College Guidance for All: A Randomized Experiment in Pre-College Advising

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Abstract

Pre-college advising programs exist in most disadvantaged high schools throughout the United States. These programs supplement traditional advising by high school guidance counselors and attempt to help underrepresented and disadvantaged students overcome the complexities of the postsecondary admission and financial aid processes. Existing evidence on these programs often uses within-school randomization where spillovers and alternative supports may confound estimates. We provide the first evidence on a whole school intervention resulting from a school-level randomized controlled trial in the United States. The college access program we study uses a near-peer model where a recent college graduate works at the school assisting students in the application and enrollment process. Pooled results across the first three years of program implementation find no significant impacts on overall college enrollment. However, subgroup analyses reveal positive, significant effects among the groups most targeted by the intervention: Hispanic and low-income students. Most of the impact comes through increasing two-year college enrollment, but this appears to be new entrants rather than inducing students to move from four-year to two-year colleges. The observed positive effects for these subgroups attenuate over time. We attribute this drop in the estimated impact to departures in fidelity of the experiment. Even among the cohorts for which we find positive enrollment impacts, we find no significant impacts on college persistence. © 2019 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Improving college access and completion is critical to reducing economic inequality within the United States and to increasing the United States' international competitiveness; yet planning for, applying to, attending, and succeeding in college are not easy for many families. Many well-qualified students are discouraged from pursuing higher education by avoidable barriers such as a lack of information about college admissions and financial aid (Avery & Kane, 2004). College advising is one of the key mechanisms by which policymakers, foundations, and high schools attempt to aid students as they navigate the college access "gauntlet" (Advisory Committee on Student Financial Assistance, 2005; Klasik, 2012), and across the country, there are thousands of college access programs that provide assistance to underserved students. These programs are so widespread that the umbrella National College Access Network claims the college access programs it represents provide supports to over two million students annually (National College Access Network, 2017).

The diversity of college access programs is staggering, even within the same school or community. These programs vary dramatically by their sponsoring organizations, funding sources, organizational structure, target populations, and interventions employed to improve college preparation and increase postsecondary enrollment (Gandara, 2001). A few of these programs operate nationally (e.g., Upward Bound, TRIO, and GEAR UP), but many are small and local, and therefore do not lend themselves well to rigorous evaluation and have limited external validity.

Better understanding the magnitude of the effects of college access programs is essential as current federal budget proposals suggest slashing the federal investment in TRIO and GEAR UP programs (a set of the most wide-scale access programs), which currently receive over a billion dollars annually from the federal government. Additionally, investments by school districts, states, and non-profit organizations in college access programs are substantial. The Gates, Kresge, and Lumina Foundations as well as many other national and local foundations have each devoted millions of private dollars to expand and improve these programs. Identifying causal effects of these programs at scale is essential for making informed policy decisions as policymakers seek to identify the most efficient allocation of funds to improve educational opportunity and success. Numerous states have launched endeavors to increase degree attainment (such as Tennessee's Drive to 55), and college access programs are viewed as an important component of those efforts.

On the whole, we know very little about the efficacy of these programs. Although some programs have conducted small-scale evaluations, few have done so using rigorous causal methods (Maynard et al., 2014). Establishing valid counterfactuals for students participating in college access programs is challenging due to the selection bias of schools and students choosing to work with the program, a challenge we overcome using an experimental design. In recent years, there have been a number of studies implementing randomized control trials to evaluate college access programs (Avery, 2013; Berman, Bos, & Ortiz, 2008; Carrell & Sacerdote, 2016; Oreopoulos, Brown, & Lavecchia, 2017; Phillips & Reber, 2018); however, they all examine within-school effects of programs targeting specific students within a population. While these studies are greatly informative about the efficacy of student-targeted interventions, the diversity of college access programs necessitates further consideration of the efficacy of programs targeting an entire school. Our study is the first experimental analysis of a whole-school college access program model in the United States. Examining a school-wide intervention dramatically reduces concerns of spillover effects and other within-school confounders that can limit within-school research designs. Additionally, our analysis is the first to test a scaled-up model of a whole-school intervention, and is therefore the largest study to date, incorporating nearly 40,000 students.

Our primary research question is whether providing information and support to high school students improves their likelihood of enrolling in postsecondary education. Given the ubiquity of college access programs with similar goals, the answer to this question sheds light on the value of the investment in college access programs generally. To estimate causal impacts, we exploit the random assignment used in the expansion of a large college access program called Advise Texas (Advise TX). In the 2010/2011 school year, Texas piloted the program in 15 high schools. In the following year, the state expanded the program to nearly 120 schools. This expansion offered us the opportunity to randomly assign the program to high schools across the state, thereby avoiding selection of schools. The program is a whole-school model in which one college adviser (a recent college graduate) is assigned to work full-time in the high school to assist with college preparation and enrollment. As we describe, our experiment has significant limitations, and there was a significant lack of compliance throughout the experiment.

Using administrative data from the Texas Higher Education Coordinating Board (THECB), our experimental analysis shows mixed results. Pooled results across the first three years of the program reveal no overall impact of having a college adviser randomly placed in a high school to assist with college enrollment. Despite the lack of compliance in the experiment, we are able to estimate fairly tight confidence intervals, which preclude large impacts of the program. However, we do find that enrollment outcomes improve for several groups of disadvantaged students that the program prioritizes. Low-income and Hispanic students experience increases of 2 to 3 percentage points on immediate college enrollment in the fall after high school graduation. These effects are concentrated among two-year college enrollments, but results do not suggest the program shifts students from four-year to two-year colleges as we observe no change in four-year enrollments. In terms of later college outcomes, we find no impact on college persistence with relatively tight confidence intervals. While the enrollment impacts for low-income students persist over two consecutive high school graduating cohorts, the impacts for other groups attenuate in the second and third years, eventually turning slightly negative but insignificant. There are several potential explanations for the attenuation that we assess. We remain most confident in our estimates of the program's impact in its first year of implementation, and these estimates suggest that the program has a small positive impact on two-year college enrollment for Hispanic and low-income students but no effect on bachelor's degree attainment rates.

We structure the paper as follows. The next section provides background information on college advising and specific programmatic details on Advise TX. The third section outlines the experimental design, data, and empirical strategies. Our fourth section provides the empirical results, and our fifth section discusses the results and provides estimates of cost effectiveness.

BACKGROUND AND INSTITUTIONAL DETAIL

College Advising for High School Students

Traditionally, high school students learn about college through their guidance counselor, and a long literature documents the extent to which guidance counselors are overwhelmed by the large numbers of students seeking assistance. In 2013, the nationwide average ratio of students to high school guidance counselors was 470:1, and the ratio in Texas was 462:1 (American Counseling Association, 2014). The lack of support has been particularly acute among low-income and minority students (Avery & Kane, 2004; Lee & Ekstrom, 1987).

There have been a number of studies focused on specific mechanisms that may affect students' likelihoods of attending college. Focused interventions, implemented through randomized control trials, demonstrate that college enrollment increases when students receive help with financial aid forms (Bettinger, et al., 2012), when students receive encouragement over the summer between high school and college (Castleman, Arnold, & Wartman, 2012; Castleman, Page, & Schooley, 2014), and when high-achieving students receive fee waivers and informational supports in choosing among colleges (Hoxby & Turner, 2015).

