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AREA CONFERENCES 2022

Economics of Education

Munich, 2 – 3 September 2022

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April 2022

Abstract

We combine a large multi-site randomized control trial with administrative and survey data to demonstrate that intensive advising during high school and college leads to large increases in bachelor's degree attainment. New causal forest methods suggest that these increases are driven primarily by improvements in the quality of initial enrollment. Program effects are consistent across sites, cohorts, advisors, and student characteristics, suggesting the model is scalable. While current and proposed investments in postsecondary education focus on cutting costs, our results suggest that investment in advising is likely to be a more efficient route to promote bachelor's degree attainment.

Keywords: College, intensive advising, degree attainment, mobility

*We are grateful to Bottom Line for partnering with us on designing an experimental evaluation of their program. We are grateful for financial support from the Michael & Susan Dell Foundation, the Coalition for Evidence-Based Policy, and the Laura and John Arnold Foundation. We thank seminar participants at the 2017 CESifo Economics of Education meeting and Columbia University and Peter Bergman, Mark Hoekstra, Richard Murphy, and Jonah Rockoff for helpful comments. A pre-registered analysis plan is available here: https://osf.io/fg7hs/

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1 Introduction

A college education remains an effective channel through which children born into low-income families can achieve greater economic opportunity. Among those born to parents in the bottom quintile, youth who attend college are two and a half times as likely to make it to the top income quintile (Chetty et al. 2017). The economic and non-pecuniary benefits of college are most pronounced among bachelor's degree-holders. For instance, adults with a bachelor's degree earn near $20,000 more per year than adults with some college credits but no degree (Ma, Pender, and Welch, 2016).

At the same time, socioeconomic gaps in college completion have widened; while half of people from high-income families obtain a bachelor's degree by age 25, only one in ten people from low-income families do (Bailey and Dynarski, 2011). Differences in preparation explain some of the attainment gap, but disparities in college success by family income persist even upon control for academic achievement (Bailey and Dynarski, 2011; Belley and Lochner, 2007).

These persistent disparities have motivated efforts to increase degree attainment among students from lower income backgrounds. Most efforts have centered on the role of college costs, including significant expansions to the need-based federal Pell Grant and the adoption and continued promotion of "free college" programs. While much academic and policy attention has focused on affordability barriers as a central factor contributing to socioeconomic gaps in college completion, evidence suggests that a lack of financial resources may not be the primary driver of gaps in degree attainment (Bulman et al. 2021). Increasing research shows that many financial aid packages have more modest effects on degree attainment than one might expect given the generosity of the awards (Angrist, Autor, and Pallais 2021; Barr et al. 2021; Bettinger et al. 2019; Dynarski 2008; Fitzpatrick and Jones 2016; Sjoquist and Winters 2012). One reason for these relatively modest impacts may be that most aid packages do little or nothing to address informational and assistance barriers that likely impede low-income students from navigating the application process or selecting institutions from which they are most likely to graduate and receive a meaningful return.[1] In this paper, we investigate how the provision of personalized college guidance and support via intensive

[1] Consistent with this, some relatively light-touch interventions have had outsized effects on college enrollment or enrollment quality (Pallais 2015; Bettinger et al., 2012; Hoxby and Turner, 2013, Castleman, Page, and Schooley, 2015).

advising influences bachelor's degree attainment.

College advising programs are an important supplement to the limited college advising students receive within their high schools.[2] These initiatives are widespread — college advising organizations serve two million students per year (more than half the number of high-school graduates each year).[3] Yet despite the volume of programs and the magnitude of financial investment in these organizations, rigorous evidence of the impact of advising on students' college success is fairly limited. A few studies demonstrate increased enrollment in college, but those that track students beyond initial enrollment suggest that these effects fade (Avery 2013; Bettinger and Evans, 2019; Carrell and Sacerdote 2017).[4] Our study provides new evidence on the extent to which intensive college advising generates increases in bachelor's degree receipt. Given the sizeable share of students who drop out from college after the first year and the many benefits associated with degree receipt, it is critical to understand whether initial enrollment effects translate into increased degree attainment.

Our paper contributes precisely-identified evidence of the effect of an intensive college advising program on low-income students' bachelor's degree attainment.[5] We conducted a multi-cohort and multi-site, randomized controlled trial of the Bottom Line (BL) college advising program, which operates in several cities in Massachusetts, New York, and Illinois, drawing students from several hundred high schools.[6]

The BL model is divided into two distinct stages: Access advising and Success advising. Access advisors provide individualized advising to students from the summer before senior year of high

[2]College guidance only accounts for approximately 20 percent of the typical high school counselor's workload (Bridgeland and Bruce 2011). Consistent with this, recent evidence suggests that high-school counselors vary in their effectiveness at improving students' behavioral and academic outcomes during and just after high school (Mulhern 2020). Mulhern makes clever use of the quasi-random assignment of counselors to students in Massachussetts to demonstrate that better counselors can generate reductions in the likelihood of suspension and modest increases in the likelihood of high school graduation and college-going. Students assigned a 1 SD better counselor are 1.4 percentage points more likely to attend college and 1.1 percentage points more likely to persist into their second year. While the empirical approach does not allow for the evaluation of the effectiveness of high school counselors per se, being assigned one of the very best counselors instead of one of the very worst increases college going by around 2 percentage points.

[3]Estimates from National College Attainment Network (NCAN).

[4]In Avery (2013), the estimated "intent to treat" effect for any enrollment became negative from the fall to spring semester and the estimated effect on four-year enrollment fell roughly a third, from 15.1 to 9.7 percentage points. In Bettinger and Evans (2019), modest initial impacts of 1-2 percentage points (which were primarily on two-year enrollment) dissipated over time with a non-significant 0.6 percentage point increase in the likelihood of enrollment in the second year. In Carrell and Sacerdote (2017), those assigned to treatment were roughly 4 percentage points less likely to be enrolled in the second year, conditional on enrollment in the first.

[5]Bottom Line serves students from families that make less than 200 percent of the federal poverty line.

[6]Bottom Line is also in the process of exploring scaling to additional states. BL's site in Chicago opened in 2014 and is not included in our analysis.

school through the summer after high school. Like many college access programs, advisors work with students on identifying well-matched colleges to apply to and on completing and submitting college applications. Advisors encourage students to apply to and attend four-year colleges and universities, with the goal of increasing bachelor's degree attainment among the population of economically-disadvantaged, academically college-ready students. In particular, advisors encourage students to consider a set of target institutions that BL has identified as providing students with an optimal combination of quality and affordability. Success advising is a unique component of the BL model. For students who enroll at one of the target institutions (approximately 50 percent of advisees choose to do so), BL continues to provide individualized, campus-based support to students for up to six years following high school.

We find that students randomly offered intensive college advising are substantially more likely to earn a bachelor's degree within 5-6 years of high school. Pooling across experimental cohorts (high school graduating classes of 2015 and 2016), students offered advising were 7.6 percentage points more likely to earn a bachelor's degree within five years of high school; this represents a 16 percent increase relative to the control group. Students in the first experimental cohort were 9.6 percentage points more likely to earn a bachelor's degree within six years of high school, an 18 percent increase relative to the control group. These effects on bachelor's attainment are as large as any rigorous estimates we have come across in the economics literature.

In the face of declining social mobility, intensive advising may be an effective policy strategy to promote greater economic opportunity for economically-disadvantaged, academically college-ready students. Indeed, the estimated impact of advising on bachelor's degree attainment is roughly as large as the (conditional on aptitude) gap in degree attainment between children from families in the first and fourth quartile of the income distribution (Belley and Lochner 2007). While the observed degree effects are quite consistent across different types of students, the fact that BL primarily serves students of color further suggests that substantial expansion could contribute to increased racial equity and mobility as well.

Degree effects appear driven primarily through shifts to higher quality enrollment. Pooling across cohorts, students offered intensive advising were 9.1 percentage points (13 percent) more likely to attend four-year institutions, with almost all of this marginal enrollment occurring at institutions with high graduation rates.[7]

We use new causal forest methods to demonstrate the strong relationship between the likelihood that intensive advising shifts an individual into a higher quality institution and the likelihood that it increases that individual's BA attainment.[8] Individuals whom intensive advising likely shifts to higher quality institutions (those with above-median PTEs on initial enrollment at an institution with an above-median graduation rate) appear to account for nearly all of the estimated treatment effects on bachelor's degree attainment. Consistent with this conclusion, conditioning on initial enrollment and enrollment quality in a simple mediation analysis suggests that most of the treatment effect on BA attainment operates through initial college choice, with perhaps a modest role for the ongoing advising provided to some students while in college.[9] These causal forest methods could be applied in numerous other settings where reseachers are interested in better understanding the role of mediating factors in driving longer-term impacts.

Detailed advisor-student interaction data and student surveys indicate that advisors appear to play a particularly important role in shaping school choice. Advisors spent an average of 10-15 hours with each student from the summer before senior year of high school through when students transition to college. While treated and control students were equally likely to apply to college and for financial aid, treatment students applied to significantly more colleges. Furthermore, most students noted that BL staff were very important in their application and decision process.

We also leverage the quasi-random assignment of advisors to investigate how advising behavior, as well as advisor backgrounds and characteristics, affect student outcomes and to investigate the potential scalability of the intensive advising model.[10] We find little relationship between advisor demographics or behaviors and advisor effectiveness. Indeed, when we estimate advisor

[7]We estimate an 8.8 percentage point (32 percent) increase in the likelihood that an individual attends a four-year college with an above-median graduation rate.

[8]To do so we estimate personalized treatment effects (PTEs) on enrollment at an institution with above-median graduation rates and PTEs on BA attainment using causal forest methods. Causal forests use machine learning to estimate complex relationships between baseline covariates, treatment assignment, and outcomes of interest. Based on these relationships and each subject's covariates, causal forests then estimate individual treatment effects for each subject. We provide further explanation of causal forest methods in the Mechanisms section and Appendix B below (Athey and Imbens, 2016; Athey and Wager, 2019).

[9]The estimated "treatment effect" on BA attainment falls to 1.6 percentage points once we condition on whether an individual enrolls in a two-year, four-year, or four-year with above-median graduation rates in the fall after high-school graduation. This result is only suggestive, however, given the strong assumptions underlying mediation analyses.

[10]Student baseline characteristics are balanced across advisors, consistent with random assignment.

fixed effects, 29 out of the 30 advisors have a positive effect on four-year college enrollment.[11] This lack of heterogeneity across different types of advisors suggests that BL has a well-developed set of advisor recruitment, selection, training, and development processes for ensuring advisor success, and supports the potential scalability of the model, since results do not appear to be driven by a small set of particularly high-performing advisors.[12]

Taken collectively, these results suggest that the intensive advising model has the potential to maintain its positive impact with diverse populations in numerous settings. To put the magnitude of these effects into context, Figure 1 illustrates that the direct cost per additional bachelor's degree generated by intensive advising appears to be substantially lower than that for programs that provide significant financial assistance for college. Aid programs increase bachelor's degree attainment by one percentage point or less per $1,000, with most rigorously-evaluated aid programs increasing attainment by less than 0.5 percentage points per $1,000. By contrast, BL advising increases bachelor's degree attainment by over two percentage points per $1,000. Indeed, the advising model appears to be as cost-effective at increasing bachelor's degree attainment as any rigorously evaluated policy of which we are aware. While maintaining implementation quality at scale for intensive advising may be more challenging than for aid programs, the consistency of impacts across cohorts, sites, student groups, and advisors suggests that providing cities with a combination of financial and technical assistance to replicate the model could be a more efficient approach to increasing bachelor's degree attainment than offering students additional aid.

2 Background

Our study builds on a relatively recent body of work aimed at rigorously evaluating the short-term effects of college advising. Unlike with financial aid, the structure of how advising is provided has afforded few opportunities for quasi-experimental evaluation. This has resulted in a handful of heterogenous and small-scale randomized investigations of advising programs. Carrell and Sacerdote (2017) find evidence that a combination of near-peer mentoring and fee waivers led to substantial

[11] An F-test on the advisor fixed effects indicates that we cannot reject the null of equivalent advisor effects on four-year college enrollment (p-value of 0.902).

