The Forgotten Summer: Does the Offer of College Counseling After High School Mitigate Summer Melt Among College-Intending, Low-Income High School Graduates?

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Abstract

Despite decades of policy intervention to increase college entry and success among low-income students, considerable gaps by socioeconomic status remain. To date, policymakers have overlooked the summer after high school as an important time period in students' transition to college, yet recent research documents high rates of summer attrition from the college pipeline among college-intending high school graduates, a phenomenon we refer to as "summer melt." We report on two randomized trials investigating efforts to mitigate summer melt. Offering college-intending graduates two to three hours of summer support increased enrollment by 3 percentage points overall, and by 8 to 12 percentage points among low-income students, at a cost of $100 to $200 per student. Further, summer support has lasting impacts on persistence several semesters into college. © 2014 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Policymakers have employed a broad range of strategies over the last several decades to reduce disparities in college entry and success by family income. These include efforts to improve students' academic readiness, to increase college affordability for low- and moderate-income families, and to simplify information about college and financial aid (Bettinger et al., 2012; Cortes, Goodman, & Nomi, 2013; Deming & Dynarski, 2009; King, 2011). Few of these initiatives, however, have focused on the summer after high school graduation, or on building students' capacity to independently complete tasks required for successful college matriculation and persistence.

Yet, even after students have been accepted to and decided to attend college, successful matriculation is contingent on students completing a number of tasks during the summer, at a time when they no longer have access to high school counselors and have yet to access support resources at their intended college. For instance, colleges typically require students to take placement tests and complete an abundance of paperwork, including housing and medical forms, over the summer months. Completing these tasks may be particularly daunting for low-income and first-generation college-bound students whose family members may lack experience with the college-going process. In addition, it is only in the summer after high school graduation when students must confront the reality of paying the first college term

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bill, which often includes unanticipated costs, such as required health insurance coverage. For college-intending students, successfully navigating the post-high school summer thus requires a level of financial and college literacy that may be unrelated to their ability to succeed in the classroom. As a result, students who have already surmounted many obstacles to college enrollment and who would potentially earn high returns to postsecondary education may, nonetheless, fail to matriculate. We refer to this phenomenon as "summer melt."

In previous empirical work drawing on data from the Education Longitudinal Study of 2002, we estimated that among students who intended to transition directly from high school into college, approximately 10 percent fail to do so. We observe even higher rates of summer melt among students of low socioeconomic status (SES). For example, in a sample of college-intending graduates from the Boston Public Schools, we estimate that approximately one in five students failed to enroll in the fall (Castleman & Page, 2013). These findings are consistent with descriptive evidence from multiple school districts serving large shares of low-income students, including the Fulton County Schools (FCS; Matthews, Schooley, & Vosler, 2011), Fort Worth Independent School District (Daugherty, 2012), and the Chicago Public Schools (Roderick et al., 2008). Our estimates are also consistent with the findings from qualitative interviews indicating that, even after paying a deposit to a particular college, low-income students struggle to evaluate financial aid offers and complete all necessary requirements to enroll (Arnold et al., 2009).

Motivated by these empirical results, we conducted a pilot experimental study of summer college counseling for college-intending students in a network of innovative high schools in Providence, RI (Castleman, Arnold, & Wartman, 2012). The results were quite striking: Treatment group students who were offered summer assistance from their high school counselors were 14 percentage points more likely to enroll immediately in college than students in the control group. Despite the pilot study's small sample size, these effects were large enough to achieve statistical significance. In addition, the cost of summer counseling in this pilot study was less than $200 per student, suggesting that summer support may be a cost-effective strategy for promoting college enrollment among low-income students.[1] Nevertheless, because of the small sample size in this study as well as the unique setting in which it was conducted, we sought to investigate whether summer college counseling support would positively impact students' college outcomes at a larger scale and in more mainstream educational settings.

Therefore, in this paper we report the results of two larger scale experiments designed to determine whether summer counseling can increase rates of college enrollment and persistence among high school graduates from large urban public school districts. In the summer of 2011, we collaborated with uAspire, a college access organization headquartered in Boston, MA, and FCS (GA) to replicate the model of college counseling used in the Providence pilot study. Like the pilot study, the cost of summer outreach and counseling was about $100 to $200 per student. Across the sites, the offer of summer counseling increased the probability of fall college enrollment by approximately 3 percentage points; this effect corresponds to a nearly 20 percent reduction in summer melt. In Boston, we find even more pronounced impacts on whether students continuously enrolled into the fall of sophomore year in college: Students who were offered additional counseling the summer before freshman year were 7.8 percentage points more likely to persist into sophomore year than students who were not offered summer support. These persistence impacts suggest that summer support can promote more stable enrollment

[1] By comparison, the financial aid literature has consistently found that $1,000 in need-based grant aid increases enrollment by 3 to 6 percentage points (Dynarski, 2003; Kane, 2003).

beyond the first semester. Further, the summer outreach was particularly beneficial for the lowest income students in each site. In Boston, where we are able to observe students' expected family contribution (EFC) to the cost of college, the summer outreach improved on-time enrollment by 12 percentage points among those students with a zero EFC. In Fulton County, where we were able to observe student-level free/reduced-price lunch (FRL) status, summer outreach and the offer of support increased immediate enrollment by more than 8 percentage points among FRL students. These results indicate that summer outreach and support may be particularly beneficial among college-intending students from low-income backgrounds.

We structure the remainder of the paper into several sections. In the next section we review the literature pertinent to summer barriers to college enrollment. In the following section we describe our research design. We then present our results. Finally, we conclude with a discussion of the implications for policy and research.

LITERATURE REVIEW

The economic and nonpecuniary benefits of higher education are substantial, and may be particularly pronounced for low-income students (Dale & Krueger, 2002; Goldin & Katz, 2008; Oreopoulos & Salvanes, 2011). Becker's (1964) model of human capital investments assumes that students are aware of these benefits and suggests that students will pursue a college education if the present discounted value (PDV) of the benefits of higher education exceeds the PDV of the costs of going to college. For low-income students, however, the time, effort, and psychic costs associated with completing college and financial applications may be particularly high, and these costs may deter academically qualified students from pursuing higher education. A number of studies document also how informational barriers at early stages in the college and financial aid application processes can lead students to make suboptimal decisions about whether to enroll in college (Avery & Kane, 2004; Bettinger et al., 2012; Dynarski & Scott-Clayton, 2006). Informational barriers continue to be problematic during the summer months, even for those students who have gained acceptance into a college or university. Students receive a considerable volume of required paperwork from their intended college or university over the summer months. Particularly for students and families lacking college and financial literacy, it may be difficult to complete these forms properly (Arnold et al., 2009).

Furthermore, recent behavioral research suggests that short-term costs weigh heavily in individuals' analyses, even if these investments would result in long-term gain (see, e.g., Chabris, Laibson, & Schuldt, 2008), and even minor cost barriers may deter students from completing key stages of the college application or choice processes (Pallais, 2013). The tuition bills students receive the summer after high school often contain unanticipated charges that are particularly likely to trigger this cost aversion. For instance, Massachusetts state law (G.L. c.15A, § 18) requires all colleges and universities in the state to enroll students in their institution's health insurance plan by default.[2] If students are covered by their parents' insurance, they can opt out of the college plan, but must submit a waiver to do so. If students do not submit a waiver, the tuition bill that they must pay in July or August can be anywhere from $500 to $2,000 higher than students expected, since health insurance typically is not included in published estimates of the cost of attendance or in the financial aid award letters that students receive in the spring.[3] This unanticipated

[2] Retrieved from http://www.mass.gov/eohhs/consumer/insurance/more-programs/student-health-insurance.html on January 8, 2012.
[3] We thank the advisors at uAspire for highlighting this issue.

expense may sufficiently increase short-term costs to the point where students on the margin of enrolling decide not to matriculate.

In short, traditional and behavioral economic theory suggests a variety of reasons why low-income, college-intending students may change or abandon their postsecondary plans during the summer months. Nevertheless, there are several reasons why the summer after high school is potentially an ideal time for policy intervention to help students achieve their postsecondary plans. First, students who have been admitted and paid a deposit to a college have already surmounted several key obstacles to college access; the remaining obstacles are relatively easy to address in comparison. In particular, among those with relatively low to moderate levels of unmet financial need, a moderate amount of counseling and support could help most students access their college's web portal or properly waive the cost of their college's health insurance plan if already covered by their parents' policy. Second, students may be more responsive to outreach and support over the summer months. Whereas college may have seemed light years away in the waning days of high school, students may feel a greater sense of urgency when they receive their tuition bill during the summer. Third, there is an ample supply of high school counselors to assist students in the summer, since many of them are employed formally by their school districts only for the academic year.

