Finishing the Last Lap: Experimental Evidence on Strategies to Increase Attainment for Students Near College Completion

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ABSTRACT

Nearly half of students who enter college do not graduate. The majority of efforts to increase college completion have focused on supporting students before or soon after they enter college, yet many students drop out after making significant progress towards their degree. In this paper, we report results from a multi-year, large-scale experimental intervention conducted across five states and 20 broad-access, public colleges and universities to support students who are late in their college career but still at risk of not graduating. The intervention provided these "near-completer" students with personalized text messages that encouraged them to connect with campus-based academic and financial resources, reminded them of upcoming and important deadlines, and invited them to engage (via text) with campus-based advisors. We find little evidence that the message campaign affected academic performance or attainment in either the full sample or within individual higher education systems or student subgroups. The findings suggest low-cost nudge interventions may be insufficient for addressing barriers to completion among students who have made considerable academic progress.

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1. Introduction

College enrollment rates have increased steadily over the last several decades, yet the probability of degree attainment among enrollees has stagnated. Just over half of students who start college complete within six years of entry (Bound, Lovenheim, & Turner, 2010; Shapiro et al., 2016). Low-income students and students of color are significantly less likely to graduate than their high-income and white peers; these disparities have only widened over time (Bailey & Dynarski, 2011; Chetty, Friedman, Saez, Turner, & Yagan, 2020).

To date, most efforts to increase college completion rates have focused on supporting students before or soon after they enter college. For example, several interventions have focused on encouraging students to attend higher-quality colleges from which they are more likely to graduate, supporting students to apply for federal student aid, and helping students overcome procedural obstacles to matriculation that arise before students arrive on campus (Barr & Castleman, 2021; Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012; Castleman & Page, 2015; Hoxby & Turner, 2013). Colleges and universities have also devoted considerable attention to students' first-year experiences in colleges, with interventions ranging from structured learning and advising supports (e.g. CUNY ASAP), learning communities, and first-year seminars, to improving remediation policies for students who enter college academically underprepared (Bettinger & Long, 2009; Culver & Bowman, 2020; Martorell & McFarlin, 2011; Schnell & Doetkott, 2003; Scott-Clayton, Crosta, & Belfield, 2014; Scrivener and Weiss, 2014; Visher, Weiss, Weissman, Rudd, & Wathington, 2012).

Evidence suggests that these strategies can increase the share of students that successfully navigate the transition to college and make progress towards their degree. However, many students who persist beyond the first year of college remain at substantial risk of withdrawing prior to earning their degree. More than 40 percent of college students who do not graduate leave after their second year of college (Bowen, Chingos, & McPherson, 2009; Shapiro et al., 2014). Recent evidence also suggests that one in three dropouts complete at least three-quarters of the credits typically required to graduate before they withdraw (Mabel & Britton, 2018). Across the country this translates into approximately 400,000 students per college entry cohort who have earned substantial credits but do not have a degree to show for it.[1]

A combination of limited support for more advanced students and novel challenges that arise as students approach completion contribute to these high rates of late withdrawal. The road to completion becomes increasingly self-directed as structured student support services taper off after the first year of college (Scott-Clayton, 2015). Students may therefore struggle to make and follow through on complicated decisions, such as determining which courses to take to fulfill their degree requirements, when academic advising is limited and often difficult to access. The non-monetary costs of navigating a challenging environment alone may also be difficult for older students who lead busy lives and have limited networks of academic support outside of school.

In this paper, we present experimental evidence from a large-scale intervention, called Nudges to the Finish Line (N2FL), that we designed in close partnership with 20 colleges and universities to increase completion among students who had made significant progress towards their degree and were still actively enrolled in college.[2] We implemented N2FL in partnership with public higher education institutions in New York City, Virginia, Texas, Ohio, and Washington

[1] These estimates are based on results from Mabel and Britton (2018), who find that 14 percent of all degree-seeking students attending public colleges in Florida and Ohio completed three-quarters of the credits typically required for graduation but did not earn an associates or bachelor's degree. On average those students enrolled in college for 3.2 years and paid $11,500 per year in out-of-pocket expenses (Horn & Paslov, 2014). Nationwide, state appropriations and grants also subsidize the cost of attending public colleges and universities by $10,000 per year on average (Schneider, 2010). Of the 15.5 million students enrolled in degree-seeking programs in the United States, this equates to approximately 2.2 million students who have earned substantial credits but no degree with substantial costs to individuals and to taxpayers.

[2] Students were eligible to participate if they had completed at least half of the credits typically required for associate or bachelor's degree attainment at two- and four-year colleges, respectively.

State during the 2016-17 through 2018-19 school years. We designed N2FL as a text message campaign that: (1) encouraged students to connect with campus-based academic and financial resources; (2) reminded them of upcoming and important deadlines; and (3) invited students to engage via text with dedicated college advising staff.[3] Students received approximately one message per week over the course of 2-3 semesters. The study sample includes 21,533 students across the 20 partner institutions.

Several recent papers have found null impacts from large-scale nudge campaigns that aimed to improve postsecondary outcomes (Avery et al., 2020; Bird et al., 2021; Gurantz et al., 2021; Page et al., 2019). The design of N2FL differed in important ways from these studies, which led us and partners to believe the intervention could effectively support higher rates of degree attainment among students with substantial credits. First, in order to foster trust and perceived legitimacy among students, we designed the campaign so that all messages were delivered by a specific advisor at students' college or university.[4] Second, the messages actively encouraged personal engagement and interaction (via text) between students and advisors; earlier text-based nudge campaigns that found positive impacts on students incorporated this interactive feature (Castleman and Page, 2015; Castleman and Page, 2016; Oreopoulos and Petronijevic, 2019). Third, by virtue of advanced students having less access to support than high school students or students early in college, we expected N2FL outreach to provide a more pronounced treatment contrast. Finally, to ensure the content was relevant to students at each college or university, we worked closely with advising staff at each institution to customize the message content, sequencing, and frequency of outreach to their institutional context.

[3] Students did not have access to this type of text-based advising at two of the 20 institutions.

[4] This stands in contrast to recent ineffective nudge campaigns where messages came from a state or national organization with whom students had at best a tenuous relationship.

That being said, most of the prior applications of nudges in postsecondary education have focused on encouraging students to complete discrete and consequential tasks, such as applying for financial aid. The efficacy of N2FL's nudges depended on students engaging in more sustained behavior change, such as meeting regularly with an advisor or taking advantage of course tutoring services. More recent studies have argued that nudges may be less effective when they are focused on promoting these types of ongoing behaviors (Oreopoulos and Petronijevic, 2019; Page, Lee, and Gelbach, 2020).

Results from a multi-cohort randomized trial of N2FL suggest that text-based nudges are not effective at addressing the barriers to completion experienced by students who have made substantial progress towards a degree. We find little evidence of effects on academic performance or attainment in the full sample and across colleges. Our statistical power is such that we can reject effects of 1.8 percentage points or larger on the probability of re-enrollment or graduation. We also find no evidence of varying impacts of the N2FL nudges based on students' baseline predicted probability of dropout prior to earning a degree. We analyze numerous dimensions of treatment fidelity and the institutional context to investigate why N2FL may not have been effective. For instance, we explore whether impacts vary based on the rate at which college advisors responded to students' texts. We also investigate whether N2FL was differentially effective based on whether the college had parallel texting campaigns. None of these analyses reveals institutional contexts, advisor practices, or other dimensions of project implementation that are associated with heterogeneous treatment impacts.

Our paper makes several important contributions. Ours is the first paper of which we are aware to investigate whether interactive, text-based nudges can improve attainment among students who have made substantial progress towards a degree and who are still in college. Several interventions have attempted to increase re-enrollment and success among students with substantial credits who had already withdrawn, with limited efficacy (Adelman, 2013; Ortagus, Tanner, & McFarlin, 2021).[5] Second, our paper shows that the limited efficacy of nudges in postsecondary education is not a function of the level of implementation or the lack of access to text-based advising, as prior papers have hypothesized (e.g. Bird et al., 2021). We find null impacts even though the nudges were sent by colleges and universities with whom students had a direct connection and invited students to connect with college advisors via text. Finally, by leveraging detailed data on the institutional context in which N2FL took place and data on treatment implementation and fidelity, we are able to investigate more deeply than prior papers factors that could contribute to the efficacy of nudge interventions in higher education.

The remainder of this paper is structured as follows. In Section 2, we provide a brief discussion of the obstacles to completion that disadvantaged populations face at broad access institutions and elaborate on which barriers the N2FL intervention is designed to address. In Section 3, we present details on the research design, including the participating schools, intervention components, study sample, randomization procedure, and empirical strategy. We present results in Section 4 and conclude in Section 5 with a discussion of our findings and their implications.

2. Obstacles to College Completion

A large body of evidence suggests that the costs to completing college are steep and may increase as students progress through school. Many students experience high time and effort costs to completion because they enter college academically unprepared (Bettinger, Boatman, & Long,

[5] For example, through Project Win-Win, a partnership between the Institute for Higher Education Policy and the State Higher Education Executive Officers, sixty postsecondary institutions attempted to re-engage former college-goers requiring 9 or fewer credits to earn an associate degree (IHEP, 2013).

2013). Resource constraints at broad-access public institutions in the United States, where the majority of postsecondary students attend, have escalated those costs by creating a shortage of student supports at many institutions (Bound et al., 2010; Deming & Walters, 2017).

Resource deficiencies are an especially large impediment to student progress because the college environment at most broad-access institutions is complicated and difficult to navigate. For example, the volume of courses offered at open-enrollment institutions and the array of program requirements make it hard for students to know which courses to take in a given term to make efficient academic progress (Nodine, Jaeger, Venezia, & Bracco, 2012; Schneider & Yin, 2011). With student-to-counselor ratios frequently exceeding 1,000:1, advising is also extremely limited, and institutional bureaucracies make it hard for students to access individualized assistance (Grubb, 2006; Scott-Clayton, 2015). According to survey research, one-third of community college students never use academic advising as a result, even though nearly half of students do not understand their graduation requirements or what courses count towards their degree (Center for Community College Student Engagement, 2015; Rosenbaum, Deil-Amen, & Person, 2006).

Within this isolated and confusing landscape, several studies find large effects from interventions that provide students entering college with enhanced mentoring, tutoring, and other supports (Angrist et al., 2009; Bettinger & Baker, 2014; Castleman & Page, 2016; Clotfelter, Hemelt, & Ladd, 2016; Scrivener et al., 2015). However, because these supports are costly, institutions typically target resources to first-year students and the impacts of early interventions fade out over time (Rutschow, Cullinan, & Welbeck, 2012; Visher, Weiss, Weissman, Rudd, & Wathington, 2012). Completing complex tasks may therefore remain a formidable barrier for students as they continue to progress in school.

Furthermore, as students age and take on more responsibilities outside of school (Erisman & Steele, 2015; U.S. Department of Education, 2017), the attention to devote to difficult tasks may become increasingly limited and lead to more frequent oversight of important deadlines and higher psychic costs (e.g., mounting stress, anxiety, and impatience) when obstacles arise. All of these factors may contribute to short-sighted perceptions that the immediate costs to continuation exceed the unrealized future benefits of earning a degree (Cadena & Keys, 2015; Gurantz, 2015).[6] These factors also suggest that targeted interventions may be a cost-effective investment towards increasing degree attainment for students on the margin of completing college. On the other hand, if the costs to completion for late-stage students are primarily due to other factors, such as academic skill deficiencies that make it difficult for students to pass specific course requirements in their major, then nudge interventions may have little impact on academic progress and motivate the need for more resource-intensive strategies to lower rates of late departure.