[12] The New York City program only began with the high-school graduating class of 2012 and thus provides a more direct test of the scalability of the program. We find similarly large effects of the program there.

increases in the share of female students who completed two years of college, while Avery (2013)'s pilot RCT of a program (N=238) providing standardized test tutoring in addition to assistance with college choice and applications found positive effects on four-year enrollment.[13] Both studies provide evidence that advising can increase initial enrollment, but neither study investigates impacts on degree attainment or persistence beyond the second year in college. Furthermore, in both cases the estimated effect of advising on enrollment appeared to dissipate over time, suggesting that marginal enrollees were less likely to persist. In Avery (2013), the estimated "intent to treat" effect for any enrollment became negative from the fall to spring semester and the estimated effect on four-year enrollment fell roughly a third, from 15.1 to 9.7 percentage points. In Carrell and Sacerdote (2017), those assigned to treatment were roughly 4 percentage points less likely to be enrolled in the second year, conditional on enrollment in the first. Bettinger and Evans (2019) overcome concerns related to the confounding role of spillovers in evaluating a near-peer model of advising at the school level. They find more modest effects of school-level access to a near-peer advisor (1.1 percentage points), with these effects similarly dissipating in the second year after high school (0.6 percentage points).[14]

Bos et al. (2012) evaluate a similar near-peer mentoring program in a somewhat poorer and more Hispanic population in Los Angeles and find no overall effects on enrollment or four-year enrollment. Other evaluations of large-scale quasi-advising programs targeted at poor students (such as the federally-funded Upward Bound program) found no impact on enrollment (Seftor, Mamun, and Shirn, 2009).[15]

[13]Avery also has two interesting small-scale studies (Avery 2010; Avery 2014) that focus on *very high achieving students* and the extent to which mentoring (or tele-mentoring) can influence college match, finding suggestive effects on college match.

[14]Bettinger and Baker (2014) demonstrate that individualized coaching for *current* college students can generate moderate (3-4 pp) increases in persistence and "degree" attainment, although the data do not allow for the observation of college type or quality, nor differentiation between certificate completion and receipt of an associate's or bachelor's degree. Their study focuses on a different population than our study or the other studies that have rigorously evaluated college advising programs: their students have already enrolled in college (and thus having no aspect of college choice advising) and have a mean age of 31. In addition to the aforementioned uncertainty regarding credential type, the authors do not have access to data on most students' socioeconomic or racial/ethnic background, so it is unclear whether the in-college coaching program the authors study reduces persistent economic and racial inequalities in postsecondary attainment. Oreopoulos and Petronijevic (2019) also investigate an individualized coaching model with first-year students at the University of Toronto. Unlike with the Bettinger and Baker (2014) study or the BL model, however, coaches were current upperclass undergraduates working part-time on the project. Neither text-based nor face-to-face coaching led to improved student grades or persistence.

[15]In a somewhat different context (Ontario), Oreopoulos and Ford (2016) evaluate a schoolwide intervention in which students participated in a series of school-based workshops in which they received guidance on choosing postsecondary programs to pursue, completed applications, and applied for financial aid. Students assigned to these

An important contrast between Bottom Line and these advising programs is the intensity of the program model. Bottom Line employs professional advisors and supports students during high school and throughout college (if they attend a BL "target" college, which approximately 50% do). As we show in Figure 2, the average student spends 10-15 hours with their advisor before transitioning to college, which represents a more intensive level of engagement than the programs described above.

2.1 Bottom Line

BL began in Boston in 1997 and now operates programs in Boston, Worcester (MA), New York City, and Chicago. Students are initially admitted into the Access program, which provides students with college and financial aid application support during high school. BL actively promotes the Access program through high schools and non-profit partners in each community. Students apply to the Access program during the second half of their junior year of high school. BL collects a substantial amount of self-reported academic and demographic information from students through the application, and verifies self-reported family income and academic performance information through tax records and high school transcripts, respectively. Students are eligible for BL if their families make less than 200 percent of the federal poverty guidelines and if they have a high school GPA of 2.5 or higher.[16] The program serves a sizeable share of the students that meet the program eligibility requirements in its focal cities – 60-70 percent in the Boston area (i.e., the Boston and Worcester sites). The high rate of take up suggests that were BL to scale, it would likely reach most eligible students in small/medium cities.[17]

BL advisors begin working with admitted students between the end of their junior year and the start of their senior year of high school. Advisors work full time. All advisors have a college

workshops were 2.9 percentage points more likely to go to college, but the effect was driven entirely by increasing enrollment at community college, and the study does not provide longitudinal data on whether treated students persisted in or graduated from college at higher rates.

[16] An earlier regression discontinuity design evaluation of the BL program in the Boston area found suggestive evidence that BL positively impacts college enrollment (Castleman and Goodman, 2016). The paper does not examine impacts on degree attainment, and the results are imprecisely estimated (including enrollment effects as small as negative 20 pp and as large as 40 percentage points), rely on a manipulable running variable, and are local to the 2.5 GPA threshold determining eligibility for the program.

[17] This estimate is based on a market analysis BL contracted a consulting firm to conduct in its Massachusetts markets. While BL was not operating in New York at the time of the market analysis, the firm's estimates suggest that BL's reach in New York only serves a small share of the roughly 12,000 students in the city who meet the program eligibility requirements.

degree and 17 percent have a master's degree. Most advisors are female (75 percent) with roughly a quarter black and a quarter Hispanic. The median advisor age is 26. Advisors have an average caseload of 50-60 students and meet with each student for an hour every three or four weeks during senior year, at BL's office in each community. BL advisors provide comprehensive college and financial aid support for students, ranging from creating lists of potential schools and supporting students with essay writing and application completion, to applying for financial aid, searching for scholarships, interpreting financial aid award letters, and selecting a college or university that aligns with a student's goals and circumstances. Outside of direct time with students, advisors work on additional advising-related activities, like developing customized college lists for students, reviewing students' college essays, and analyzing students' award letters.

Once students have chosen where to enroll in college, students who plan to attend one of BL's target institutions are invited to continue into the Success program.[18] The target institutions are primarily moderately selective four-year colleges and universities, approximately two-thirds of which are public institutions. Campus-based advisors at each target institution meet regularly with students once they have matriculated in college; first-year students meet with advisors approximately three to four times per semester, while older students meet with an advisor twice a semester on average.[19] Success advisors provide a combination of academic support (e.g., course selection and making use of advising and tutoring services) and social support (e.g., helping students adjust to a new environment, getting involved with activities and student groups), and advise students on how to balance academic, work, social, and family commitments. The cost of providing Access and Success supports is approximately $4,000 per student$.

3 Experimental Design

We collaborated with BL staff to modify its student application processes in the spring of 2014 and spring of 2015 to incorporate a lottery design into BL's selection of applicants. Among students who met the BL eligibility criteria (GPA of at least 2.5 and family income below 200 percent of

[18] Appendix Table A1 shows the list of encouraged institutions at each site

[19] Advisors adjust meeting frequency based on student need, meeting more regularly with students who are experiencing some form of challenge in college. Success advisors typically serve students across 2-3 different campuses, and work with 30-40 students per campus.

the poverty line), we randomized students to either receive an offer to participate in the BL Access advising program or to be in a control group that did not receive any BL services. In each site BL had minimum commitments to its funders and community partners of the number of students it had to serve, which are reflected in the treatment/control ratios we report in Table A2.[20]

3.1 Data

Our data come from four primary sources: the BL application, BL advisor interaction data, two surveys we conducted with students during the spring of their senior year in high school and the fall after high school graduation, and the National Student Clearinghouse. The BL application collects rich student-level baseline information, including race/ethnicity, gender, whether the student is the first in their family to go to college, whether they were working with another college access organization at the time they applied for BL, their high school GPA and SAT/ACT scores (if they had taken the exam), family income, and whether they had a sibling who had participated in BL. The interaction data contain detailed information on each interaction students had with an advisor, including the topic discussed, assistance the advisor provided with this topic, and narrative comments from the advisor about their interaction with students. Our spring of senior year survey asked students where they applied to college and whether they had been accepted to each institution; whether and when students applied for financial aid; whether students received assistance reviewing their financial aid award letters; and a series of questions about factors influencing students' decisions about whether and where to enroll in college. Our fall after high school survey asked about students' enrollment intensity, campus engagement, course taking, and employment. The National Student Clearinghouse (NSC) provides student by term-level college enrollment and degree attainment data, with coverage across more than 97 percent of college enrollments in the country.

3.2 Baseline Equivalence

In Table 1, we report results from models in which we regress student-level baseline characteristics on the treatment indicator and site by cohort fixed effects. Across 20 baseline measures we only

[20] The first experimental cohort includes 1,429 students and the second experimental cohort includes 993 students.

find 2 statistically significant differences between the treatment and control group at the 10 percent level, which is probabilistically what we would expect given the number of tests we conduct.

4 Empirical Strategy and Results

We estimate the effects of an offer to participate in BL on college enrollment, enrollment quality, persistence, and degree attainment. As the proportion assigned to treatment varied by site and cohort, we control for site by cohort fixed effects. In most specifications we condition on covariates to increase precision. Our basic specification is:

$$y_i = \alpha + \beta X_i + \theta Treatment_i + \sum_j \gamma_j l_{ij} + \varepsilon_i \tag{1}$$

where y_i is generally a post-secondary education outcome for individual i and X_i includes baseline demographic controls (gender, race, citizenship), measures of family resources and background (parents' AGI, parental employment status, household size, first generation status, whether sibling went to college), measures of aptitude (standardized GPA, state standardized test scores), and measures of college guidance resources (whether student is working with another advising organization, whether sibling participated in BL). The l_{ij} are site by cohort fixed effects, which are included because the probability of being assigned to treatment varies by site and cohort. The coefficient of interest is θ, which is the intention to treat (ITT) estimate.

4.1 Enrollment

Table 2 contains our estimates of the impact of BL on students' enrollment and enrollment quality. The point estimate in the first row of column (2) shows that assignment to treatment increases the likelihood of college enrollment by 5.3 percentage points (6.5 percent). The primary focus of the BL model is promoting four-year and higher-quality college enrollment, and in turn, bachelor's degree attainment. Estimates in the second row of Table 2 indicate even larger effects on four-year enrollment, with a 9.1 percentage point increase; this is a 13 percent increase relative to four-year enrollment in the control group. As expected given BL's emphasis on four-year enrollment, estimates in the third row indicate a reduction in two-year enrollment (4 percentage points) contributed to the rise in four-year enrollment.

The remaining rows in Table 2 estimate the impact of BL on the quality of institutions at which students enroll. Treated students are substantially more likely to attend institutions with higher graduation rates, lower default rates, higher post-enrollment earnings, and higher mobility among attendees.[21] For instance, BL students are 11 percentage points more likely to attend institutions with above-median earnings for graduates; this represents a 17 percent increase relative to the control mean.[22]

Despite college enrollment increasing dramatically over the last thirty years, the rate of four-year college completion has barely increased. Many college students fail to make it past the first year, underscoring the importance of evaluating the effects of intensive advising past the point of initial enrollment.

In Figure 3 we compare enrollment rates over time for the BL students compared to the control group. We measure enrollment each term (Fall or Spring) starting one year after high school and continuing through five years after high school. The top panel presents overall enrollment rates over time while the bottom panel presents enrollment rates over time at four-year institutions. Starting in the first Fall semester after high school, and continuing through the Spring semester four years after high school, BL students maintain a substantially higher rate ($\approx$5 percentage points) of enrollment than control students (panel (a)). We observe a very similar but more pronounced pattern of increased four-year enrollment over time, with BL students maintaining a higher rate of enrollment ($\approx$10 percentage points) starting the first Fall after high school and continuing four years after high school (panel (b)). In both cases, we observe virtually no difference in enrollment starting five years after high school as a sizeable share of students assigned to BL earn bachelor's degrees and leave college.[23]

[21] Specifically, we estimate effects on whether an individual is initially observed enrolled at a four-year institution with an above median 6-year graduation rate (53.8), with a student loan default rate below the median (0.07), with earnings above the median for all college enrollees ($35,800) in Chetty et al. (2017), and with "mobility" above the median college in Chetty et al. (2017).

