Finishing the Last Lap: Experimental Evidence on Strategies to Increase College Completion for Students At Risk of Late Departure

Eric P. Bettinger
Stanford University School of Education and NBER

Benjamin L. Castleman
University of Virginia

Zachary Mabel
College Board

February 2019

ABSTRACT

Nearly half of students who enter college do not graduate and completion disparities by race and income are large. While the majority of efforts to increase degree attainment have focused on supporting students before or soon after they enter college, many students drop out after spending significant time in school. In this paper, we report pilot-year results from an experimental intervention we conducted across five states and nine broad-access, public colleges and universities to help students graduate. The intervention provided students late into college with personalized text messages that prompted them to identify goals associated with finishing their degree, encouraged them to connect with campus-based academic and financial resources, and reminded them of upcoming and important deadlines. We find little evidence of effects on academic performance or attainment in the full sample, although post-randomization anomalies unrelated to intervention efficacy at two institutions make results from this sample difficult to interpret and potentially downward-biased. In our preferred sample that excludes those institutions, the intervention decreased fall-to-spring dropout by 14 percent, from 17.5 to 15 percent. Among students in this sample at greatest risk of dropout based on their background and prior enrollment experiences, outreach increased degree completion after one year by 6 percentage points, or 38 percent.

The contents of this article were developed under grant R305N160026 from the U.S. Department of Education, Institute of Education Sciences. We are grateful for the ongoing collaboration of our postsecondary system and institutional partners in the design and implementation of this intervention. Thank you also to Chris Avery, Matt Chingos, David Deming, Albert Liu, Bridget Terry Long, Lashawn Richburg-Hayes, Silvia Robles, Michael Weiss and seminar participants at the EPPE Colloquium at the Harvard Graduate School of Education for helpful comments and suggestions on earlier versions of this paper. All errors, omissions, and conclusions are our own.

1. Introduction

The economic return to completing college is large and increasing (Autor, 2014; Avery & Turner, 2012; Carnevale, Jayasundera, & Gulish, 2016), but the likelihood of degree attainment among those who attend is lower than in previous decades: 55 percent of students who currently start college complete within six years of entry (Bound, Lovenheim, & Turner, 2010; Shapiro et al., 2016). Particularly concerning is the fact that low-income students and students of color are significantly less likely to graduate than their high-income and white peers, and these disparities have widened over time (Bailey & Dynarski, 2011; Chetty, Friedman, Saez, Turner, & Yagan, 2017). For example, the gap in bachelor's degree attainment between high- and low-SES students who attend four-year college within two years of completing high school exceeds 25 percentage points (U.S. Department of Education, 2015). With social mobility in decline in the United States and the payoff to degree attainment on the rise, increasing college completion rates among populations at high risk of dropout is an essential component of broader strategies to create more equitable opportunities for economic prosperity.

To date, the majority of efforts to address completion inequities have focused on supporting students before or soon after they enter college. For example, several interventions have focused on helping students apply to college, complete the cumbersome application for federal student aid, and overcome procedural obstacles to matriculation that arise before students arrive on campus (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012; Carrell & Sacerdote, 2013; Castleman & Page, 2015; Hoxby & Turner, 2013; Pallais, 2015). Considerable attention has also been devoted to improving the effectiveness of remediation policies for students who enter college academically underprepared (Bettinger & Long, 2009; Martorell & McFarlin, 2011; Scott-Clayton, Crosta, & Belfield, 2014).

Despite these investments, patterns of college dropout suggest that addressing the completion problem may also require supporting students long after they arrive on campus. For example, more than 40 percent of college students who do not earn degrees 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, and students of color are 1.5 times more likely to drop out at this point relative to White students (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, despite per-student public expenditures of almost $32,000 and average private investments of almost $40,000 by students and their families towards their college education.[1]

While the literature on early departure from college has identified significant barriers to completion, comparatively less is known about the obstacles to completion for upper-division students. One likely explanation is that the road to completion becomes increasingly self-directed as structured student supports 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 difficult to access. The psychic costs to 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.

