 3 are identical to those in Table 2 though generally 2.25 times larger, suggesting an increase in CollegePoint attendance of 6.1 percentage points and an increase in the average graduation rate of the college attended of 1.8 percentage points. One concern about interpreting the TOT estimates is if the simple offer of assistance changes students' self-conceptions, inducing them to try harder irrespective of the actual advising. Given our prior work we believe this impact is implausible, especially as students shifted specifically to CollegePoint colleges – which are a priority of the advising program but not reflected in the introductory materials or other outreach directed towards treated students.

We do not find evidence that high-achieving students who were more academically isolated, as identified by Hoxby and Avery (2013), experienced positive impacts from the program. Only 3% of the high-achieving, low- and middle-income students were literally the only student in their high school who scored in the top 10% nationwide, though roughly 12% were the only low- or middle-income student in the top 10% but attended schools with high-income students in this top 10% range. Various regressions focused on students just in these isolated high schools – from the truly isolated (495 students) to those in schools with fewer than ten high-achieving students (4017 students) – all produce null results; the maximum impact on CollegePoint colleges was 0.9 percentage points with a standard error of 1.8.[10]

4.3. Impacts of adviser assignment on college attendance

Each treated student was also randomly assigned to one of 23

[10] Alternate regressions with high school fixed effects, that necessarily focus on students in schools with larger numbers of low- and middle-income, high-achieving students, produce results similar and even slightly larger than those in Table 2, with statistically significant and positive impacts on CollegePoint score sending and attendance of 0.44 score sends and 2.9 percentage points, respectively.

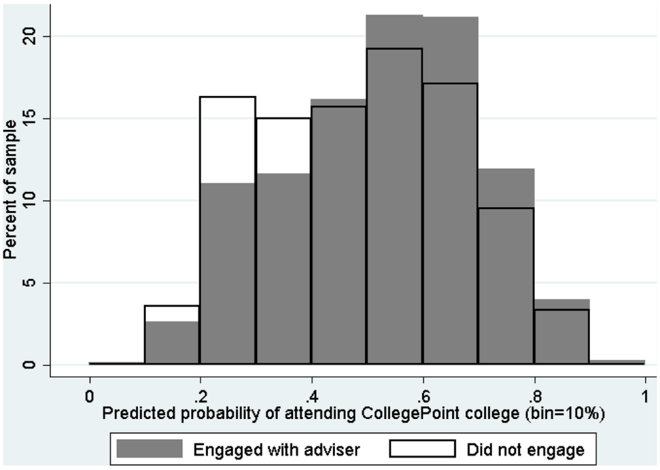


Fig. 2. Adviser engagement rates based on predicted likelihood of attending a CollegePoint college.
Notes. Predicted probabilities derive from a logistic regression using control group students that controlled for: student ethnicity; gender; parental education; school urbanicity; whether they took the SAT zero, one, or two or more times; a cubic of initial SAT math and verbal scores, school-level free and reduced price lunch, and school size.

advisers, which we use to test for evidence of homophily in treatment effects. For example, did female students randomly assigned to a female adviser have more positive outcomes than male students assigned to a female adviser? One challenge is that we have relatively few advisers, with 15 females versus 8 males, and 17 white advisers with four African-American, one Asian, and one Hispanic adviser. As such, these results might be suggestive of potential homophily effects but not conclusive. Appendix Table 2 confirms that being assigned a female or white adviser is not correlated with student background characteristics.

Overall, Table 3 shows some differences in enrollment outcomes based on being assigned to a same sex adviser. The first row of Table 3 shows that being assigned to an adviser of the same sex leads to a two percentage point increase in the likelihood of engaging with the adviser, and leads students to be 1.5 (2%) and 1.2 (2%) percentage points more likely to either enroll in college or enroll in a four-year college, respectively. When we disaggregate by sex, we find that the impacts are essentially null for females but positive for males; in other words, males assigned to male adviser exhibit better outcomes than when they are assigned to female advisers, yet females assigned to female advisers exhibit similar outcomes as females assigned to male advisers. Overall, males who are randomly assigned to a male adviser are 2.3 percentage points (3%) more likely to attend college overall, with roughly two-thirds of this effect being driven by increases in four-year college attendance.