3. Research Design

We partnered with a diverse array of broad-access, public two- and four-year institutions across the country to implement N2FL. All our partner institutions accept 75 percent or more of the applicants that apply. Sixty percent of students attending our partner institutions enrolled part-time, 32 percent received federal Pell Grants, and 50 percent were students of color. The average graduation rate within 150 percent of the expected time (e.g. 6 years for a 4-year degree) reported by our partner institutions was 29 percent. Of the 20 institutions that participated in N2FL, three are community colleges and three are four-year colleges in the City University of New York system; seven are community colleges in the Virginia Community College System; three are

[6] To inform our intervention design, Persistence Plus also conducted student focus groups at each institution participating in the pilot year during spring and summer 2016. The most common challenges students identified in those sessions were not knowing what steps to take to graduate and where to turn for help on campus when challenges arose.

community colleges in Texas; two are four-year public universities in the University of Texas system; and two are four-year public institutions in Ohio and Washington State. We pre-registered our evaluation of N2FL at the Open Science Framework.[7]

3.1. Eligibility Criteria and Sample

Degree-seeking students were eligible to participate in the study if they: 1) were actively enrolled, 2) had an active cell phone number on record with their institution, and 3) completed at least 50 percent of the credits typically required for degree completion prior to intervention launch.[8] We established broad eligibility criteria to examine heterogeneity in treatment effects by predicted risk of dropout.

Based on the eligibility criteria above and the size of enrollments at our partner institutions, we recruited 21,533 students to participate in the study. Of this experimental sample, we randomly assigned 13,826 to the treatment group and 7,727 to the control group. Students assigned to the control condition did not receive any text messages as part of the intervention but maintained access to the support structures typically available on their campus. However, as discussed above, outreach to students, especially upper-division students, is limited at many public colleges and universities. Therefore, the relevant counterfactual is that control group students did not receive personalized support unless they had the time, motivation, and awareness to seek it out.

In columns 2-3 of Table 1, we present summary statistics by treatment status for the students in the analytic sample. To examine the extent to which the sample reflects the population of undergraduates attending broad-access, public institutions nationally, we report (in column 1)

[7] The pre-registration for the study is available here: https://osf.io/xas3t/.

[8] At two-year institutions, students in pursuit of associate degrees who had completed 30 or more college-level credits were eligible to participate. At four-year institutions, bachelor's degree-seeking students who had completed 60 or more college-level credits were eligible for the study. In practice, many students in the study sample were potentially closer to degree completion prior to intervention launch. One-third of students had completed at least 75 percent of the credits typically required for degree completion before outreach began.

analogous statistics for a nationally representative sample using data from the National Postsecondary Student Aid Study of 2012 (NPSAS:12). Finally, we report (in column 4) balance between the treatment and control experimental conditions.

Across both treatment and control groups, approximately 43 percent of students in the study sample were male, 50 percent were students of color, and the average age of students at the start of the intervention was 21.5 years. Approximately half of our experimental sample attended two-year institutions and half attended four-year institutions. Students had earned an average of 65 college-level credits and completed 91 percent of the credits they had attempted prior to the start of the intervention. Students in the study sample on average had a 30 percent chance of dropping out prior to earning their degree based on the predictive models we developed using historical data from partner institutions (see Appendix 2 for more details on these models).

Our experimental sample is fairly representative of the national student population attending broad-access public institutions with respect to sex (43 percent male versus 44 percent) and racial/ethnic composition (50 percent in both samples). Based on our institutional recruitment strategy, students attending four-year institutions are overrepresented in our sample (48 percent versus 22 percent nationally). As a result, on average the students in our study are slightly younger than the typical enrollee at public broad-access institutions (21.6 years versus 27.1 years).

3.2. Intervention Design

N2FL consisted of a pilot phase (2016-2017 academic year) and a subsequent scale phase (2017-2018 and 2018-2019 academic years). Across both phases, 21 institutions participated in the study. Nine institutions participated in the pilot phase, eight of which also participated in the scale phase. We recruited an additional 12 institutions, for a total of 20, to participate during the scale phase. With the exception of one institution that only participated in the pilot phase, we estimate intervention impacts off a sample that includes participants in the pilot phase, scale phase, or both.[9] All nine pilot institutions used a text messaging model and platform called Persistence Plus, whereby both the automated messages and follow-up responses (to students who wrote back) were automated and personalized to students' use of keywords in their response.[10] Two of the pilot institutions, Ohio University and University of Washington-Tacoma, continued to use Persistence Plus during the scale phase.

The other eighteen scale phase institutions adopted an interactive two-way text messaging campaign that actively promoted opportunities for students to connect with advisors at their campus directly via text. Eligible students who were randomly assigned to treatment received approximately one pre-scheduled text message each week over the course of 2-3 semesters, depending on the institutional partner. These messages were sent automatically by the text messaging vendor Signal Vine according to a predetermined content schedule and delivery timeline that we developed collaboratively with advisors at each partner institution. We provide a sample of message content in Appendix 1.

We present in Appendix Table A1 the start and end dates of messaging, the number of terms over which students were messaged, and student and advisor engagement statistics for each of our twenty scale phase institutions. Messaging start and end dates depended on each institution's preference. Most institutions began messaging students during the 2018 calendar year. Students at

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[9] We exclude 500 students at one institution that only participated in the pilot phase because we observe a large initial enrollment difference between treated and control students at that campus. During the pilot phase, we randomized students in late summer before fall enrollments finalized and message outreach began after classes started. The imbalance therefore occurred due to the timing of randomization, not as a result of message outreach, and would likely bias estimates of intervention impacts.

[10] For example, during the spring term students who reported uncertainty about their remaining math requirements received the following messaging: "Last semester you were unsure whether you had any math requirements left to graduate. Were you able to get that sorted out?". Students who replied "Yes" then received the following response: "Fantastic! If you're currently taking any math courses remember that you can always visit the Math Lab in [on-campus location] for free tutoring."

most institutions received automated messages for 2-3 semesters, depending on each institution's preference.[11]

The topics and frequency of scheduled messages stayed fairly consistent across institutions. Messages were sent approximately once per week and prompted students to complete important tasks (e.g., register for the next semester's courses), encouraged them to use campus resources (e.g., tutoring centers, financial aid office), and addressed feelings of stress and anxiety (e.g., financial hardships). We worked with each partner institution to tailor pre-scheduled message content to their institutional context, such as inserting the name of campus-specific tutoring centers or modifying the tone to fit their student population.

The messages leveraged several key behavioral insights: (1) *Increase informational salience:* To simplify the process of accessing on-campus resources, one set of messages encouraged students to connect with campus-based academic and financial resources and provided them with specific contact and location information where assistance was available.[12] (2) *Promote implementation intentions*: A second set of messages reminded students of upcoming deadlines and encouraged them to make implementation plans that increase the likelihood of task completion (Milkman, Beshears, Choi, Laibson, & Madrian, 2011; Nickerson & Rogers, 2010).[13] (3) *Set positive social norms:* A third set of messages amplified descriptive informational norms to motivate action (Cialdini, 2016; McDonald & Crandall, 2015).[14]

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[11] Pilot phase institutions had six semesters of messaging: two semesters during the pilot phase and four semesters during the scale phase.
[12] For example, the following message encouraged students to use tutoring resources: "Hi [student name], using the [name of campus tutoring center] can help you do well on midterms & boost your grades. Can I help connect you?".
[13] For example: "Hi [student name], Summer and Fall 2019 registration opens today. Don't miss your chance to secure a seat in the courses you need to graduate. What day do you plan to register?"
[14] For example: "Hi! Did you know 580,000+ New Yorkers filed FAFSA by this day last year? Join your peers and visit FAFSA.gov now to get the most aid." Some messages moreover embedded infographics to reinforce the call to action and increase the salience of relevant information.

One distinguishing feature of the N2FL intervention was the ability for students to write back to the scheduled messages with any questions or requests for help and get connected with campus advisors via text. Indeed, most scheduled messages encouraged students to text back by posing a final question designed to encourage student response and engagement. Each partner institution identified a specific advisor or staff team to monitor the text messaging inbox for student replies and to respond to students' questions or requests for assistance.

Staffing models varied across institutions: Some institutions elected to use professional or faculty advisors, while others appointed general staff (e.g., administrative assistants) to staff the messaging inbox and reply to students who texted in. The language of scheduled messages was modified to match the nature and scope of each designated staff's role. Specifically, institutions whose professional advisors had the capacity to support students directly (e.g., choosing which courses to register for or filling out the FAFSA) sent automated messages that offered direct assistance. In contrast, institutions that used a general administrative assistant offered assistance with connecting with the appropriate resources. A summary of the four primary staffing models that emerged can be found in Appendix Table A2.

As we also show in Figure 1, message engagement rates varied substantially among both students and advisors across institutions. Student response rates were generally high across institutions, with approximately half or more of students responding at all institutions. That being said, institutional-level student response rates ranged from a low of 44 percent at Blinn College (Texas) to a high of 78 percent at Lehman College (CUNY). The average institution-level student response rate was 58 percent. Advisor response rates (to messages sent by students) also tended to be quite high, though there was more heterogeneity in institution-level advisor response rates than for student response rates: advisor response rates ranged from as low as 33 percent at

Kingsborough Community College (CUNY) to a high of 88 percent at Thomas Nelson Community College (VCCS). Because higher or lower advisor response rates affected a key design component of the intervention (the availability of advising via text), we investigate whether this feature of institutional heterogeneity was correlated with N2FL efficacy.

3.3. Data and Measures

The data for this study consists of student-level administrative records maintained and provided by our institutional partners for both study participants and previous cohorts of students. The specific data elements vary across schools due to availability, but in general we observe baseline demographic and academic measures (e.g., gender, race, high school GPA and college entrance exams, etc.) and term-by-term records of students' financial aid receipt, enrollment intensity (e.g., credits attempted), academic performance (e.g., credits completed, term and cumulative GPA, etc.), and degree receipt. Most of our partner institutions also routinely collect enrollment and degree information from the National Student Clearinghouse (NSC) on previously enrolled students. We also relied on NSC data when it was available to capture transfer, enrollment, and degree information at non-participating institutions.

We use these data in three ways. First, we used the historical data provided by each institution to develop school-specific dropout prediction models. We present details about the model construction process in Appendix 2 and report descriptive statistics of the study sample by tercile of predicted dropout risk in Appendix Table A3. Second, we use the data to assess whether students randomly assigned to the treatment and control conditions appear to be equivalent in expectation on observable and unobservable dimensions. Third, we use the data to evaluate the impact of the intervention on students' academic progress spanning different time horizons. In our main tables we focus on impacts within four terms of the start of the intervention--the longest time horizon we can observe for all institutions.[15] In appendices we report impacts of the intervention for the subset of earlier-participating interventions for which we can observe outcomes six terms after the start of the intervention. We report on four primary outcome measures over these time horizons: whether students re-enrolled or graduated, the cumulative number of credits earned following intervention, whether students graduated, and for students attending community colleges, whether they transferred to a four-year institution.

3.4. Randomization Procedure and Baseline Equivalence

To investigate whether impacts of message outreach varied with predicted risk of dropout, we randomly assigned students to receive message outreach using a block randomization procedure that afforded greater statistical power to examine evidence for heterogeneity of treatment effects.[16] We implemented this procedure by predicting the probability of dropout for currently enrolled students using the dropout models we developed. The models include a robust set of covariates correlated with whether students drop out before earning a degree, including: (1) Fixed student attributes and time-variant measures before students completed one-half of the credits typically required for graduation, such as age, assignment to remediation status, and whether the student temporarily stopped out before completing one-half of their required credits to graduate; (2) measures of academic performance and financial aid receipt in the term students completed one-half of their credit requirements, such as attempted credits, cumulative GPA, and the cumulative proportion of attempted credits that were earned; and (3) measures of enrollment experiences and financial aid receipt after surpassing the one-half credit threshold analogous to those captured in 1) above. The model effectively differentiated between late dropouts and non-

[15] One of the institutions only provided data through three terms following the intervention. To preserve our sample, we include this institution's students in our main tables. Results are robust to excluding this institution as well.
[16] In our study proposal to the Institute of Education Sciences, we proposed to examine heterogeneity on this student background dimension alone. We therefore designed our study with this analysis in mind.

late dropouts in the historical samples: the probability that a randomly chosen late dropout was assigned a higher risk rating than a randomly chosen student who did not drop out ranged from 0.75-0.875 across the models. We describe the prediction models we developed more fully in Appendix 2.