RESEARCH DESIGN

During the summer of 2011 we collaborated with two educational agencies, uAspire and FCS, to conduct summer counseling interventions. uAspire is a Boston-based, nonprofit organization that provides college financial aid advising and scholarships to high school students.[4] FCS is a large urban school district in the metro-Atlanta area of Georgia with more than 90,000 students in 100 schools.[5]

uAspire Sample and Staffing Structure

uAspire is particularly focused on ensuring that students can pursue an affordable postsecondary plan. This mission underlies its two primary programmatic efforts. The first of these programs is uAspire's High School Advising Program, which places financial aid advisors in every high school in the Boston Public Schools. uAspire advisors spend at least one day per week working individually with students in their assigned school(s) for the entire school year. During the 2010 to 2011 academic year, out of more than 4,000 seniors enrolled in Boston public high schools, 61 percent participated in a uAspire-hosted group workshop, and 71 percent met individually with an advisor at least once.

The second program is the Last Dollar Scholarship, which aims to help Boston students pay for college costs that are not met by their final financial aid packages (including federal, state, institutional, and other support). Students who work with uAspire during the academic year through the High School Advising Program are eligible to apply. Each year, uAspire provides grant funds to all students who fall within a designated awardable range of unmet financial need.[6] During summer 2011, 929 students applied for Last Dollar Scholarships, and uAspire awarded 106

[4] More information about uAspire can be found at http://www.uaspire.org.
[5] More information about the FCS can be found at http://portal.fultonschools.org/About_Fulton/Pages/Fulton_Facts.aspx.
[6] The awardable range varies from year to year, depending on the volume of applications and the scholarship budget for that given year, with priority given (typically) to students with lower levels of unmet need.

scholarships ranging in value from $500 to $5,000. We included 927 of the 929 applicants in the sample for this study, excluding two who were eligible for other summer supports.[7]

uAspire selected 11 advisors to staff the intervention and divided them into four teams for the purposes of pairing experienced and novice advisors and to provide backup counseling options during the summer vacations for each of the advisors. We assigned each of the 927 Last Dollar Scholarship applicants to a team of advisors, matching applicants to teams with the advisor who had worked with them before wherever possible. We then randomized students to treatment and control groups within each advising team.[8] Our research team conducted the randomization and provided caseload rosters to each team of advisors. The uAspire intervention ran from June 27, 2011 through August 10, 2011.

FCS Sample and Staffing Structure

In selecting FCS high schools to participate in summer outreach and counseling, we took into consideration the geographic distribution of the district's 14 traditional high schools. The district is physically bisected by the City of Atlanta and the Atlanta Public Schools, creating two distinct regions that differ substantially in socioeconomic and demographic enrollment. We selected for participation the three high schools in the southern region of the district and the three high schools in the northern region of the district with the highest estimated rates of summer melt among the previous cohort's (class of 2010) college-intending high school graduates.[9]

We used student responses to the FCS Senior Exit Survey administered to 12th graders in May 2011 to target students for participation in the intervention. Specifically, we included students who reported they (1) planned to pursue postsecondary education following high school, (2) had applied to at least one postsecondary institution, and (3) had been accepted to at least one postsecondary institution.[10] A total of 1,446 students met these criteria. Of these, we randomly selected 80 students from each school to receive proactive outreach from FCS counselors, for a total of 480 students assigned to the treatment group. Staff in the FCS Assessment and Accountability Department conducted the randomization and provided caseload rosters to each participating counselor.[11] The remaining 966 students not selected for proactive outreach comprised the control group.

FCS district leadership worked with the head school counselor in each school to select two counselors to staff the intervention. FCS counselors spend only a small portion of their time during the academic year on the college application and

[7] These scholarship applicants are class of 2011 graduates from 42 different high schools within the Boston Public School District, including traditional comprehensive high schools, exam schools, charter schools, and district pilot schools.

[8] For each advisor, uAspire identified a maximum summer counseling caseload, ranging from 10 to 47 students, depending on advisors' other summer work responsibilities. If an advisor's caseload was not sufficient to include all treatment group students with whom that advisor had worked during the academic year, then some of those students were assigned to another advisor.

[9] These rates were estimated by comparing counselors' expectations of their students postsecondary plans to the actual postsecondary enrollments exhibited by class of 2010 graduates.

[10] In order to meet a target of 80 treatment group students at each school, at two of the intervention schools the third criterion (accepted to at least one postsecondary institution) was expanded to include students reporting they were still waiting to hear about their acceptance. Across the six high schools, 96 percent of students meeting these criteria were class of 2011 district graduates, with 4 percent of students failing to graduate at the time of spring high school graduation.

[11] Within each FCS school, randomization was stratified by student gender and FRL status. For both the Boston and FCS samples, we conducted the randomization process in Stata, making use of the runiform() function for the generation of random numbers.

college choice processes, so we provided counselors with supplemental training on how to help students apply for federal and Georgia-specific financial aid.[12] The FCS intervention ran from June 6, 2011 through July 22, 2011.

Intervention Design

We randomly selected treatment group students to receive proactive outreach from a uAspire advisor or FCS counselor over the course of the summer, while the control group students did not receive outreach.[13] Nevertheless, in both sites, counselors were instructed not to deny support to any control group student who actively sought help. Counselors made multiple attempts to contact each treatment group student to offer support and used a variety of outreach methods: phone, e-mail, and text and Facebook messaging.[14] Upon reaching students, uAspire advisors offered each a $25 gift card incentive to attend an in-person meeting; we were not able to incorporate student incentives in the FCS experiment.[15] uAspire advisors primarily met with students at the uAspire Center for College Affordability (CCA) in Boston's city center, while FCS counselors who met with students in person primarily used the school from which they were working. Most consultations in Boston occurred in person, while in FCS, counselors depended on phone conversations to provide most of their support.

We provided uAspire advisors with a protocol for the outreach and support they were to provide. During the first in-person meeting, counselors completed a college-assessment protocol that we designed to achieve three purposes. First, counselors reviewed the student's financial aid award letter and provided guidance based on the student's level of unmet financial need. Second, counselors briefed the student on the calendar of key summer deadlines at the college the student planned to attend, and helped the student understand and complete paperwork the student had already received from that college. Finally, the counselor assessed whether the student faced social or emotional barriers to college enrollment in the fall.[16]

At the conclusion of the assessment meeting, counselors helped students create a list of personalized tasks they needed to complete in order to start college that fall. Throughout the rest of the summer, counselors followed up with students individually to check on their progress in completing these tasks. Subsequent to the initial assessment meeting, much of the communication between counselors and students happened via phone, e-mail, and text, though counselors also conducted in-person follow-up meetings with students when they felt it important to do so.

uAspire's focus on college affordability strongly infused advisors' interactions with students. uAspire's organizational philosophy, articulated by leadership and through written documents, generally discourages students from assuming loan burdens in excess of $10,000 to $15,000 annually. During trainings in advance of the summer intervention, uAspire leadership coached advisors to caution students from taking

[12] The latter training topic was particularly important, since at the time of the experiment, students who maintained high GPAs in high school qualified for up to a full scholarship at in-state public institutions and up to $4,000 at in-state private institutions through the Georgia HOPE and Zell Miller Scholarship programs.

[13] In Boston, treatment and control group students alike were told prior to the start of the intervention that individualized counseling would be available from uAspire over the summer. In FCS, students were not made aware of the program prior to its commencement. Control group students who initiated contact with uAspire/FCS received the same level of support as those in the treatment group.

[14] We use the term *counselor* to refer generally to both FCS counselors and uAspire advisors.

[15] uAspire advisors reported that while students were grateful to receive a gift card, it did not appear to be the primary driver for students deciding to take up the offer of a one-on-one advisor meeting.

[16] Materials we developed to guide counselors' interactions with students are available upon request.

out large debt to finance their college plans. During the summer, advisors reported encouraging students whose college plans would have required assuming large debt to instead delay their enrollment or consider an alternative college.

In contrast, counselors in the FCS experiment were encouraged to use an intake form that listed numerous tasks required for college enrollment during their initial contact with students, but were not provided with specific protocols for outreach or support. Rather, we urged the counselors to follow their existing professional protocols for working with students as they were expected to do during the academic year. Counselors in both sites logged whether and when they interacted with students (both treatment and control). Counselors indicated that many of their interactions with students focused on issues of financial aid. Counselors also reported addressing a variety of informational questions, such as how to access a college's web portal, how to complete required paperwork, and what the matriculation process entailed.