Our focus is on more holistic programs in which an adviser provides a host of services and college planning. These have proven more difficult to evaluate for several reasons. First, college access programs are diverse in nature and size, and they contain varied levels of student supports, counseling, and academic help. Few programs are adopted at a sufficient scale to facilitate a large-scale evaluation with random assignment. In their systematic review of the efficacy of college advising

programs, Maynard and colleagues (2014) report results for many studies with only a few hundred students or less.

Another problem in the evaluation of college access programs is selection bias. Even when programs exist on a large enough scale to facilitate evaluation, these programs purposefully target schools with large proportions of disadvantaged students, and more motivated students likely select into receiving high doses of the treatment. This makes rigorous evaluation that eliminates selection critical for measuring program effects. Even studies employing quasi-experimental methods to reduce selection effects offer questionable treatment estimates. Of the 18 evaluations of broadly defined college access programs that use experimental or quasi-experimental designs, 11 rely on some form of matching design to estimate the effects of the program (Maynard et al., 2014). In nearly all cases, the randomized control trials provide smaller impact estimates than the quasi-experimental studies, suggesting that matching techniques do not fully account for bias and that randomized control trial designs are necessary to accurately measure effects.

To date, the most rigorously evaluated programs show positive, although somewhat mixed results. A majority of studies find that advising programs often impact the choice of school of attendance rather than inducing students at the margin of college attendance to enroll. For example, one of the largest evaluations to date focused on Upward Bound. Upward Bound is a cohort-based model where a small group of disadvantaged students are targeted early in their academic careers (typically around eighth grade) and followed throughout high school. Upward Bound had no impact on college enrollment although it moved a small percent of students from two- to four-year schools (Myers et al., 2004). Similarly, other college programs, such as College Possible, Bottom Line, and SOURCE, steer students toward four-year colleges or colleges with lower dropout rates (Avery, 2013; Berman, Bos, & Ortiz, 2008; Castleman & Goodman, 2018).

In Canada, Oreopoulos, Brown, and Lavecchia (2017) and Oreopoulos and Ford (2016) show that mentoring programs and integrating college-going information into the curriculum can improve college attendance among Canadian youth. Phillips and Reber (2018) find that virtual advising (including text-based reminders of key tasks and deadlines) can improve intermediary outcomes, such as taking the SAT and submitting financial paperwork on time, but does not increase college enrollment (although it may improve enrollment outcomes for Hispanic students who speak Spanish at home). Cunha, Miller, and Weisburst (2018) find that Texas GO Centers, which provide information and peer guidance to high school students, increase the rate of applying to and being accepted to college, but produce no overall effect on college enrollment, persistence, or completion. They do find, however, a small positive effect on four-year college enrollment among Hispanic students.

Perhaps the most optimistic study has been the recent evaluation of the Dartmouth Mentoring Program (Carrell & Sacerdote, 2016). In this program, Dartmouth undergraduates helped students apply for college and provided $100 in incentive money to students who completed college applications. The program led to improved college attendance for women, and importantly, they found that the effects were largest for students who lacked parental support.

While many of these programs focus on providing similar services (e.g., assistance with completing college and financial aid applications), there are some key differences in our setting. Programmatically, most evaluated college access programs are cohort-based models where students are specifically targeted because of a set of characteristics. Students were often targeted early in their academic careers or were assigned based on some criteria related to students' backgrounds and potential to attend college. In our context, advisers were charged with working across the entire set of students at the school, typically focusing most of their efforts on seniors and prioritizing those students who might have the least support outside of school.

Perhaps the most important difference relates to scale. With the exception of Oreopouolos and Ford (2016), who randomly assigned schools an intervention integrating three college information workshops into the Canadian high school curriculum, all of these prior interventions were done as short-term, within-school evaluations. Researchers typically used randomization to choose which students received services. As a result, the control group students attended the same schools and readily interacted with treated students. The potential spillovers that may exist in such a setting could likely downward bias any results, which may explain why most programs have failed to demonstrate impacts on the extensive margin of college attendance. Our analysis identifies treatment effects for a school-wide model that incorporates any within-school spillovers as part of the overall treatment effect. The use of a school-wide model is also advantageous in that it allows advisers and teachers to provide assistance to students at the classroom level without having to consider students' treatment statuses. Moreover, our program focuses on a scaled-up model of an intervention. Many of the other programs evaluated to date involve aspects of the treatment that may be implausible in a scaled-up, whole-school setting. For example, the cash payments to students in the Dartmouth program may be politically or financially implausible in most settings, or the level of intensity in College Possible (on average 42 adviser meetings per student) may be unsustainable when scaled-up over a typical school, district, or state.

Additionally, most evaluations of college access programs follow a college access program over a short time horizon. One of the criticisms of randomized experiments is that the comparisons fail to account for long-run adaptations that might be made in the presence of a newly established intervention (Shanzenbach, 2012). These adaptations would become more present as a program increases its scale and could undermine the short-term results. Our study contributes to better understanding longer-term program impacts as we provide some evidence that these adaptations may have impacted efficacy two to three years after the initial implementation.

Our study complements the existing literature by providing an evaluation of a large-scale implementation of a college access program across 111 schools including over 38,000 students. The program is a whole-school model and represents a more scaled-up version of a college access program than provided in extant literature. Given the randomization of schools in Texas, our study could potentially provide the best evidence to date on the effectiveness of similar programs, as well as provide valuable insight on challenges and best practices associated with college access programs in other states.

Advise TX

The primary goal of Advise TX is to raise the rates of college enrollment and completion among low-income, first-generation college, and underrepresented high school students in Texas. The program employs the model of the College Advising Corps (CAC) which operates in 14 other states and is headquartered in Chapel Hill, North Carolina. The program model integrates supplementary supports in the form of a college adviser assigned to a specific high school. The advisers address primarily informational barriers to college enrollment, although they also assist with academic and financial barriers.[1]

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[1] Nationally, CAC works with sophomores and juniors to increase their likelihood of taking AP exams and other college preparation courses. In Texas, however, the size of the graduating cohort was sufficiently large that it impeded significant interaction with underclassmen. The Texas advisers described their efforts as "triage" for high school seniors.

Advise TX partners with colleges and universities in the state to recruit and train recent college graduates from these partner institutions to serve as full-time college advisers in disadvantaged high schools. Advisers participate in a six-week, residential summer training program prior to their placement in a high school. The advisers serve as near-peer mentors and often have characteristics closely aligned with the population of students they serve at the high schools. For example, most advisers are themselves first-generation college graduates. Advisers agree to serve for one year with the option to renew for a second year. While in the schools, advisers work in close collaboration with guidance counselors, teachers, and administrators within their school to foster a school-wide "college-going" culture.

Although advisers serve all students at the school, their work primarily focuses on low-income and first-generation college students who, due to a lack of information and misperceptions about costs and aid, are historically underrepresented in postsecondary education. Advisers offer direct support to students in the form of individual advising sessions, group sessions with students, and group sessions with students and parents.

Advisers also work most closely with seniors. Across the 2012/2013 academic year, the Advise TX program, including Advise TX schools that were not part of the experiment sample, served schools with 52,425 seniors and had direct contact with 84 percent of them at least once. The advisers logged over 180,000 individual student meetings and over 110,000 group meetings with seniors across the state. Conditional on having contact with the advisers, seniors received an average of 3.5 individual meetings and 2.1 group meetings.