The bottom half of Table 3 shows larger positive impacts when non-white students are assigned to a non-white adviser, relative to a white adviser in our sample. In this instance, we define an ethnic match as including all African-American, Asian, and Hispanic students matched to an adviser of the same ethnicity, noting again that we have only six non-white advisers, with four of them African-American. When white students are assigned to a white adviser we see almost no differences in outcomes than when assigned to a non-white adviser, except for a small decline in total SAT score sends. In contrast, non-white students assigned to a non-white adviser send SAT score to roughly 0.8 (15%) more CollegePoint colleges, are 3.7 percentage points (4%) more likely to attend a four-year college, and 4.6 percentage points (8%) more likely to attend a CollegePoint college. These shifts lead non-white students to attend more selective colleges, as evidenced by small increases in the college's SAT percentile and graduation rates, though also leads students to attend more expensive colleges. One caveat is that we cannot observe the actual price students pay at these schools, and the average net price value we observe does not account for cost

Table 3
Impacts of random assignment to same sex or ethnicity adviser, intent-to-treat estimates.

	(1) Engagement	(2) SAT score sends Non-CollegePoint colleges	(3) CollegePoint colleges	(4) Attendance (NSC data) Any	(5) Four-year	(6) CollegePoint college	(7) Barron's Ranking Top 3	(8) 4 or higher	(9) College characteristics SAT percentile	(10) Graduation rate	(11) Net price: $30-48K
Adviser matching on sex											
Same sex adviser	0.020*	−0.012	−0.030	0.015*	0.012^{+}	−0.001	0.005	0.004	−0.083	−0.004	113.151
	(0.009)	(0.035)	(0.091)	(0.006)	(0.007)	(0.009)	(0.009)	(0.008)	(0.236)	(0.004)	(127.275)
Female and same sex adviser	0.018	−0.046	−0.053	0.006	0.009	0.004	0.011	−0.001	0.195	0.001	336.420^{+}
	(0.014)	(0.051)	(0.133)	(0.009)	(0.010)	(0.014)	(0.013)	(0.011)	(0.340)	(0.006)	(184.467)
Male and same sex adviser	0.022^{+}	0.019	−0.009	0.023**	0.014	−0.006	−0.000	0.009	−0.342	−0.009^{+}	−91.233
	(0.013)	(0.048)	(0.126)	(0.009)	(0.010)	(0.013)	(0.013)	(0.011)	(0.328)	(0.005)	(176.463)
Control group mean	42.6%	1.7	4.6	86.7%	82.7%	52.1%	61.0%	19.9%	88.6	72.8%	$12,281
Adviser matching on ethnicity											
Same ethnicity adviser	−0.020	−0.078^{+}	0.188	0.006	0.008	0.009	0.013	0.001	0.314	0.003	292.488^{+}
	(0.013)	(0.046)	(0.118)	(0.008)	(0.009)	(0.012)	(0.012)	(0.010)	(0.307)	(0.005)	(168.131)
White and same ethnicity adviser	−0.010	−0.099	−0.232	−0.008	−0.013	−0.019	−0.021	0.013	−0.158	−0.006	−182.912
	(0.016)	(0.061)	(0.155)	(0.011)	(0.012)	(0.016)	(0.016)	(0.013)	(0.414)	(0.007)	(222.704)
Non-white and same ethnicity adviser	−0.033^{+}	−0.050	0.775**	0.026*	0.037**	0.047*	0.060**	−0.016	0.897^{+}	0.015^{+}	930.203**
	(0.019)	(0.072)	(0.184)	(0.013)	(0.014)	(0.019)	(0.019)	(0.016)	(0.460)	(0.008)	(258.203)
Control group mean	46.3%	1.7	5.1	88.1%	84.3%	56.0%	64.3%	18.4%	89.6	74.6%	$12,050

Notes: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. All estimates compare the randomized offer of virtual advising to a same sex or ethnicity adviser, restricted to only students in the treatment sample. Pooled regressions include student gender and ethnicity dummies.