[1] These estimates are based on results from Mabel and Britton (2017), 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.

These potential barriers suggest that providing students with information to simplify decision-making, guidance on where they can turn for help, and encouragement to persist in school could meaningfully increase degree completion and lower attainment gaps. However, most efforts to reduce late dropout to date have not provided students with proactive guidance and support. Instead, they have attempted to re-engage individuals who have already withdrawn, despite the fact that reconnecting with individuals who have left is difficult and has limited the efficacy of those efforts (Adelman, 2013).[2]

In this paper, we report on a low-cost, randomized intervention to increase completion among students who have earned substantial credits and are actively enrolled, but remain at risk of dropout. In partnership with Persistence Plus, a Boston-based company that designs and delivers student support interventions using behavioral science principles, we conducted the pilot phase of the Nudges to the Finish Line (N2FL) project during the 2016-17 academic year at nine broad-access, public higher education institutions in New York City, Virginia, Texas, Ohio, and Washington State. With our feedback, Persistence Plus developed a text messaging campaign which: (1) prompted students to identify goals associated with finishing their degree; (2) encouraged them to connect with campus-based academic and financial resources; (3) reminded them of upcoming and important deadlines; and (4) addressed feelings of stress, anxiety, and other psychological hurdles that could impede student progress. In seven of the sites the messages were fully automated: messages contained closed-ended prompts that students could respond to and receive additional information based on their responses. However, in these sites students did not receive any additional access to text-based advising as part of the intervention. In two other sites, students received the automated messages and also had access to a dedicated

[2] 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).

college advisor who could respond to their text replies and provide more intensive support as needed.

The intervention sample in the pilot year consisted of 3,804 students and is representative of students attending public, urban institutions in the United States. Students were eligible for the study 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. To examine whether the intervention produced differential effects according to students' risk of dropout, we developed dropout prediction models at each institution as a function of student attributes, academic performance, prior enrollment experiences, and financial aid receipt using data on previous cohorts of students. We then used the models to predict the probability of dropout for actively enrolled students and randomly assigned students to treatment arms within schools using a procedure that grouped individuals with similar predicted risk of dropout into randomization blocks.

Results from the pilot year indicate that outreach to students late into college can increase attainment. Although we find no evidence of differential effects on completion by predicted risk of dropout in the full sample, post-randomization anomalies unrelated to intervention efficacy at two institutions make results from the full sample difficult to interpret and potentially downward-biased. These challenges arose in part because we randomized students in late summer before fall enrollment had stabilized at all institutions. Figure 1 illustrates the timing of randomization relative to the start of the academic year and our intervention.[3]

[3] On average, we relied on preliminary fall registration lists generated three weeks before the start of term to randomize students, and we completed randomization 16 days before the start of term on each campus. This timeline was necessary to allow adequate time for data transfers between the research team, institutional partners, and text messaging platform in order to begin messaging students during the first week of classes.

Results from our preferred sample, which excludes the two institutions where we observe post-randomization anomalies, indicate large completion effects for students at high risk of dropout. Completion rates for students in the top tercile of dropout risk increased 6.1 percentage points, from 16.2 percent to 22.3 percent, after one year of intervention. We also find suggestive evidence of smaller, but still positive average treatment effects on intermediate student outcomes. For example, the intervention in our preferred sample increased the probability of fall-to-spring re-enrollment by 2.5 percentage points, representing a 4 percent increase over the control group mean. These impacts, while preliminary, were generated from an intervention that cost only $100 per student, including start-up costs, and compare favorably to cost-effective interventions that target students at earlier points along the degree pipeline and that are higher touch.

The remainder of this paper is structured as follows. In Section 2, we discuss 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 our research design, including the participating schools, study sample, predictive models of dropout, intervention components, randomization procedure, and empirical strategy. We present the study results in Section 4 and conclude in Section 5 with a discussion of our findings and their implications.

2. Literature Review: Obstacles to College Completion

The cost to completing college is substantial. Many students experience high time and effort costs to completion because they enter college academically unprepared (Bettinger, Boatman, & Long, 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).