Within each institution, we then ranked students by dropout risk and randomly assigned students with similar probabilities of dropout to either the treatment or control conditions. At most institutions, we randomly assigned students to one of three treatment arms: a control condition and two variants of the treatment group, one of which received a set of messages focused more on academic barriers and another that received a set of messages tailored more to address financial obstacles (though students in both groups received messages about both academic and financial barriers and resources). However, in all analyses we aggregate treated students into a pooled treatment group because we do not observe evidence of differential effects by variant of message outreach.[17]

In column 4 of Table 1, we show that random assignment appears to have created equivalent groups of students in the treatment and control conditions. In both tests of equivalence on individual covariates and in our test for joint equivalence across all covariates, we fail to detect any significant differences between treatment and control students.[18] And as we show in Appendix

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[17] Spillovers are unlikely in our context because the substantial majority of institutions in our experimental sample are large, broad-access institutions that enroll primarily commuting populations. As a result, the probability that students in the study sample interact with each other on a regular basis is lower than is expected among students at smaller and primarily residential institutions.

[18] Because the covariates we use to test for treatment balance are also used to generate the dropout risk predictions and construct the blocks in our randomization, we would mechanically not expect much variation between treatment and control groups on these covariates. This is not a concern, however, given that with a large sample randomization is expected to result in statistically-equivalent groups on both observable and unobservable dimensions. Indeed, when we estimate models with no controls included, the results are very similar to those we estimate with covariates in the model (results available upon request).

Tables A4-A6, the treatment and control groups are also well-balanced within each of our three higher education system partners (CUNY, THECB, and VCCS).

3.5. Empirical Strategy

To evaluate the effects of message outreach on academic progress and performance, we estimate intent-to-treat (ITT) models of the following form using ordinary least squares or linear probability models:

$$Y_{ib} = \alpha + \beta T_{ib} + \delta_b + \zeta X_{ib} + \varepsilon_{ib}, \tag{1}$$

where Y_{ib} is one of the four academic outcomes described above for student i in randomization block b. T_{ib} is the treatment indicator set to one for students assigned to receive text-message support and zero otherwise. δ_b denotes randomization block fixed effects, which are groupings of students within each institution assigned a similar probability of dropout by the prediction models we developed for each college.[19] The coefficient of interest in this model is β, which represents the causal estimate of being assigned to receive text-based outreach. The set of student-level covariates (X_{ib}) is comprised of indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), number of credits earned prior to the intervention, percent of attempted credits earned prior to the intervention, and whether the student had ever transferred prior to the intervention. We do not include campus fixed effects in the model, as time-invariant differences across campuses are already controlled for through the block dummies. ε_{ib} is a student-specific random error term, and in all results we report robust standard errors that allow for heteroskedasticity in the error term.

[19] Specifically, within each institution-by-intervention wave, we rank ordered students by their predicted probability of dropout and then used a nearest-neighbor approach to construct the randomization blocks. We constructed 2,534 blocks in total. The average block in the study sample includes 8.5 students.

We examine heterogeneity of treatment effects by dropout risk by estimating models of the following form:

$$(2) \qquad Y_{ibk} = \sum_{k=1}^{3} \beta_k'(T_{ib} * DR_{ibk}) + \delta_b' + \zeta'^{X_{ib}} + \varepsilon'_{ib},$$

where, as before, i and b respectively index students and blocks, and DR_{ibk} is an indicator for whether a student's predicted probability of dropout is in tercile k. All other terms in the model are defined as above. This specification allows for estimation of treatment effects separately by tercile of predicted dropout risk, whereby tercile one categorizes students with low relative risk of dropout, tercile two captures students with medium risk of dropout, and tercile three denotes students with high predicted risk of dropout according to the college-specific dropout prediction models we developed using historical data from each institution.[20]

Finally, we investigate additional sources of potential heterogeneity in the impacts of N2FL in several exploratory analyses: by participating higher education system; by institutional staffing levels; and by whether institutions had ongoing texting campaigns in parallel to N2FL. We conduct these analyses by adding appropriate interaction terms to equation (1), but the results are unchanged if we instead conduct these analyses within each sub-sample.

4. Results

4.1 Overall impacts

In the top panel of Table 2, we present estimates of N2FL's impact on our primary outcomes of interest, measured four terms following the start of the intervention at each institution. Across experimental conditions, most students (76.2 percent) re-enrolled or graduated within four terms, and the N2FL interactive text messages did not significantly increase re-enrollment or

[20] Our estimates of heterogeneous impacts by baseline risk are robust to whether we group students into above- versus below-median groups, quartiles, or use a continuous measure of baseline risk.

graduation rates. We can rule out impacts of 1.8 percentage points or larger on the probability of re-enrollment or graduation. We similarly do not observe impacts of the treatment on the number of credits students accumulated. Likewise, when we investigate impacts of N2FL on degree attainment alone, we also find no significant effects. Fifty-nine percent of the control group completed their degree within four terms, and we can rule out treatment impacts of 0.9 percentage points or larger. Among students at two-year institutions, we find no impact of N2FL on transfer to four-year institutions and we can rule out impacts of 1.0 percentage points or larger. Finally, in the bottom panel of Table 2, we show that there are similarly no effects of N2FL within six terms of the intervention for the subset of institutions for whom we can observe outcomes over that time frame. None of our estimates is significant, and if we were to apply multiplicity adjustments given the number of estimated impacts (here and throughout the paper), it would only further accentuate our lack of identification of significant impacts.

4.2 Impacts by predicted baseline risk of withdrawal

In Figure 2, we present heterogeneous impacts of N2FL on the probability of re-enrollment or graduation and the probability of graduation alone four terms following the start of the intervention by tercile of predicted risk of withdrawal. As expected, we observe the highest rates of re-enrollment or completion and degree attainment among students in the bottom tercile of risk. For instance, 70.7 percent of students in the control group in the bottom tercile earned a degree within four terms, compared with 45.9 percent of control students in the top tercile. Once again, we do not observe significant impacts of N2FL across any of the risk terciles on any of the primary outcomes, and in all cases can rule out even moderate treatment effects. In Appendix Table A7, we show that the null effects of N2FL across the distribution of predicted risk holds across other academic outcomes (i.e., credit accumulation and transfer among two-year enrollees) both four and six terms following intervention launch.

4.3 Impacts by higher education system

We implemented N2FL across five states, with most institutions participating in one of three higher education systems: the City University of New York (CUNY), the Texas Higher Education Coordinating Board (THECB), and the Virginia Community College System (VCCS). These systems differ in their governance structure, public expenditures in higher education, institutional context, and student composition, so it is possible the impacts of N2FL would vary across systems. We investigate whether this is the case in Figure 3. As with the absence of heterogeneity by predicted risk, we find no evidence of impacts of N2FL on the probability of re-enrollment/graduation or graduation alone four terms post-launch across any of the three higher education systems, and we can rule out the possibility of moderately-sized effects. We similarly do not find significant impacts by system on other academic outcomes and time horizons, as we show in Appendix Tables A8-A9. We further show in Appendix Table A10 that the impacts of N2FL do not vary by predicted baseline risk within each higher education system. Finally, as we show in Appendix Table A11, the impacts of N2FL do not vary across two- and four-year institutions in the aggregate.

4.4 Mechanisms

While we maintained a consistent core of scheduled message content across institutional partners, by virtue of working with 20 institutions, there were still potentially important differences in treatment implementation and institutional context that could lead to differences in N2FL efficacy. For instance, as we describe above, institutions varied in whether the staff member responding to student messages was a dedicated professional advisor, part of a team of advisors, or a non-advisor staff member who made connections to other advisors on campus. Institutions also varied in their overall level of advising support (which we proxy for using the advisor:student ratio), whether institutions required students to meet with academic advisors, and whether they had other texting campaigns operating in parallel with N2FL.

In Appendix Table A12, we investigate whether treatment efficacy varied based on any of these factors. We treat these investigations as exploratory since we are underpowered to detect impacts across numerous sub-groups. Nonetheless, we fail to find any significant differences in N2FL efficacy by whether institutions used professional advisors, a team of advisors, a staff “connector”, or in the case of two institutions, automated responses (Appendix Table A12, Panel A). Nor do we find significant differences by whether institutions had larger or smaller advisor:student caseloads, required students to meet with advisors, or had parallel texting campaigns in place (Appendix Table A12, Panel B).

Finally, as we describe above, we observed meaningful heterogeneity in advisor response rates across campuses. In Appendix Figure A1, we investigate whether the impacts of N2FL varied by advisor responsiveness to text messages students sent in response to scheduled outreach they received. Appendix Figure A1 plots treatment effects on our primary outcomes four terms post-intervention by quartile of advisor responsiveness.[21] We find no evidence of N2FL impacts on any of the main outcomes across the distribution of advisor responsiveness; furthermore, the confidence intervals of the effect estimate by quartile of advisor responsiveness overlap considerably. We conclude that N2FL had no impact on the likelihood of college persistence or degree completion, even among students paired with highly responsive and engaged advisors.

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[21] The within-quartile outcome means for Panel A are: Q1 = 75.6%; Q2 = 65.8%; Q3 = 75.1%; and Q4 = 74.2%. The within-quartile outcome means for Panel B are: Q1 = 29.9; Q2 = 29.4; Q3 = 31.0; and Q4 = 24.2. The within-quartile outcome means for Panel C are: Q1 = 58.5%; Q2 = 48.4%; Q3 = 60%; and Q4 = 42.0%.

5. Discussion

Many college students within reach of graduation remain at risk of dropping out before they earn a degree. Although leaving without a degree may be a rational human capital investment decision for some, reducing late dropout is likely to benefit many near completers given the prevalence of the phenomenon and the high returns to degree completion for most college enrollees. We developed the N2FL intervention to examine if text-based outreach offers a scalable solution to support students at risk of late dropout while they remain enrolled in college. To our knowledge, previous interventions targeted to this population have strictly attempted to re-engage individuals after they have already withdrawn from school. The findings in this study provide strong evidence that low-touch interventions such as text-based outreach may not be an effective policy tool to reduce the incidence of late dropout from college. We estimate null impacts on persistence and completion in the overall sample, separately by students' baseline predicted risk of dropout, and across numerous dimensions of treatment fidelity and institutional context.

The most immediate question is what explains the null impacts of N2FL. Our findings are consistent with several other recent nudge interventions in the education arena that have not scaled successfully (Bergman, Denning, and Manoli 2019; Bird et al. 2021; Gurantz et al. 2021). However, unlike N2FL, those interventions relied on messages delivered from organizations with which students did not have close, pre-existing connections, leaving open the possibility that the efficacy of outreach campaigns requires participation of local entities that students trust. We find null impacts in this study despite partnering with institutions to design message content and sequencing, sending all messages from a specific advisor at the students' institution, and encouraging students and advisors to interact in real-time using two-way texting capabilities. The findings in this study therefore suggest that nudging at scale in postsecondary education is often not effective for other reasons.

We posit three alternative explanations for the null findings in this study. One possibility is that the messages were not salient enough to students to foster meaningful engagement with advisors on campus. Text-based outreach has become increasingly widespread over the past decade and colleges must compete more in recent years for the attention of students. Although we observed high student and advisor response rates in N2FL overall, we cannot rule out that college students may have reached a point of text message saturation, such that the efficacy of outreach campaigns launched five or 10 years ago will be more limited today.

Alternatively, because N2FL relied on the existing advising infrastructure of colleges and universities to engage with students, it is possible that the intervention asked too much of college staff with large caseloads and competing demands. This may be especially true in the context of upper-division students at risk of dropout, who may face acute academic and financial barriers that require more intensive assistance than two-way texting or traditional models of advising can provide (Mabel & Britton, 2018; Ortagus, Skinner & Tanner, 2020). The intensity of support at-risk students need may also explain the success of more resource-intensive interventions in college like one-on-one coaching programs (Bettinger & Baker, 2014; Oreopoulos & Petronijevic, 2017), which often have low student-coach caseloads and augment, rather than depend on, the traditional advising capacity of colleges. A third possibility is that N2FL may have engaged students too late into their college careers. As evident from the promising impacts of comprehensive college support interventions (Dawson, Kearney & Sullivan, 2020; Evans et al., 2020; Weiss et al., 2019), there may be important benefits to programs that engage students throughout their college career. If that is the case, then upper-division students at risk of dropout may benefit most from interventions that begin earlier and offer continuous support.