Data Sources and Descriptive Statistics

In this study we rely on three primary sources of data, which we have matched at the student level: (1) uAspire's student database; (2) FCS administrative records; and (3) college enrollment records for the fall of 2011, spring of 2012, and fall of 2012 from the National Student Clearinghouse (NSC).[17]

In Table 1, we present descriptive statistics for the Boston (uAspire) and FCS samples. In Boston (column 1), students of color comprise more than 90 percent of the sample (32 percent of students are black, 24 percent are Latino, and 20 percent are Asian).[18,19] Nearly 85 percent of the Boston sample completed the Free Application for Federal Student Aid (FAFSA). Of those who did complete the FAFSA, 62 percent had an EFC of zero and another 23 percent had an EFC that was nonzero, but still within the range of Pell-eligibility.[20] Perhaps the most striking demographic feature is that 65 percent of sample students are female. This is not necessarily surprising, however, given a decades-long trend of females enrolling in college at considerably higher rates than males (Goldin, Katz, & Kuziemko, 2006). Of students reporting a specific intended postsecondary institution on the Last Dollar Scholarship application (94 percent of the sample), 85 percent intended to enroll at a four-year institution, and 51 percent intended to enroll at a public institution.

We are also able to observe in the uAspire data the cost of attendance at students' intended college, and for those who provided complete financial aid information, what aid they received. uAspire students faced an average cost of attendance of nearly $30,000, likely reflective of the fact that nearly half of the students intended to enroll at a private institution.[21] Among those who provided complete financial

[17] The NSC is a nonprofit organization that houses student degree and enrollment information for colleges and universities in the United States. At the time of our writing, approximately 94 percent of colleges and universities nationwide participated in the NSC. For more information, see http://www.studentclearinghouse.org.

[18] These percentages correspond roughly to the Boston Public School population as a whole: 87 percent are students of color (Boston Public Schools, 2012).

[19] Seven percent of Last Dollar Scholarship applicants did not report race or ethnicity information.

[20] According to the Federal Pell Grant Program's Payment Schedule for Determining Full-Time Scheduled Awards in the 2011 to 2012 Award Period, students with EFCs no greater than $5,273 qualified for Pell Grant funding. For additional detail, see http://www.ifap.ed.gov/dpcletters/attachments/P1101Attach.pdf.

[21] Total cost of attendance is based on a uAspire calculation that includes institutional costs, such as tuition, fees, housing, and meal plan costs, as well as estimates of non-institutional costs, such as books and the cost of travel, where applicable.

Table 1. Descriptive statistics for the Boston uAspire and Fulton County samples.

	Boston uAspire (1)		Fulton County (2)	
	Mean	*N*	Mean	*N*
Demographic and prior student achievement characteristics				
White	0.09	863	0.39	1,446
Black	0.32	863	0.49	1,446
Hispanic	0.24	863	0.06	1,446
Asian	0.20	863	0.04	1,446
Multiracial	0.10	863	0.02	1,446
Other race/ethnicity	0.05	863	0.00	1,446
Female	0.65	927	0.54	1,446
Completed the FAFSA	0.85	927	0.78	1,446
Zero EFC	0.62	784	–	–
Nonzero EFC in Pell-eligible range	0.23	784	–	–
EFC above Pell-eligible range	0.15	784	–	–
Free/reduced lunch status	–	–	0.37	1,446
11th-grade GA high school graduation test in mathematics	–	–	0.10 (0.93)	1,409
11th-grade GA high school graduation test in language arts	–	–	0.08 (0.94)	1,409
HS GPA	–	–	2.92 (0.54)	1,379
GA HOPE eligible	–	–	0.47	1,379
Characteristics of the college/university where students intend to enroll				
Intend on public institution	0.51	869	0.82	1,271
Intend on four-year institution	0.85	869	0.87	1,446
Total cost of attendance	$29,301 (14,952)	820	–	–
Total loans	$5,326 (2,055)	639	–	–
Total grants	$19,305 (13,512)	639	–	–

Source: uAspire student database and Fulton County Schools administrative and high school exit survey records.

Notes: The uAspire sample comprises summer 2011 Last Dollar Scholarship applicants. For the uAspire sample, FAFSA completion is based on students submitting their student aid report along with their scholarship application. For uAspire, the 49 percent who do not intend to enroll in a public institution intended to enroll in a private institution, and the 15 percent who did not intend to enroll in a four-year institution intended to enroll in a two-year institution. For the Fulton sample, FAFSA completion is based on students' self-report. Among those Fulton students who reported an intended institution, the 18 percent who did not intend to enroll in a public institution intended to enroll in a private institution, and the 13 percent who did not intend to enroll in a four-year institution intended to enroll in a two-year institution. Specific college of intention was missing for 175 students. Therefore, we are missing information on type of postsecondary institution (public vs. private) for 175 students. EFC denotes expected family contribution. For continuous variables, we report standard deviation in parentheses.

aid information, students received an average of $19,305 in grant aid and $5,326 in loans.

The FCS sample (column 2) includes lower proportions of students of color (61 percent). The sample is also more balanced on gender than the Boston sample (54 percent female). Approximately 78 percent of the FCS sample reported completing the FAFSA. While a similar proportion of the FCS sample intended to enroll at four-

year institutions (87 percent), a considerably higher proportion intended to enroll at public institutions (82 percent). Although we lack academic achievement data in Boston, for the FCS sample our data include students' high school GPA as well as math and English language arts (ELA) scaled scores on the Georgia High School Graduation Test (GHSGT), taken at the end of 11th grade. For the class of 2011, GA state law required that students pass the GHSGT examinations to graduate from high school. The FCS students in our sample are relatively high performing; on average, college-intending students in the participating FCS earned a high school GPA of 2.92, and 47 percent of these students met the GPA criteria of 3.0 to be eligible for the HOPE Scholarship. Further, the average student in the sample scored 0.10 standard deviations and 0.08 standard deviations above the district average on the 11th grade state assessments in mathematics and ELA, respectively. Finally, 37 percent of students qualified for FRL.

For at least two reasons, we anticipate measures of SES (FRL status in Fulton and EFC in Boston) to be particularly important covariates in our analyses. First, financial barriers to enrollment are more likely to be a binding constraint for students from low-income families; by helping students acquire additional grant aid, waive costs such as health insurance, or maximize their borrowing from federal and state sources, summer outreach may help students reduce costs to the point where college becomes affordable. Second, it is plausible that students from low-income backgrounds are more likely to be the first in their family to go to college, and therefore more likely to lack access to family resources to help with completing required paperwork and other summer tasks. Offering these students professional guidance may therefore have a larger impact on whether they successfully matriculate in college.

In Table 2, we assess the baseline equivalence of the treatment and control groups for the pooled sample and for the Boston and FCS samples, separately. With a comprehensive set of baseline covariates, testing baseline equivalence for each covariate individually can lead to the detection of significant differences due to an increase in Type I error rates (Hansen & Bowers, 2008). We therefore employ two different methods for assessing baseline equivalence for the full set of baseline covariates simultaneously. First, we utilize a probit model to regress the indicator for treatment on the vector of baseline covariates described in Table 1 together with the appropriate set of fixed effects and indicators for baseline missingness. Here, we focus on the associated Wald (χ^2) statistic for assessing baseline equivalence. Hansen and Bowers (2008) note that with such an approach, a large vector of baseline covariates coupled with moderate sample size can also lead to increased Type I error in the associated omnibus test. Therefore, we additionally utilize the omnibus measure of baseline balance developed and described by Hansen and Bowers (2008) that, based on simulation evidence, does not suffer from this bias even in moderate sample sizes. This approach also utilizes a χ^2 omnibus test. With both approaches and in all samples, we fail to reject the null hypothesis of baseline equivalence. The omnibus statistics from the two different approaches are similar, suggesting that the probit results do not suffer notably from a finite-sample bias. In Appendix Table A1, we present analogous omnibus test results for all subgroups defined by student SES that we consider in subsequent analyses. Across subgroups, we find no evidence of overall baseline imbalance.[22]

[22] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787. Assessing baseline covariates individually, we found no differences in the pooled sample or in the Boston sample. We did detect certain modest differences in the Fulton County

Measures

To evaluate the impact of the interventions on students' on-time college enrollment in the year after high school graduation as well as their subsequent persistence, we focus on the following dichotomous enrollment outcome variables: (1) enrollment in college in the fall immediately following high school graduation, (2) enrollment in college continuously for the fall and spring semesters after high school graduation, and (3) enrollment in college continuously for the first three semesters after high school graduation (i.e., persistence into the sophomore year). We also create dichotomous measures for whether students enrolled and persisted at (1) the specific institution in which they intended to enroll as of high school graduation and (2) the type of institution (i.e., two year vs. four year, public vs. private) in which they intended to enroll as of high school graduation. Outcomes in these latter categories allow us to assess the impact of the intervention on a different dimension of the stability of students' plans: whether the offer of summer counseling increased the probability that students were able to follow through on their specific postsecondary plans from senior year in high school.