Students also struggle to make consequential decisions about their educational experience because the 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, institutions typically target these interventions and resources to first-year students, and the impacts of early interventions fade out over time even though students continue to encounter complex tasks as they progress in school (Rutschow, Cullinan, & Welbeck, 2012; Visher, Weiss, Weissman, Rudd, & Wathington, 2012). 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).[4] 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.

Despite indications that these obstacles are particularly salient to students late into college, the causes of late dropout in particular and strategies to reduce its incidence are not well known. This is largely because most studies have examined the effects of intervening with students early in college and it is unclear if the factors that prevent students from finishing, or the importance of those factors, evolve over time. If the obstacles to completion for late dropouts are predominantly the product of limited information, complex decisions, and psychological factors such as cognitive overload and impatience, then low-cost interventions like N2FL may offer as effective a treatment to the late dropout issue as they have to earlier bottlenecks in college, such as summer melt or students failing to renew their financial aid [see Castleman, Schwartz, & Baum, (2015) and Lavecchia, Liu, & Oreopoulos (2014) for comprehensive reviews of this literature]. On the other hand, if the challenges 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 informational and behavioral interventions may have little impact on academic progress and motivate the need for more resource-intensive strategies

[4] 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.

to lower rates of late departure.[5] We shed light on the potential causes of late dropout by examining the impacts of offering students ongoing guidance and support designed to lower continuation costs and emphasize the future benefits to completion.

3. Research Design

We partnered with a diverse array of broad-access, public two- and four-year institutions across the country during the 2016-17 school year to provide additional support to upper-division students at risk of withdrawing from college. All of our partner institutions accepted 75 percent or more of the applicants that apply. Forty percent of students attending our partner institutions enrolled part-time, 40 percent received federal Pell Grants, and 61 percent were students of color. The average graduation rate within 150 percent time reported by our partner institutions was 36 percent. Of the nine institutions, three are community colleges in the City University of New York and Virginia Community College Systems. The remaining six are four-year public institutions in New York City, Texas, Ohio, and the State of Washington.

3.1. Eligibility Criteria and Sample

Degree-seeking students were eligible to participate in the study if they had: 1) registered to enroll in fall 2016 before the start of term, 2) an active cell phone number on record, and 3) completed at least 50 percent of the credits typically required for degree completion through summer 2016.[6] Although the goal of the intervention was to provide enhanced support to students at risk of late dropout, we established broad eligibility criteria during the pilot year to examine heterogeneity in treatment effects by predicted risk of dropout.

[5] There is some empirical evidence that students experience these types of barriers as well. In their study of the late dropout phenomenon, Mabel and Britton (2017) find that late dropouts have a harder time passing coursework as they progress in school compared to graduates and may only need to pass 3-4 additional courses in their major to graduate.

[6] 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.

Based on the eligibility criteria above and the size of enrollments at our partner institutions, we recruited 3,804 students to participate in the study. In columns 3-6 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, urban public institutions nationally, we report analogous statistics for a nationally representative sample using data from the National Postsecondary Student Aid Study of 2012 (NPSAS:12) in column 1.[7] In column 2, we report descriptive statistics for all students who met the minimum credit threshold at our partner institutions to show the extent to which the study sample generalizes to the broader set of upper-division students attending those schools.

Across both treatment and control groups, approximately 40 percent of students in the study sample are male, 55 percent are students of color, and the average age of students at the start of the intervention was 24.8 years. On average, students in the study sample had a 29 percent chance of dropout according to the prediction models we developed using historical data from partner institutions. Figure 2 shows the distribution of predicted dropout probabilities in the study sample. The standard deviation of the predicted dropout risk distribution is 18 percentage points, the interquartile range spans 16 to 38 percent, and the minimum and maximum values are 0 and 97 percent, respectively. Section 3.2 provides additional details about the models we developed to predict the likelihood of dropout for study participants.