Typically, advisers assist seniors with the college search process, college application process, and financial aid process. This work can include encouraging students to consider a wide range of postsecondary options accounting for the fit of student to college, taking students on college visits, establishing time lines, applying for fee waivers, interpreting communications from colleges such as offers of admission and financial aid, and a host of other general supports as students navigate the college admission and enrollment process. For example, advisers assisted 52 percent of seniors in Advise TX schools with completing college applications and arranged college visits with 12 percent of the seniors. As a marker of the low-income population with whom they work, advisers helped 20 percent and 33 percent of seniors obtain fee waivers for the ACT and SAT, respectively, and they helped over 33,000 students receive college application fee waivers.

Advisers also work with underclassmen to encourage students to consider and plan for higher education and focus on specific preparatory activities such as studying for and taking the SAT or ACT. In the 2012/2013 school year, Advise TX advisers met with over 25,000 juniors, 6,000 sophomores, and 4,000 freshmen, representing a substantial portion, but not the majority, of their efforts. Advisers typically prioritize meeting with students who are underrepresented in higher education, including underrepresented minorities, low-income students, and first-generation students.[2] In Appendix Table A1, we show the average number of interactions that students in these groups had with advisers in 2016/2017, the year in which we have the most complete data.[3] In each case, the prioritized group had more interactions with the adviser.

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[2] In our adviser tracking data, we can track outcomes for all three groups. In the Texas administrative data, we can track outcomes for underrepresented minorities and low-income students but not first generation students.

[3] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 1. Descriptive statistics and balance check of student characteristics in pretreatment (2009/2010).

Variable	All TX high schools		All experimental high schools		All treatment high schools		Raw difference T-C		T-C difference with lottery controls	
	Mean	Stdev.	Mean	Stdev.	Mean	Stdev.	Difference	Std. Error	Difference	Std. Error
White	0.391	0.488	0.227	0.419	0.203	0.403	−0.038	0.037	−0.036	0.024
Black	0.134	0.340	0.181	0.385	0.225	0.418	0.071[+]	0.036	0.088**	0.022
Hispanic	0.420	0.493	0.545	0.498	0.521	0.500	−0.040	0.057	−0.060*	0.025
Other race	0.055	0.229	0.047	0.211	0.051	0.220	0.007	0.011	0.008	0.007
URM	0.553	0.497	0.726	0.446	0.745	0.436	0.031	0.042	0.028	0.024
Female	0.497	0.500	0.502	0.500	0.504	0.500	0.003	0.008	0.002	0.007
FRL	0.363	0.480	0.474	0.499	0.478	0.500	0.007	0.042	−0.014	0.021
Age	17.181	0.611	17.200	0.614	17.170	0.596	−0.049**	0.017	−0.041**	0.014
Fall College Enrollment	0.519	0.500	0.497	0.500	0.512	0.500	0.024	0.018	0.015	0.011
N	274,623		38,201		14,270		38,201		38,201	

Notes: [+]p < 0.10; *p < 0.05; **p < 0.01. This table uses student level data from the 2009/2010 school year. The regressions estimating the difference between treatment and control use clustered standard errors at the school level (111 schools). Treatment assignment in the first year of the treatment is used as measure of treatment.

EXPERIMENTAL DESIGN & DATA

When the Advise TX program planned its expansion after its initial pilot year, we collaborated with the Texas Higher Education Coordinating Board to identify and randomly select high schools to receive the program. The THECB identified a sampling frame of 418 high schools in the state with at least 35 percent free/reduced price lunch participation, less than 70 percent of graduating students attending college within a year, and less than 55 percent of students experiencing a "distinguished" college-prep curriculum. These schools were invited to apply, and 237 did so. These 237 schools were ranked on the above three criteria as well as percent of underrepresented minority and a qualitative "fit" component that was assigned a one to four value by Advise TX staff based on the school's organizational capacity. All schools that applied were given an aggregate score based on these criteria, and the top 84 schools were automatically selected for the program. The bottom 42 schools were eliminated from consideration. The remaining 111 schools were considered eligible for random assignment to the program and constitute our experimental sample.

To ensure geographic diversity, we blocked on region of the state. These 111 schools were divided into 32 geographic regions, and a lottery was held within each region to select treatment schools. Thirty-six schools were randomly chosen for treatment assignment out of the set of 111 across the regions.

Table 1 provides descriptive statistics at the student level for demographic variables and for the fall college enrollment outcome measured in the 2009/2010 pretreatment year, the year of data used to rank the order of schools as described above. The first column of numbers contains means for all Texas high schools followed by schools in the experimental sample and then treatment schools. Given the selection criteria and goals of the Advise TX program, schools in the experiment have a higher share of minority and low-income students than all Texas high schools, but college enrollment rates are quite similar.

Table 1 also investigates balance in pretreatment covariates and the pretreatment college enrollment outcome across treatment and control schools. We run block control regressions of each variable on treatment assignment and report the treatment coefficient and standard error in the last two rows of Table 1. There do appear to be differences in the racial makeup of the schools assigned to treatment, with treatment schools more likely to have higher percentages of black students and lower percentages of Hispanic students than control schools.[4] During the randomization, Advise TX used only the aggregate percentage of underrepresented minorities ("URM" in Table 1), and the treatment and control samples are balanced on this variable. Age also appears slightly imbalanced within blocks, as students in treated schools are younger by 0.04 years, corresponding to about 15 days younger. A chi-squared test of the joint significance of the covariates accounting for blocking and clustering fails to reject the null hypothesis of balance (Hansen & Bowers, 2008). Although the randomization within regional blocks yielded some minor differences, we control for these variables in our analyses below and believe the random assignment process produced reasonably equivalent treatment and control groups.

We observe student-level outcome data provided by THECB for the first three years of the treatment (2011/2012, 2012/2013, and 2013/2014 school years), hence, we estimate results at the student level despite the school-level nature of the intervention, thereby creating a clustered randomized control trial. Due to randomized treatment assignment, we can employ the regression model below to identify the causal effects of having the Advise TX program assigned to a high school on individual college enrollment outcomes.

$$y_{isj} = \alpha_j + \boldsymbol{X_{isj}\beta} + \delta * Treatment_{isj} + \varepsilon_{isj} \tag{1}$$

Student i at school s in region j receives a value of one for the binary treatment variable if the student was enrolled in a high school assigned to treatment. Because we blocked on region, we include region fixed effects, α_j. We also include available student-level demographic information such as gender, race, and low-income status as covariates to increase precision in vector $\boldsymbol{X_{isj}}$. We estimate our binary outcomes using linear probability models for ease of interpretation. It is debatable whether clustering standard errors by school or by school-by-year level is preferable. We choose a more conservative approach of using standard errors which cluster at the school level since consecutive cohorts of graduating students may be related within schools. The Texas administrative data from the THECB track all students who graduate from Texas public high schools into all public institutions of higher education within Texas. We augment the THECB data with National Student Clearinghouse data enabling us to track enrollments into out-of-state and private postsecondary institutions.