[22] The estimates in Table 2 are essentially unchanged upon incorporation of covariates or the estimation of average marginal effects from specifications that interact risk set indicators with treatment status.

[23] As we show in Appendix Figures A1 and A2, these patterns of overall and four-year enrollment over time are quite similar for both experimental cohorts.

4.2 Degree Attainment

In Table 3 we present estimates of BL's impact on degree attainment. The point estimates in column 2 show that, starting four years after high school, BL generates large increases in the share of students that earn a bachelor's degree. BL students are 6.2 percentage points more likely to earn a bachelor's degree within four years after high school; this represents a 23 percent increase relative to the control group mean of 26.8 percent. By five years after high school, this grows to a 7.6 percentage point effect relative to the control mean of 47.1 percent. By six years after high school (which we observe just for the first experimental cohort), the control mean bachelor's degree attainment rate rises to 52.8 percent. Nonetheless, BL students remain much more likely (9.6 percentage points) to earn a bachelor's degree.

The next four rows in Table 3 show that BL's impacts on degree attainment result mainly from increasing the share of students who graduate from high-quality institutions, those with higher graduation rates, lower default rates, higher average earnings among their graduates, and higher mobility rates. For instance, BL students were 7.6 percentage points more likely to graduate from an institution with above-median average earnings for graduates, a 15 percent increase relative to the control group.

The final two rows in Table 3 show that a moderate share of BL's impact on bachelor's degree attainment likely results from diverting students who would otherwise have attended two-year institutions and received associate's degrees to instead enroll at four-year institutions and complete their bachelor's degree. For instance, BL students were 3.0 percentage points less likely to earn an associate's degree within five years.

For context, these bachelor's attainment effects are similar in size to those estimated in a recent experimental evaluation of Buffett Scholars, a program that provides merit-based aid for low-income students. While the attainment effects are similar in size, the Buffett program cost per treated applicant is roughly *eight times* the amount spent per BL-offered student. The marginal degrees generated by BL advising appear to be larger per direct dollar spent than that of any large financial aid program that has been rigorously evaluated (Figure 1).

Indeed, the effects of BL on bachelor's degree attainment appear to be as large or larger than any intervention for which researchers have rigorously estimated treatment effects. This includes the estimated effects of early childhood education on bachelor's degree attainment, which range from null effects for Perry Preschool to null to somewhat similar effect sizes as what we estimate for Head Start (Anderson 2008; Thompson 2018; Bailey, Sun, Timpe 2020). But these programs are considerably more expensive, particularly when one considers that the investment is made 14 or more years in advance of college enrollment. Assignment to a smaller class in primary school also produces much smaller effects on bachelor's degree attainment (0.9 pp), and does so at significantly greater cost (roughly $14,000 in 2015 dollars) (Dynarski, Hyman, Schanzenbach 2013).[24]

In Figure 4, we explore the extent of heterogeneity in effects across common subgroups defined by race, gender, high school performance, access to alternative college access programs, and family resources. We see some suggestive evidence of smaller treatment effects for those with below-median high school GPAs in panel (a), but this difference appears to be driven by low levels of degree attainment in this group four years after high-school graduation. Looking five years after high-school graduation, BL's positive impacts on bachelor's degree attainment are quite consistent across student groups.

5 Mechanisms

5.1 Explaining Bachelor's Treatment Effects Using Causal Forests

We first outline the key channels through which the advising model may influence subsequent degree attainment. First, there may be relatively direct effects of advising during the senior year of high-school, such as increases in access to financial aid via FAFSA filing. We see little evidence on this particular margin. While we cannot rule out the role of other direct effects such as increased motivation to achieve a degree or improved preparation to navigate the post-secondary experience, there is little evidence from the content of advising meetings that would suggest that these channels would play an important role. Rather, advisers spend significant amounts of time working with

[24] Increases in school quality also appear to produce smaller increases in bachelor's degree attainment (Deming et al. 2014). Attending a first-choice school in Charlotte-Mecklenberg produced smaller effects on degree attainment (4.7 pp), although it is difficult to scale these effects by an associated cost. Lottery-based evaluations of charter middle schools suggest no increases in bachelor's degree attainment (https://files.eric.ed.gov/fulltext/ED594043.pdf). Even charter schools that produced very large increases in academic achievement and on-time benchmarks failed to produce persistent increases in college attainment (Dobbie and Fryer 2015).

students on the selection of and application to high-quality colleges that are well-matched to a student's aptitude and interests.

We have shown in the prior sections that participation in BL college advising generates large increases in the share of low-income students that attend higher-quality four-year institutions. It could be the case that shifting students to attend higher-quality institutions results in the higher rates of degree attainment that we observe. But it is also possible that the students induced to attend higher-quality colleges and universities were inframarginal to bachelor's degree attainment and that BL is affecting degree completion among a different population of students (e.g., through the ongoing Success advising provided to many students).

One traditional approach to distinguishing these explanations is to examine patterns of subgroup heterogeneity to see if the subgroups with larger treatment effects on initial enrollment quality are also the subgroups with larger treatment effects on bachelor's degree attainment. This type of analysis is usually conducted across commonly defined subgroups, e.g., race, gender, baseline level of achievement, etc. In our case, the lack of heterogeneity across these subgroups results in this approach being uninformative about the role of institution quality in mediating the bachelor's degree effects that we estimate. Instead, we use recently developed causal forest methods in a new way to investigate the extent to which we can attribute increases in bachelor's degree attainment to BL's impact on students attending higher-quality institutions.[25]

Causal forests are a machine learning-driven enhancement to traditional methods for investigating treatment heterogeneity (Athey and Imbens, 2016; Athey and Wager, 2019). Rather than rely on researchers to specify subgroups to include in heterogeneity analysis, causal forest methods use machine learning to investigate the complex relationships between baseline covariates and treatment effects. The intuition behind the method is to create "trees" that iteratively split the sample in ways that maximize treatment effect heterogeneity. For example, one tree might split on gender and then GPA below or above 3.0, and so on. Individuals in the resulting "leaves" of the tree share

[25]See Appendix B for additional discussion of the approach and details of our implementation. Application of causal forests in economic policy analysis has been very limited to date. The few applications of which we are aware use causal forests to better understand treatment effect heterogeneity (as opposed to disentangling the role of potential mediating factors). For example, Carlana et al. (2022) use the approach to provide a deeper understanding of how the effects of an offer to receive tutoring and career counseling on school track choice differ across different types of high-ability immigrants.

similar combinations of baseline covariates that are predictive of the treatment effect associated with their group. To prevent overfitting, the treatment effect associated with these "leaves" is estimated using a holdout sample that was not used in the production of the tree. Using a new random training (and hold out) sample each time, the algorithm produces a series of these trees that are referred to as a "forest". This "forest" can then be used to generate personalized treatment effects (PTEs) for each individual by averaging across the treatment estimate associated with each "leaf" that an individual falls into in each tree. These PTEs indicate the expected magnitude of impact the treatment has for an individual subject (on a particular outcome) based on their observable characteristics. Researchers have used causal forests to enhance treatment heterogeneity analysis, but applications of causal forests to educational interventions remain very limited, and we are unaware of any that use the method to explore mechanisms in the fashion we pursue in this paper.

To investigate whether BL's impacts on enrollment quality are resulting in higher rates of degree attainment, we estimate separate PTEs for two outcomes: (1) enrollment at a four-year institution with above-median graduation rates, and (2) bachelor's degree attainment within five years. In Figure 5, we display the relationship between the PTEs we estimate for attendance at higher-quality institutions (X axis) and the PTEs we estimate for bachelor's degree attainment (Y axis).[26] We observe a strong positive relationship between the PTEs: students for whom BL has the largest positive personalized treatment effects on enrollment quality tend to be the same students for whom BL has the largest PTEs on bachelor's degree attainment.[27] In other words, assignment to BL advising appears to increase degree attainment more for students who are more likely to enroll in a high quality college as a result of BL advising.

This analysis suggests shifting students to enroll at higher-quality institutions may be the primary driver of BL's impact on bachelor's degree attainment. Of course, this analysis does not address the possibility that the impact of in-college "Success" advising on bachelor's degree attainment may be larger among students more likely to be shifted into higher-quality institutions as

[26] In the figure, we collapse the PTEs for bachelor's degree attainment into means within bins of size 0.02. The size of each dot reflects the sample size represented.

[27] Appendix Figure A6 shows a similar but weaker relationship between the individual PTEs we estimate for attendance at a four-year institution (X axis) and the individual PTEs we estimate for bachelor's degree attainment (Y axis).

a result of the initial advising. We find little evidence to support this: There is minimal relationship between the likelihood that an individual is shifted into a higher-quality institutions and that an individual attends a target institution where Bottom Line students would be eligible to receive in-college advising (Appendix Figure A4).[28]

To further reinforce that in-college advising does not seem to be driving higher bachelor's degree attainment rates among students shifted to higher-quality institutions, we can look at individuals with above- versus below-median PTEs on enrollment at four-year institutions with above-median graduation rates. While individuals with above- versus below-median PTEs on our quality enrollment measure enroll in target institutions at similar rates (46 versus 43 percent), those with above-median PTEs experience large ($\approx$15 percentage point) impacts on bachelor's degree attainment, while there is no effect on attainment for students with below-median PTEs on enrollment quality.

As further evidence, we present in Appendix Table A3 a simple mediation-style analysis in which we include indicators for students' initial enrollment and enrollment quality in our basic specification to estimate treatment effects. The coefficient on treatment is substantially attenuated, to 1.6 percentage points (n.s.), which suggests that most of BL's impact on attainment stems from the effect Access program advisors have on students' college choices.[29] The modest positive coefficient on the treatment indicator may indicate that the ongoing advising provided by Success advisors does help some students persist to degree. Consistent with this, we do see some evidence that individuals who, based on their covariates, are likely to attend BL target institutions absent treatment have larger PTEs (and actual estimated treatment effects) in terms of 5-year BA attainment; this is true despite these individuals having smaller PTEs (and actual estimated treatment effects) on the four-year enrollment and school quality margins. In other words, among those ex ante likely to attend BL target schools, assignment to treatment is generating more marginal degrees per marginal enrollee. This suggests that the Success program may contribute somewhat to

[28]The coefficient from a corresponding regression is 0.299 (se 0.241), confirming the weak and statistically insignificant relationship between an individual's probability of being shifted into a higher-quality institution and their likelihood of attendance at a target institution.

[29]Interpreting this result seriously requires strong assumptions regarding the presence of confounders along multiple margins as well as the stability of the relationship between mediating factors and subsequent outcomes across treatment groups. With those caveats in mind, we still think the estimates are informative.

increased persistence, in addition to the important role of Access advisers in shaping school choice.

5.2 Exploring Access Advisor Effectiveness

How are advisors so effective in shaping school choice? In the final results sections we attempt to more rigorously measure what advisors do and what appears to be effective, leveraging survey data, rich student-advisor interaction data, and the quasi-random assignment of students to advisors.

BL maintains detailed data on advisor-student interactions. Advisors record a note detailing the date, mode of contact, and purpose of each interaction. They also enter a written summary of the substance of the meeting. Appendix Table A4 contains summary statistics generated from these data for the period between the beginning of the Access advising program (May of student's Junior year of high school) and the transition period to college (August after a student's Senior year of high school). As seen in the table, nearly every student assigned to treatment (97 percent) had at least one interaction with an advisor during this period. Over the 15-month period, advisors interacted with students an average of 13 times, with the majority of these interactions occurring as in-person meetings in the advisor's office.