Our experimental sample is fairly representative of the national student population attending broad-access, urban public institutions with respect to sex (40 percent male versus 44 percent) and academic achievement (average GPAs are 2.96 and 2.85 among students in the

[7] We defined broad-access institutions to be two- and "open admission" or "minimally selective" four-year institutions in the 2012 National Postsecondary Student Aid Study (NPSAS:12). Institutional selectivity is reported directly in NPSAS:12 and is based on an index of five measures: 1) whether the institution has any academic admission requirements, 2) number of applicants, 3) number of students admitted, 4) whether the institution requires college entrance exam scores, and if so, 5) the interquartile range of exam scores for admitted students.

study and national samples, respectively). However, students attending four-year institutions are overrepresented in our sample (66 percent 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 (24.8 years versus 27.1 years), and students of color are also slightly overrepresented in our sample (55 percent versus 49 percent). The experimental sample is also representative of all credit-eligible students at our partner institutions on most observable dimensions, although students in the study sample were at slightly greater risk of dropout on average according to our prediction models (29 percent versus 22 percent). This difference is due to the fact that the experimental sample contains a larger share of students attending community colleges (34 percent versus 26 percent of all credit-eligible students), and risk of dropout is higher on average among two-year college-goers.

In order to begin messaging students at the start of fall 2016, we randomized students in late summer before fall enrollments finalized. This timeline was necessary to allow for adequate time for data transfers between the research team, institutional partners, and text messaging platform. Unfortunately, as shown in panel A of Figure 3, the timing of randomization produced an initial fall enrollment imbalance between treated and control students at one institution that we attribute to chance. In expectation, randomization should have yielded equivalent fall enrollment rates in treatment and control groups; however, at one site (college 1) treated students were 8 percentage points more likely to enroll in fall 2016. At three standard deviations above the expected null difference, the probability of observing an imbalance of this magnitude is 0.2 percent. The fall enrollment imbalance is also not a function of message outreach, which did not begin until after classes started, but it likely contributes to the downstream effects on fall completion (+5 percentage points) and spring re-enrollment (-5 percentage points) we observe at this site. In our preferred sample we exclude this site to avoid upwardly biasing our treatment effect estimates on degree completion and downwardly biasing our treatment effect estimates on re-enrollment.

At a second site (college 2 in Figure 3), treated students were 12 percentage points less likely to graduate in fall 2016 for reasons that are also plausibly unrelated to the effectiveness of the intervention. High-risk students at this institution were substantially less likely to enroll in fall 2016 (a similar issue as college 1 of differential enrollment rates post-randomization, although in the opposite direction). In Appendix Table A1, we show that students in the top tercile of dropout risk were 7 percentage points less likely to enroll initially and the fall degree effect (-16 percentage points) is largest for this group.[8]

Historical completion rates at college 2 also indicate the negative "effect" on fall completion is driven by an atypically high completion rate among control group students, not a lower completion rate among treated students. The completion rate in fall 2016 for treated and control group students was 25 percent and 38 percent, respectively, whereas the completion rate among credit-eligible students in each of the previous two pre-intervention terms was 22 percent.[9] The inflated completion rate among control group students at college 2 may have occurred because total credit attainment is a noisy proxy for proximity to completion (Complete College America, 2011; Zeidenberg, 2015). As a result, control group students at college 2 may have been closer to completion than their treatment group peers on average, even though both groups completed a similar number of credits on average at the start of fall 2016. This issue was corroborated by another institution in the same system as college 2 subsequent to the start of

[8] The overall effect on fall completion declines by 25 percent when we exclude the top tercile of students.

[9] As shown in Figure 3 and Appendix Figure A1, we also find no evidence of negative impacts on other outcomes, such as credits completed or GPA, at any sites (including college 2) which we would expect to be associated with declines in degree completion. In column 4 of Appendix Table A2, we further show that the point estimate on fall completion at college 2 is the only negative, statistically significant outlier compared to the effect estimates at all other colleges.

intervention. Advisors at that campus noted discrepancies between the measures of credit attainment we observe in the data and alternative measures we do not observe that the institution uses to track academic progress. Taken together, the evidence suggests that the magnitude and direction of the fall completion impact at college 2 are also driven by factors unrelated to the intervention.