The above analytical approach provides intent-to-treat (ITT) estimates; however, compliance with treatment assignment is approximately 75 percent (Table 2). Five schools of the 36 assigned to treatment subsequently declined to accept an adviser. Advise TX requires data sharing, dedicated space, and administrative oversight. Many schools who initially applied were unable or unwilling to comply with these requirements. Additionally, nine control schools received an adviser in part to make up for the five treatment schools that declined to participate and in part due to

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[4] We had multiple blocks where all of the schools were highly polarized. For example, Block 16 had three schools that were either highly Hispanic or highly African-American. Blocks were designed by geography and were balanced by the overall share of URM students. In Block 16, we would have had imbalance in the proportion of black or Hispanic students no matter which school had been selected in the lottery. If we exclude block 16, we reduce the imbalance.

Table 2. Treatment compliance in year one (2011/2012).

Panel A: School level			
	Treatment received	Control received	Total
Treatment Assigned	31	5	36
Control Assigned	9	66	75
Total	40	72	111
Lottery controlled regression of treatment received on treatment assignment		0.745 (0.072)	
Panel B: Student level			
	Treatment received	Control received	Total
Treatment Assigned	12,529	1,324	13,853
Control Assigned	3,267	21,004	24,271
Total	15,796	22,328	38,124
Lottery controlled regression of treatment received on treatment assignment		0.774 (0.070)	

Notes: For year 2011/2012, first year of treatment. Standard error is clustered at the school level in the student level regression.

philanthropic financial gifts provided by funders that conditioned their gift on participation of specific control schools. Although we had randomly constructed a waitlist with the schools assigned control status, program staff violated the waitlist in three instances thereby undermining the randomization of the waitlist.[5] We focus on intent-to-treat estimates throughout our results tables, although simple Wald estimators can be used to estimate the treatment on the treated (TOT) effect inflating the intent-to-treat effects by approximately 33 percent. In Table A2, we report the instrumental variable estimates that provide treatment on the treated estimates across subgroups and cohorts for three college enrollment outcomes.[6]

RESULTS

First Year Impacts

We report intent-to-treat results for the first year of Advise TX on any college enrollment in the fall after high school graduation in Table 3. The first two columns present the treatment effect on the full sample with and without covariate controls; given the slight imbalance in the racial composition of the schools discussed above, we preference the estimates with covariates. We observe a statistically insignificant 1.1 percentage point increase in college enrollment at treatment schools. We might not be surprised that the point estimate is small given the intervention adds only

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[5] For example, after seeing Advise TX operate in some Fort Worth schools, a local donor offered complete funding for the program as long as Advise TX would expand into all schools, including control schools, in the local area.

[6] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 3. Intent-to-treat first-year college fall enrollment results.

	Full sample		Black		Hispanic		Low-Income	
Treatment	0.022[*]	0.011	0.019	0.012	0.022[+]	0.020[+]	0.038[**]	0.019
	(0.011)	(0.010)	(0.017)	(0.016)	(0.012)	(0.011)	(0.014)	(0.012)
Covariates		Yes		Yes		Yes		Yes
Block Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.558		0.599		0.515		0.498	
R^2	0.022	0.090	0.009	0.064	0.038	0.085	0.025	0.083
N	38,124	38,124	6,659	6,659	21,852	21,852	19,677	19,677

Notes: [+]$p < 0.10$; [*]$p < 0.05$; [**]$p < 0.01$. Each cell reports the coefficient on treatment assignment in 2011/2012 for each sample using a linear probability model. Standard errors are reported in parentheses and are clustered at the school level. Covariates include gender, race, age, whether the student was on free/reduced price lunch, whether free/reduced price lunch was missing, and whether the entire school was on free/reduced price lunch.

one college adviser in an entire high school. One noisy estimate of the effect of an additional high school counselor on college enrollment suggests an additional counselor might increase college going by 10 percentage points (Hurwitz & Howell, 2014), but those results appear to be driven by increasing from one to two counselors at very small schools. Schools in our sample are, on average, over four times as large likely resulting in a diluted effect. We further consider effects by school size below.

Given the program's goals and the prioritized populations, advisers may have a larger effect on specific subgroups. The subsequent columns of Table 3 report treatment effect estimates on minority and low-income subsamples. We observe positive but insignificant point estimates for black students and a 2 percentage point effect on Hispanic students that is significant at the 10 percent level in the model with covariates. We observe a similar effect for low-income students of 2 percentage points, but with a *p*-value of 0.103. Overall, we conclude that the program likely had a 1 to 2 percentage point effect on college enrollment in its first year, concentrated among Hispanic and low-income populations, although we do not have enough power to precisely estimate effects smaller than 2 percentage points.

This overall enrollment effect masks important differences in enrollment patterns across institutions. Table 4 displays the intent-to-treat estimates for fall college enrollment outcomes separated by two-year and four-year college enrollment. In the full sample and across all three subgroups, we observe larger treatment effects on enrollment at two-year institutions than at four-year institutions. Overall, the program increased two-year college enrollment by 2.4 percentage points in its first year with larger effects for Hispanic students of 3.4 percentage points and marginally significant effects of 2 percentage points for low-income students. Given the two-year enrollment rates of control students, these estimated effects correspond to a 6.3 percent increase for the full sample, a 9.1 percent increase for Hispanic students, and a 5.5 percent increase for low-income students. In contrast, we see no evidence of effects for black students and no movement in four-year college enrollment rates with point estimates close to zero in each sample. The program's overall college enrollment effects are driven by increases in two-year college enrollment, and, importantly, these effects do not appear to be at the cost of four-year enrollments. The program improves college enrollment rates for students at the margin of two-year college attendance without shifting students away from four-year colleges. We note

Table 4. Intent-to-treat first-year college fall enrollment results for two-year versus four-year enrollment.

	Two-Year enrollment				Four-Year enrollment			
	Full sample	Black	Hispanic	Low-Income	Full sample	Black	Hispanic	Low-Income
Treatment	0.024*	0.009	0.034*	0.020+	−0.007	0.006	−0.006	0.006
	(0.012)	(0.019)	(0.013)	(0.012)	(0.010)	(0.016)	(0.009)	(0.010)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.380	0.383	0.374	0.365	0.237	0.277	0.184	0.175
R^2	0.034	0.016	0.041	0.038	0.077	0.057	0.060	0.055
N	38,124	6,659	21,852	19,677	38,124	6,659	21,852	19,677

Notes: +$p < 0.10$; *$p < 0.05$; **$p < 0.01$. Each cell reports the coefficient on treatment assignment in 2011/2012 for each sample using a linear probability model. Standard errors are reported in parentheses and are clustered at the school level. Covariates include gender, race, age, whether the student was on free/reduced price lunch, whether free/reduced price lunch was missing, and whether the entire school was on free/reduced price lunch.

that this may imply impacts on students not at the top of the academic preparation distribution, although we do not directly observe any academic performance measures in our data.

We note that although we did not see a statistically significant difference in pretreatment college enrollment effects, the pretreatment point estimate for treatment schools is approximately 1.5 percentage points higher than for control schools. If this point estimate is indicative of a real pretreatment difference that remains stable in the subsequent treatment years, it could account for much of the positive effect we observe. It would suggest the treatment effects we observe may be overestimates, and that the true enrollment effect is closer to 1 percentage point for overall two-year enrollments with higher rates among Hispanic students.