Although we are unable to pin down the precise reason(s) why N2FL produced null impacts, our findings are clear that college students at risk of late dropout likely require higher-touch intervention. Yet the reality is that high-touch interventions are expensive and many colleges, especially broad-access institutions that serve most students at risk of late dropout, operate on tight budgets. Helping more college students cross the finish line will require institutions to target resources to at-risk students who stand to benefit most. We embedded predictive modeling into the design of N2FL to help colleges identify which students experienced the largest gains from message outreach. While we find no impacts of message outreach on persistence and completion across the distribution of predicted baseline risk, the null effects found in this study do not necessarily reflect that predictive models convey limited utility for colleges. At the same time, recent research demonstrates that predictive models do not perform equally well for all student subgroups and that the specific modeling strategy used in predictive analytics can result in different student risk assignments (Bird et al., 2021). Additional research is needed to critically investigate whether the use of predictive analytics in higher education leads to more effective, efficient, and fair targeting of students for success-oriented interventions.

Further research is also needed to determine if more intensive student support interventions that have proven effective in other contexts can lower rates of late dropout from college. To maximize the cost-effectiveness of those strategies, we encourage researchers to embed predictive analytics into future research, as we have done in this study, to help policymakers and college leaders better distinguish between marginal and inframarginal students. We believe this is the most feasible strategy for reconciling the tension between the resource-intensive supports that many college students appear to need and the resource-constrained environments in which most higher education institutions operate.

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Table 1. Pre-treatment characteristics of experimental sample by treatment condition and summary statistics of nationally representative sample of undergraduates attending public two- and non-selective four-year institutions

	(1)	(2)	(3)	(4)
		Experimental Sample		
	NPSAS Sample	Treated Students	Control Students	T-C Difference
Male	0.438	0.433	0.442	0.000
Black	0.175	0.153	0.146	-0.001
Hispanic	0.220	0.223	0.208	-0.003
White	0.507	0.388	0.416	0.003
Race other	0.098	0.129	0.130	0.001
Race missing	0.000	0.108	0.100	-0.001
Age	27.10	21.58	21.39	0.102
Enrolled in public 2-year institutions	0.781	0.522	0.467	0.000
Enrolled in public 4-year institution	0.219	0.478	0.533	0.000
Cumulative credits earned before intervention		61.76	65.98	-0.205
Share of credits earned before intervention		0.906	0.908	0.000
Transferred into current school		0.295	0.308	-0.006
Predicted risk of dropout		0.297	0.294	0.000
P-value on *F*-test for joint significance				0.741
Number of Students:	58,410	13,826	7,727	21,553

*** $p<0.01$ ** $p<0.05$ * $p<0.10$

Notes: The data in column 1 is from the National Postsecondary Student Aid Study of 2012 (NPSAS:12). Summary statistics in column 1 are calculated using survey sampling weights. The data in columns 2-4 are from partner institution administrative records. Means are reported in columns 2 and 3. Estimates of post-randomization balance are reported in column 4 from OLS/LPM models that include randomization block fixed effects.

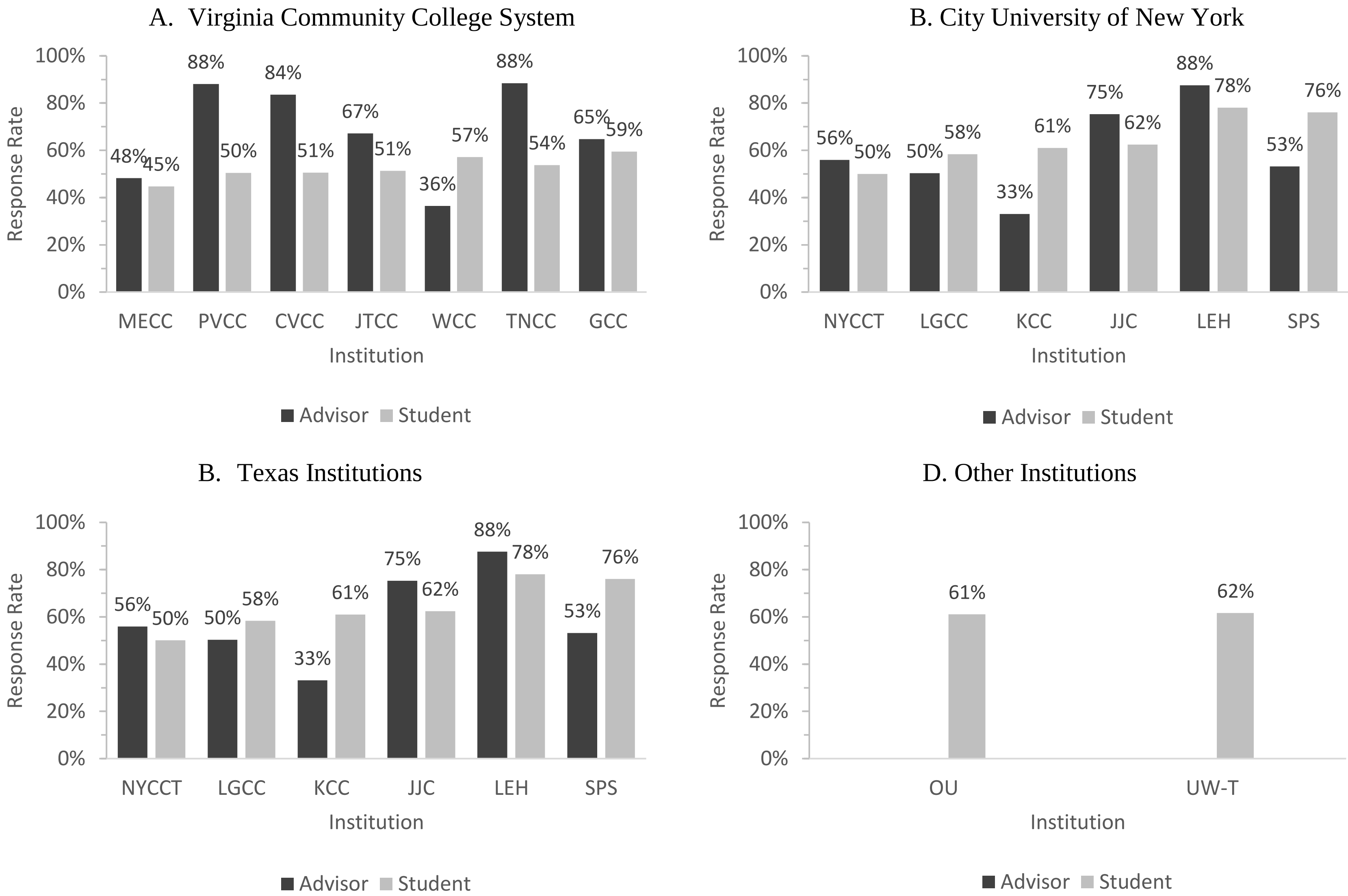
Table 2. Estimates of intervention effects on academic outcomes

	(1) Re-Enrolled or Graduated	(2) Number of Credits Accumulated	(3) Graduated	(4) Transferred to Four-Year
Four Term Outcomes:				
Treatment Impact	.0059	.421	-.0027	-.008
	(0.006)	(0.263)	(0.006)	(0.009)
Control Mean	.762	34.823	.591	.459
Observations	21553	21553	21553	10534
Six Term Outcomes:				
Treatment Impact	-.0036	.088	-.0065	-.011
	(0.007)	(0.416)	(0.007)	(0.012)
Control Mean	.815	42.623	.755	.526
Observations	12879	12879	12879	6788

*** $p<0.01$ ** $p<0.05$ * $p<0.10$

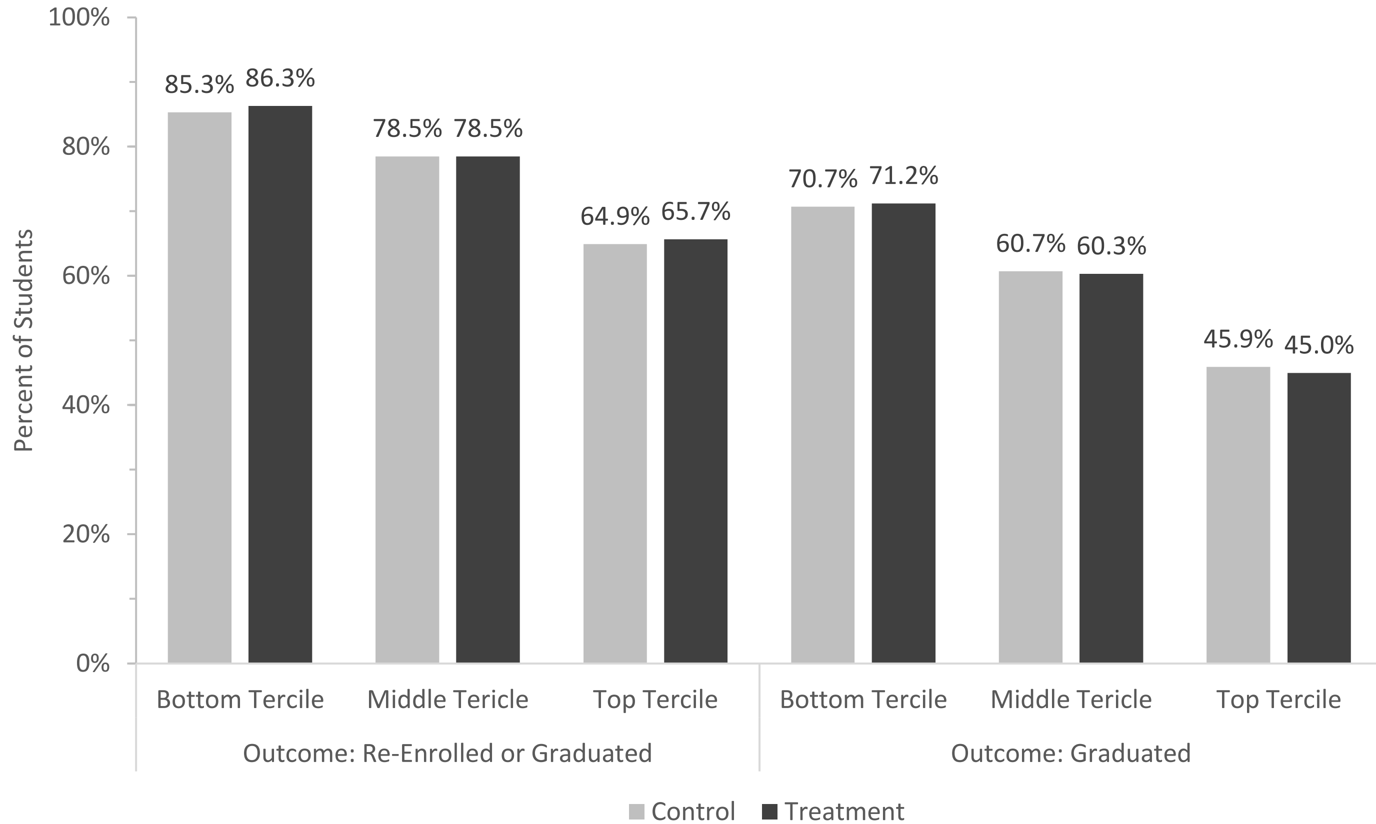
Notes: Estimates are from OLS/LPM models that include randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses. Column 4 only includes students at 2-year colleges.