The anomalies at college 1 and college 2 influence the estimated treatment effects. For example, across all sites the effect on fall-to-spring re-enrollment is only negative at college 1, as shown in panel C of Figure 1. As a result, if message outreach truly increased the likelihood of persistence, including college 1 in the sample will attenuate the estimated treatment effect.[10] We therefore present estimates from the full sample as well as for samples in which we exclude college 1, or college 2, or both institutions. Excluding either one of these sites yields evaluation samples of 3,304 students; excluding both yields a sample of 2,804 students. Our preferred estimates are from the latter sample that is free of initial enrollment imbalances and control group aberrations.

3.2. Predictive Models of Dropout

To examine which upper-division students stand to benefit from targeted outreach and support, we developed logistic regression dropout prediction models at each partner institution using data on historical cohorts of students. We modeled 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

[10] Likewise, college 2 has an outsize influence on the estimates of degree impacts among high-risk students in the full sample because: 1) college 2 is a two-year institution where students are at greater risk of late dropout than at four-year colleges on average, and 2) the "effect" on fall completion at college 2 is more than twice as large in magnitude as degree impacts across all other sites.

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.[11]

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. However, 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 take the following general form:

(1) $\Pr(Y_i \mid X_i) = P(\alpha + \gamma * Pre50_i + \omega * At50_i + \delta * Post50_i)$, where $(j) = \frac{1}{1+e^{-(j)}}$.

In this model, $Pre50_i$ is a vector of fixed student attributes and time-variant measures before students completed one-half of the credits typically required for graduation. Where available, the 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,

[11] Due to cost constraints, recruitment was limited to 500 students per campus during the pilot year. At institutions where the sample of eligible students exceeded this number, we also used the dropout predictions to exclude the most inframarginal students from the study sample. Restrictions were employed at 7 of the 9 participating schools. Excluded students had dropout probabilities below 10 percent or above 75 percent on average. The maximum lower-bound for exclusion was set to 15 percent (at two institutions), and the minimum upper-bound was set to 60 percent (also at two institutions).

$Pre50_i$ 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. $At50_i$ is a vector of characteristics in the term students completed one-half of their credit requirements. Where available, the vector includes the following measures: number of attempted credits, cumulative GPA, the cumulative proportion of attempted credits that were earned, and the amount of financial aid received. $Post50_i$ contains measures of enrollment experiences and financial aid receipt after surpassing the one-half credit threshold analogous to those captured in the vector of $Pre50_i$ predictors.

Our preferred models clearly 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 dropout ranged from 0.75-0.875 across the models.[12] Across all institutions we also observed similar model performance in both the development and validation samples. Students in the experimental sample who graduated in 2016-17 were also at lower risk of dropout on average compared to students who did not graduate. The average predicted probability of dropout in the control group was 25.5 percent among students who graduated compared to 30.4 percent among students who did not graduate.

[12] Subsequent to intervention launch, we also developed random forest classification models to compare against the performance of the logistic regression models used in this study. Logistic regression and random forest models performed very similarly. Using random forests 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) on average. The risk ratings generated by the two modeling approaches also correlate around 0.90 or higher.

In Table 2, we report descriptive statistics of the full study sample by tercile of predicted dropout risk.[13] 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 95 percent of their attempted credits prior to intervention launch, whereas top-tercile students completed 89 percent of their attempted credits. High-risk students were also more likely to attend community college and to have transferred into their current institution. The average risk rating among community college students was 42 percent, 17.5 percentage points higher than the predicted risk of dropout for four-year enrollees in the study sample; as a result, 61 percent of top-tercile students attended a community college compared to 11 percent of bottom-tercile students. High-risk students were also more likely to be older, male, and students of color.