The primary predictor of interest for each intervention is *TREATMENT*, an indicator for student assignment to the treatment group or the control group. We additionally include controls for students' gender, race/ethnicity, FAFSA completion status, and intentions to enroll at a two-year institution, a four-year public institution, or a four-year private institution. For students from Boston, we control for a categorical measure of EFC, and for students from FCS, we additionally control for FRL status, high school GPA, eligibility for the GA HOPE scholarship, and 11th-grade math and language arts achievement scores.[23] Our models include fixed effects for advising team in Boston and high school in FCS, as we randomly assigned students within these units.[24]

sample. For instance, treatment group students scored on the order of 0.10 standard deviations higher on standardized assessments of math and language arts. We further probed the detected imbalance and its impact on our results. Specifically, we investigated whether the imbalance in students' test scores was concentrated at particular segments of their respective score distributions by dividing the test score distributions into approximate 5-percentile bins and comparing average scores by regressing test scores within each bin on an indicator for treatment, along with school fixed effects. While parameter estimates associated with the treatment indicator largely fail to achieve statistical significance, the largest test score differences are concentrated in the tails of the test score distributions. Given these patterns of imbalance, we conducted sensitivity checks in which we trimmed the FCS sample by removing students who scored in the top or bottom 5 percent of the test score distributions in either math or language arts. We then reassessed baseline equivalence for the trimmed sample overall and within subgroups defined by FRL status. In the trimmed overall sample, baseline equivalence is attained across covariates with the exception of the indicator of multiracial status. Given the small share of students with this designation, we anticipate that this difference was of little consequence. Further, we examined whether the experimental groups were equivalent at baseline within the subset of students who qualify for FRL—the population of students we anticipate being particularly impacted by the offer of additional outreach. Baseline equivalence holds for all covariates in the trimmed FRL sample, and the magnitude of analytic results (and patterns of statistical significance) do not differ for the Fulton FRL students between the full and trimmed samples. For the non-FRL sample, we find imbalance in the share of treatment students who intend to enroll in a public institution both before and after trimming the sample. The extent of this imbalance does not impact our interpretation of the intervention impacts for non-FRL students. These additional analyses further increase our confidence that our results and conclusions are not threatened by the imbalance that we observed in a few of the baseline covariates when examined individually. In the remainder of the paper, we focus our attention on impact results based on fully controlled models. All results referenced here are available upon request.

[23] We account for missing values of baseline covariates with indicators for missingness in our analyses.

[24] The inclusion of fixed effects allows us to difference out idiosyncratic differences across high schools (Fulton) or advising groups (uAspire) and focus on within-unit differences between students assigned to the treatment group and control group. We additionally utilize procedures for obtaining robust standard

Table 2. Probit regression results predicting treatment assignment to assess baseline equivalence.

	Pooled sample (1)	uAspire (2)	Fulton County (3)
Black	0.081	−0.011	0.162
	(0.097)	(0.115)	(0.121)
Hispanic	0.142	0.089	0.211
	(0.113)	(0.124)	(0.165)
Asian	0.014	–	–
	(0.120)	–	–
Multiracial	0.026	0.114	−0.324
	(0.145)	(0.162)	(0.269)
Other	0.350†	0.333	–
	(0.211)	(0.208)	–
Race/ethnicity missing	0.181	0.174	–
	(0.186)	(0.182)	–
Female	−0.034	−0.045	−0.016
	(0.057)	(0.089)	(0.075)
FAFSA completed	0.003	−0.048	−0.013
	(0.085)	(0.359)	(0.089)
Zero EFC	−0.107	−0.071	–
	(0.175)	(0.358)	–
Pell-eligible EFC	−0.036	−0.002	–
	(0.194)	(0.367)	–
Non-Pell-eligible EFC	−0.176	−0.199	–
	(0.210)	(0.377)	–
Free/reduced lunch	0.023	–	0.016
	(0.092)	–	(0.093)
11th-grade mathematics achievement	0	–	0.001
	(0.002)	–	(0.002)
11th-grade language achievement	0.002	–	0.002
	(0.002)	–	(0.002)
Missing 11th-grade mathematics achievement	−0.373	–	−0.088
	(1.277)	–	(1.301)
Missing 11th-grade language achievement	1.218	–	1.116
	(0.767)	–	(0.771)
HS GPA	0.012	–	−0.024
	(0.136)	–	(0.139)
GA HOPE eligible	0.115	–	0.126
	(0.130)	–	(0.130)
Missing HS GPA	0.14	–	0.104
	(0.365)	–	(0.369)
Intend on two-year institution	−0.358	−0.171	−0.071
	(0.260)	(0.270)	(0.148)
Intend on four-year public	−0.3	−0.449	0.14
	(0.260)	(0.273)	(0.126)
Intend on four-year private	−0.25	−0.581*	0.247†
	(0.265)	(0.291)	(0.147)
Intend on four-year unspecified	−0.405	–	–
	(0.282)	–	–
Cost of attendance	0.00	0.00	–
	(0.000)	(0.000)	–
Missing cost of attendance	−0.197	−0.036	–
	(0.251)	(0.264)	–
Grant aid	0.00	0.00	–
	(0.000)	(0.000)	–

Table 2. Continued.

	Pooled sample (1)	uAspire (2)	Fulton County (3)
Loan aid	0.00	0.00	–
	(0.000)	(0.000)	–
Missing grants and loan aid	−0.469*	−0.608**	–
	(0.215)	(0.223)	–
Intercept	−0.26	0.522	−1.505
	(1.115)	(0.389)	(1.047)
χ^2 omnibus test	26.52	16.95	20.96
(*P*-value)	(0.545)	(0.527)	(0.180)
χ^2 omnibus test, Hansen and Bowers, 2008	25.80	16.70	20.50
(*P*-value)	(0.585)	(0.540)	(0.154)
N	2,373	927	1,446

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database and Fulton County Schools administrative and high school exit survey records.

Notes: Coefficients presented from probit regressions predicting treatment assignment from baseline covariates and fixed effects. For Fulton County, fixed effects pertain to the high school. For Boston uAspire, fixed effects pertain to the advising group. Robust standard errors are shown in parentheses. EFC denotes expected family contribution.

Empirical Strategy

In order to investigate the impact of the treatment offer on the binary outcomes of interest, we utilize probit models and present results both for the cross-site pooled sample and separately for each site. In addition, we examine results separately for Boston by EFC category and for FCS by FRL status, so that we are able to investigate impacts particular to students from lower income backgrounds. With a first set of models, we obtain intent-to-treat (ITT) estimates of the effect of offering summer counseling to students. These models take the following basic form:

$$\Pr\left(Y_{ij} = 1\right) = \Phi(\alpha_j + \beta \times TREATMENT_{ij} + \boldsymbol{X}\boldsymbol{\gamma} + \varepsilon_{ij}), \quad (1)$$

where for student i assigned to advising team j, Y_{ij} represents a dichotomous college enrollment outcome; α_j is a fixed effect for an advising team in Boston or high school in FCS; $\boldsymbol{X}$ includes a vector of student-level covariates, and ε_{ij} is an individual-level error term.[25] Here, the primary coefficient of interest is β, which represents the causal impact of the treatment offer on the outcome of interest. For ease of interpretation, in our results, we present marginal effects of assignment to treatment holding all covariates at the average. This marginal effect corresponds to the predicted change in probability of the outcome of interest (such as fall college

errors in deriving all estimates of treatment impact in order to allow for heteroskedasticity in error distributions across groups. For further discussion, see Wooldridge (2003).