Given the lack of compliance to treatment assignment noted in Table 2, the treatment effects reported above are larger for schools that actually had an adviser working in the school. As observed in Table A2, the two-year college treatment on the treated effects for Hispanic students and low-income students in the first cohort is 4.5 and 2.4 percentage points, respectively.[7]

College Application and Persistence Outcomes

We now consider two other observable and pertinent outcomes. Advise TX uses the number of college applications submitted by each student as a performance measure under the assumption that the college adviser will improve the college-going culture of the school and directly assist students with completing college applications. We observe the number of college applications submitted by each student to any public institution of higher education in Texas and assess whether the advisers increase the number of college applications to these institutions in panel A of Table 5.

We do not observe a large effect on the number of applications. The only marginally significant result exists for low-income students, and the effect is small

[7] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 5. First-year intent-to-treat estimates on college application and persistence outcomes.

| | Panel A: College application outcome | | | | | | | |
| | Full sample | | Black | | Hispanic | | Low-Income | |
	Number of apps	Binary applied	Number of apps	Binary applied	Number of apps	Binary applied	Number of apps	Binary applied
Treatment	0.031	0.013	0.124	0.024	0.032	0.021[+]	0.099[+]	0.031[**]
	(0.050)	(0.010)	(0.088)	(0.020)	(0.054)	(0.011)	(0.053)	(0.011)
Control Mean	0.646	0.601	0.813	0.654	0.589	0.559	0.586	0.537
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.061	0.090	0.074	0.079	0.050	0.094	0.067	0.080
N	38,123	38,124	6,659	6,659	21,851	21,852	19,676	19,677

| | Panel B: College persistence outcome | | | |
	Full sample	Black	Hispanic	Low-Income
Treatment	0.006	0.010	0.009	0.017
	(0.010)	(0.017)	(0.011)	(0.012)
Control Mean	0.398	0.380	0.364	0.334
Covariates	Yes	Yes	Yes	Yes
Block Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.081	0.064	0.067	0.071
N	38,124	6,659	21,852	19,677

Notes: [+]$p < 0.10$; [*]$p < 0.05$; [**]$p < 0.01$. Each cell reports the coefficient on treatment assignment for each outcome and for each sample. The number of applications is measured continuously. The binary applied outcome is an indicator for applying to at least one institution. College persistence is a binary measure of whether students were enrolled in a second year of higher education. Binary outcomes are measured using linear probability models. Standard errors are reported in parentheses and are clustered at the school level. Covariates include gender, race, age, whether the student was on free/reduced price lunch, whether free/reduced price lunch was missing, and whether the entire school was on free/reduced price lunch.

at a tenth of an application. This implies the program induced one out of every 10 low-income students to apply to an additional college. This is not overly surprising given that Texas has a common application, and, conditional on having applied to one college, applying to an additional college literally involves only a click of a button.

We also examine whether the program affected students at the margin of applying to any college using a binary measure of applying to higher education. Here, we observe stronger results with a 2 percentage point effect among Hispanic students and a 3 percentage point effect among low-income students. Advisers are motivating some students who would not otherwise apply to college to take a major step toward enrolling. These findings comport with the small increases we observe in two-year college enrollment among Hispanic and low-income students.

Although advisers focus on college enrollment, they may improve the fit or "match" between students and institutions through the advising process. This improved match may result in increased persistence (Bowen, Chingos, & McPherson, 2009); therefore, we also examine college persistence outcomes as a test for this improved match hypothesis and report results in panel B of Table 5. Across all of the

Table 6. Intent-to-treat college fall enrollment estimates pooled and in program years one, two, and three.

	Full sample		Black		Hispanic		Low-Income	
Pooled Years	0.006	0.000	0.009	0.005	0.004	0.004	0.029***	0.015
	(0.009)	(0.008)	(0.014)	(0.012)	(0.011)	(0.010)	(0.011)	(0.010)
		0.542		0.568		0.506		0.487
2011/2012 (Year 1)	0.022*	0.011	0.019	0.012	0.022+	0.020+	0.038**	0.019
	(0.011)	(0.010)	(0.017)	(0.016)	(0.012)	(0.011)	(0.014)	(0.012)
Control Mean		0.558		0.599		0.515		0.498
2012/2013 (Year 2)	0.013	0.006	0.025	0.018	0.009	0.009	0.046**	0.030**
	(0.010)	(0.009)	(0.017)	(0.016)	(0.013)	(0.012)	(0.013)	(0.011)
Control Mean		0.561		0.588		0.526		0.502
2013/2014 (Year 3)	−0.014	−0.016	−0.015	−0.015	−0.014	−0.012	0.005	−0.004
	(0.011)	(0.010)	(0.017)	(0.016)	(0.013)	(0.011)	(0.012)	(0.011)
Control Mean		0.510		0.520		0.480		0.462
Covariates		Yes		Yes		Yes		Yes
Block Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: +p < 0.10; *p < 0.05; **p < 0.01. Each cell reports the coefficient on treatment assignment for each year and for each sample using a linear probability model. Standard errors are reported in parentheses and are clustered at the school level. Covariates include gender, race, age, whether the student was on free/reduced price lunch, whether free/reduced price lunch was missing, and whether the entire school was on free/reduced price lunch. R^2 and sample size vary by year and sample.

samples, we observe small, positive, and statistically insignificant results. Although the results reported in Table 5 are unconditional on college enrollment, conditioning on college enrollment does not change the conclusion that there is no evidence that having a college adviser in your high school improves your institutional match, as far as that match results in increased persistence.[8]

Perhaps we should not be surprised by the null findings on number of applications and college persistence as all of the observed enrollment effect applies to two-year colleges. Students typically do not apply to more than one two-year college. Furthermore, the average persistence rates at two-year colleges are generally lower than at four-year colleges, so any matching benefit the advisers may be achieving might be countered by lower persistence among students induced to attend two-year colleges. Regardless of the reason, we do not observe any significant impacts on persistence in Table 5.

Treatment Effects over Time

Thus far, we have reported results for the initial year of the program, the 2011/2012 academic year. We have two subsequent years of data for the 2012/2013 and 2013/2014 academic years. We report intent-to-treat effects of the three pooled years and the separate years of program implementation in Table 6 (we replicate first year treatment effects from Table 3 for comparison). We note that the pooled results re-

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[8] Our data do not allow us to track students beyond the second year of college enrollment. Using the NSC data (which tends to understate enrollments relative to the state data), we also find no significant impacts on persistence after the first year. As in the impacts in Table 5, we lack power to detect extremely small impacts.

veal no statistically-significant program impacts when controlling for our preferred model including covariates in either the full sample or for any subgroup. However, these pooled results mask important differences in treatment effect estimates over time. Focusing on the full sample results, we observe the positive 2 percentage point treatment effects observed in year one falling to an insignificant 1 percentage point effect in year two and declining to a negative point estimate in year three of the program. This pattern generally holds for each subgroup, with the treatment effect declining over time such that there are only null or negative treatment effects by the third year of the program. We consider several possible explanations for this reduction in treatment effect over time below.