differentials across ethnic background that may be smaller for non-white students. Thus non-white students are being encouraged to attend colleges that are nominally more expensive, though there may be unobserved changes in financial aid packages or other institutional behaviors that mitigate this cost.[11]

5. Conclusion

We find that providing an offer of virtual advising to low- and middle-income, high-achieving students increases enrollment in colleges with higher graduation rates. High-achieving students have strong demand for receiving this additional support, with 44% taking up the advising initiative. Students are also responsive to the specific ideas proffered by counselors, as they increased their score sends and enrollment in exactly those colleges identified in the initiative. Given this observed responsiveness, interventions should have very strong priors that the information being offered is actually likely to benefit students. Overall, inducing students to apply to a wider range of more selective colleges is supported by prior literature as likely to produce positive impacts (Hoekstra, 2009; Zimmerman, 2013). Prior work suggests this likely occurs through a close connection with a counselor and dialogue regarding a student's interests, as low-touch interventions that do not strongly prescribe certain colleges produce few effects (Barr & Castleman, 2017; Gurantz et al., 2019; Hyman, 2020). Future information on degree completion rates for this initiative, as well as other similar projects, will be vital for determining whether encouraging students to attend more selective colleges ultimately improves labor force outcomes commensurate with any changes in student debt.

The increases in average selectivity do not appear to coincide with increases in the expected cost of these colleges for low-income students, though these results should be interpreted with caution. Most importantly, we cannot observe what treatment and control group students ultimately pay for colleges, which might be highly variable after taking into account how colleges shape their incoming class and students' background characteristics. In addition, even statistically significant changes in enrollment at high-graduation colleges are unlikely to produce large changes in the average costs faced by a large pool of students, as highlighted by results in Table 2 and associated figures. We find one area where costs may change significantly, as non-white students assigned to non-white advisers appear to attend colleges with higher graduation rates and an expectation of increased tuition costs for low-income students.

These results point to the potential for technology to facilitate remote counseling efforts. Multiple efforts were made to lower the barrier to engaging with an adviser. First, we used an “opt-out” model, in which the only ask of students was to respond to their adviser with a few pieces of information to confirm their identity and eligibility for their program. This may matter as “opt-in” approaches may dramatically lower participation rates (Bergmann, Lasky-Fink, & Rogers, 2019). The initial outreach minimized some costs by leveraging existing College Board channels of email, text and direct mail via their participation in PSAT and SAT exams, rather than requiring counselors to actively identify and convince students, or broader advertising campaigns. The College Board and College Advising Corps also worked to develop robust student-level data sharing to accelerate the advising timeline and reach students earlier in the process.

The findings also highlight potential challenges that advising initiatives must consider moving forward. We did not find that “academically isolated” students – those in high schools with relatively few high-achieving students – were significantly impacted by the initiative, though these results suffer from weaker statistical power. We also find some evidence that students with lower propensity to attend a higher graduation rate college were less likely to engage after receiving the offer of virtual advising. Although virtual advising programs may be a scalable solution for motivated students who are willing to engage with their adviser, more work is needed to develop messages that target and motivate students based on their background characteristics and future plans.

Acknowledgements

We are grateful to the leadership and staff at the College Advising Corps for their partnership on this project. We are grateful to Bloomberg Philanthropies for providing philanthropic support for CollegePoint, and to the CollegePoint program staff for their leadership and collaboration. We also recognize the many colleagues at the College Board that supported the planning, execution and evaluation of this program. Any errors or omissions are our own. All authors except Eric Bettinger were employees of the College Board when this research was conducted, and declare that they have no relevant or material financial interests that relate to the research described in this paper.

Appendix

Appendix Table 1

.

Activity	College point	CB-CAC Collaboration
Identification	~30k students identified (~15k assigned to each organization)	
Assigned to treatment/control	Outreach to all Treatment status assigned after intake	Treatment assigned before outreach@@@@Outreach only to treatment group
Outreach/Recruitment	• Direct mail from CollegePoint • Email from College Board • Phone calls on behalf of CCB	• Direct mail from CB • Email from CB • Text from CB
Intake	Student signs up (https://www.collegepoint.info)	• Student contacts adviser • Adviser confirms student information and eligibility on CB list • Adviser offers times for first meeting
Assigned to advising organization	Random assignment after intake, names provided to advising org	Student (assigned to CAC) agrees to the schedule for the first meeting
First substantive voice-to-voice interaction	Hold first session (voice-to-voice)	

[11] Using NPSAS data we find that African-American and Hispanic students have smaller cost differentials between more and less selective colleges, relative to white and Asian students. In other words moving from a less selective to more selective college is likely to have a smaller real impact on net price for African-American and Hispanic students, even though the sticker price is obviously similar.