Figure 2 illustrates the frequency of student-advisor interactions by months since the beginning of the advising program. As the BL model begins at the end of a student's junior year of high school, month 0 is set to equal May of 2014 for the high school class of 2015 and May of 2015 for the high school class of 2016. As illustrated in the figure, advisors begin interacting with students in the summer after their junior year and continue interacting with most students at high levels into the spring of the following year. Advisors spend an average of 10 to 15 hours working directly with each student

In addition to illustrating the high and persistent level of student-advisor interaction, the interaction data provide a way to quantify what advisors are spending time on during these meetings. The bottom third of Appendix Table A4 indicates that most meetings involve working on applications (3.47 meetings per student) or financial aid (2.03 meetings per student).

In general, advisors try to guide students to choose schools with relatively low costs and high graduation rates. The set of schools that possess these traits tends to coincide with the set of target colleges where BL has a continued advising presence.

Whereas the administrative data provide a good indication of how advisors spend their time helping students, they provide relatively little indication of the specific changes in students' actions, behaviors, and/or attitudes that led to the pronounced impacts we observe on college enrollment and persistence. To better understand the channels through which the BL advising may have affected students' college decisions and outcomes, we turn to survey data.

We conducted a survey of both treatment and control group students in the first cohort during their spring of the senior year of high school (2015). We asked about students' college and financial aid application decisions and behaviors; where they had been accepted as of the time of the survey; and the sources of advising and support students relied on when making college and financial aid decisions (for treatment group students, this included questions about their BL advisor). Approximately 56 percent of students responded to the survey, with roughly equal response rates among treatment and control group students.[30]

One interesting finding that emerges is that nearly all survey respondents in the control group — those who applied for BL but were not selected to participate — applied to college and for financial aid, even in the absence of BL advising (Table 4). This suggests that control group students were able to access college planning guidance and support from other sources, or had sufficient motivation and college aspirations to complete these tasks independently. Students in both experimental groups also applied to a large volume of colleges and universities — 10 on average for control group students and 13 on average for students in the treatment group.

In terms of students' responses about sources of college and financial aid advising, treatment students rate BL advising as the most important source of guidance; 58 percent of treatment students indicated that BL advising was "very important" in their application and decision process. In contrast, only 21 percent of control group students indicated that "staff at other college access programs" were very important. Both groups ranked support from parents ($\approx$ 60 percent), advisors ($\approx$ 50 percent), and teachers ($\approx$ 30 percent) as very important.

Interestingly, among students who ranked parents, advisors, or teachers as important, treat-

[30]The response rate for the control group was 0.558, with a 0.016 (se 0.029) coefficient on a treatment indicator variable, controlling for site by cohort indicators. Appendix Table A5 suggests little selection or differential selection into survey response. As seen in the table, observables of respondents are similar to those of the full sample. Similarly, the characteristics of treated respondents are broadly similar to control respondents.

ment students were less likely to say they discussed college-related issues (e.g., which colleges to apply to or how to apply for financial aid) with these other adults. This suggests treatment students received more guidance on these topics from BL advisors, and perhaps felt less need to turn to other (and potentially less-informed) sources of advising for this information.

5.3 Do Effects Vary across Advisors?

One question related to mechanisms and to the potential scalability of BL is whether the large observed treatment effects on enrollment, persistence, and degree attainment vary across advisors. [31] Figure 6 plots estimates of advisor fixed effects on college enrollment and four-year college enrollment. As seen in the Figure 6, 29 out of 30 advisors have a positive fixed effect on BL's focal outcome, four-year enrollment. This is further evidence of the potential scalability of the BL model.[32]

Of course, the preponderance of positive advisor fixed effects may reflect some sorting of students who need the most help to the most effective advisors. The inclusion of baseline covariates, suggests that this is not the case. After controlling for a rich set of student baseline covariates, 29 out of 30 have a positive effect on four-year enrollment.

Despite the suggestion of positive impacts across nearly all advisors, these figures do not necessarily indicate the causal effect of particular advisors on student outcomes. Advisors in high schools and other college success organizations often have some say in who they counsel. Advisor preferences (and thus characteristics) could therefore be correlated with the ability, family background, and motivation of their students. Similarly, many college success organizations intentionally match advisors to students based on similarity of backgrounds or interests, hoping that shared experiences will result in a better match and a higher likelihood of helping the student.

BL staff follow a different approach, essentially assigning students to advisors at random. BL staff describe the Access advisor assignment process as a "pretty blind assignment to fill each advisor's caseload (when they come in and who is available to meet with them)." This discussion

[31] If only advisors with certain characteristics/behaviors are effective and advisors with these characteristics are in short supply, it would be more difficult to scale the program.

[32] We note that it is, of course, possible that the quality of advisors falls with expansion, but argue that the consistency of effects across current advisors suggests the importance of the BL model to ensuring consistent delivery of services. Indeed, despite quickly scaling into the NYC area we observe similar effects there.

suggests that student assignment to advisor may be as good as random.

We explore this notion more formally in Appendix C, providing evidence consistent with the quasi-random assignment of students to advisors. Having established quasi-random assignment, we proceed with an investigation of the effects of advisor characteristics and behaviors on student success (details in Appendix C). We find no observable relationship between advisor gender or race and student success (Table 5).[33] The lack of heterogeneity across different types of advisors further suggests the potential scalability of the program.[34] The BL model appears effective across nearly all advisors, and there is little relationship between advisor characteristics and advisor effectiveness.

6 Discussion

Through a multi-cohort, multi-site RCT, we provide robust evidence that intensive advising generates large increases in degree attainment, with these impacts primarily driven by increased attainment at higher quality institutions. Given declining social mobility over time, intensive advising appears to be an effective policy strategy to promote greater economic opportunity for economically-disadvantaged, academically college-ready students. While the observed degree effects are quite consistent across different types of students, the fact that BL primarily serves students of color furthermore suggests that substantial expansion of the BL model could contribute to increased racial equity and mobility in the U.S.

We use new casual forest methods to demonstrate that BL's impact on attainment is generated primarily by shifting students to enroll at higher-quality institutions. Our approach could be applied in numerous other settings where reseachers are interested in better understanding the role of mediating factors in driving longer-term impacts. For example, researchers might use the approach in disentangling the role of cognitive versus non-cognitive skill improvements in influencing high-school graduation, college-going, or the commission of crime; the role of different health

[33]There is some suggestive evidence that assigning a student to an advisor who tends to have more application meetings with their assignees may increase the likelihood that that student goes to college, suggesting that BL's increased focus on this aspect of advising may be important. It is clear that advisors that have more application meetings are more effective, but the results merely suggest that the extent of interaction is causing the higher enrollment rates. It may be that advisors who have more application meetings have some other characteristics that makes them a better advisor.

[34]We have also investigated the presence of racial or gender interaction effects (for example, does a black student benefit more from a black advisor). We find no evidence of important interactions of this type.

care interventions in subsequent morbidity or mortality; or the role of neighborhood attributes in influencing a child's subsequent outcomes. By more exhaustively investigating the presence of and relationship between heterogeneity in intermediate and longer-term outcomes, causal forest methods allow for a deeper exploration of the role of mediating factors than traditional approaches that explore heterogeneity across researcher-specified subgroups.

The conclusions from our causal forest analyses are consistent with analyses of administrative and survey data that underscore the role of advisors in shaping student application and college choices. We cannot rule out the role of the ongoing in-college advising in promoting persistence and attainment, but the evidence suggests that it perhaps plays a modest role in generating additional degrees.

The consistency of our results across sites, cohorts, advisors, and students indicates that intensive advising may provide a scalable solution to reducing the income gap in college enrollment and success. We find that intensive advising generates large impacts across multiple program sites operating in different states under local program leadership. The New York site had been in operation for only a few years prior to the RCT. Large positive effects of the BL model there provide direct evidence of scalability and suggest that the program reaches maturity and efficacy more rapidly than many other programs.

We believe it is particularly noteworthy how consistent advisors are at improving student outcomes. From a scalability perspective this is highly important, since it suggests that a combination of coherent organizational leadership, successful staff recruitment and training, and effective curriculum are driving the results we observe, rather than a handful of particularly strong advisors who may be hard to identify and recruit in other contexts. Of course, it is possible that the quality of advisors could fall with program expansion in an area, but high rates of take up in the Boston area (BL reaches 60-70% of eligible students) suggest that advisor quality can be maintained.

Further supporting the notion that the model may be scalable, the colleges targeted by BL account for a substantial (40%) and responsive share of nearby four-year enrollment suggesting that intensive advising is not just crowding out other students.[35] An open question though is whether

[35]Between 2003 and 2014, enrollment at BL target colleges rose nearly 30 percent (7,690 students), while enrollment at Ivy league institutions in the same areas rose roughly 3.6 percent (142 students). Were BL primarily targeting students towards highly selective institutions with limited changes to total enrollment over time (e.g., the Ivies),

the effects are likely to be similar if access was expanded to additional individuals who are less likely to apply. While we cannot answer this question definitively, we think it is likely for two reasons. First, eligible non-applicants tend to be somewhat more disadvantaged, a group for which we find larger effects among applicants. Second, our estimated treatment effects are generated off of cohorts of applicants for which BL aggressively recruited students to meet sample size targets for the experiment; these sample sizes were substantially larger than the number of students served in prior cohorts. In some sense then, the estimated average treatment effects incorporate the treatment effects for individuals who would not have applied absent the experiment.

Taken collectively, these results suggest that intensive advising has the potential to maintain its positive impact with diverse populations in numerous settings. It is also impressive that BL has generated large and growing impacts on students' postsecondary outcomes given that two of the markets in which it operates (New York and Boston) are fairly saturated with other college advising organizations. The impacts of intensive advising could be even larger if applied in communities where students have little or no existing access to college advising supports.[36] Even assuming similar effects in other communities, broad adoption of the advising model would substantially reduce attainment gaps; the estimated impact of advising on bachelor's degree attainment is roughly as large as the (conditional on aptitude) gap in degree attainment between children from families in the first and fourth quartile of the income distribution.

As we describe earlier and show in Figure 1, the BL model is also more cost-effective than rigorously evaluated aid programs that provide students with additional financial assistance for college. Maintaining implementation quality at scale for intensive advising may be more challenging than for aid programs, but the consistency of impacts across sites, cohorts, student groups, and advisors suggests that providing cities with a combination of financial and technical assistance to

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the concern about crowd out would be more pronounced. BL target institutions also appear to increase capacity in response to demand; regressing log enrollment on log applications in first differences indicates that enrollment rises by 5.4 percent for every 10 percent increase in applications. In contrast, enrollment levels at elite and Ivy league institutions are unresponsive to the number of applications. The point estimate for BL target institutions is 0.54 (se, 0.043), while that for Ivy and elite institutions is -0.02 (se, 0.021). Based on authors' calculations using IPEDS data restricted to commuting zones containing the BL target colleges.

[36] Appendix Table A6 illustrates similar treatment effects on enrollment and bachelor's degree attainment by site. The Boston area includes the Boston and Worcester sites. The latter is much smaller (337 students) so for precision we combine with the Boston site. However, impacts on BA within five years are very similar for the Boston (7.2 pp) and the Worcester sites (7.4 pp).

replicate the model could be a more efficient approach to increasing bachelor's degree attainment than offering students additional aid. Indeed, the advising model appears to be as cost-effective at increasing bachelor's degree attainment as any rigorously evaluated policy of which we are aware.