[25] We recognize the potential for bias in statistical estimates that derive from probit models with fixed effects due to the incidental parameter problem. Simulation results by Greene (2004) suggest that with the number of group-level fixed effects included in our model, together with the number of observations per group (ranging from 168 to 377), our results should not suffer from such bias. In addition, we ran all analyses using linear probability models (LPMs) with the same set of fixed effects and covariates as an additional check. In some instances, coefficients were somewhat larger and in other cases somewhat smaller, but both the substantive conclusions and the patterns of statistical significance remained unchanged. These LPM results are available upon request.

matriculation) for the average student assigned to proactive outreach, compared to the average student not assigned to receive outreach. We consider variation in the treatment effect by student SES by fitting model (1) separately by EFC category in Boston and by FRL status in FCS.[26]

Next, we utilize instrumental variables (IV) estimation to assess local average treatment effects for the impact of communicating with a counselor on each enrollment outcome. Because a student's willingness to communicate with a counselor is potentially endogenous to other student characteristics that could determine college enrollment outcomes, we use random assignment to treatment as an IV for whether a student communicated with a counselor (Murnane & Willett, 2011). A student is identified as having communicated with a counselor if he talked with a counselor on the phone or engaged with a counselor via e-mail, text, or Facebook. We fit the following IV probit model:[27]

$$\Pr(COMMUNICATE_{ij} = 1) = \Phi(\alpha_j + \delta \times TREATMENT_{ij} + \boldsymbol{X\gamma} + \varepsilon_{ij}), \tag{2}$$

$$Y_{ij} = \alpha'_j + \lambda \times \widehat{COMMUNICATE}_{ij} + \boldsymbol{X\pi} + \varepsilon_{ij}. \tag{3}$$

In the first stage (equation (2)), we use the treatment indicator to isolate the variation in whether students communicated with a counselor (*COMMUNICATE*) that was exogenously determined by treatment assignment. In the second stage (equation (3)), we use predicted values of *COMMUNICATE* to identify the causal effect of communicating with a counselor on each college enrollment outcome, making the exclusion restriction assumption that the intervention impacted postsecondary enrollment outcomes only for those students induced to communicate with a counselor by virtue of random assignment to treatment. λ represents the causal effect of communicating with a counselor on each enrollment outcome. As the exclusion restriction implies, this effect is identified only for those who would communicate with a counselor as a result of being randomly assigned to the treatment group (i.e., the compliers; Angrist, Imbens, & Rubin, 1996). We include the same set of covariates in both the first and second stages of the model. As with the ITT analyses, the results that we present correspond to the marginal effect of communicating with a counselor or advisor on the outcomes of interest, holding all covariates at their average. In addition, we discuss results for both the pooled and site-specific samples in addition to examining differential impacts of communicating with a counselor by SES.[28]

RESULTS

College-intending high school graduates are responsive to the offer of summer counseling. Among these students, the offer of summer counseling support has a strong,

[26] We prefer fitting separate models over a single model that includes a treatment-by-SES measure interaction based on preliminary analyses, which revealed that parameter estimates associated with baseline covariates also differed significantly by FRL status, for example, such that a fully interacted model would be most appropriate.

[27] In equations (3) and (4), α_j and α'_j are advising team or high school fixed effects. We differentiate notation in the first- and second-stage models, as we expect different coefficients on the fixed effects in the first and second stages.

[28] Similar to the sensitivity checks that we ran on the ITT models, we additionally fit these instrumental variables models using a two-stage least squares approach that assumes linear models in both the first and second stage equations. As with the ITT results, effects of interest and patterns in statistical significance do not change with the model specification. These results are available upon request.

Table 3. Intervention take-up by treatment and control group students.

	Pooled	uAspire	Fulton
Outcome: communicated with an advisor or counselor			
Treatment	0.510***	0.728***	0.344***
	(0.019)	(0.023)	(0.024)
Control group take-up rate	0.012	0.039	0.008
Pseudo-R^2	0.477	0.487	0.406
N	2,373	927	1,446
Outcome: met with an advisor or counselor			
Treatment	–	0.497***	–
		(0.026)	
Control group take-up rate	–	0.022	–
Pseudo-R^2	–	0.339	–
N	–	927	–

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database and Fulton County Schools administrative and high school exit survey records.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Robust standard errors are shown in parentheses.

positive impact on immediate college enrollment for students overall and a particularly pronounced impact for lower income students in both sites. In addition, these impacts persist into the fall of their sophomore year in college.

In Table 3, we present uncontrolled intervention take-up rates for treatment and control group students. The pooled results (column 1) indicate that overall virtually no control group students and just over half of treatment group students communicated with a counselor during the summer months. Among students in the control group in Boston, approximately 4 percent initiated contact with a uAspire advisor, and 2 percent actually met with an advisor (column 2). In contrast, more than 75 percent of students in the treatment group communicated with an advisor. Not all of these students opted to meet with an advisor, and some scheduled meetings but did not show up. Just over half (52 percent) of students in the treatment group had at least one in-depth meeting with a uAspire advisor (bottom panel, column 2), with an average of about 1.5 meetings per student among those who did meet with an advisor.[29] In Boston, students across the EFC categories communicated with counselors at similar rates, but the lower income students were somewhat more likely to take up the opportunity to meet.

In FCS, district counselors reached out to nearly all treatment group students, but a much smaller proportion of students (approximately 35 percent) had any communication with a district counselor (column 3).[30] FCS students from lower income backgrounds were much more likely to have communicated with a counselor: approximately 25 percent of non-FRL students in the FCS treatment group

[29] One possible explanation for the gap between communication with an advisor and actual meetings is that a number of students stated during outreach that they felt confident in their college plans and did not need to meet with an advisor. According to the uAspire advisors, however, this confidence was often unfounded; students who initially said they had everything in order frequently noted substantial barriers to their enrollment in later conversations with advisors.

[30] FCS counselors were able to provide accurate information regarding the students with whom they communicated, but these records did not differentiate which students they actually met with.

Table 4. Impact of the offer of summer counseling on college enrollment and persistence.

	Pooled sample (1)	Pooled sample (2)	uAspire (3)	Fulton (4)
Outcome: immediate enrollment				
Treatment	0.041*	0.033*	0.046†	0.022
	(0.017)	(0.016)	(0.027)	(0.020)
Control group enrollment rate	0.789	0.827	0.784	0.854
Baseline covariates		√	√	√
Pseudo-R^2	0.081	0.218	0.125	0.288
N	2,373	2,373	927	1,446
Outcome: continuous first-year enrollment				
Treatment	0.047*	0.039*	0.065*	0.019
	(0.018)	(0.018)	(0.029)	(0.023)
Control group enrollment rate	0.749	0.785	0.742	0.811
Baseline covariates		√	√	√
Pseudo-R^2	0.079	0.231	0.146	0.88
N	2,373	2,373	927	1,446
Outcome: continuous enrollment into sophomore fall				
Treatment	0.059**	0.050*	0.078**	0.026
	(0.021)	(0.022)	(0.033)	(0.029)
Control group enrollment rate	0.639	0.663	0.638	0.680
Baseline covariates		√	√	√
Pseudo-R^2	0.09	0.231	0.140	0.292
N	2,373	2,373	927	1,446

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Robust standard errors are shown in parentheses. Baseline covariates include all variables represented in Table 1 together with indicators for covariate missingness. Control group enrollment rates reported are predicted rates at average values of all baseline covariates included in the model.

communicated with a counselor, while nearly 54 percent of FRL students did so (Appendix Table A2).[31]

In Table 4, we present ITT effects of the summer intervention on immediate fall enrollment (top panel), continuous first-year enrollment (middle panel), and continuous enrollment into sophomore fall (bottom panel) for the pooled sample (columns 1 and 2) and the sites separately (columns 3 and 4). For the pooled sample, we present the uncontrolled main effect of treatment on enrollment and the effect of treatment after controlling for baseline covariates. For all other samples, we present the main effect of treatment after controlling for baseline covariates.

Controlling for baseline covariates, the offer of summer counseling increased immediate postsecondary enrollment by 3.3 percentage points for students in the treatment group relative to students in the control group (column 2). This represents

[31] Importantly, in both sites, counselors had access to student-level information on these measures of SES. Therefore, where differences in rates of counselor interaction emerge, we are not able to disentangle whether they are the result of variation in student responsiveness or in counselor effort by student SES. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

Table 5. Impact of the offer of summer counseling on college enrollment and persistence, by socioeconomic status.

	uAspire			Fulton	
	Zero EFC, Pell-elig. (1)	Nonzero EFC, Pell-elig. (2)	EFC non-Pell (3)	FRL (4)	Non-FRL (5)
Immediate enrollment	0.123***	0.024	−0.108†	0.085†	−0.002
	(0.035)	(0.063)	(0.064)	(0.048)	(0.019)
	[0.763]	[0.833]	[0.943]	[0.634]	[0.928]
Continuous first-year enrollment	0.139***	0.036	−0.160*	0.030	0.008
	(0.038)	(0.052)	(0.072)	(0.049)	(0.022)
	[0.726]	[0.851]	[0.957]	[0.593]	[0.894]
Continuous enrollment into sophomore fall	0.132**	0.157*	−0.140	0.022	0.025
	(0.044)	(0.071)	(0.097)	(0.049)	(0.029)
	[0.644]	[0.662]	[0.789]	[0.392]	[0.809]
N	487	177	120	536	910

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. Robust standard errors are shown in parentheses, and control group enrollment rates are shown in brackets. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Baseline covariates include all variables represented in Table 1 together with indicators for covariate missingness. Control group enrollment rates reported are predicted rates at average values of all baseline covariates included in the model. EFC denotes expected family contribution.

almost a 20 percent reduction in the rate of summer melt experienced by control group students.[32] The treatment had a similar impact on whether students enrolled continuously throughout their freshman year in college. Controlling for baseline covariates, students targeted for proactive outreach were 3.9 percentage points more likely to be enrolled in college for both the fall and spring semesters immediately after high school graduation. Further, in the bottom panel of Table 4, we observe that the treatment impact persists (and even increases somewhat in magnitude) into the fall semester of sophomore year. Taken together, these results indicate that proactive counselor outreach improved not only immediate postsecondary enrollment, but also college persistence into sophomore year.