Figure 1. Engagement statistics for students and advisors at partner institutions by system



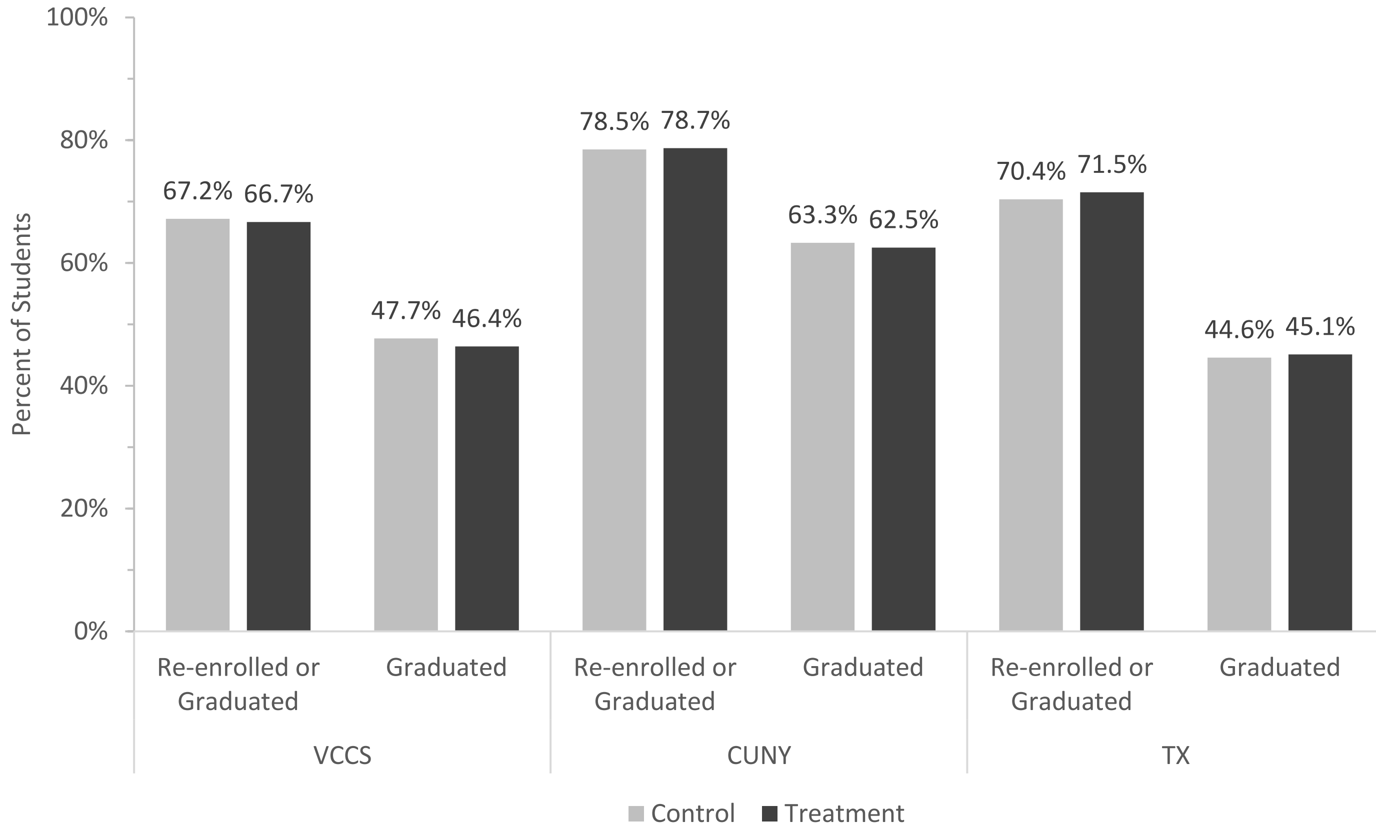
Note: Student response rates are averaged based on cohort sample size at institutions with multiple intervention waves. Advisor response rates are averaged across cohorts and are not available for the fall 2016 intervention wave. Advisor response rates are not reported for Ohio University and the University of Washington-Tacoma because responses were automated at those institutions.

Figure 2. Estimates of intervention effects on the probability of re-enrollment/graduation four terms following intervention launch by tercile of dropout risk



Notes: None of the treatment-control contrasts are statistically significant at the 10 percent level. Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Risk ratings terciles are defined within an institution.

Figure 3. Estimates of intervention effects on the probability of re-enrollment/graduation four terms following intervention launch by system



Notes: None of the treatment-control contrasts are statistically significant at the 10 percent level. Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention.

Appendix 1: Sample Text Message Content

Spring 2018 Semester	
March 12, 2018 3:00 PM	Hi [first_name], I’m <advisor name>, a <institution> advisor, & I’m here to support you to finish your degree! Is there anything I can help you with now?
March 14, 2018 3:00 PM	Hi [first_name]! Early registration for fall term starts 3/26. Can I help you choose courses that will help you finish & earn your degree?
March 20, 2018 3:00 PM	Hi, it’s <advisor name>. Students share they miss meals & face other financial hardships. We have resources for those facing these challenges. Can I help connect you?
March 26, 2018 3:00 PM	Hi, it’s <advisor name>. FAFSA.gov is open for the 2018-2019 school year, and applying early gets you the most financial aid. Have you started FAFSA yet?
March 26, 2018 3:00 PM	Hi, it’s <advisor name>. Lots of students get grants that help them finish their degree and graduate to a rewarding career. Can I help you do FAFSA this year? *<Infographic with average financial aid award for students at this institution>*
March 28, 2018 3:00 PM	Hi [first_name]! Putting in time at the <campus tutoring center name> math and writing centers will help you succeed in your classes! Can I help you get connected there? *<infographic with information about the campus tutoring center>*
March 28, 2018 3:00 PM	Hi [first_name]! Doing well in classes can bring you closer to graduating & a higher income! Can I help connect you to the <campus tutoring center name> math and writing centers? *<infographic with information about the campus tutoring center>*
April 4, 2018 3:00 PM	Hi [first_name], I want you to graduate! Some students take courses that don’t count for their degree. Can I help you register for classes that fit your program? *<infographic with visualization of time to degree for students at institution who take classes that count toward their degree vs. those that do not>*
April 4, 2018 3:00 PM	Hi [first_name], I know college can feel expensive. Can I help you register for classes that fit your program, so you don’t pay for courses you don’t need?

	<infographic with visualization of relationship between excess courses and time to degree>
April 10, 2018 3:00 PM	Hi, [first_name]! Now's a great time to think about summer. Taking courses can bring your diploma much sooner. Want to look into summer courses together?
April 10, 2018 3:00 PM	Hi, [first_name]! Now's a great time to think about summer. Taking courses can bring your diploma and a good-paying job much sooner. Can I help you look into courses?
April 17, 2018 3:00 PM	Hi! Have you had a chance to apply for financial aid for next year? FAFSA.gov is now open. Applying early gets you the most aid. Let me know if I can help.
April 17, 2018 3:00 PM	Hi! Did you know 268,000+ Virginians filed FAFSA by this date last year? Join your peers and visit FAFSA.gov now to get the most aid. Can I help?
April 23, 2018 3:00 PM	Hey, it's <advisor name>. With finals coming up, I wanted to check if you've used SMARThinking online tutoring or the <campus tutoring center name>. Can I help you get connected? *<infographic with visualization of relationship between using academic supports and student academic performance>*
April 25, 2018 3:00 PM	Doing well on exams brings you closer to your degree. Pick 2 hours each day to study using this calendar. Can I help you make a study schedule? *<infographic with a fillable schedule for dates/times students plan to study>*
April 25, 2018 3:00 PM	Studying for finals pays off! Grads in [program] earn an average of $[earnings] per year! Can I help make a study schedule?
May 8, 2018 3:00 PM	Hey, it's <advisor name>. Congrats on finishing another semester--you're one term closer to your degree. I'll reach out again in the fall. Happy summer!
Fall 2018 Semester	
August 22, 2018 4:00 PM	Hi [first_name], it's <advisor name>. I'm happy to have you back on campus! I'm here to help you continue staying on track. How do you feel about the start of the term? *<infographic with general tips for starting the semester>*
August 27, 2018 4:00 PM	Hi! 500+ <institution> students used resources like the <campus tutoring center name> last spring to pass classes & move toward graduation. Do you have the academic support to succeed?

	<R: tncc_f18_mathcenter>
September 5, 2018 4:00 PM	Hey, it's <advisor name>. The start of the fall semester can be challenging for students who juggle classes w/ family and work. How's the transition going for you?
September 17, 2018 4:00 PM	Hi [first_name], you may be on track to graduate soon--congrats! Last day to apply for Fall 2018 graduation is 10/1. Can I help you do a graduation check? <*infographic with step-by-step guidance for checking eligibility to graduate*>
September 24, 2018 4:00 PM	Hi [first_name]! Priority registration for Spring semester starts 10/22. Can I help you check which courses you still need to graduate?
October 1, 2018 4:00 PM	Hi! FAFSA.gov opened this week for the 2019-20 school year. Apply early to get more $$ toward your courses. Can I help you apply? <*infographic comparing the average aid amount for students who apply before vs. after the priority deadline*>
October 1, 2018 4:00 PM	Hi! FAFSA.gov opened this week for the 2019-20 school year. Apply early to get all the free $$ you're eligible for. Can I help? <*infographic comparing the average aid amount for students who apply before vs. after the priority deadline* >
October 8, 2018 4:00 PM	Hi. I know financial stress like budgeting or loans can distract students from focusing on school & graduating on time. Do you face these challenges? <*infographic with information about resources for students facing financial hardships – e.g.., budgeting workshops*>
October 15, 2018 4:00 PM	Hi [first_name], using the <campus tutoring center name> math & writing center can help you do well on midterms & boost your grades before end of term. Can I help connect you? <*infographic with information about the math tutoring center*>
October 15, 2018 4:00 PM	Hi [first_name], make your tuition $$ count by taking advantage of academic resources like the <campus tutoring center name> math & writing center. Can I help connect you? <*infographic with information about the math tutoring center*>
October 22, 2018 4:00 PM	Hi, priority registration for Spring 2019 starts today! Register before Sunday to maximize your chances of getting the classes you need to graduate. Can I help?

	<infographic with information about steps to register for courses>
November 5, 2018 4:00 PM	Hi [first_name], paying tuition on time guarantees your seat in the Spring 2019 courses you need to graduate. If you already registered, last day to pay your tuition is Fri, 12/7. If you register after 12/7, tuition is due the day you register. Do you feel on track to pay on time? *<infographic with information about spring semester tuition payment deadlines>*
November 12, 2018 4:00 PM	Hi [first_name]! Final exams are in 1 month. Many students use the <campus tutoring center name> math & writing center to prepare for exams & boost their GPA. Can I help connect you? *<infographic with information about academic support resources>*
November 19, 2018 4:00 PM	Hey, it’s <advisor name>. This week’s break from classes is a great time for you to get the FAFSA in for the 2019-2020 school year, if you haven’t already. *<infographic with step-by-step guidance for completing the FAFSA>*
November 26, 2018 4:00 PM	Hi [first_name]! Only 2 weeks until final exams. A study plan can help you do well on finals & graduate on time. Can you set aside 2 hrs each day to study? *<infographic with a fillable schedule for dates/times students plan to study>*
November 26, 2018 4:00 PM	Hi [first_name]! Only 2 weeks until final exams. A study plan can help you pass finals & advance towards a rewarding career. Can you set aside 2 hrs each day to study? *<infographic with a fillable schedule for dates/times students plan to study>*
December 10, 2018 4:00 PM	Hi, you’re so close to passing your exams & moving closer toward your degree! The <campus tutoring center name> is open this week if you need tutoring. Can I help?
December 19, 2018 4:00 PM	Hi, congrats on moving closer toward your degree! Setting New Year goals can keep you on track to graduate on time. Which of these goals can you commit to? *<infographic with a checklist of goals to keep students on track to graduate >*

Appendix 2: Predictive Models of Dropout

To examine which students with substantial credits stand to benefit from targeted outreach and support, we developed dropout prediction models at each partner institution using data on historical cohorts of students. We predicted the probability of dropout after students completed 30 or 60 college-level credits at two- and four-year colleges, respectively, as a function of time-invariant student characteristics, measures of students' enrollment experiences and performance in college, and measures of financial need and aid receipt. We then assigned risk ratings to students in the experimental sample using the dropout prediction models.[22] We assigned risk ratings using logistic regression models in the pilot phase and random forest classification models during the scale phase. As we discuss in more detail below, the two models performed very similarly in absolute terms; however, we switched modeling strategies over time because the random forest models performed slightly better in relative terms.