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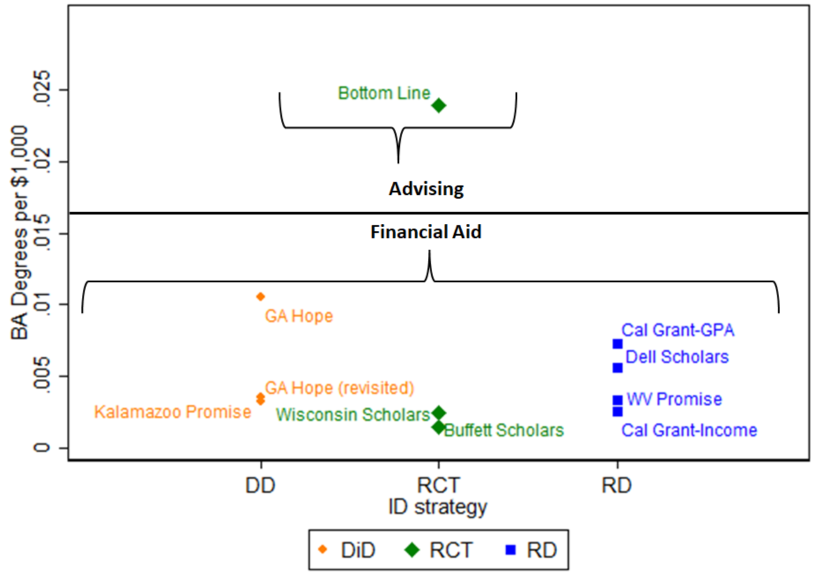
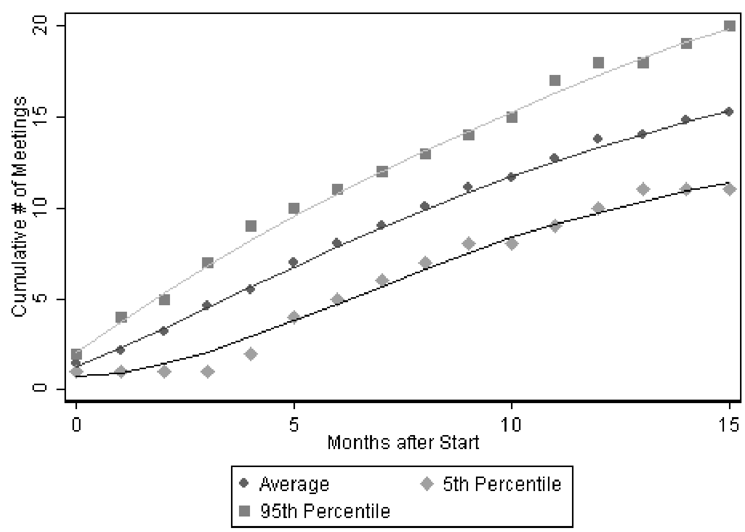
Figure 1: Bachelor's Degrees Generated per $1,000 of Direct Cost: Advising and Financial Aid



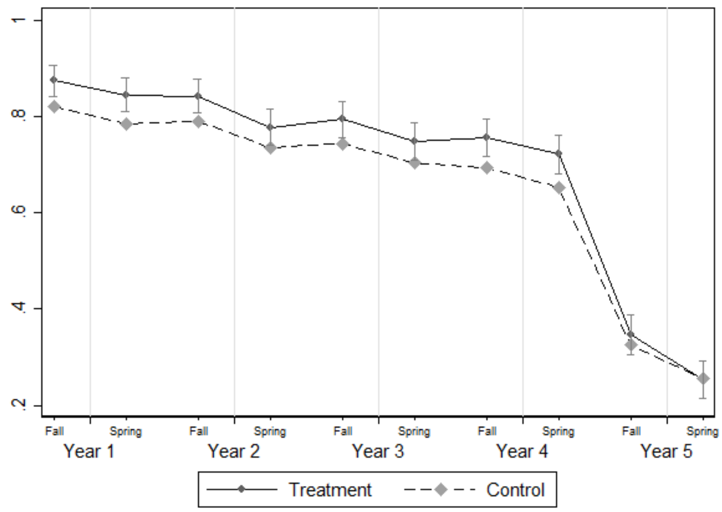
Note: Estimates produced by dividing reported estimate of the effect of a program on bachelors degree attainment by the corresponding cost (inflated to 2016 dollars).

Figure 2: Advisor Interaction Patterns over Time

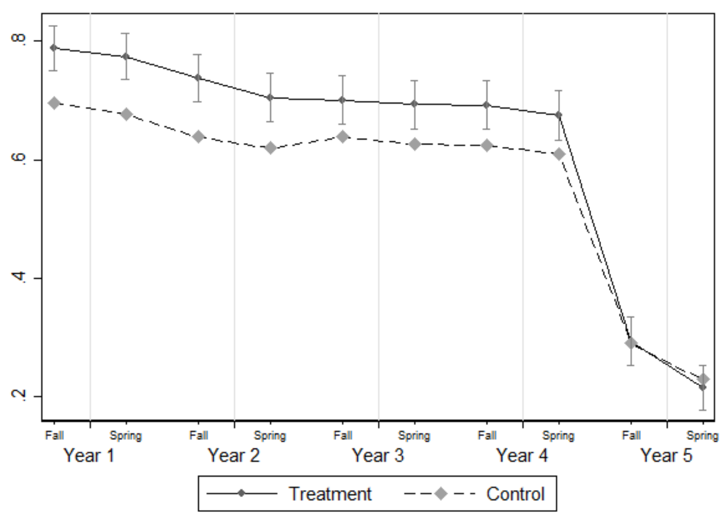


Note: Statistics derived from BL data. Month 0 is May of each high school class' junior year.

Figure 3: College Enrollment Over Time



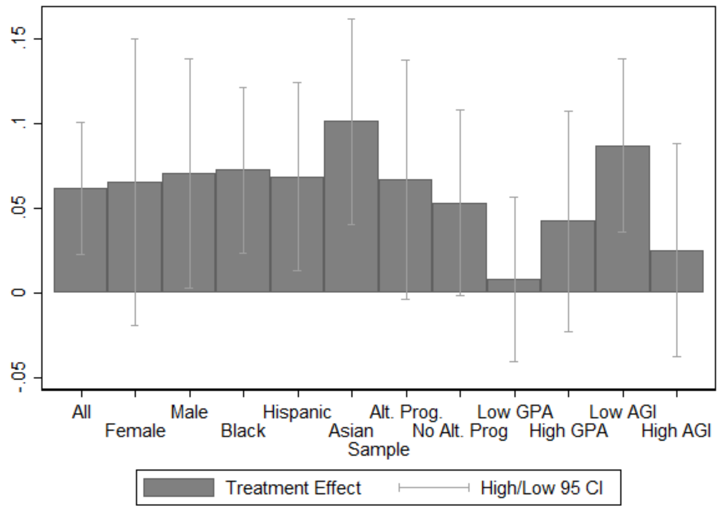
(a) Enrolled



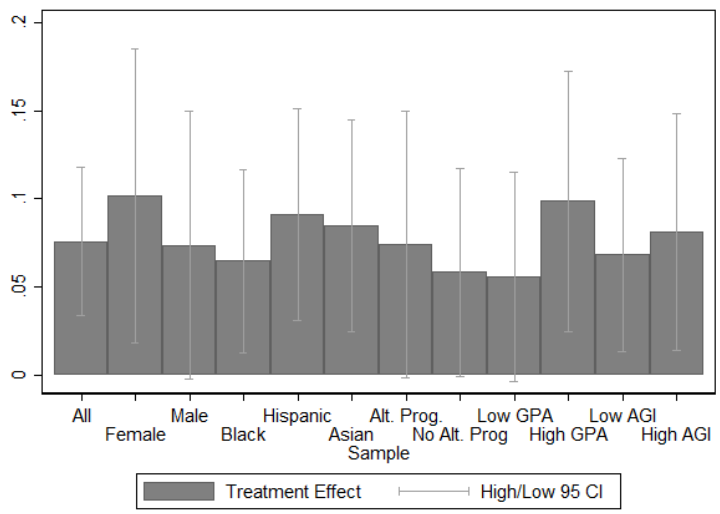
(b) Enrolled Four-Year

Note: Statistics derived from BL and NSC data. Year 1 is the academic year beginning in the fall after each high school class' senior year. Treatment line points adds estimated treatment effect from equation (1) to the control mean.

Figure 4: Bachelor's Degree Attainment: Heterogeneity



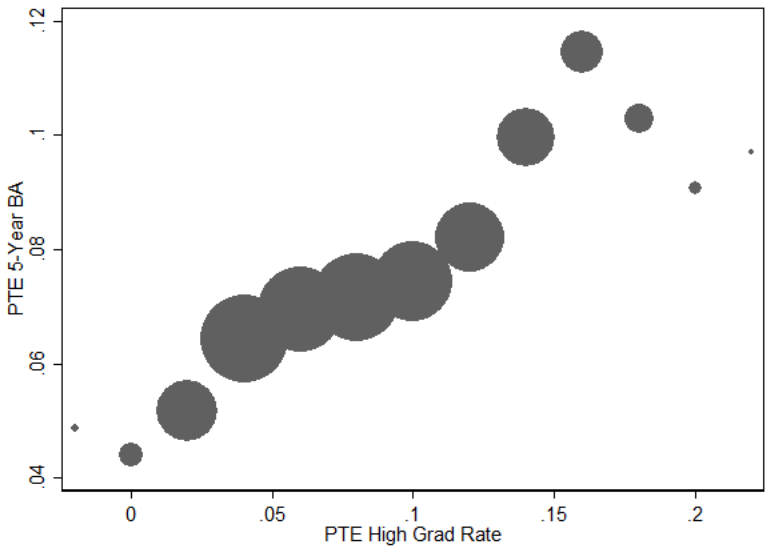
(a) 4 Year Attainment



(b) 5 Year Attainment

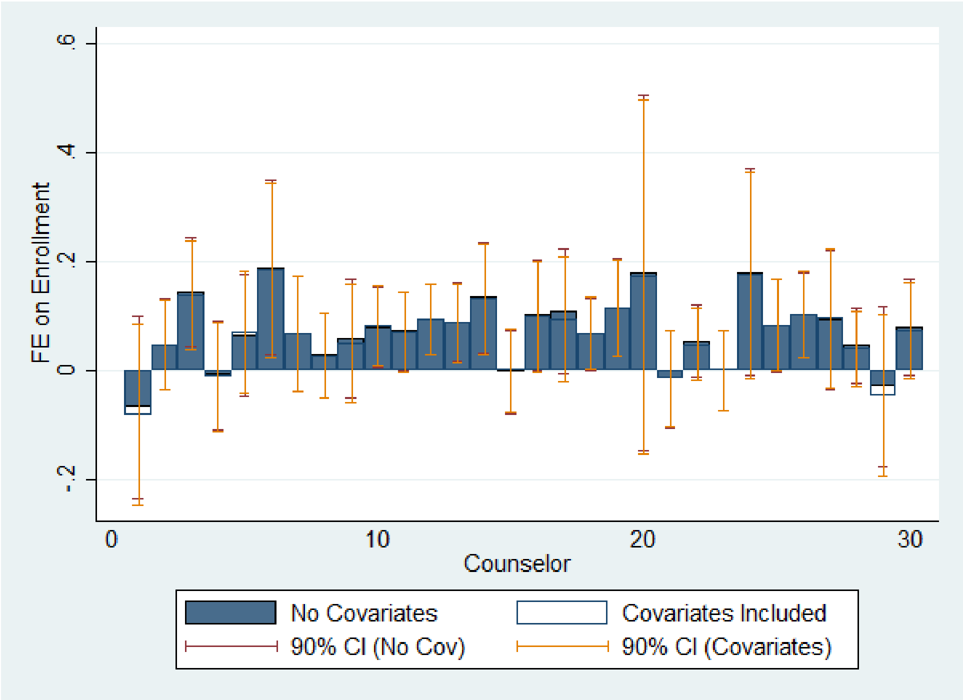
Note: Statistics derived from BL and NSC data. Treatment estimates provided by estimating equation (1) within each subgroup.