Appendix Table 2
Balance of observed covariates on assignment to adviser type.

	Female	Parent BA	White	Hispanic	African-American	Asian	SAT: First score	School size	City	Suburb	Town	Rural	FRPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Assigned female adviser	−0.008	0.007	−0.003	0.008	−0.002	−0.003	2.100	13.893	0.020*	−0.008	−0.010*	−0.004	0.002
	(0.010)	(0.009)	(0.010)	(0.007)	(0.009)	(0.004)	(1.693)	(23.059)	(0.009)	(0.009)	(0.005)	(0.006)	(0.005)
Assigned white adviser	−0.001	−0.006	−0.016	0.008	0.011	−0.004	0.737	−20.387	0.012	0.002	0.000	−0.011	0.000
	(0.011)	(0.010)	(0.011)	(0.008)	(0.010)	(0.005)	(1.852)	(25.502)	(0.010)	(0.010)	(0.005)	(0.006)	(0.005)
P-value of joint test	0.722	0.533	0.322	0.422	0.497	0.610	0.460	0.517	0.085	0.666	0.075	0.252	0.881

Notes: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$.

Appendix Table 3
Impacts of virtual advising, Instrumental variables estimates.

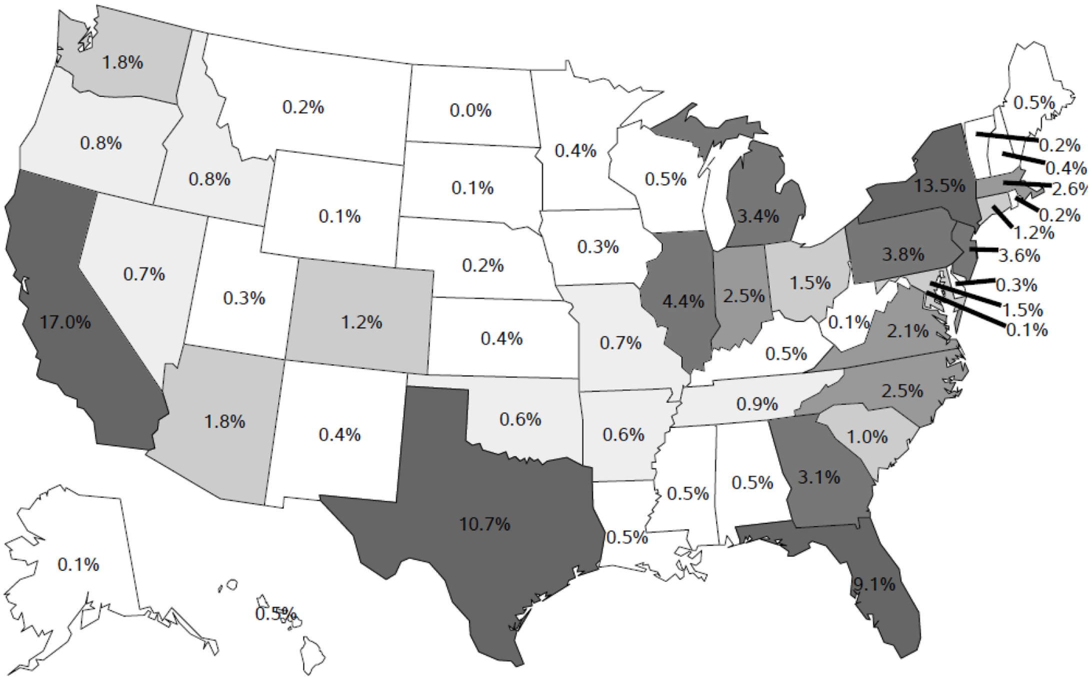
Description	First-stage engagement	(1) SAT score sends Non-CollegePoint colleges	(2) CollegePoint colleges	(3) Attendance (NSC data) Any	(4) Four-year	(5) CollegePoint college	(6) Barron's Ranking Top 3	(7) 4 or higher	(8) College characteristics SAT percentile	(9) Graduation rate	(10) Net price: $30-48K
All students	0.434**	0.002	0.720**	0.007	0.022	0.060**	0.039^{+}	−0.031^{+}	0.383	0.017*	−66.739
	(0.008)	(0.077)	(0.197)	(0.014)	(0.016)	(0.021)	(0.020)	(0.018)	(0.499)	(0.008)	(272.439)
Control group mean		6.0	4.3	87.3%	82.7%	50.0%	60.9%	26.1%	88.5	72.1%	$12,391
Sample size		16256	16256	16256	16256	16256	16256	16256	12654	14023	14134