The declining treatment effects over time present a puzzle, and we consider four separate hypotheses that may explain the pattern of results. The first potential explanation focuses on treatment compliance. Even in the first year, compliance was only 75 percent (Table 2). This was due to five initially assigned treatment schools backing out of their commitment and not accepting an adviser, combined with nine control schools receiving an adviser. Compliance continued to deteriorate over the subsequent years, with some schools leaving the treatment and some control schools receiving an adviser as the program expanded.[9] By the 2015/2016 school year, 20 of the 36 schools initially assigned treatment had left the treatment and did not have an adviser, and 17 of the initial 75 control schools had an adviser. This eroding compliance over time dilutes the treatment contrast, possibly leading to the attenuation of the effects we observe.

However, when we examine treatment on the treated effects by instrumenting for treatment receipt with treatment assignment (Table A2), we observe similar patterns of positive effects for Hispanic and low-income students in the first year declining to no effects in year three.[10] The decline in the TOT results are roughly aligned with the decline observed in the ITT results suggesting that adjusting for compliance does not substantially explain the falling estimates.

A second hypothesis as to why the treatment effect fade-out occurred involves the size of schools in compliance with treatment assignment. A reasonable hypothesis is that size is a mediating factor in the treatment. One adviser in a school of 50 graduating seniors can work with a higher fraction of the student body, and at a higher level of dosage, than a school with a graduating class of 700. If smaller schools experience a larger treatment effect and if smaller treatment schools are less likely to maintain an adviser over the treatment period, we might expect the treatment effect to decline over time. In the distribution of treatment school size, the median, 75th percentile, and 90th percentiles of schools who had an adviser did not change over time. By contrast, the 10th and 25th percentiles changed as the program moved towards large schools, so compliance is related to school size. The treatment effect might be greater at smaller schools because our dependent variable is measured in enrollment rates. If any individual counselor can affect a fixed number of students, then the impact on rates will be smaller in schools with larger student bodies. We also have descriptive evidence that advisers in smaller schools can devote more time to each student. In one small treatment school, 90 percent of students met with an

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[9] Evidence from supplementary qualitative work documented that schools left the treatment conditions in subsequent years for different reasons (Bettinger et al., 2014). In several cases, the university partner cancelled the program because the schools were too distant from the institution. In one case, the lead guidance counselor did not want to renew the adviser because of poor performance. In another case, there was a competing college access program in the school, and the adviser left to work in that competing program.
[10] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

adviser over 10 times, while the average number of meetings is 3.5 meetings per student across all treated schools.

However, when we estimate the treatment impact interacted with size, we find few significant results. We estimate differential size effects using several alternative specifications (linear size, log size, and binary indicators for small schools), but we find little evidence of differential impact (Table A3).[11] Although we lack statistical power to fully identify how size interacts with treatment, the lack of significance does not support the hypothesis that size is the primary mechanism of explaining the fade-out of treatment effects over time.

A third possible reason for the decline of treatment effects is that control schools adopted substitutes for the Advise TX program. While we have documented the compliance with respect to Advise TX, we have not documented the existence of other college access programs. Control schools not assigned the program may adopt an effective alternate program thereby reducing the experimental treatment effect. While potentially serving as a good test of general equilibrium for widespread program adoption, this possible substitution among control schools potentially undermines identifying the program-specific effects in our experiment. Unfortunately, we do not have a comprehensive list pre- and post-treatment in the number and types of college advising programs available in control and treatment schools. However, in 2015/2016, we conducted a survey inventory of programs at treatment and control schools. We asked schools to provide a year-by-year accounting of the availability of a wide array of common college access programs (e.g., AVID, TRIO, GEAR UP) hosted at the school between 2010/2011 and 2015/2016.[12] Although 35 of the 36 treatment schools provided data, only 69 percent (52 of 75) of the control schools replied. Our data are not completely representative of the control sample; yet, examining the number of programs available each year provides a sense of how control schools responded to not being assigned the Advise TX program.

Examining the number of programs active over time reveals suggestive evidence of the substitution pattern. Figure 1 provides the number of programs (including Advise TX) in each academic year active at treatment and control schools. In the last pretreatment year in 2010/2011, the number of programs is very similar between assigned treatment and control schools with a statistically insignificant 0.35 program difference. In the first two years of treatment, we see the treatment schools growing in the number of programs relative to control schools due to the adoption of Advise TX. However, after the 2012/2013 academic year, we observe the number of programs in control schools increasing while the number in treatment schools remains constant. By the 2014/2015 academic year, the difference in number of programs between treatment and control schools is less than in the first year of treatment. Hence, there appears to be some empirical support for the substitution hypothesis.

A fourth hypothesis is that schools made within-school staff adjustments in response to program adoption. Treatment schools may divert guidance counselor resources away from college preparation and information activities once an Advise TX adviser arrives in the school. We again rely on the treatment and control school surveys to evaluate this hypothesis by examining schools' responses to the question: "When a student comes to a guidance counselor with a college related question, what is the typical process for handling that question at your school?" Forty percent of schools that were assigned treatment answer that the student is referred to a specific guidance counselor who oversees college-going activities, but only 25 percent

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[11] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[12] Schools could also report additional college advising programs not explicitly named on the survey.
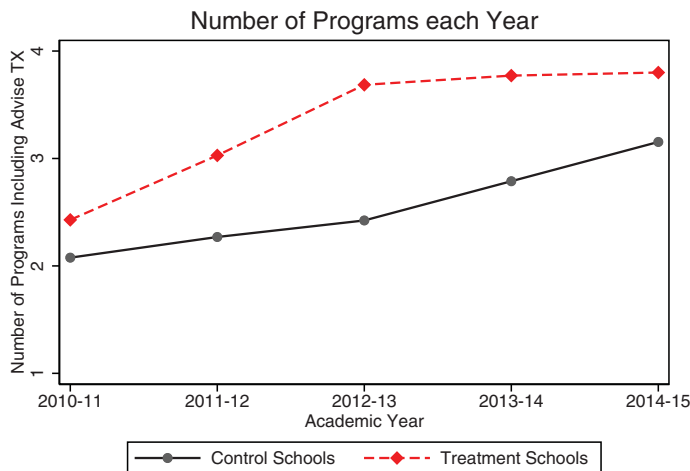
Figure 1. Number of College Advising Programs Available in Treatment and Control Schools over Time. [Color figure can be viewed at wileyonlinelibrary.com]

of control schools respond in this way. This 15 percentage point difference may be a result of guidance counselors at treatment schools reducing their workload on college-related activities by sending them to the Advise TX adviser. Observing a potential change in this measure over time would provide stronger support for this hypothesis, but in the absence of such longitudinal data, the cross-sectional treatment and control contrast provides suggestive support for the staffing adjustment hypothesis.