Figure 5: The Relationship Between Personalized Treatment Effects on High Quality Enrollment and 5-Year Bachelor's Degree Receipt

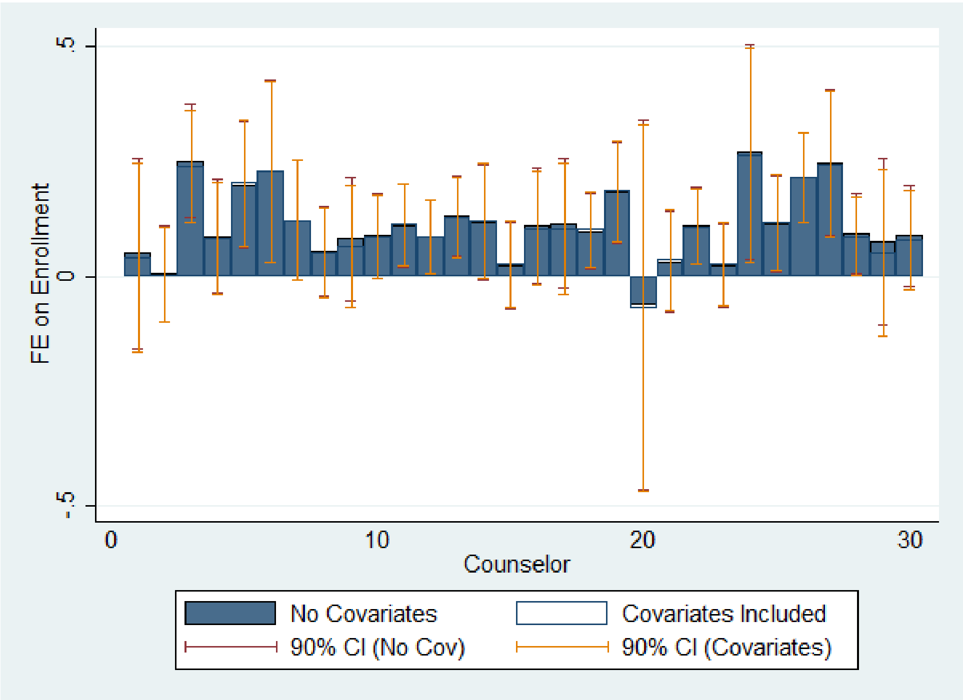


Note: Statistics derived from BL data. PTEs produced using covariates included in main specification. See text and Appendix B for additional details. We collapse the individual PTEs for bachelor's degree attainment into means within bins of size 0.02. The size of each dot reflects the sample size represented.

Figure 6: Advisor Fixed Effect Estimates



(A) College Enrollment



(B) Four-Year College Enrollment

Note: Estimates derived from basic specification but replacing treatment indicator with advisor fixed effects.

Table 1: Descriptive Statistics and Randomization Tests

	Control Mean (1)	Treatment (2)
Female	0.697	0.004
		(0.021)
Black	0.302	0.022
		(0.021)
Hispanic	0.325	-0.008
		(0.021)
Asian	0.246	-0.009
		(0.020)
Other Race	0.094	0.001
		(0.014
Citizen	0.787	-0.039**
		(0.019)
Verified GPA	3.264	-0.004
		(0.027)
Parent AGI	22520	393
		(840)
Household Size	4.26	-0.003
		(0.074)
Mom Employed	0.641	.005
		(0.023)
Mom Employed (missing)	0.144	-0.007
		(0.016)
Dad Employed	0.435	0.053**
		(.024)
Dad Employed (missing)	0.446	-0.004
		(.023)
First Generation	0.811	.000
		(.019)
Sibling College	0.389	-0.004
		(.023)
Sibling College (missing)	0.059	-0.011
		(.010)
Sibling Bottom Line	0.075	.001
		(.013)
Sibling Bottom Line (missing)	0.074	-0.001
		(0.012)
Other Program	0.444	-0.009
		(.022)
Observations		2422

Note: Column (1) contains control group means. Each cell in column (2) contains a coefficient from a separate regression of the observed characteristics on a treatment indicator variable, controlling for site by cohort (i.e., risk set) indicators. Robust standard errors in parentheses. * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 2: Effects on Enrollment in College

	Control Mean (1)	Treatment (2)
Any College	0.822	0.053***
		(0.017)
4-Year College	0.697	0.0914***
		(0.020)
2-Year College	0.128	-0.040***
		(0.014)
High Grad Rate	0.275	0.088***
		(0.020)
Low Default	0.291	0.072***
		(0.020)
High Earnings	0.631	0.110***
		(0.020)
High Mobility	0.608	0.103***
		(0.020)
BL Target College	0.327	0.101***
		(0.022)
Observations		2422

Note: Column (1) contains control group means. Each cell in column (2) contains a coefficient from a separate regression of each outcome variable (indicated by the row title) on a treatment indicator variable, controlling for site by cohort (i.e., risk set) indicators as well as the covariates indicated in Table 1. Robust standard errors in parentheses. * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3: Effects on Degree Attainment

	Control Mean (1)	Treatment (2)
BA Degree (4 Years)	0.268	0.062***
		(.020)
BA Degree (5 Years)	0.471	0.076***
		(0.022)
BA Degree (6 Years)[a]	0.528	0.096***
		(0.027)
High Grad Rate BA	0.252	0.082***
		(0.0189)
Low Default BA	0.250	0.072***
		(0.023)
High Earnings BA	0.502	0.076***
		(0.026)
High Mobility BA	0.480	0.083***
		(0.026)
AA Degree (4 Years)	0.106	-0.033**
		(.013)
AA Degree (5 Years)	0.128	-0.030**
		(0.014)
Observations		2422

Note: Column (1) contains control group means. Each cell in column (2) contains a coefficient from a separate regression of each outcome variable (indicated by the row title) on a treatment indicator variable, controlling for site by cohort (i.e., risk set) indicators as well as the covariates indicated in Table 1. a: BA Degree (6 Years)[a]) is only estimated for Cohort 1. Robust standard errors in parentheses. * (p<0.10) **(p<0.05), ***(p<0.01).

Table 4: Student Completion of College and Financial Aid Milestones (Cohort 1)

	Control Mean (1)	Treatment (2)
Proportion Applying	0.988	0.009
		(0.007)
Number of Applications	9.75	2.91***
		(0.336)
Costs Important	0.50	0.09*
		(0.05)
Filled Out FAFSA	0.97	0.017
		(0.05)
Met to Review Award Letter	0.66	0.18*
		(0.09)
College Access Advisor Important	0.21	0.37*
		(0.22)
Observations		813

Note: Statistics derived from survey of individuals in cohort 1. See text for additional details. Column (1) contains control group means. Each cell in column (2) contains a coefficient from a separate regression of each variable (indicated by the row title) on a treatment indicator variable, controlling for site by cohort (i.e., risk set) indicators as well as the covariates indicated in Table 1. Robust standard errors in parentheses. (p<0.10) **(p<0.05), ***(p<0.01).

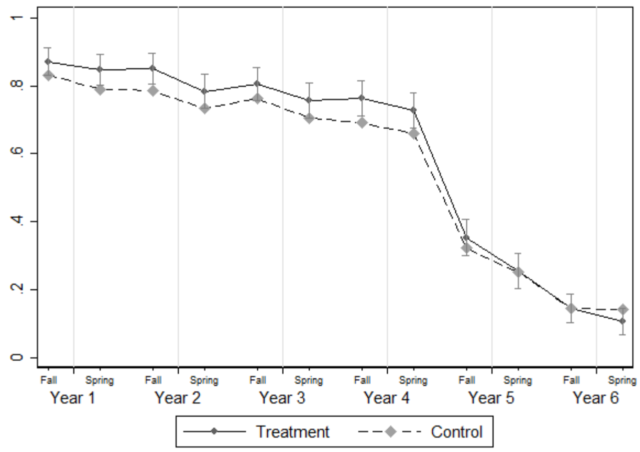
Table 5: Relationship Between Advisor Characteristics and Enrollment Outcomes

	Enrolled		Enrolled 4-Year		Enrolled High Grad		BA 5 Years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Counselor Characteristics								
Female	0.011	0.011	-0.006	-0.012	0.004	0.000	0.010	-0.001
	(0.027)	(0.027)	(0.035)	(0.034)	(0.042)	(0.041)	(0.043)	(0.042)
Black	-0.041	-0.042	-0.045	-0.048	-0.023	-0.013	-0.010	-0.007
	(0.029)	(0.029)	(0.037)	(0.037)	(0.045)	(0.044)	(0.046)	(0.045)
White	-0.033	-0.032	-0.034	-0.032	-0.018	-0.010	-0.012	-0.003
	(0.029)	(0.029)	(0.037)	(0.037)	(0.045)	(0.044)	(0.046)	(0.045)
Hispanic	-0.020	-0.019	-0.024	-0.023	-0.008	-0.010	0.010	0.018
	(0.028)	(0.028)	(0.036)	(0.035)	(0.043)	(0.042)	(0.044)	(0.043)
Application Meetings	0.064**	0.066**	0.055	0.054	-0.009	-0.027	0.005	-0.008
	(0.032)	(0.032)	(0.040)	(0.040)	(0.049)	(0.048)	(0.050)	(0.049)
Financial Aid Meetings	-0.033	-0.037	-0.043	-0.050	-0.037	-0.034	-0.016	-0.028
	(0.044)	(0.044)	(0.055)	(0.055)	(0.067)	(0.066)	(0.069)	(0.068)
Covariates		X		X		X		X

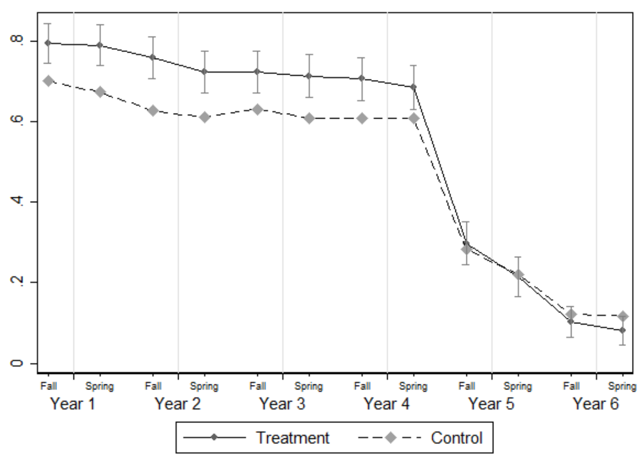
Note: Each column contains estimates from a separate regression of a dependent variable (in columns) on a set of advisor characteristics. Application meetings and financial aid meetings variables provide a measure of the average number of meetings of each type per student for each advisor. The variable is constructed using a leave one out procedure, so that each individual is assigned the average number of meetings occurring between every other student with the same advisor. Robust standard errors in parentheses. * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Appendix A: Supplemental Figures and Tables

Figure A1: College Enrollment Over Time: First Cohort



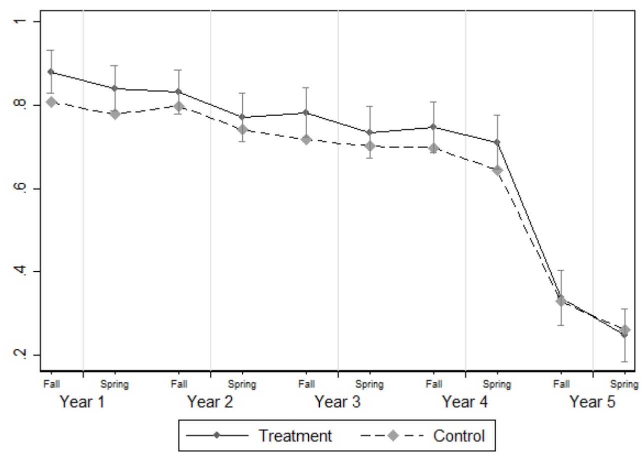
(a) Enrolled



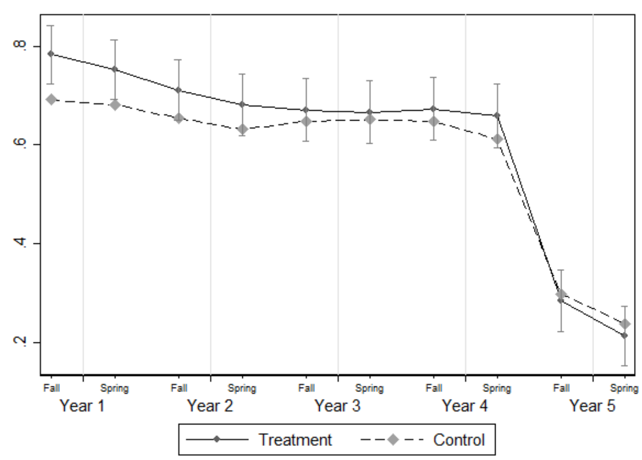
(b) Enrolled Four-Year

Note: Statistics derived from BL data. Year 1 is the academic year beginning in the fall after each high school class' senior year. Treatment line points adds estimated treatment effect from equation (1) to the control mean. Sample restricted to cohort 1.