These overall effects were driven mainly by stronger overall impacts for the Boston uAspire site (column 3), where students in the treatment group were 4.6 percentage points more likely to enroll immediately (top panel), 6.5 percentage points more likely to remain continuously enrolled through freshman year (middle panel), and nearly 8 percentage points more likely to enroll and persist into sophomore year (bottom panel). By contrast, the overall impact in Fulton was positive in direction, but not significant (column 4).

In both sites, these overall results mask substantial heterogeneity of impacts by SES. In Table 5, we present fully controlled models investigating the impact of the intervention on immediate and continuous enrollment for each of the SES

[32] Control group rates reported are predicted rates at average values of all baseline covariates included in model.

subgroups. In both sites, the offer of summer counseling had the largest impact on the lowest SES students. Outreach improved fall college enrollment by 12.3 percentage points among Boston students with an EFC of zero (column 1). In Boston, students in the treatment group with EFCs of zero were approximately 13 percentage points more likely to remain enrolled through freshman year and into sophomore year than their control group counterparts. The intervention had a positive though statistically insignificant effect on whether Pell-eligible students with an EFC greater than zero enrolled immediately or remained continuously enrolled through freshman year. However, the treatment increased the probability that students remained continuously enrolled into sophomore year by over 10 percentage points. The magnitude of this impact may reflect that these students were better positioned to complete required summer tasks the summer after freshman year than their control group counterparts who did not receive guidance on how to navigate summer requirements.

While lower income students in Boston benefited considerably from summer outreach, students in the treatment who were outside the Pell-eligible range were actually considerably *less* likely to enroll in college than their control group counterparts. While at first glance this result appears confusing, it is consistent with uAspire's focus on affordability. Students outside the Pell awardable range were considerably more likely to intend on a private institution with a higher cost of attendance and, on average, received substantially less grant aid than students with lower EFCs. As we describe earlier, uAspire's practice was often to encourage these students to delay their enrollment until they had additional college savings or to find a more affordable postsecondary plan. During the course of the intervention, uAspire leadership often reminded advisors that "not all melt is bad," if it meant that students were avoiding poor financial decisions. Given this organizational philosophy, it is not surprising that non-Pell-eligible students who received outreach from a uAspire advisor were less likely initially to enroll in college than non-Pell-eligible students who did not receive outreach. On the other hand, when we look at whether students were enrolled in college the third semester after high school (fall 2012), we find that non-Pell-eligible students were enrolled at similar rates regardless of treatment group status. Seventy-eight percent of the non-Pell-eligible students in the treatment group were enrolled in college during the third semester, compared with 80 percent of non-Pell-eligible students in the control group.

In FCS, summer outreach increased immediate fall enrollment by 8.5 percentage points among students qualifying for FRL (column 4). The direction of the treatment effect on whether FRL students remained continuously enrolled was positive, but not significant. We find no impact of the treatment on non-FRL students in Fulton across any of the enrollment outcomes considered. Taken together, these results lend support to our hypothesis that proactive summer outreach should have a more pronounced impact on students from the lowest income backgrounds.

In Table 6, which follows the same structure as Table 4, we examine the impact of the treatment on whether students kept their postsecondary plans from the end of high school. We find among those students for whom specific postsecondary plans were documented, the treatment offer had a significant impact on whether they followed through with their stated plans.[33] Across sites, treatment group students were 4.5 percentage points more likely to enroll in their intended college immediately after high school, 4.3 percentage points more likely to remain enrolled at their intended institution throughout the first year of college, and 5.3 percentage

[33] We ran tests for baseline equivalence (analogous to those presented in Tables 3 and 4) for additional subgroup analyses and found no indication of baseline imbalance for the various subgroups examined.

Table 6. Impact of the offer of summer counseling on whether students enrolled and persisted at their intended postsecondary institution.

	Pooled sample (1)	Pooled sample (2)	uAspire (3)	Fulton (4)
Outcome: immediate enrollment				
Treatment	0.053**	0.045*	0.064*	0.030
	(0.020)	(0.020)	(0.030)	(0.026)
Control group enrollment rate	0.728	0.759	0.726	0.786
Baseline covariates		√	√	√
Pseudo-R^2	0.079	0.187	0.096	0.258
N	2,140	2,140	869	1,271
Outcome: continuous first-year enrollment				
Treatment	0.051*	0.043*	0.067*	0.023
	(0.021)	(0.021)	(0.032)	(0.028)
Control group enrollment rate	0.697	0.727	0.689	0.753
Baseline covariates		√	√	√
Pseudo-R^2	0.073	0.185	0.094	0.252
N	2,140	2,140	869	1,271
Outcome: continuous enrollment into sophomore fall				
Treatment	0.060**	0.053*	0.074*	0.032
	(0.023)	(0.024)	(0.035)	(0.033)
Control group enrollment rate	0.563	0.575	0.554	0.590
Baseline covariates		√	√	√
Pseudo-R^2	0.08	0.171	0.071	0.246
N	2,140	2,140	869	1,271

$^{\dagger}P < 0.10$; $^{*}P < 0.05$; $^{**}P < 0.01$; $^{***}P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Robust standard errors are shown in parentheses. Baseline covariates include all variables represented in Table 1 together with indicators for covariate missingness. Control group enrollment rates reported are predicted rates at average values of all baseline covariates included in the model.

points more likely to remain enrolled into the fall of sophomore year (column 2). As with the enrollment outcomes we presented earlier, the effects of the intervention on whether students kept their postsecondary plan were largest in Boston and positive though not significant in Fulton. In Table 7, we present results of these same analyses by SES subgroup. Here again, initial positive impacts of outreach are isolated to those with a zero EFC; the impacts for those with Pell-eligible EFCs are substantial for all outcomes and statistically significant by the third semester of enrollment. Similarly, the impacts for the Fulton FRL sample are practically although not statistically significant.

In Table 7, we again observe negative impacts for those students with non-Pell-eligible EFCs. While in Table 5 we showed that these students were no less likely to be enrolled during the third semester after high school, they were nearly 14 percentage points less likely to have continued to their originally intended institution. Non-Pell-eligible students in the treatment group were therefore substantially *more* likely than their control group counterparts to opt for a postsecondary institution different from their original intentions. This finding is consistent with the hypothesis that these students may have heeded uAspire's advice to delay their enrollment and select a postsecondary option that represented a better financial choice.

Table 7. Impact of the offer of summer counseling on whether students enrolled and persisted at their intended postsecondary institution, by socioeconomic status.

	uAspire			Fulton	
	Zero EFC, Pell-elig. (1)	Nonzero EFC, Pell-elig. (2)	EFC non-Pell (3)	FRL (4)	Non-FRL (5)
Immediate enrollment in intended institution	0.091*	0.062	−0.104	0.067	0.014
	(0.041)	(0.056)	(0.087)	(0.055)	(0.024)
	[0.721]	[0.824]	[0.863]	[0.502]	[0.880]
Continuous first-year enrollment in intended institution	0.099*	0.098	−0.176†	0.030	0.019
	(0.043)	(0.063)	(0.091)	(0.055)	(0.027)
	[0.685]	[0.762]	[0.871]	[0.479]	[0.852]
Continuous enrollment into sophomore fall in intended institution	0.100*	0.178*	−0.115	0.060	0.008
	(0.048)	(0.079)	(0.103)	(0.050)	(0.036)
	[0.580]	[0.547]	[0.678]	[0.265]	[0.737]
N	467	173	117	434	837

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. Robust standard errors are shown in parentheses, and control group enrollment rates are shown in brackets. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Baseline covariates include all variables represented in Table 1 together with indicators for covariate missingness. Control group enrollment rates reported are predicted rates at average values of all baseline covariates included in the model. EFC denotes expected family contribution.