The potential for staff adjustments has important implications. One criticism of randomized experiments is that the underlying treatment changes as individuals and organizations adjust to the treatment. These general equilibrium-type effects could cause the short-run impacts to overstate the impact of a program in the short-run compared to its impact when conducted at scale. In particular, Advise TX was intended to be a complement to existing services, but we have some suggestive evidence that it might have become a substitute over time. Such a shift in the program's intent and interaction with existing services also has broad implications for other randomized experiments where the treatment may vary once scaled.[13]

In summary, it appears likely that the observed drop in positive treatment effects over time is due to a combination of annually declining compliance to treatment assignment, additional college advising program adoption in control schools, and substitution of resources away from college advising in treatment schools. These issues speak to the importance of monitoring compliance throughout the course of longitudinal experimental analyses. They also suggest general limitations to the clustered design of the experiment: It is difficult to observe compliance within schools and difficult to identify heterogeneous program effects by school characteristics given the relatively small number of randomized units. In contrast, student-level randomization would have provided substantially more power and would have enabled a school-to-school comparison of effects. However, such a research design is impos-

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[13] Throughout the research, we regularly informed Advise TX on the nature of the results. Upon becoming aware of the possible decrease in school services, Advise TX modified their school contracts to include a clause about continued effort by school guidance counselors. The responsiveness of Advise TX to these findings provides some evidence on the potential of researcher-practitioner partnerships.

sible for a program designed to function at the school level such as Advise TX and would have introduced substantial risk of within-school spillover effects, a concern we have with prior within-school randomization evaluations of college advising.

CONCLUSION

Our results lead us to conclude that the program improved two-year college enrollment rates, mostly for Hispanic and low-income students, which supports the general findings in the literature that providing information and college assistance improves college enrollment. Our results also extend the current literature by experimentally establishing that a holistic, school-wide college access program improves postsecondary enrollment outcomes for underserved high school students. In contrast to several prior experimental studies that have targeted interventions on selected students, we demonstrate that a school-wide program can have positive enrollment effects at the margin of attendance. The effects we observe are substantially smaller than those reported by Carrell and Sacerdote (2016), but that is likely explained by the difference in targeting and dosage in their intervention. At their largest high schools, roughly 30 students were treated by a team of advisers for multiple hours each week for several weeks. Combined with financial incentives, this intervention increased college enrollment rates for women by 14 to 15 percentage points. This effect is substantially larger than our observed effect of a few percentage points on college attendance, but the Advise TX intervention targeted the entire school with one adviser working with many times the number of graduating high school students and investing in the preparation activities of underclassmen. The targeted intervention studied by Carrell and Sacerdote also relies on pre-identifying students at the margin of attending college. If that pre-identification is correct, their program may be more efficient. If, however, there is error in that pre-identification, students who need the intervention may be overlooked. Although the Advise TX program may waste resources on students who would have otherwise attended college or who have a very low probability of attendance even with assistance, it likely reaches a higher percentage of students who can be switched to a postsecondary enrollee.

Our results are more aligned with those of Oreopoulos and Ford (2016) who examine a school-wide intervention focused on incorporating assistance with completing college applications into the senior high school curriculum in Canada. They find the intervention increased two-year college enrollments by 5 percentage points, only slightly larger than our treatment on the treated effects, albeit in a very different international context.

Results from our large-scale, state-wide analysis of the Advise TX college access program provide important considerations for policymakers. Our results provide evidence in cautious support of continued funding at the federal, state, and institution levels towards college access programs targeting low-income and minority populations, where we observe the largest two-year college enrollment results. The lack of observed positive effects on four-year enrollments and persistence, however, suggests programs may need to focus renewed efforts on promoting baccalaureate degree attainment, financial aid to enable students to afford four-year colleges, and better match to improve persistence.

The results also suggest current college access programs should reflect on their program design. While we cannot definitely attribute effects of Advise TX to the near-peer design of the intervention, other qualitative work on the impact of Advise TX suggests that this plays an important role (Bettinger et al., 2014). They find that students seemed "connected" with advisers because of the fact that the advisers had recently been in similar high schools and were closer to their age.

Based on our analysis of the decline of effects over time, we also encourage programs to consider how their intervention can supplement rather than supplant existing college advising services. Funders and programs can solicit guarantees from schools that program adoption will not lead to a replacement of established college advising activities, and schools can monitor how their established college advising services may change after program adoption.

Of special interest is the extent to which the program covers its costs. If we accept the pooled three-year treatment effect estimates of no impact, then the program would not be cost-effective. However, given our potential explanations of the attenuating effects over time, the cost-benefit analysis is more nuanced. If schools are substituting guidance counselor efforts with the Advise TX adviser, then any benefit of the program observed in its first year of implementation falls to zero. If, however, the treatment effect diminishes due to control schools adopting additional programs, then the program still provides a benefit. Under this scenario, it is valuable to consider the cost-benefit analysis given the first-year treatment effects. We provide such a detailed cost-benefit analysis in Table A4 and summarize it here.[14] Our point estimate on the effect of the treatment on two-year college enrollment (Table 4, column 1) suggests that 11.1 additional students per high school attended community college as a result of Advise TX. Given our lack of finding of any significant impact on persistence, we make the assumption that all of these students acquired just one year of college.

If indeed the program did not lead to differential attendance except in the first year, then 11.1 students acquired "some college" and nothing more. If we use the College Board (2016) earnings for some college, it suggests that earnings are $4,900 higher per year than they would have been with only a high school diploma. Because that estimate likely incorporates some students at four-year colleges who dropout after two or three years, we take a more conservative approach, by cutting this estimate of the benefits of some college by half.[15] This suggests that earnings increased by $27,165 in each school after the first year that students attended just one year of college.

The biggest cost to Advise TX is not the adviser costs. The average cost for the advisers is roughly $59,000 per school which includes salary and overhead. The largest cost is the foregone income from attending college. The 11.1 students who now attend college forego some earnings. Using College Board data (on returns to high school) and the National Center for Education Statistics (wages of currently enrolled college students), we estimate that each student foregoes almost $21,000 per year. This is likely an overestimate given the high unemployment rates of high school graduates who do not attend college in the years just after graduation, but it serves as a conservative estimate. As students drop out of college and join the workforce, these foregone wages decline and students now start experiencing some of the returns to college. We estimate that community college tuition and fees balance out with state and federal need-based financial aid programs given that most of the impact occurs with free/reduced lunch students.

We can then compute the lifetime increase in earnings by combining the costs and benefits. If we assume some college completion, with a conservative 5 percent discount rate, we compute that average lifetime gains per school per year is nearly $156,000. As long as the returns to one year of community college exceed $1,570

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[14] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[15] This estimate is even more conservative than the data in Texas would suggest. Using Texas data from the American Community Survey, the annual earnings differential for less than one year of college relative to a high school diploma is $5,407.

per year, then the program generates positive returns given the number of students who are impacted and the low cost per school.

We view these as conservative estimates for a variety of reasons. First, we have been liberal in estimating the foregone wages. High young adult unemployment and low entry wages should lower the foregone wages. Second, we have assumed that CAC does not affect time to degree, subsequent return to college, or other long-run outcomes. Finally, we also ignore any non-pecuniary benefits of college that would likely improve the returns for college (Oreopoulos & Petronijevic, 2013). If we lift any of these assumptions, the estimated return swells.

Even with our most conservative estimates, we find an internal rate of return of just over 4 percent. We know of no other estimates of the return to college access programs. Our result is similar or better than the return to financial aid models. For example, Dynarski (2008) finds a 9 percent rate of return for Georgia Hope. Other financial aid programs such as the Ohio College Opportunity Grant (Bettinger, 2015) suggest returns that are closer to 1 percent.