Wave 1	0.452**	0.007	0.493^{+}	−0.005	0.011	0.032	0.033	−0.039	0.169	0.012	−300.630
	(0.011)	(0.101)	(0.263)	(0.019)	(0.022)	(0.028)	(0.028)	(0.025)	(0.684)	(0.012)	(358.052)
Control group mean		5.5	4.0	87.1%	82.3%	48.5%	59.6%	27.4%	88.5	71.8%	$12,136
Sample size		8203	8203	8203	8203	8203	8203	8203	6374	7032	7092
Wave 2	0.416**	−0.005	0.965**	0.019	0.034	0.089**	0.045	−0.022	0.639	0.023^{+}	182.475
	(0.011)	(0.118)	(0.297)	(0.020)	(0.022)	(0.031)	(0.030)	(0.026)	(0.730)	(0.012)	(414.814)
Control group mean		6.4	4.6	87.5%	83.0%	51.5%	62.2%	24.9%	88.4	72.4%	$12,638
Sample size		8053	8053	8053	8053	8053	8053	8053	6280	6991	7042
Low-income	0.447**	0.190^{+}	1.141**	0.011	0.029	0.055*	0.030	−0.016	0.341	0.013	297.083
	(0.010)	(0.101)	(0.261)	(0.018)	(0.020)	(0.027)	(0.026)	(0.023)	(0.643)	(0.011)	(352.670)
Control group mean		6.3	4.6	86.9%	82.4%	52.1%	62.4%	24.1%	89.1	73.5%	$12,047
Sample size		9397	9397	9397	9397	9397	9397	9397	7355	8078	8137
Middle-income	0.415**	−0.272*	0.115	0.000	0.012	0.066*	0.052	−0.053^{+}	0.469	0.024^{+}	−588.017
	(0.012)	(0.119)	(0.301)	(0.022)	(0.025)	(0.033)	(0.033)	(0.030)	(0.792)	(0.013)	(428.793)
Control group mean		5.5	3.9	87.7%	83.0%	47.2%	58.9%	28.8%	87.6	70.2%	$12,839
Sample size		6859	6859	6859	6859	6859	6859	6859	5299	5945	5997

Notes: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. All estimates compare the randomized offer of virtual advising to control group students not offered virtual advising.

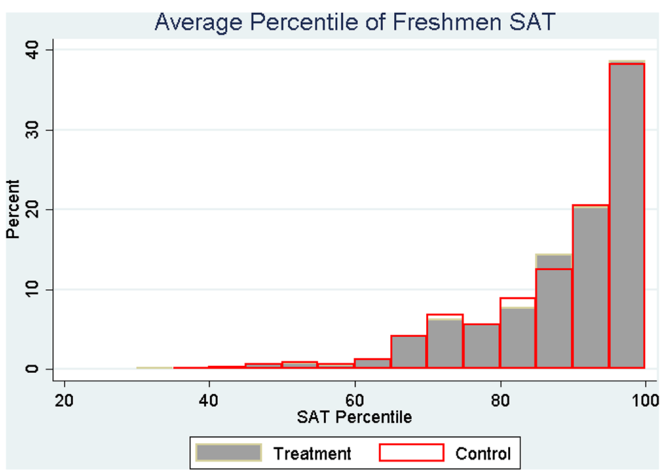
Appendix Table 4
College attendance outcomes, National Student Clearinghouse data.