Figure A2: College Enrollment Over Time: Second Cohort



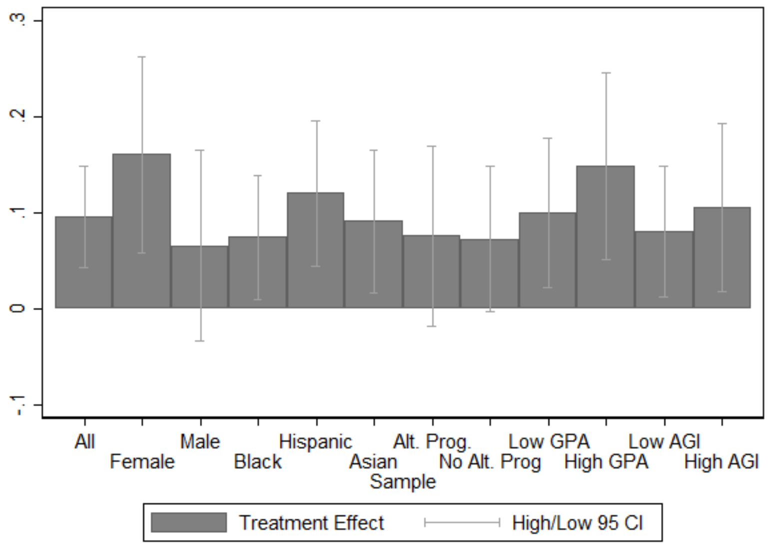
(a) Enrolled



(b) Enrolled Four-Year

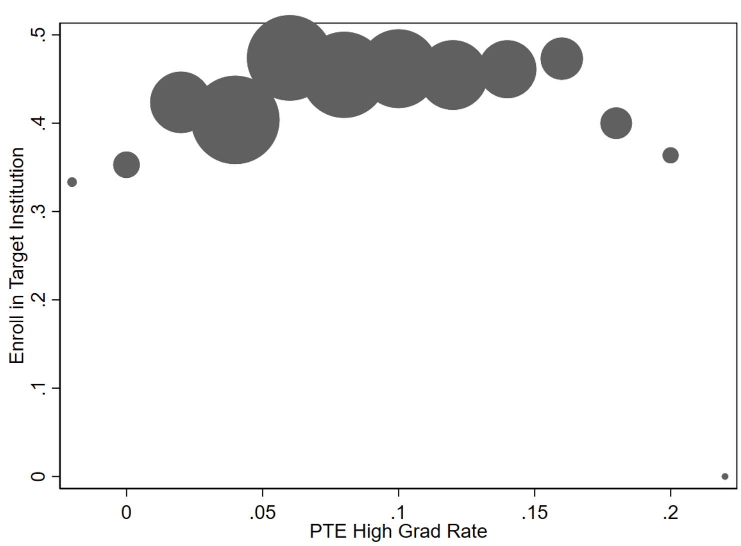
Note: Statistics derived from BL data. Year 1 is the academic year beginning in the fall after each high school class' senior year. Treatment line points adds estimated treatment effect from equation (1) to the control mean. Sample restricted to cohort 2.

Figure A3: Bachelor's Degree Attainment (6 Years): Heterogeneity, First Cohort



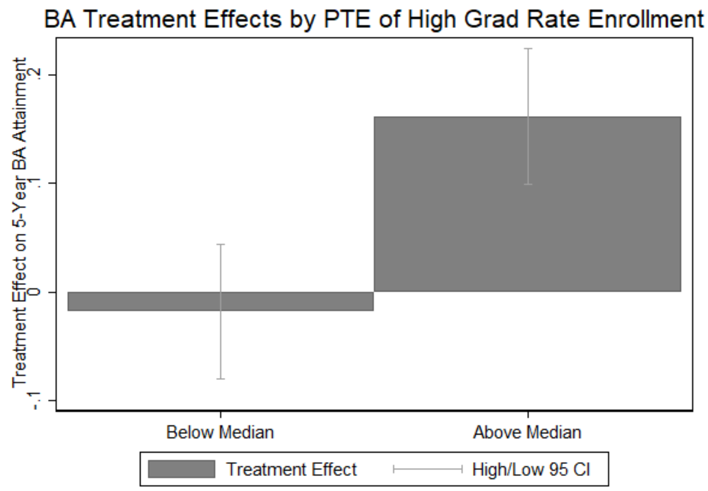
Note: Statistics derived from BL and NSC data. Treatment estimates provided by estimating equation (1) within each subgroup.

Figure A4: The Relationship Between Personalized Treatment Effects on High Quality Enrollment and Enrollment in a Target Institution



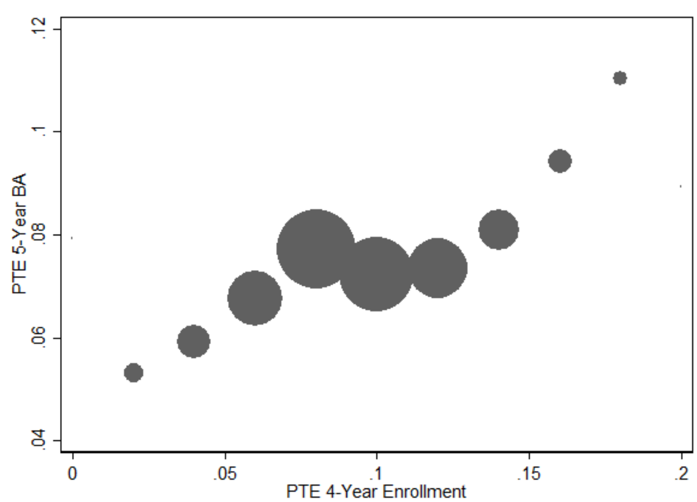
Note: Statistics derived from BL data. PTEs produced using covariates included in main specification. See text and Appendix B for additional details. We collapse the individual enrollment into means within bins of size 0.02. The size of each dot reflects the sample size represented.

Figure A5: Effects on 5-Year Bachelor's Degree Attainment: Heterogeneity by High Grad Rate PTE



Note: Statistics derived from BL data. PTEs produced using covariates included in main specification. See text for additional details.

Figure A6: The Relationship Between Personalized Treatment Effects on Four-Year Enrollment and 5-Year Bachelor's Degree Receipt



Note: Statistics derived from BL data. PTEs produced using covariates included in main specification. See text for additional details. We collapse the individual PTEs for bachelor's degree attainment into means within bins of size 0.02. The size of each dot reflects the sample size represented.

Table A1: Encouraged Colleges

College Names	Graduation Rate	Tuition and Fees	Net Price (0-48K)
Bentley University	84.1	41110	20544
Boston College	92.2	45622	16196
Boston University	83.9	44910	23573
Bridgewater State University	54.4	8053	14680
Buffalo State SUNY	48.1	7022	8021
CUNY Hunter College	45.7	6129	5258
CUNY John Jay College of Criminal Justice	43.1	6059	3993
CUNY Lehman College	34.9	6108	3297
CUNY New York City College of Technology	13.6	6069	5220
CUNY York College	25.6	6096	4590
Clark University	79.8	39550	18293
College of the Holy Cross	92.9	44272	15607
Fitchburg State University	50.8	8985	9013
Fordham University	81	43577	23352
Framingham State University	51.5	8080	12515
MCPHS University	66.4	28470	29807
Northeastern University	78.5	41686	20140
SUNY at Albany	64.4	8040	11019
Saint Joseph's College-New York	67.5	21878	10292
Salem State University	45.4	8130	11800
St Francis College	51.9	20700	9448
State University of New York at New Paltz	72.7	7083	9844
Suffolk University	55.9	31716	22900
The Sage Colleges	51.8	28000	14834
University of Massachusetts-Amherst	70.4	13258	12437
University of Massachusetts-Boston	37.9	11966	8084
University of Massachusetts-Dartmouth	49.9	11681	12581
University of Massachusetts-Lowell	53.8	12097	10258
Wentworth Institute of Technology	64	29200	25754
Worcester Polytechnic Institute	83.5	42778	27224
Worcester State University	51	8157	10907
Mean	59.6	20854	13919

Table A2: Treatment and Control Assignments

	Boston	New York	Worcester	Total
Control	193	450	92	735
Treatment	860	582	245	1,687

Table A3: Basic Mediation Analysis for 5-Year Bachelor's Degree Attainment

Explanatory Variable	(1)
2-Year College	0.105***
	(0.036)
4-Year College	0.427***
	(0.027)
High Grad Rate	0.279***
	(0.020)
Treatment	0.016
	(0.020)
Observations	2422

Note: The table contains the estimats from reestimating our basic specification while simultaneously conditioning on enrollment and enrollment quality. Each row contains the coefficient on an explanatory variable. As in our basic specification, we control for site by cohort (i.e., risk set) indicators. Robust standard errors in parentheses. * (p<0.10) **(p<0.05), ***(p<0.01).

Table A4: Advisor Interaction Patterns

	Mean
Ever Interact with Student (proportion):	0.97
Office Meeting	0.95
Phone Meeting	0.32
Interactions per Student (number):	13.06
By Medium:	
Office Meeting	8.81
Phone Meeting	0.42
Text or Email	0.28
By Subject:	
First Meeting	2.13
Second Meeting	1.37
Application Meeting	3.47
Financial Aid Meeting	2.03
Missed Meetings	0.59
Estimated Contact Time per Student (hours):	10-15

Note: Statistics calculated from BL data. Sample for rows (1)-(3) includes all students assigned to treatment and has a sample size of 1687. Remaining rows are restricted to the 97.2 percent of students assigned to treatment who had any post-assignment interaction with BL. Sample size for these rows is 1639.

Table A5: Survey Response Balance

	Full Sample	Respondents	
	Control Mean	Control Mean	Treatment
	(1)	(2)	(3)
Female	0.697	0.755	-0.051
			(0.035)
Black	0.302	0.335	-0.029
			(0.036)
Hispanic	0.325	0.335	-0.018
			(0.037)
Asian	0.246	0.265	0.033
			(0.036)
Other Race	0.094	0.086	0.009
			(0.023)
Citizen	0.787	0.767	-0.070**
			(0.034)
Verified GPA	3.264	3.320	0.018
			(0.043)
Parent AGI	22520	22295	-720
			(1456)
Household Size	4.26	4.32	-0.014
			(0.13)
Mom Employed	0.641	0.532	-0.017
			(0.040)
Mom Employed (missing)	0.144	0.171	-0.040
			(0.028)
Dad Employed	0.435	0.352	0.069*
			(0.039)
Dad Employed (missing)	0.446	0.494	-0.040
			(0.040)
First Generation	0.811	0.820	-0.048
			(0.032)
Sibling College	0.389	0.367	0.010
			(0.039)
Sibling College (missing)	0.059	0.053	-0.002
			(0.017)
Sibling Bottom Line	0.075	0.053	0.017
			(0.019)
Sibling Bottom Line (missing)	0.074	0.065	0.009
			(0.021)
Other Program	0.444	0.412	-0.047
			(0.038)
Observations	2422	813	

Note: Column (1) contains control group means for the full sample. Column (2) contains control group means for survey respondents. Each cell in column (3) contains a coefficient from a separate regression of the observed characteristics on a treatment indicator variable, controlling for site by cohort (i.e., risk set) indicators. Response rates did not differ significantly between control and treatment groups. Response rate for control group was 0.558, with a 0.016 (se 0.029) coefficient on a treatment indicator variable, controlling for site by cohort indicators. Robust standard errors in parentheses. * (p<0.10) **(p<0.05), ***(p<0.01).