At each institution, we evaluated the performance of several candidate prediction models by splitting the historical data into development and validation samples to identify which model best distinguished between students who dropped out and students who graduated or were still enrolled in the historical data. The specific covariates included in each model differed slightly across institutions based on data availability. In general, we compared the performance of models that only included predictors up to the term that students completed one-half of the credits typically required for graduation to models that also included measures of their enrollment history and aid receipt after completing one-half of their credits. The models that consistently performed best captured information on students before and after they completed one-half of the credits typically required to graduate. These models include the following general set of predictors:

[22] Due to cost constraints and institutional preferences, recruitment was limited at some campuses. At institutions where the number of eligible students exceeded the number of recruitment slots, we also used the dropout predictions to exclude the most inframarginal students from the study sample.

1) Fixed student attributes and time-variant measures before students completed one-half of the credits typically required for graduation. Where available, this vector includes the following measures: age, gender, race/ethnicity, assignment to remediation status, whether the student transferred into their current institution and whether the student temporarily stopped out before completing one-half of their required credits to graduate. To capture changes in student circumstances over time that may influence risk of dropout, the vector also includes an indicator of whether students changed majors between when they first entered the institution (or system) and when they completed one-half of their credits, as well as within-student standard deviations of the following measures: Expected Family Contribution (EFC), the amount of financial aid received (entered separately by aid type), and the number of credits attempted per term.
2) Measures of academic performance and financial aid receipt in the term students completed one-half of their credit requirements. Where available, this vector includes the number of attempted credits, cumulative GPA, the cumulative proportion of attempted credits that were earned, and the amount of financial aid received.
3) Measures of enrollment experiences and financial aid receipt after surpassing the one-half credit threshold analogous to those captured in 1) above.

Our preferred models effectively differentiated between late dropouts and non-late dropouts in the historical samples. For example, the probability that a randomly chosen late dropout was assigned a higher risk rating than a randomly chosen student who did not drop out ranged from 0.75-0.875 across the models.[23] Students in the experimental sample who graduated were also at lower risk of dropout on average compared to students who did not graduate. The average predicted probability of dropout was

[23] On average, replacing logistic regression models with random forest algorithms increased the probability that a randomly selected late dropout was assigned a higher risk rating than a randomly selected non-dropout by less than 3 percentage points (2-3 percent). The risk ratings generated by the two modeling approaches correlate around 0.90 or higher.

23.2 percent among students who graduated compared to 38.3 percent among students who did not graduate.

In Appendix Table A3, we report descriptive statistics for the full study sample by tercile of predicted dropout risk. The average risk rating in the bottom, middle, and top tercile is 0.13, 0.26, and 0.50, respectively. Students at greatest risk of dropout exhibited higher rates of course failure and erratic credit loads as they progressed in school. For example, bottom-tercile students completed 96 percent of their attempted credits prior to intervention launch, whereas top-tercile students completed 85 percent of their attempted credits. High-risk students were also more likely to be older, male, and identify as Black or Latinx.

Appendix 3: Nudges to the Finish Line statistical power calculations from the original Institute for Education Sciences Proposal (submitted in calendar year 2015)

Power analysis

For our power calculations, we use Optimal Design software. In these calculations we consistently assume 80 percent power over a 95 percent confidence interval. We also assume that the blocking variables explain 40 percent of the variation in the outcome, but we do not assume additional power coming from included covariates, given that the blocking variable is constructed based on an index of many of these covariates. We further assume that the treatment variability is 0.10 across sites and that two people are in each block as a result of our pairwise matching method. Our power calculations are based on the assumption that the treatment variance can be represented as follows:

$$V(\beta) = \frac{\tau}{J} + \frac{4\sigma^2}{Jn}$$

This is the standard case where τ represents the treatment variability across blocks, J is the number of blocks, and n is the number of people in each block (2 in our case).

In our Development phase, we anticipate recruiting 8 campuses with 500 students in the experiment per campus. This gives us roughly 2000 matched pairs of students. Under these conditions, the Minimum Detectable Effect Size (MDES) is 0.072. Mabel and Britton (2015) show that 25 – 30 percent of students who had completed 75 percent of their total credit requirements withdrew from college. Therefore, if the stop-out rate was 30 percent in our sample, the MDES of 0.072 would correspond to a 3.2 percentage point change in stop-outs. If our blocking variables have even more explanatory power (i.e. explaining 60 percent of the variation), then we can detect impacts as small as 2.7 percentage points in the Development phase.

To place this MDES in context, Bettinger and Baker (2012) examined the effect of coaching students in their first year of college. They found short-run impacts of approximately 0.110 standard deviations. Although the student supports we are providing are likely not as intensive as those that Bettinger and Baker investigated, our MDES of 0.072 is comparatively strong in the overall sample.

In the Efficacy phase, we anticipate recruiting 17 institutions with 1500 students at each campus for our experiment. If we only consider the Efficacy phase (rather than pooling the data with our Development phase), we should have 12,750 matched pairs of students. Under the same assumptions as above, we should have power to identify impacts as small as 0.028 standard deviations, or about 1.2 percentage points. If our blocking variables explain 60 percent of the variation in outcomes, then we can detect effects as small as 1.0 percentage point. This gives us an extremely strong MDES relative to the prior literature on student supports.

As we mentioned above, there are some permutations of the treatment that we would like to examine. If, for example, we test two treatments in two campuses during the Development phase, we would have 1000 students or 333 matched triads. We would have power to identify impacts as small as 0.144 standard deviations in this setting. This is a high MDES relative to other studies, especially given the exploratory nature of such iterations, and we might accept slightly less power knowing that we can increase the sample if needed in our Efficacy phase when we have substantial power. If, for example, we were to accept 70 percent power and a 90 percent confidence interval, we could detect impacts as small as 0.11 standard deviations, which approaches the magnitude of the short-run impacts in the Bettinger and Baker study.

These power calculations illustrate that there are limitations to the number of permutations we might be able to test; however, they also give credence to our overall development plan where testing different intervention design variations and features during the Development phase will help us to refine the treatment before the Efficacy phase begins. As we explain above, attrition in our case is an outcome of interest given the coverage of our data, and so we do not treat it in the power calculations.

Appendix Table A1. Engagement statistics for students and advisors at partner institutions, presented by system

Panel A: VCCS Institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mountain Empire Community College	Piedmont Virginia Community College	Central Virginia Community College	John Tyler Community College	Wytheville Community College	Thomas Nelson Communit y College	Germanna Community College
Start of messaging	Fall '18	Fall '16	Fall '18	Spring '18	Fall '18	Fall '16	Spring '18
End of messaging	Fall '19	Spring '19	Fall '19	Fall '19	Fall '19	Spring '19	Spring '19
Terms messaged	3	6	3	4	3	6	3
Percent students responded	44.74	50.49	50.59	51.31	57.14	53.77	59.46
Advisor response rate	48.21	88.03	83.55	67.13	36.43	88.43	64.8
N Treated Students	76	629	171	766	84	867	296

Panel B: CUNY Institutions

	New York City College of Technology	LaGuardia Community College	Kingsborough Community College	John Jay College	Lehman College	School of Professional Studies
Start of messaging	Spring '18	Fall '16	Spring '18	Spring '18	Fall '16	Fall '16
End of messaging	Spring '19	Spring '19	Fall '18	Spring '19	Fall '19	Spring '19
Terms messaged	3	6	2	3	6	6
Percent students responded	50.05	58.34	61	62.41	78.09	76.06
Advisor response rate	55.97	50.31	33.11	75.31	87.58	53.2
N Treated Students	756	1,333	622	1,368	751	389

Appendix Table A1, continued. Engagement statistics for students and advisors at partner institutions, presented by system

Panel C: Texas Institutions

	Blinn College	Austin Community College	University of Texas at Arlington	Alamo Colleges	University of Texas of the Permian Basin
Start of messaging	Fall ‘18	Fall ‘18	Fall ‘18	Spring ‘19	Fall ‘16
End of messaging	Spring ‘19	Fall ‘19	Fall ‘19	Fall ‘19	Spring ‘19
Terms messaged	2	3	3	2	3
Percent students responded	44.03	47.70	56	62.07	68.80
Advisor response rate	77.64	71.34	77.34	71.23	61.19
N Treated Students	1,000	423	1,000	608	659

Panel D: Other Institutions

	Ohio University	University of Washington Tacoma
Start of messaging	Fall ‘16	Fall ‘16
End of messaging	Spring '19	Spring '19
Terms messaged	6	6
Percent students responded	61.04	61.59
Advisor response rate	--	--
N Treated Students	1,003	1,025

Appendix Table A1, continued. Engagement statistics for students and advisors at partner institutions, presented by system

Panel E: All Institutions

	All Institutions
Percent students responded	58.36
Advisor response rate	66.16
N Treated Students	13,826

Notes: This table presents student and advisor engagement statistics for the institutions included in the study. Note that some institutions had multiple cohorts of students; in that case, we averaged the student response rate based on cohort sample size. Note that advisor response rate is averaged across each cohort and we do not have data on advisor response rate from the Fall 2016 cohorts. The start date presented for each institution is that of the earliest cohort at that institution. Finally, advisor response rate is inapplicable for Ohio University and the University of Washington Tacoma since responses were automated at those institutions.

Appendix Table A2: N2FL Staffing Models

Model	**Example Advisor Background(s)**	**Advisor Role**	**Sample Message**	**Institutions**
Professional Advisor	Hired specifically for project	Direct assistance with tasks (e.g., registering for courses, financial aid applications)	Hi, it's <Professional Advisor>. With finals coming up, I wanted to check if you've used <Support Center> for help with classes. Can I help you get connected?	PVCC, TNCC, SPS, JJC, LAGCC, ACC, Lehman, WCC, Alamo
Faculty Advisor	University faculty	Direct assistance with questions in their specialization (e.g., course selection) and recommending campus resources for other questions (e.g., financial aid)	Hey, it's <Faculty Advisor>. As you're planning for spring, think about picking up an extra course. This can help you graduate sooner. Can I help you choose another class?	NYCCT
Staff Point Person	Administrative assistant on student engagement team	Direct students to the resource most appropriate for providing assistance	Hi <Student>! Registration for fall and summer starts 4/2. Have you talked to an advisor about the next classes you need to take in your program?	JTCC, UTPB, Blinn, MECC, CVCC, UT Arlington
Segmented Advising	Mix of campus staff (e.g., some faculty advisors coupled with a career services counselor)	Leveraged multiple staff depending on question (e.g., student replies to automated questions about course registration went to an Academic Advisor's portfolio)	Hi, it's <Advisor>. Fafsa.gov is now open for the 2018-2019 school year and applying early gets you the most financial aid. Have you started FAFSA yet? [student replies are routed to a Financial Advisor's inbox]	KBCC

Appendix Table A3. Pre-treatment characteristics of experimental sample by tercile of predicted dropout risk

	(1) Bottom Tercile	(2) Middle Tercile	(3) Top Tercile
Male	0.412	0.027***	0.038***
Black	0.116	0.030***	0.070***
Hispanic	0.207	0.010	0.020***
White	0.420	-0.015**	-0.049***
Race other	0.150	-0.027***	-0.039***
Race missing	0.106	0.001	-0.002
Age	20.59	1.141***	1.533***
Enrolled in public 2-year institutions	0.482	0.024***	0.037***
Enrolled in public 4-year institution	0.518	-0.024***	-0.037***
Cumulative credits earned before intervention	65.50	-2.640***	-4.456***
Share of credits earned before intervention	0.957	-0.034***	-0.110***
Transferred into current school	0.292	-0.002	0.001
Predicted risk of dropout	0.130	0.128***	0.371***
Number of Students:	7,192	7,183	7,178

*** p<0.01 ** p<0.05 * p<0.10

Notes: Notes: Means are reported in column 1. Differences relative to bottom-tercile students are reported in columns 2 and 3 from OLS/LPM models. Estimates include school by cohort fixed effects. Risk terciles are defined within a school. The respective means by tercile are 0.13, 0.26, and 0.50.