Sample	*N*	(1) CollegePoint colleges Barrons ranking 1	(2) 2	(3) 3	(4) 4-7	(5) Non-College Point colleges Barrons ranking 1	(6) 2	(7) 3	(8) 4-7
All students	16256	0.008	0.013^{+}	0.005	0.001	0.000	−0.007*	−0.002	−0.015^{+}
		(0.007)	(0.008)	(0.005)	(0.002)	(.)	(0.003)	(0.005)	(0.008)
Control group mean		18.6%	21.3%	8.2%	1.9%	0.0%	3.1%	9.7%	24.3%
Wave 1 (Spring outreach)	8203	0.003	0.007	0.005	−0.001	0.000	−0.002	0.002	−0.017
		(0.010)	(0.011)	(0.007)	(0.003)	(.)	(0.004)	(0.008)	(0.011)
Control group mean		19.2%	21.1%	6.6%	1.7%	0.0%	2.8%	10.0%	25.7%
Wave 2 (Fall outreach)	8053	0.014	0.018^{+}	0.004	0.002	0.000	−0.011**	−0.005	−0.013
		(0.010)	(0.011)	(0.008)	(0.004)	(.)	(0.004)	(0.007)	(0.011)
Control group mean		18.0%	21.5%	9.8%	2.1%	0.0%	3.4%	9.4%	22.9%
Low-income	9397	0.005	0.013	0.008	0.000	0.000	−0.007^{+}	−0.004	−0.009
		(0.010)	(0.010)	(0.006)	(0.003)	(.)	(0.004)	(0.007)	(0.010)
Control group mean		21.5%	21.6%	6.9%	2.1%	0.0%	3.6%	8.7%	22.1%
Middle-income	6859	0.012	0.013	0.001	0.001	0.000	−0.007^{+}	0.002	−0.023^{+}
		(0.010)	(0.011)	(0.008)	(0.004)	(.)	(0.004)	(0.009)	(0.012)
Control group mean		14.7%	21.0%	10.0%	1.6%	0.0%	2.4%	10.9%	27.2%

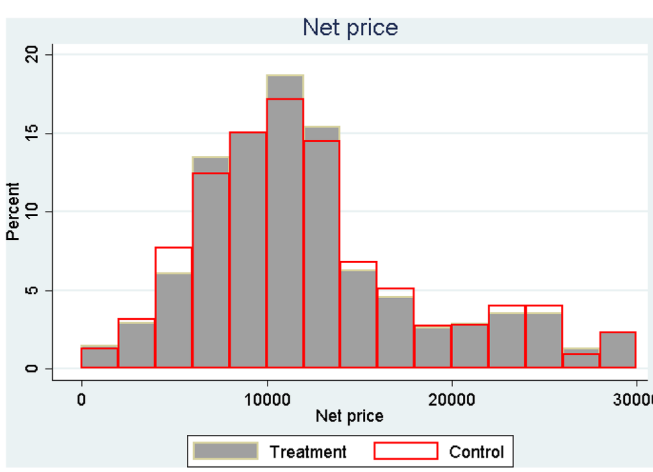
Notes: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$. All estimates compare the randomized offer of virtual advising to control group students not offered virtual advising.



Appendix Fig. 1. Geographic distribution of low- and middle-income high-achieving students in the experimental sample



Appendix Fig. 2. Comparison of college SAT percentile score for treatment and control groups, four-year enrollees only.
Notes. Results from a histogram comparing the full distribution of treatment and control groups for four-year college attendees only. Bins are 5 percentile points.



Appendix Fig. 3. Comparison of college net price for families earning $30,000 to $48,000 for treatment and control groups, all college enrollees.
Notes. Results from a histogram comparing the full distribution of treatment and control groups for all college attendees only. Bins are $2000.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2020.101974.

References

Bailey, M. J., & Dynarski, S. M. (2011). Inequality in postsecondary education. In G. J. Duncan, & R. J. Murnane (Eds.). *Whither opportunity* (pp. 117–132). Russell: Sage.

Barr, A., & Castleman, B. L. (2017). *The bottom line on college counseling*.