Table A6: Effects on Degree Attainment by Site

	Boston Area		NYC	
	Control Mean (1)	Treatment (2)	Control Mean (3)	Treatment (4)
Enrolled	0.811	0.046*	0.829	0.059***
		(.025)		(0.022)
Enrolled 4-Year	0.653	0.090***	0.724	0.089***
		(.029)		(0.026)
Enrolled High Grad	0.298	0.047*	0.260	0.119***
		(.028)		(0.028)
BA Degree (5 Years)	0.418	0.074**	0.504	0.072**
		(0.030)		(0.031)
Observations		1390		1032

Note: Odd columns contains control group means. Each cell in an even column contains a coefficient from a separate regression of each outcome variable (indicated by the row title) on a treatment indicator variable, controlling for site by cohort (i.e., risk set) indicators as well as the covariates indicated in Table 1. Robust standard errors in parentheses. * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Appendix B: Causal Forest Approach

We apply recently developed causal forest methods in a new way to investigate the extent to which we can attribute increases in bachelor's degree attainment to BL's impact on students attending higher-quality institutions. The basic intuition of our approach is that if the effects on bachelor's degree attainment are operating (at least to some degree) through shifts in college choice, then we should see stronger treatment effects on bachelor's degree attainment among individuals who are more likely to attend higher-quality institutions as a result of intensive advising. Because we cannot recover individual treatment effects from the RCT, we use causal forest methods and our extensive set of baseline covariates to generate a personalized treatment effect (PTE) for each individual in the sample. We then explore the relationship between PTEs on the college choice and degree attainment margins to determine whether the effects on college choice likely mediate the degree attainment effects we observe.

Causal forests are a machine learning-driven enhancement to traditional methods for investigating treatment heterogeneity (Athey and Imbens, 2016; Athey and Wager, 2019). Rather than rely on researchers to specify subgroups to include in heterogeneity analysis, causal forest methods use machine learning to investigate the complex relationships between baseline covariates and treatment effects. The intuition behind the method is to create "trees" that iteratively split the sample in ways that maximize treatment effect heterogeneity. For example, one tree might split on gender and then GPA below or above 3.0, and so on. Individuals in the resulting "leaves" of the tree share similar combinations of baseline covariates that are predictive of the treatment effect associated with their group. To prevent overfitting, the treatment effect associated with these "leaves" is estimated using a holdout sample that was not used in the production of the tree.[37] Using a new random training (and hold out) sample each time, the algorithm produces a series of these trees that are referred to as a "forest". This "forest" can then be used to generate personalized treatment effects (PTEs) for each individual by averaging across the treatment estimate associated with each "leaf" that an individual falls into in each tree. These PTEs indicate the expected magnitude of

[37] Using this holdout sample rather than the sample used to fit the trees to estimate the treatment effects makes this approach "honest" (as in an "honest causal tree"). Further refinements that pre-split the data before estimating the honest causal forest produce similar results in our context. This approach maintains a second holdout sample used for neither training nor estimation of treatment effects within leaves.

impact the treatment has for an individual (on a particular outcome) based on their observable characteristics. Researchers have used causal forests to enhance treatment heterogeneity analysis, but applications of causal forests to educational interventions remain very limited, and we are unaware of any that use the method to explore mechanisms in the fashion we pursue in this paper.

An immediate question is why this method is any better than other options. Why would causal forests be better than pursuing traditional approaches of exploring heterogeneity within researcher-specified subgroups or by interacting baseline covariates with the treatment indicator? These approaches, while useful (and employed in the paper), have limitations. They tend to limit the scope of investigation by focusing on the construction of simple subgroups defined by one or perhaps two demographic characteristics. But perhaps it is the interaction of multiple attributes that most strongly predicts the magnitude of a treatment effect. For example, information on the availability of a benefit may only be useful for those who are (1) capable of making use of a benefit, and (2) unlikely to be aware of it in the absence of the information. In the context of college choice advising, it may be the case that the individuals with scope to make high-quality college choices are those able to gain admittance to better colleges (for example, those with higher GPAs or test scores). However, it may also be the case that many students with higher GPAs and test scores are already aware of these tradeoffs (for example, those with higher incomes or with parents or siblings with college experience). As a result, there may be little heterogeneity in effects across GPA/test score or family income/first generation measures, but significant heterogeneity when we examine those with higher GPAs *and* and lower family incomes.[38] Further, standard approaches to heterogeneity often have continuous measures that enter linearly (when interacted with a treatment indicator) or are used to split the sample into subsets (e.g., when a researcher explores effects among individuals with above- and below-median GPAs). But perhaps it is individuals in the middle of some distribution (e.g., GPA) that benefit from treatment, while the top and bottom do not. Or perhaps it is individuals in the middle of the GPA distribution *and* from families at a particular segment in the income distribution. The causal forest algorithm is not confined to a narrowly defined set of “expected” dimensions of heterogeneity.

[38] A researcher could use traditional analytic methods to interact GPA and family income with a treatment indicator to explore heterogeneity in this instance, but only if there was a strong a priori reason for doing so. Increasingly complex or unexpected interactions would naturally be met with suspicion and concerns about overfitting the data.

One concern then is that the causal forest overfits the data by allowing for the formation of unusual subgroups defined by the interaction of covariates. The approach overcomes this concern though by splitting the sample into one sample to construct subgroups (i.e., leaves) and another to estimate treatment effects. If the algorithm overfits in the process of choosing covariate splits and interactions to define subgroups, this overfitting will be reflected in the estimation of treatment effects out of sample within subgroups defined by the same characteristics.[39] This is more disciplined than the standard approach of using full sample data to estimate heterogeneity within an "expected" set of subgroups, where the researchers get to decide what goes in the "expected" set.

Returning to our context, we use this method to better understand the contribution of shifts in institution quality to increases in bachelor's degree attainment. As noted in the text, it could be the case that shifting students to attend higher-quality institutions results in the higher rates of degree attainment. But it is also possible that the students induced to attend higher-quality colleges and universities were inframarginal to bachelor's degree attainment and that BL is affecting degree completion among a different population of students (e.g., through the ongoing Success advising provided to many students).

To investigate whether BL's impacts on enrollment quality are resulting in higher rates of degree attainment, we estimate separate PTEs for two outcomes: (1) enrollment at a four-year institution with above-median graduation rates, and (2) bachelor's degree attainment within five years.

For each outcome we estimate 5,000 trees. To ensure that we are capturing real heterogeneity in treatment effects, we use "honest" estimation, beginning each of the 5,000 iterations by splitting the sample in half. The first subsample serves as a holdout sample in which we will estimate the treatment effect within each terminal leaf (i.e., set of covariate partition interactions) of the estimated tree. The second subsample serves as the training sample in which we estimate the tree structure. We use the full set of measures listed in Table 1 as potential covariates to form trees. Unless noted otherwise, we use the default parameters suggested by Athey and Wager (2019).

[39] In other words, the estimated heterogeneity in treatment effects across leaves out of sample will be smaller than that generated within the training data. Further refinements that pre-split the data before estimating the honest causal forest produce similar results in our context. This approach maintains a second holdout sample used for neither training nor estimation of treatment effects within leaves.

The algorithm chooses covariate splits so as to maximize the difference in treatment effects in the resulting leaves of the tree. Once the tree structure is established within the training sample, treatment effects are estimated with equivalently defined leaves in the holdout sample. For example, it might be determined within the training sample that the optimal tree structure to maximize heterogeneity in treatment effects results in splitting the sample into (1) individuals with GPAs above 3.0 and incomes above the poverty line, (2) individuals with GPAs above 3.0 and incomes below the poverty line, and (3) individuals with GPAs above below 3.0.

The estimation of treatment effects within these subgroups would then be estimated within the holdout sample. Each individual in the full sample would then be assigned the treatment effect corresponding to the leaf into which they fall based on their covariates. An individual's personalized treatment effect (PTE) is calculated as the simple average of treatment effects from each terminal leaf in which they fall across the 5,000 trees. We then use these PTEs to explore relationships between treatment effects on mediating and final outcome measures as discussed in the paper.

Appendix C: Quasi-random Advisor Assignment

We explore the notion of random assignment of students to advisors more formally by conducting a set of randomization tests. In Table C1, we explore the relationship between a number of advisor characteristics and baseline student characteristics. Formally, we estimate the following specification:

$$C_i = \alpha + \beta X_i + \sum_j \gamma_j l_{ij} + \varepsilon_i \tag{2}$$

where C_i are observable demographic characteristics of the advisors and measures of the extent to which a advisor meets with his or her assigned students, and X_i includes baseline demographic student characteristics. The l_{ij} are site by cohort fixed effects which control for site by cohort variation in the pool of students randomized across advisors.

The advisor interaction measures (in columns(5) through (8), indicate the average number of meetings of each type that a advisor holds over the course of the program. For example, the dependent variable in column (5) is the average number of meetings about applications that a advisor has had with each of his or her students. We follow a leave-one-out procedure to eliminate the possibility that a particular student could influence his or her advisor's score via their own behavior; thus, our variable of interest takes the form $X_{-i,s}$. The estimates in Table C1 suggest little relationship between advisor observables characteristics (or behavior) and baseline individual student characteristics, supporting the argument that advisors are as good as randomly assigned. F tests for the joint significant of all the pre-determined variables are generally insignificant, illustrating that particular types of students do not appear to be assigned to particular types of advisors.[40] Similarly, columns (6)-(9) indicate that particular types of students do not appear to be assigned to advisors who exhibit different advising tendencies. This suggests that students are as good as randomly assigned to advisors.

[40]The lone exception is for white advisors, a result that appears to be driven by white advisors adjusting verified GPAs rather than non-random assignment. If we exclude verified GPA from the regression, the remaining variables are not predictive of having a white advisor.

Table C1: Tests of Random Advisor Assignment

Couns. Chars.	(1) Female	(2) Black	(3) White	(4) Hispanic	(5) # of App.	(6) # of Fin. Aid	(7) # of Office	(8) # of Contacts
Baseline Covariates:								
Parent AGI	-0.000	0.000	-0.000	-0.000	-0.000	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Household Size	0.006	-0.002	0.012**	0.001	0.005	-0.001	0.006	-0.002
	(0.007)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.010)	(0.015)
Verified GPA	-0.008	0.014	-0.022	-0.014	-0.021	-0.002	-0.057*	-0.120***
	(0.020)	(0.018)	(0.018)	(0.020)	(0.014)	(0.013)	(0.030)	(0.045)
Female	0.013	-0.006	0.032	-0.030	0.030*	0.018	0.072**	0.087
	(0.024)	(0.021)	(0.021)	(0.023)	(0.016)	(0.016)	(0.036)	(0.053)
White or Asian	-0.006	-0.037	0.023	0.034	0.030	0.025	0.057	0.108
	(0.042)	(0.038)	(0.038)	(0.042)	(0.029)	(0.028)	(0.063)	(0.094)
Black	-0.062	0.027	0.015	-0.016	-0.038	0.013	-0.021	-0.014
	(0.041)	(0.036)	(0.036)	(0.040)	(0.028)	(0.026)	(0.061)	(0.090)
Hispanic	-0.053	0.002	0.007	-0.008	-0.018	-0.006	-0.051	-0.036
	(0.041)	(0.037)	(0.037)	(0.041)	(0.028)	(0.027)	(0.062)	(0.091)
Observations	1,596	1,596	1,596	1,596	1,596	1,596	1,596	1,596
R-squared	0.007	0.008	0.013	0.005	0.010	0.004	0.010	0.009
Prob>F	0.362	0.262	0.0208	0.702	0.0912	0.728	0.106	0.155
Mean	0.727	0.228	0.281	0.282	3.591	2.121	9.151	13.58

Note: Each column contains a regression of a different advisor characteristic on the full set of covariates, controlling for site by cohort indicators. The average # of meetings variables are constructed using a leave one out procedure, so that each individual is assigned the average number of meetings occurring between every other student with the same advisor. Robust standard errors in parentheses. * (p<0.10) **(p<0.05), ***(p<0.01).