Appendix Table A4. Pre-treatment characteristics of VCCS experimental sample by treatment condition

	(1)	(2)	(3)
	Experimental Sample		
	VCCS Treated Students	VCCS Control Students	VCCS T-C Difference
Male	0.431	0.428	0.003
Black	0.182	0.183	-0.001
Hispanic	0.093	0.102	-0.008
White	0.621	0.614	0.007
Race other	0.039	0.044	-0.004
Race missing	0.064	0.057	0.007
Age	19.97	19.73	0.247
Enrolled in public 2-year institutions	1.000	1.000	--
Enrolled in public 4-year institution	0.000	0.000	--
Cumulative credits earned before intervention	35.49	35.52	-0.001
Share of credits earned before intervention	0.856	0.856	0.001
Transferred into current school	0.109	0.108	0.002
Predicted risk of dropout	0.459	0.458	0.001
P-value on *F*-test for joint significance			0.837
Number of Students:	2,889	1,447	4,336

*** p<0.01 ** p<0.05 * p<0.10

Notes: The data in columns 1-3 are from VCCS partner institution administrative records. Means are reported in columns 1 and 2. Estimates of post-randomization balance are reported in column 3 from OLS/LPM models that include randomization block fixed effects.

Appendix Table A5. Pre-treatment characteristics of CUNY experimental sample by treatment condition

	(1)	(2)	(3)
	Experimental Sample		
	CUNY Treated Students	CUNY Control Students	CUNY T-C Difference
Male	0.378	0.376	0.005
Black	0.209	0.202	0.006
Hispanic	0.281	0.287	-0.008
White	0.174	0.176	0.000
Race other	0.180	0.172	0.008
Race missing	0.157	0.163	-0.005
Age	22.27	22.06	0.123
Enrolled in public 2-year institutions	0.441	0.453	0.000
Enrolled in public 4-year institution	0.559	0.547	0.000
Cumulative credits earned before intervention	60.88	60.57	-0.023
Share of credits earned before intervention	0.933	0.931	0.001
Transferred into current school	0.306	0.309	-0.010
Predicted risk of dropout	0.245	0.247	0.0004*
P-value on *F*-test for joint significance			0.805
Number of Students:	5,219	2,532	7,751

*** p<0.01 ** p<0.05 * p<0.10

Notes: The data in columns 1-3 are from CUNY partner institution administrative records. Means are reported in columns 1 and 2. Estimates of post-randomization balance are reported in column 3 from OLS/LPM models that include randomization block fixed effects.

Appendix Table A6. Pre-treatment characteristics of TX experimental sample by treatment condition

	(1)	(2)	(3)
	Experimental Sample		
	TX Treated Students	TX Control Students	TX T-C Difference
Male	0.456	0.476	-0.020
Black	0.082	0.090	-0.008
Hispanic	0.317	0.307	0.009
White	0.381	0.379	0.002
Race other	0.104	0.104	0.000
Race missing	0.116	0.120	-0.004
Age	21.84	21.88	-0.045
Enrolled in public 2-year institutions	0.550	0.550	0.000
Enrolled in public 4-year institution	0.450	0.450	0.000
Cumulative credits earned before intervention	61.55	62.25	-0.678**
Share of credits earned before intervention	0.890	0.892	-0.002
Transferred into current school	0.347	0.358	-0.011
Predicted risk of dropout	0.262	0.262	0.000
P-value on *F*-test for joint significance			0.413
Number of Students:	3,690	1,845	5,535

*** $p<0.01$ ** $p<0.05$ * $p<0.10$

Notes: The data in columns 1-3 are from TX partner institution administrative records. Means are reported in columns 1 and 2. Estimates of post-randomization balance are reported in column 3 from OLS/LPM models that include randomization block fixed effects.

Appendix Table A7. Estimates of intervention effects by tercile of dropout risk and outcome horizon

	(1)	(2)	(3)	(4)
	Re-Enrolled or Graduated	Number of Credits Accumulated	Graduated	Transferred to Four-Year
Four Term Outcomes:				
Treatment x Bottom Tercile	.01	.614	.005	-.022
	(0.008)	(0.440)	(0.010)	(0.017)
Treatment x Middle Tercile	0	.314	-.004	.005
	(0.010)	(0.448)	(0.011)	(0.016)
Treatment x Top Tercile	.0076	.329	-.0093	-.0069
	(0.011)	(0.479)	(0.011)	(0.015)
P-value on *F*-test of Equal Effects	.712	.867	.603	.522
Control Mean - Bottom Tercile	.853	37.523	.707	.602
Control Mean - Middle Tercile	.785	35.461	.607	.453
Control Mean - Top Tercile	.649	31.489	.459	.323
Observations	21553	21553	21553	10534
Six Term Outcomes:				
Treatment x Bottom Tercile	-.009	.337	-.003	-.017
	(0.009)	(0.707)	(0.011)	(0.020)
Treatment x Middle Tercile	.002	.231	-.007	-.001
	(0.011)	(0.707)	(0.012)	(0.021)
Treatment x Top Tercile	-.0041	-.295	-.0091	-.014
	(0.014)	(0.753)	(0.014)	(0.020)
P-value on *F*-test of Equal Effects	.757	.81	.939	.852
Control Mean - Bottom Tercile	.91	45.4	.867	.651
Control Mean - Middle Tercile	.834	42.955	.776	.52
Control Mean - Top Tercile	.701	39.517	.621	.401
Observations	12879	12879	12879	6788

*** p<0.01 ** p<0.05 * p<0.10

Notes: Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses. Risk ratings terciles are defined within an institution. Column 4 only includes students at 2-year colleges.

Appendix Table A8. Estimates of intervention effects on academic outcomes by system

	(1)	(2)	(3)	(4)
	Re-Enrolled or Graduated, Four Terms after Intervention	Number of Credits Accumulated, Four Terms after Intervention	Graduated, Four Terms after Intervention	Transferred to Four-Year, Four Terms after Intervention
Panel A: VCCS Institutions				
Treatment Impact	-.005	.635	-.013	.004
	(0.014)	(0.457)	(0.015)	(0.014)
Control Mean	.672	22.847	.477	.325
Observations	4336	4336	4336	4336
Panel B: CUNY Institutions				
Treatment Impact	.002	-.453	-.008	-.02
	(0.009)	(0.410)	(0.011)	(0.015)
Control Mean	.785	32.098	.633	.597
Observations	7751	7751	7751	4064
Panel C: TX Institutions				
Treatment Impact	0.011	0.969*	0.005	
	(0.012)	(0.533)	(0.011)	
Control Mean	0.704	25.65	0.446	
Observations	5535	5535	5535	

*** p<0.01 ** p<0.05 * p<0.10
Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses. Column 4 only includes students at 2-year colleges.

Appendix Table A9: Estimates of intervention effects on academic outcomes by system, six term outcomes

	(1)	(2)	(3)	(4)
	Re-Enrolled or Graduated, Six Terms after Intervention	Number of Credits Accumulated, Six Terms after Intervention	Graduated, Six Terms after Intervention	Transferred to Four-Year, Six Terms after Intervention
Panel A: VCCS Institutions				
Treatment Impact	0.004	0.013	-0.004	0.013
	(0.014)	(0.608)	(0.014)	(0.018)
Control Mean	0.756	24.868	0.682	0.396
Observations	3095	3095	3095	3095
Panel B: CUNY Institutions				
Treatment Impact	-.015	-1.004	-.019	-.032**
	(0.011)	(0.665)	(0.012)	(0.016)
Control Mean	.801	36.671	.721	.635
Observations	5353	5353	5353	3693
Panel C: TX Institutions				
Treatment Impact	0.002	2.840*	-0.025	
	(0.034)	(1.570)	(0.037)	
Control Mean	0.838	29.039	0.820	
Observations	500	500	500	

*** p<0.01 ** p<0.05 * p<0.10
Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses. Column 4 only includes students at 2-year colleges.

Appendix Table A10. Estimates of intervention effects on academic outcomes by predicted dropout risk and system, four terms after intervention

	(1)	(2)	(3)	(4)
	Re-Enrolled or Graduated, Four Terms after Intervention	Number of Credits Accumulated, Four Terms after Intervention	Graduated, Four Terms after Intervention	Transferred to Four-Year, Four Terms after Intervention
Panel A: VCCS Institutions				
Treatment x Bottom Tercile	-.011	.954	-.008	-.03
	(0.020)	(0.714)	(0.025)	(0.027)
Treatment x Middle Tercile	.004	.678	-.011	.029
	(0.026)	(0.798)	(0.027)	(0.026)
Treatment x Top Tercile	-.009	.263	-.02	.014
	(0.028)	(0.870)	(0.024)	(0.021)
P-value on *F*-test of Equal Effects	.895	.828	.935	.253
Control Mean - Bottom Tercile	.843	24.855	.687	.489
Control Mean - Middle Tercile	.676	23.351	.48	.306
Control Mean - Top Tercile	.499	20.335	.263	.18
Observations	4336	4336	4336	4336
Panel B: CUNY Institutions				
Treatment x Bottom Tercile	.014	.322	.003	-.041
	(0.014)	(0.702)	(0.017)	(0.026)
Treatment x Middle Tercile	-.001	-.906	-.016	.001
	(0.016)	(0.708)	(0.019)	(0.025)
Treatment x Top Tercile	-.007	-.799	-.012	-.023
	(0.019)	(0.724)	(0.019)	(0.026)
P-value on *F*-test of Equal Effects	.612	.396	.711	.5
Control Mean - Bottom Tercile	.864	36.141	.752	.698
Control Mean - Middle Tercile	.824	32.976	.658	.596
Control Mean - Top Tercile	.667	27.187	.491	.497
Observations	7751	7751	7751	4064

Appendix Table A10, continued. Estimates of intervention effects on academic outcomes by predicted dropout risk and system, four terms after intervention

	(1)	(2)	(3)	(4)
	Re-Enrolled or Graduated, Four Terms after Intervention	Number of Credits Accumulated, Four Terms after Intervention	Graduated, Four Terms after Intervention	Transferred to Four-Year, Four Terms after Intervention
Panel C: TX Institutions				
Treatment x Bottom Tercile	.024	1.051	.024	
	(0.018)	(0.927)	(0.018)	
Treatment x Middle Tercile	-.004	.954	.011	
	(0.020)	(0.923)	(0.019)	
Treatment x Top Tercile	.014	.92	-.02	
	(0.021)	(0.928)	(0.018)	
P-value on *F*-test of Equal Effects	.588	.995	.201	
Control Mean - Bottom Tercile	.767	27.161	.489	
Control Mean - Middle Tercile	.707	26.436	.436	
Control Mean - Top Tercile	.636	23.358	.412	
Observations	5535	5535	5535	

*** $p<0.01$ ** $p<0.05$ * $p<0.10$

Notes: Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses. Risk ratings terciles are defined within an institution. Column 4 only includes students at 2-year colleges.

Appendix Table A11. Estimates of intervention effects (four terms after intervention) by school level

	(1)	(2)	(3)
	Re-Enrolled or Graduated, Four Terms after Intervention	Number of Credits Accumulated, Four Terms after Intervention	Graduated, Four Terms after Intervention
Treatment x 2 Year	.00098	.411	-.01
	(0.009)	(0.372)	(0.009)
Treatment x 4 Year	.011	.43	.0045
	(0.007)	(0.372)	(0.008)
P-value on *F*-test of Equal Effects	.389	.971	.229
Control Mean - 2 Year	.669	24.957	.433
Control Mean - 4 Year	.844	43.475	.73
Observations	21553	21553	21553

*** $p<0.01$ ** $p<0.05$ * $p<0.10$

Notes: Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses.