In conclusion, results from our cluster randomized control trial provide the first causal evidence of an at-scale school-wide college access program. The evidence is mixed. While the program does not generate significant overall impacts on enrollment, the program is effective at increasing first-year college enrollment for low-income and Hispanic students, inducing them to enroll in two-year colleges when they otherwise would not enroll in college immediately after high school graduation. However, the impacts do not persist beyond the first year of implementation, and we do not find impacts on persistence after initial enrollment for the first cohort. Despite issues of compliance in the experiment, our estimates are relatively precise, allowing us to preclude large impacts even when the estimated impacts are statistically insignificant. Even so, given the low cost of the program per school, even a modest improvement in wages results in a rate of return conservatively estimated at 4 percentage points. These findings provide empirical support for policymakers choosing to continue investment in college access programs generally, although further analysis of different program structures and comparisons between whole-school interventions versus targeting individual students is a promising area for further research.

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APPENDIX

Table A1. Average number of adviser interactions by student characteristics.

Student Characteristic	# of 1:1 meetings	# of group/class meetings	# of parent meetings	# assist on applications	# SAT registrations assisted
Free/Reduced Lunch	5.1	3.9	0.3	2.3	1.0
Not FRL	3.1	2.2	0.1	0.8	0.2
First Generation	6.5	4.4	0.5	3.3	1.1
Not First Generation	5.1	2.6	0.1	1.8	0.5
Hispanic	4.7	3.9	0.3	2.2	0.9
Black	4.8	3.2	0.2	1.9	0.6
White	2.7	2.0	0.1	0.8	0.3

Notes: Data focus on all interactions in the 2016/2017 school year in Advise TX schools. This is the first year where complete student-level reporting was possible on race, income, and first generation status.

Table A2. Treatment on the treated instrumental variable estimates.

Panel A: Fall college enrollment			
	Full sample	Black	Hispanic	Low-Income
2012 Treatment	0.014	0.015	0.026^+	0.023^+
	(0.012)	(0.0207)	(0.015)	(0.014)
2013 Treatment	0.008	0.024	0.012	0.038^{**}
	(0.012)	(0.020)	(0.016)	(0.013)
2014 Treatment	-0.025^+	-0.027	-0.017	-0.005
	(0.015)	(0.030)	(0.016)	(0.015)

Panel B: Fall two-year college enrollment			
	Full sample	Black	Hispanic	Low-Income
2012 Treatment	0.031^+	0.012	0.045^*	0.024^+
	(0.016)	(0.025)	(0.019)	(0.014)
2013 Treatment	0.013	0.000	0.023	0.026^+
	(0.015)	(0.021)	(0.019)	(0.014)
2014 Treatment	0.008	-0.035	-0.002	-0.007
	(0.014)	(0.023)	(0.016)	(0.013)

Panel C: Fall four-year college enrollment			
	Full sample	Black	Hispanic	Low-Income
2012 Treatment	-0.009	0.008	-0.007	0.008
	(0.012)	(0.021)	(0.013)	(0.012)
2013 Treatment	-0.003	0.030	-0.008	0.012
	(0.011)	(0.019)	(0.010)	(0.010)
2014 Treatment	-0.016^+	0.010	-0.015	0.002
	(0.009)	(0.018)	(0.011)	(0.009)
Covariates	Yes	Yes	Yes	Yes
Block Fixed Effects	Yes	Yes	Yes	Yes
N	38,124	6,659	21,852	19,677

Notes: $^+$p $<$ 0.10; *p $<$ 0.05; **p $<$ 0.01. Each cell reports the coefficient on treatment received as instrumented using treatment assignment in 2011/2012 for each sample using a linear probability model to estimate the binary outcome. Standard errors are reported in parentheses and are clustered at the school level. Covariates include gender, race, age, whether the student was on free/reduced price lunch, whether free/reduced price lunch was missing, and whether the entire school was on free/reduced price lunch.

Table A3. Treatment effect heterogeneity by school size.

	2012 Fall college enrollment	2012 Fall two-year enrollment	2012 Fall four-year enrollment
Size = Number of students/1,000			
Treatment	0.010	0.036	−0.034
	(0.027)	(0.033)	(0.027)
Size	0.037**	0.040**	−0.003
	(0.010)	(0.012)	(0.009)
Treatment*Size	−0.002	−0.008	0.013
	(0.012)	(0.014)	(0.013)
Size = Ln(number of students)			
Treatment	−0.026	−0.026	−0.071
	(0.168)	(0.202)	(0.157)
Size	0.054**	0.054**	0.003
	(0.019)	(0.021)	(0.015)
Treatment*Size	0.005	0.006	0.009
	(0.022)	(0.027)	(0.021)
Size = Small school indicator for below median			
Treatment	0.009	0.025	−0.011
	(0.014)	(0.016)	(0.015)
Size	−0.031*	−0.031+	0.000
	(0.016)	(0.017)	(0.013)
Treatment*Size	0.002	−0.006	0.010
	(0.022)	(0.026)	(0.021)
Size = Small school indicator for below 25th percentile			
Treatment	0.017	0.038**	−0.014
	(0.011)	(0.013)	(0.012)
Size	−0.036*	−0.022	−0.022
	(0.017)	(0.019)	(0.013)
Treatment*Size	−0.035	−0.072*	0.025
	(0.024)	(0.032)	(0.021)
N	38,124	38,124	38,124

Notes: $^+p < 0.10$; $^*p < 0.05$; $^{**}p < 0.01$. Each model includes covariates and blocking fixed effects. Covariates include gender, race, age, whether the student was on free/reduced price lunch, whether free/reduced price lunch was missing, and whether the entire school was on free/reduced price lunch. Standard errors are reported in parentheses and are clustered at the school level.

Table A4. Data for cost-benefit analysis.

Variable	Values	Source
Estimated impact on college attendance	0.024	Table 4, column 1
Average class size (Senior class)	462	Table 1, panel A, "Total Students" in Treatment HS
Total students impact per school	11.1	Impact*Average Class Size
Increase in earnings for BA degree	$24,600	College Board, Education Pays, 2016, Figure 2.1
Increase in earnings for Associate's degree	$9,200	College Board, Education Pays, 2016, Figure 2.1
Increase in earnings for any college	$2,450	College Board, Education Pays, 2016, Figure 2.1. Assumes 1/2 of the "some college" return for more conservative estimate
Proportion of completing an AA/AS conditional on 2-yr start	0.30	Shapiro et al., 2016 (Figure 12)
Proportion of completing a BA/BS conditional on 2-yr start	0.09	Shapiro et al., 2016
Proportion still enrolled	0.16	Shapiro et al., 2016
Total students who achieved AA/AS	0.0	Impact*Total Students
Total students who received a BA/BS	0.0	Impact*Total Students
Total students still enrolled	0.0	Impact*Total Students
Still enrolled at 2-year	0.0	Breaking up still enrolled using Degree ratio from Shapiro et al., 2016
Still enrolled at 4-year	0.0	Breaking up still enrolled using Degree ratio from Shapiro et al., 2016
Students who received any college	11.1	Remaining students
Average gain in earnings for students at school	$27,165	Total students in each category multiplied by increase in earnings
Years of college	1	
Foregone earnings in any year	$20,948	Difference between high school earnings and NCES estimate of earnings while enrolled
Total gain in wages (5 percent discount rate)	$156,195	PDV of earnings (starting in year 7) less foregone wages during college and cost of the program