A final point to note based on the results in Table 7 is the lack of stability in students' enrollment, particularly among the control group. For example, among FRL students in Fulton, only half of those in the control group matriculated to their intended postsecondary institution, and three semesters later, only about one-quarter of students remained enrolled in that same institution.

In additional analyses that we present in Appendix Table A3, we examine the extent to which students are able to follow through on the type of institution in which they originally intended to enroll.[34] We find positive impacts of the intervention on whether students who intended to enroll at four-year institutions actually enrolled at four-year colleges and universities, with the exception of non-Pell-eligible students who were less likely to enroll at four-year institutions. The sample of students intending to enroll at two-year institutions is too small to precisely estimate impacts of the intervention, although the results provide suggestive evidence that the intervention was beneficial to uAspire students and to FRL-qualifying student in FCS in supporting two-year intending students to realize and maintain their stated postsecondary plans.

Finally, in our IV analyses, we use the treatment indicator as an instrument for communicating with a counselor. We present results of these analyses in Table 8 for

[34] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

Table 8. Instrumental variable impacts of communicating with a counselor on college enrollment and persistence.

	Pooled sample (1)	Pooled sample (2)	uAspire (3)	Fulton (4)
Outcome: immediate enrollment				
Communicated	0.0760*	0.053†	0.061†	0.050
	(0.032)	(0.029)	(0.035)	(0.052)
Baseline covariates		√	√	√
N	2,373	2,373	927	1,446
Outcome: continuous first-year enrollment				
Communicated	0.085*	0.061*	0.083*	0.040
	(0.034)	(0.031)	(0.037)	(0.054)
Baseline covariates		√	√	√
N	2,373	2,373	927	1,446
Outcome: continuous enrollment into sophomore fall				
Communicated	0.103***	0.074*	0.094*	0.052
	(0.037)	(0.033)	(0.039)	(0.059)
Baseline covariates		√	√	√
N	2,373	2,373	927	1,446

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from instrumental variable probit regressions with fixed effects, where communicating with a counselor is instrumented for with the indicator for treatment assignment. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Robust standard errors are shown in parentheses. Baseline covariates include indicators for race/ethnicity, gender, free/reduced lunch status, FAFSA completion, EFC category, characteristics of intended college or university, 11th-grade standardized test scores, and indicators for covariate missingness. First-stage impacts of treatment on communication with a counselor are similar to those presented in Table 3 and are available upon request.

the pooled and site-level samples and in Appendix Table A4 for the SES subgroups.[35] We estimate that among students with whom counselors would be able to make contact as a result of proactive outreach, those receiving proactive outreach were 5 percentage points more likely than their control group counterparts to enroll on time, 6 percentage points more likely to enroll for the full year after high school graduation, and 7 percentage points more likely to persist into the third semester of college (Table 8, column 2). As with the ITT estimates, we observe that the IV-estimated impacts are largest among the lowest income students in both samples.

CONCLUSIONS

The results of these experimental studies demonstrate that, for populations of college-intending graduates from public high schools in Boston and Fulton County, GA, proactive outreach during the summer months leads to substantially higher rates of on-time college enrollment. Fall college enrollment rates were more than 3 percentage points higher for students across the sites and 8 to 12 percentage points higher for the lowest income students in each site. The treatment also improved

[35] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

students' rates of continuous enrollment through the fall semester of sophomore year and the extent to which they followed through on and persisted with their postsecondary plans from the end of high school. Specifically, members of the treatment group were 5 percentage points more likely to be continuously enrolled through three semesters of college and, among those whose intentions were documented, over 4 percentage points more likely to enroll at their intended college or university. In short, proactive outreach and the offer of college counseling helped students, particularly those from the lowest income backgrounds, to realize and persist in their college plans.

Our finding that the offer of summer counseling increased the rates at which students remained continuously enrolled into sophomore year is particularly noteworthy. A general concern with college access interventions is that they improve rates of initial enrollment among students who ultimately have a low probability of collegiate success. One could argue that students who struggle to access and complete required paperwork during the summer are unlikely to possess the skills, either academic or problem-solving, necessary to succeed in the classroom and persevere in college. While our conclusions will be strengthened by observing students over a longer time frame, these findings suggest that the challenges students encounter over the summer may not relate directly to their ability to persist in college.

An important question to consider is why we observe substantial variation in the impacts of the interventions across the two sites. One clear difference between the sites is that students in Boston had substantially higher rates of communication with advisors, which may be explained by two primary factors. First, uAspire students had already received individualized help from a uAspire advisor during their senior year of high school, so may have been more comfortable communicating with an advisor over the summer. Given typical student-counselor ratios in public high schools, FCS students may have been comparatively surprised by and potentially reluctant to take up the offer to meet one-on-one with a district counselor. Second, the uAspire advisors were able to offer a $25 gift card incentive for students to respond. The financial benefit of communicating with an advisor may have been a motivating factor for some students in Boston. Yet another possibility, while speculative, is that students were more responsive to uAspire, given that they had applied to uAspire for a college scholarship.

Another reason why the impacts were larger in Boston may be that the uAspire advisors had substantially more experience supporting students with financial aid issues and communicating with colleges than the counselors in Fulton who, like many public school counselors, reported a lack sufficient training in financial aid topics. Financial aid related issues were among the most common on which students sought guidance and support. uAspire advisors reported a number of instances in which they were able to help students successfully appeal for additional grant aid, waive costs (such as health insurance) that the student had not anticipated, or assist students in registering for tuition payment plans. Advisors' ability to lower students' costs may have helped students on the margin of matriculating at their intended institution to enroll.

Finally, the variation in impacts across sites may be explained by the differential distributions of low-income students across the two sites. Though our proxies for low-income status are different across sites, Boston appeared to have a substantially higher share of lower income students than FCS. Since these students appear to benefit most from summer outreach, it makes sense that the impacts were larger in Boston than in Fulton.

Also noteworthy are the negative impacts that we observe for the Boston students with EFCs above the Pell-awardable range. These students faced particularly high levels of unmet financial need. Nevertheless, by the following fall, these students

were just as likely to be enrolled, although less likely to have matriculated to their originally intended institution. These patterns reflect uAspire's targeted focus on college affordability and philosophical stance that some summer melt may be a good thing if it means that students are taking the time to identify and organize postsecondary plans that are more financially viable. Of course, another important implication of this finding is that students would likely benefit substantially from additional information and counseling when applying to college so that at the time of high school graduation, their intended college represents a financially sustainable option.

To further investigate the mechanisms by which the offer of summer college counseling may have impacted students' enrollment decisions, Arnold et al. (2013) conducted focus groups and interviews with financial aid advisors and students in the Boston site. Several themes have emerged from these analyses. First, results suggest that advisors were able to help some students reduce college costs to the point that they could afford to enroll. Counselors helped students qualify for aid if they had not already, waive charges where possible, setup tuition payment plans to spread out costs over the entirety of freshman year, and, in some cases, select a more affordable institution to attend. Helping students access information appears to have been another important element of the summer support. A number of counselors identified helping students access and navigate their school's online web portal as a key part of their work over the summer. Counselors also reported spending considerable time addressing gaps in students' college literacy. For instance, a number of students thought their tuition bill was for the entire year, when it was often for the first semester only. Finally, an important part of the summer work seems to be providing students nudges to complete required tasks. As one counselor said, they were essentially filling in for what middle-class parents would do with their own children. This observation aligns with recent work in behavioral economics, which documents that prompts and reminders can have positive impacts on desired outcomes, such as whether individuals complete important medical procedures or contribute to financial savings accounts (Karlan et al., 2010; Milkman et al., 2012).

To assess the cost-effectiveness of summer outreach, we compare the intervention's impact on immediate fall enrollment to what it would cost to obtain the same impact by giving students additional grant aid. One important difference is that the per-student costs of summer intervention are paid regardless of whether students enroll, whereas grant aid is dispersed only if students enroll. Nonetheless, consider that a variety of studies have found that $1,000 in grant aid increases enrollment by anywhere from 3 to 6 percentage points, depending on the structure and target population of the grant program (Deming & Dynarski, 2009). We will assume that offering students in our sample an additional $1,000 in aid would have an impact at the high end of this range; Last Dollar Scholarship applicants' college plans are plausibly more elastic to reductions in college costs than the average high school senior who is eligible for grant aid.[36] The cost of increasing enrollment by 3.3 percentage points in the pooled sample was approximately $138,000. Given a (covariate-controlled) enrollment rate in the control group of approximately 82 percent, increasing enrollment by the same margin using grant aid would cost approximately $384,450,[37] or more than 2.5 times the cost of the summer outreach. Given the even larger treatment effects among the lowest income students in the two samples, proactive

[36] We are grateful to Raj Chetty for making this point.