Appendix Table A12. Estimates of intervention effects (four terms after intervention) by advising model and institutional characteristics

	(1)	(2)	(3)
	Re-Enrolled or Graduated, Four Terms after Intervention	Number of Credits Accumulated, Four Terms after Intervention	Graduated, Four Terms after Intervention
Panel A: Impacts by Advising Model			
Treatment x Professional Advisor	.017	.821	-.003
	(0.015)	(0.711)	(0.015)
Treatment x Team or Segmented Advisors	.002	-.112	-.001
	(0.018)	(0.793)	(0.019)
Treatment x Connectors	-.003	.193	-.006
	(0.009)	(0.330)	(0.009)
Treatment x Automated Advising	.012	.673	.001
	(0.009)	(0.535)	(0.010)
P-value on *F*-test of Equal Effects	.589	.714	.976
Control Mean - Professional Advisor	.584	25.736	.343
Control Mean - Team or Segmented Advisors	.724	31.415	.51
Control Mean - Connectors	.784	27.96	.595
Control Mean - Automated Advising	.842	48.08	.74
Observations	21553	21553	21553
Panel B: Impacts by Institutional Characteristics			
Student-to-Counselor Ratio:			
Treatment x Stu:Counselor Ratio < 250:1	.013	.693	.001
	(0.010)	(0.427)	(0.011)
Treatment x Stu:Counselor Ratio > 250:1	-.003	-.082	-.01
	(0.008)	(0.369)	(0.008)
P-value on *F*-test of Equal Effects	.199	.17	.436
Control Mean - Stu:Counselor Ratio < 250:1	.786	36.239	.63
Control Mean - Stu:Counselor Ratio > 250:1	.784	37.386	.657

Appendix Table A12, continued. Estimates of intervention effects (four terms after intervention) by advising model and institutional characteristics

	(1) Re-Enrolled or Graduated, Four Terms after Intervention	(2) Number of Credits Accumulated, Four Terms after Intervention	(3) Graduated, Four Terms after Intervention
Other Texting Programs:			
Treatment x No Other Texting Programs	.0065	.27	.0046
	(0.009)	(0.499)	(0.010)
Treatment x Other Texting Programs	.00029	.17	-.014
	(0.009)	(0.318)	(0.009)
P-value on *F*-test of Equal Effects	.614	.865	.162
Control Mean - No Other Texting Programs	.838	48.807	.712
Control Mean - Other Texting Programs	.738	26.429	.589
Required Advising:			
Treatment x No Required Advising	0	.03	-.009
	(0.011)	(0.511)	(0.011)
Treatment x Required Advising	.004	.327	-.005
	(0.008)	(0.325)	(0.008)
P-value on *F*-test of Equal Effects	.757	.623	.781
Control Mean - No Required Advising	.751	41.333	.57
Control Mean - Required Advising	.807	34.159	.695
Student Support (composite measure from survey response)			
Treatment x Below Median Student Support	.006	.326	-.001
	(0.007)	(0.352)	(0.008)
Treatment x Above Median Student Support	.006	.575	-.006
	(0.009)	(0.386)	(0.010)
P-value on *F*-test of Equal Effects	.993	.634	.703
Control Mean - Below Median Student Support	.736	34.756	.522
Control Mean - Above Median Student Support	.803	34.928	.698
Observations	21553	21553	21553

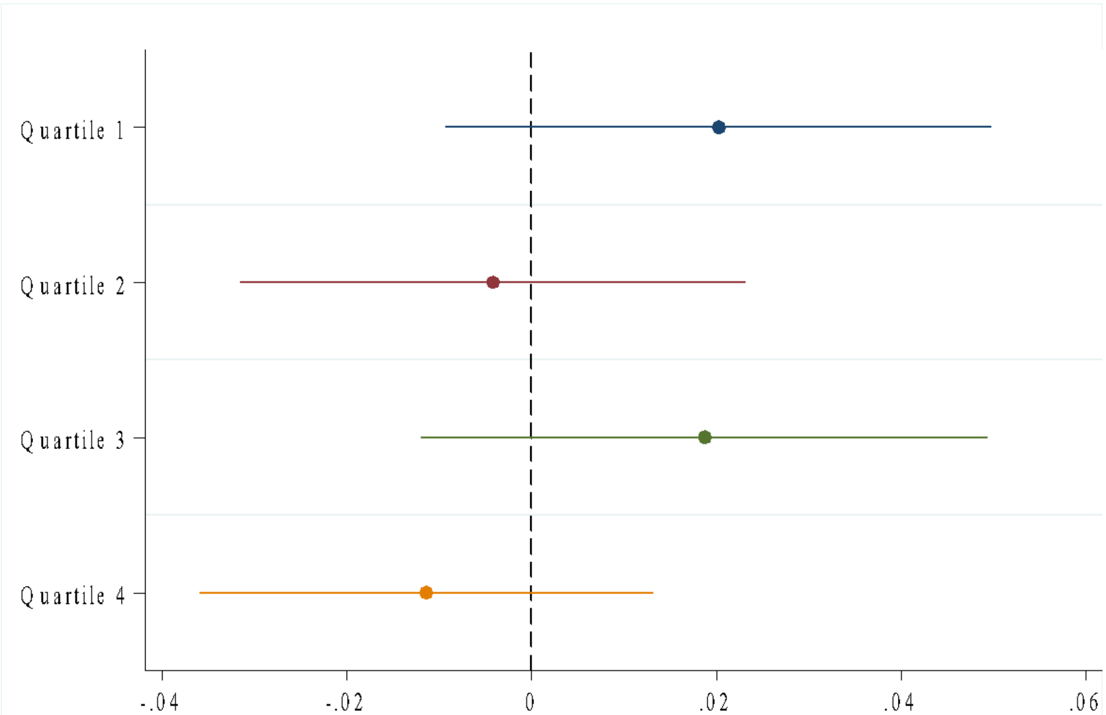
*** p<0.01 ** p<0.05 * p<0.10

Appendix Table A12, continued. Estimates of intervention effects (four terms after intervention) by advising model and institutional characteristics

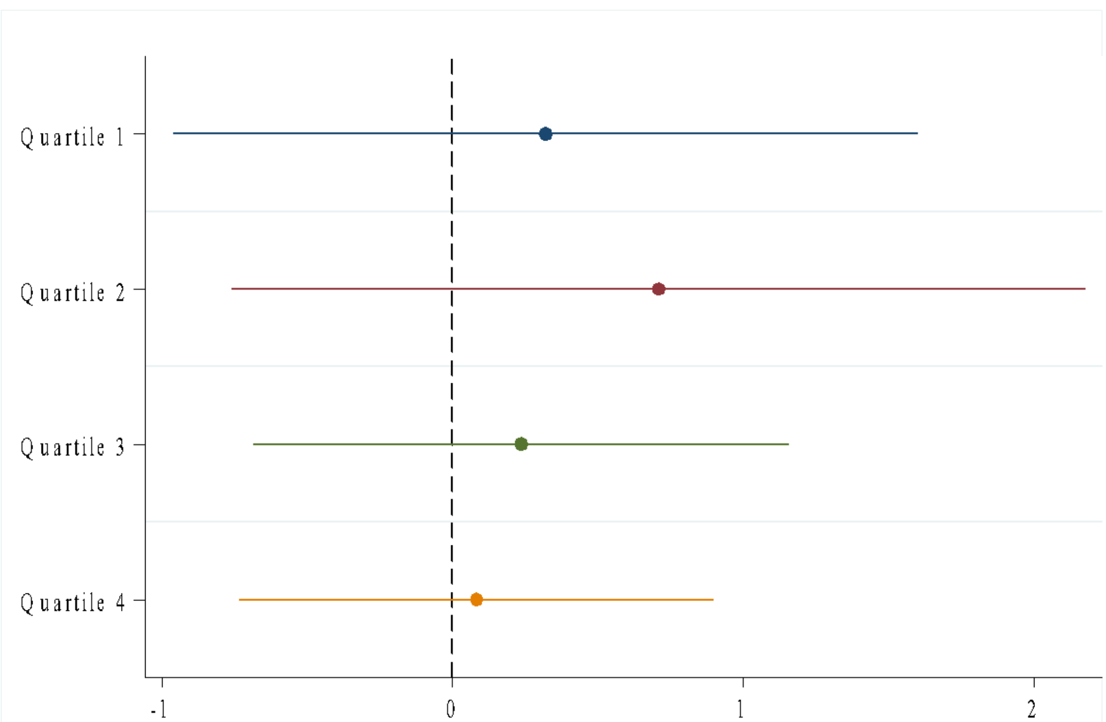
Notes: Estimates are from OLS/LPM models that include risk rating, randomization block fixed effects, and the following pre-treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Robust standard errors are reported in parentheses. Panel B reports effects based on responses to a survey sent to partner institutions in July 2020; representatives from 16 institutions responded to the survey (4 institutions did not respond). Students from the institutions that did not respond to the survey are excluded from Panel B. The composite measure of student support is based on a survey question which asked whether the following services were offered and how proactively they were offered: academic tutoring; assistance with course selection; assistance with transferring to a 4-year university; assistance with completing the FAFSA; emergency financial aid; other financial assistance (resolving financial holds, checking aid status); assistance with food or housing; and career exploration. The three possible responses and how they were coded numerically were: this was not offered (point value of 0); this was offered, students must seek this support/service out on their own (point value of 1); this was offered, campus delivered this support/service proactively to students (point value of 2). The point value of each of those responses was averaged at the institution level. The median of that mean measure of student support was then calculated across institutions and institutions were identified as having above or below median student support based on that number.

Appendix Figure A1. Estimates of intervention effects by quartile of advisor responsiveness

A. Re-Enrolled or Graduated, Four Terms after Intervention

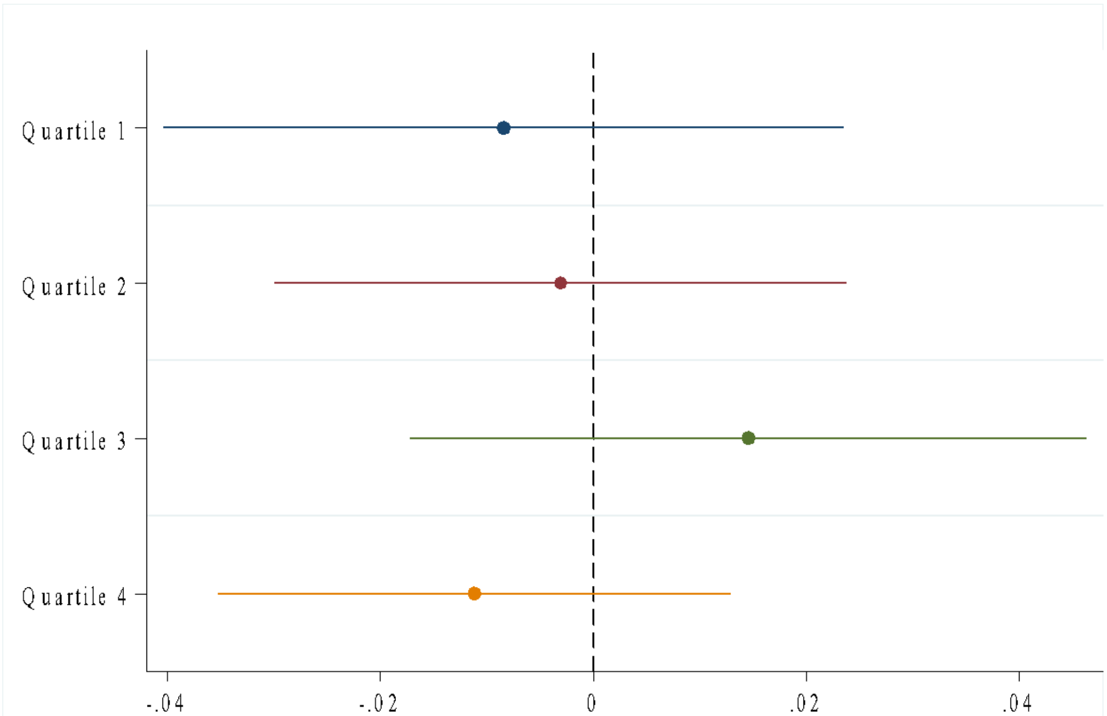


B. Cumulative Credits Completed, Four Terms after Intervention



Appendix Figure A1, continued. Estimates of intervention effects by quartile of advisor responsiveness

C. Earned Degree, Four Terms after Intervention



Notes: Each line reports coefficient estimates and 95% CIs from OLS/LPM models within each quartile group that include risk rating, randomization block fixed effects, and the following pre- treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), and transfer status at the start fall 2016, as well as continuous measures of cumulative credits completed, and the fraction of total credits attempted that were earned at the start of the intervention. Advisor responsiveness is measured as the share of student text messages that an advisor responded to at the institution by cohort level. Quartile 1 is the lowest advisor responsiveness (mean .44) and Quartile 4 is the highest advisor responsiveness (mean .82). Students from the pilot phase (launch Fall 2016) are excluded from this analysis as we do not have data on advisor responsiveness for that cohort. Additionally, students from OU and UWT are removed from this analysis as advisor responses were automated for those institutions.