Bastedo, M. N., & Jaquette, O. (2011). Running in place: Low-income students and the dynamics of higher education stratification. *Educational Evaluation and Policy Analysis, 33*(3), 318–339.

Bergmann, P., Lasky-Fink, J., & Rogers, T. (2019). Simplification and defaults affect adoption and impact of technology, but decision makers do not realize it. *Organizational Behavior and Human Decision Processes*.

Bettinger, E. P., & Evans, B. J. (2020). College guidance for all: A randomized experiment in pre-college advising. *Journal of Policy Analysis and Management*.

Bird, K. A., Castleman, B. L., Denning, J. T., Goodman, J. S., Lamberton, C., & Rosinger, K. O. (2019). *Nudging at scale: Experimental evidence from FAFSA completion campaigns.* NBER Working Paper No. 26158. Cambridge, MA.

Carrell, S. E., & Sacerdote, B. I. (2017). Why do college-going interventions work? *American Economic Journal: Applied Economics, 9*(3), 124–151.

Castleman, B. L., & Goodman, J. S. (2018). Intensive college counseling and the enrollment and persistence of low-income students. *Education Finance and Policy, 13*(1), 19–41.

Castleman, B. L., & Page, L. C. (2015). Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? *Journal of Economic Behavior and Organization, 115*, 144–160.

Castleman, B. L., Page, L. C., & Schooley, K. (2014). The forgotten summer: Mitigating summer attrition among college-intending low-income high school graduates. *Journal of Policy Analysis and Management, 32*(2), 320–344.

Cohodes, S. R., & Goodman, J. S. (2014). Merit aid, college quality and college completion: Massachusetts' adams scholarship as an in-kind subsidy. *American Economic Journal: Applied Economics, 6*(4), 251–285.

Goodman, J. S., Hurwitz, M., & Smith, J. (2017). Access to four-year public colleges and degree completion. *Journal of Labor Economics, 35*(3), 829–867.

Gurantz, O., Howell, J., Hurwitz, M., Larson, C., Pender, M., & White, B. (2019). *Realizing Your College Potential? Impacts of College Board's RYCP Campaign on Postsecondary Enrollment.* Annenberg Institute at Brown University19–40 WorkingPaper.

Hoekstra, M. (2009). The effect of attending the Flagship State University on earnings: A discontinuity-based approach. *Review of Economics and Statistics, 91*(4), 717–724.

Hoxby, C. M., & Avery, C. (2013). *The missing "One-Offs": The hidden supply of high-achieving, low income students.* Washington DC: Brookings Institution Brookings Paper on Economic Activity.

Hoxby, C. M., & Turner, S. E. (2013). *Expanding college opportunities for high-achieving, low income students.* Stanford, CA: Stanford Institute for Economic Policy Research.

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

Klasik, D. (2012). The college application gauntlet: A systematic analysis of the steps to four-year college enrollment. *Research in Higher Education, 53*(5), 506–549.

Page, L. C., Kehoe, S. S., Castleman, B. L., & Sahadewo, G. A. (2017). More than dollars for scholars: The impact of the dell scholars program on college access, persistence and degree attainment. *Journal of Human Resources*.

Page, L. C., & Scott-Clayton, J. (2016). Improving college access in the United States: Barriers and policy responses. *Economics of Education Review, 51*, 4–22.

Phillips, M., & Reber, S. J. (2019). *Does Virtual Advising Increase College Enrollment? Evidence from a Random Assignment College Access Field Experiment.* NBER Working Paper No. 26509. Boston, MA.

Smith, J. (2018). The sequential college application process. *Education Finance and Policy, 0*(JA), 1–54.

Smith, J., Pender, M., & Howell, J. (2013). The full extent of student-college academic undermatch. *Economics of Education Review, 32*, 247–261.

Sullivan, Z., Castleman, B., & Bettinger, E. (2019). *College advising at a national scale: Experimental evidence from the CollegePoint initiative.* Annenberg Institute at Brown University WorkingPaper: 19-123.

Zimmerman, J. (2013). *School choice, opportunity and access: A geographic analysis of public school enrollment in New Orleans.* Retrieved from.