[37] To arrive at this number, we multiplied the cost of increasing enrollment by 3.3 percentage points ((3.3/6) × 1,000) by the number of students in the treatment group who enrolled in college (699). This calculation assumes that the increase in the probability of on-time enrollment is linear in the size of the grant.

outreach and summer counseling may be a particularly cost-effective strategy for increasing enrollment rates among low-income, college-intending students.

Data from the Boston and Fulton County interventions as well as the Providence pilot (Castleman, Arnold, & Wartman, 2012) indicate that offering summer support is a cost-effective approach to meaningfully increasing college enrollment among low-income students. Nevertheless, there are several outstanding questions regarding how summer outreach and counseling could be conducted most efficiently and effectively. Given that many of the barriers students face are informational in nature, we currently are investigating the impact of a summer text messaging campaign on students' postsecondary outcomes. Capitalizing on cell phone and intended institution information gathered in high school exit surveys, we generated a set of text messages customized to each student's intended institution, reminding them of key summer tasks to complete. The messages are timed for delivery just before each task needs to be completed, and each message offers students the option of responding via text message to request help from a school counselor or other college advisor. The marginal cost of sending these text messages to a student is approximately $0.10 total (in addition to any cost incurred by the student by receiving the messages), so if the intervention has any discernible impact on enrollment, it would be both extremely cost-effective and very easily scalable.

In closing, a growing body of research indicates that college-intending, low-income high school graduates face a host of informational, financial, and other barriers to enrollment that may prevent them from successfully matriculating. Encouragingly, results presented here illustrate that students' postsecondary plans are highly responsive to the offer of support and guidance during the summer months. At a time when the private and social returns to a college education are particularly high, yet local, state and federal budgets are especially lean, our study suggests that summer counseling is a cost-effective approach to increasing college access among low-income students who aspire to further their education.

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APPENDIX

Table A1. Omnibus tests of baseline covariate equivalence in SES subgroups.

	Probit regression χ^2 omnibus test statistic (*P*-value) (1)	Hansen and Bowers χ^2 omnibus test statistic (*P*-value) (2)	*N*
Boston uAspire			
Zero EFC	21.7†	21.8	487
	(0.085)	(0.114)	
Pell-eligible EFC	14.0	15.2	177
	(0.450)	(0.440)	
Non-Pell-eligible EFC	15.9	20.1	120
	(0.318)	(0.167)	
Fulton County			
FRL	10.3	10.2	536
	(0.802)	(0.855)	
Non-FRL	14.8	16.3	910
	(0.394)	(0.435)	

$^{\dagger}P < 0.10$; $^{*}P < 0.05$; $^{**}P < 0.01$; $^{***}P < 0.001$.

Source: uAspire student database and Fulton County Schools administrative and high school exit survey records.

Notes: Results presented in column 1 are from a probit regression of treatment assignment on a vector of baseline covariates analogous to those in Table 2 and fixed effects. For Boston uAspire, fixed effects pertain to the advising group. Robust standard errors are shown in parentheses. Results in column 2 are based on Hansen and Bowers' omnibus test for assessing baseline equivalence.

Table A2. Intervention take-up by treatment and control group students in SES subgroups.

	uAspire			Fulton	
	Zero EFC (1)	Nonzero EFC, Pell-elig. (2)	EFC non-Pell-elig. (3)	FRL (4)	Non-FRL (5)
Outcome: communicated with an advisor or counselor					
Treatment	0.752***	0.765***	0.761***	0.522***	0.241***
	(0.031)	(0.049)	(0.078)	(0.036)	(0.029)
Control group take-up rate	0.042	0.039	0.029	0.013	0.006
Pseudo-R^2	0.506	0.512	0.510	0.412	0.389
Outcome: met an advisor or counselor					
Treatment	0.537***	0.539***	0.478***	–	–
	(0.036)	(0.059)	(0.080)		
Control group take-up rate	0.024	0.041	0.003	–	–
Pseudo-R^2	0.365	0.324	0.349	–	–
N	487	177	120	–	–

$^{\dagger}P < 0.10$; $^{*}P < 0.05$; $^{**}P < 0.01$; $^{***}P < 0.001$.

Source: uAspire student database and Fulton County Schools administrative and high school exit survey records.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. For Boston uAspire, fixed effects pertain to the advising group. Robust standard errors are shown in parentheses. EFC denotes expected family contribution.

Table A3. Impact of the offer of summer counseling on whether students enrolled at their intended type of postsecondary institution.

		uAspire				Fulton		
	Pooled sample (1)	Full sample (2)	Zero EFC, Pell-elig. (3)	Nonzero EFC, Pell-elig. (4)	EFC non-Pell (5)	Full sample (6)	FRL (7)	Non-FRL (8)
Four-year intending students								
Fall enrollment in four-year institution	0.029†	0.036	0.079*	0.012	−0.154†	0.024	0.082	0.004
	(0.017)	(0.028)	(0.038)	(0.017)	(0.085)	(0.022)	(0.055)	(0.021)
Continuous first-year enrollment in four-year institution	0.032†	0.046	0.099*	0.066	−0.217*	0.022	0.052	0.009
	(0.019)	(0.030)	(0.040)	(0.060)	(0.093)	(0.025)	(0.056)	(0.023)
Continuous enrollment in first three semesters in four-year institution	0.059**	0.086**	0.128**	0.173*	−0.081	0.036	0.043	0.029
	(0.023)	(0.035)	(0.045)	(0.076)	(0.086)	(0.032)	(0.055)	(0.032)
N	1,985	739	404	163	172	1,246	410	836
Two-year intending students								
Fall enrollment in two-year institution	0.068	0.124	–	–	–	0.033	0.123	−0.073
	(0.068)	(0.087)				(0.083)	(0.116)	(0.146)
Continuous first-year enrollment in two-year institution	0.041	0.126	–	–	–	−0.003	0.124	−0.012
	(0.067)	(0.099)				(0.078)	(0.109)	(0.152)
Continuous enrollment in first three semesters in two-year institution	0.014	0.021	–	–	–	0.026	0.050	−0.111
	(0.033)	(0.026)				(0.063)	(0.082)	(0.117)
N	325	125	–	–	–	200	125	74

$^{\dagger}P < 0.10$; $^{*}P < 0.05$; $^{**}P < 0.01$; $^{***}P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from probit regressions with fixed effects. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Robust standard errors are shown in parentheses. Baseline covariates include all variables represented in Table 1 together with indicators for covariate missingness. Two-year results disaggregated by EFC yields sample sizes too small for estimating subgroup regressions. EFC denotes expected family contribution.

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Table A4. Instrumental variables impacts of communicating with a counselor on college enrollment and persistence, by socioeconomic status.

	uAspire			Fulton	
	Zero EFC, Pell-elig. (1)	Nonzero EFC, Pell-elig. (2)	EFC non-Pell-elig. (3)	FRL (5)	Non-FRL (6)
Outcome: immediate enrollment					
Communicated	0.161***	0.033	−0.088	0.118†	−0.015
	(0.048)	(0.086)	(0.102)	(0.066)	(0.078)
Outcome: continuous first-year enrollment					
Communicated	0.180***	0.050	−0.147	0.060	0.027
	(0.049)	(0.074)	(0.103)	(0.069)	(0.085)
Outcome: continuous enrollment into sophomore fall					
Communicated	0.160**	0.185**	−0.115	0.031	0.079
	(0.052)	(0.085)	(0.120)	(0.071)	(0.098)
N	487	177	120	536	910

†$P < 0.10$; *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Source: uAspire student database, Fulton County Schools administrative and high school exit survey records, and National Student Clearinghouse.

Notes: Coefficients presented are marginal effects from instrumental variable probit regressions with fixed effects, where communicating with a counselor is instrumented for with the indicator for treatment assignment. For Boston uAspire, fixed effects pertain to the advising group. For Fulton County, fixed effects pertain to the high school. Robust standard errors are shown in parentheses. Baseline covariates include all variables represented in Table 1 together with indicators for covariate missingness. First-stage impacts of treatment on communication with a counselor are similar to those presented in Table A2 and are available upon request. EFC denotes expected family contribution.