3.3. Intervention Design

Students randomly assigned to treatment received automated text messages during the 2016-17 academic year. To inform the intervention design, Persistence Plus conducted student focus groups and interviewed school leaders at each participating institution during spring and summer 2016. Persistence Plus also spoke with school staff during fall 2016 to inform the development of message content and timing of message delivery in spring 2017. From our review of existing literature and the interviews and focus groups Persistence Plus conducted, we hypothesized that students at risk of late dropout would be more likely to misunderstand (or have little knowledge of) their academic requirements to graduate and benefit considerably from academic advising and supports, such as tutoring services. We also posited that late dropouts live busy lives which: 1) makes searching for support on campus difficult, 2) increases the likelihood of forgetting important deadlines, such as re-applying for financial aid and registering for

[13] The range of risk ratings in the bottom, middle, and top tercile are: 0-0.18, 0.18-0.32, and 0.32-0.97, respectively. The respective means by tercile are 0.13, 0.24, and 0.50.

courses, and 3) incites feelings of stress, anxiety, and frustration which can make students concentrate on immediate continuation costs over the future benefits to earning a degree.

Persistence Plus, in concert with our research team, designed the message campaign to address each of these potential barriers to completion. 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. For example, the following message encouraged students to use tutoring resources: "Many students benefit from the excellent tutors in the [CAMPUS LOCATION]. Make an appt. using [CAMPUS RESOURCE] to make the best use of your time". 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). For example: "Priority filing for the FAFSA is Fri Mar 31. Filing on time guarantees you get the max $ possible. When & where do you plan to complete the FAFSA?". A third set of messages, like the following example, leveraged psychological principles to help students set task-related goals, manage their time during the school year, and reduce their stress levels: "Many studies show that writing about what stresses you out can help you handle those feelings. Want to try it out?". Lastly, a fourth set of messages prompted students to identify their reasons for pursuing a degree and reminded students at stressful times during the semester of the responses they provided to sustain their commitment to graduation (Clark, Gill, Prowse, & Rush, 2017). A representative sample of the messages treated students received is provided in Appendix B.[14]

[14] We provide a representative sample because the complete list is almost 200 messages long, but the full list is available upon request.

Students received automated messages approximately three times per week for the duration of the academic year. Approximately half of the messages were interactive, prompting students to respond. Interactive messages were designed to encourage student engagement and to personalize follow-up messages to students. If students responded with one of the keywords the message prompted, they would receive additional, more customized content. For example, during the spring term students who reported uncertainty about their remaining math requirements received the following message: “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”. Students who replied “No” or “Unsure” received this response: “We don't want to see any missed courses derail your plans for graduation. Talk to your advisor or the [NAME OF ADVISING CENTER] in [ON CAMUPS LOCATION] soon about this issue”.

We also customized non-interactive messages to each institution and according to students’ background characteristics and prior academic record. For example, messages that reminded students to refile their FAFSA were customized to the deadlines and financial aid resources available at each institution, while messages about receiving credit for courses taken at other institutions were delivered to transfer students only.

We observed a relatively high level of engagement with the message campaign among students assigned to treatment on each campus. On average, students assigned to treatment received 45 messages and 69 percent of treated students responded to an interactive message. Treated students responded to 7 messages on average throughout the school year. Because only about one-half of the messages students received prompted interaction, students replied to approximately one-third of the messages that solicited their response.

At all participating institutions, 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.

As the sample messages above indicate, one goal of the intervention was to make it easier for students to engage with staff and access supports on campus. At one college we observe the on-campus resources students utilized in fall 2016 because they are required to swipe their ID cards when they access support services. We use this data to evaluate whether message outreach increased the likelihood and frequency with which students sought out campus supports.[15]

We present the results of this analysis in Table 3. The evidence suggests the messages had their intended affect: treated students made greater use of on-campus supports than their control group peers. In column 1, we present effects on whether students made any use of on-campus resources. None of the point estimates are significant and three of the four coefficients are negative, indicating that message outreach did not influence whether or not students made use of on-campus supports. However, estimates of effects on the amount of supports students received in column 3 are consistently positive and the coefficients are large and significant on total resource usage and the amount of tutoring support received. On average, students who

[15] We also examined whether accessing additional campus resources mediates the effects on academic progress by estimating two-stage least squares (2SLS) specifications of resource usage on achievement and attainment. The results of this analysis are inconclusive because: 1) treatment assignment is a weak instrument for resource use (the F-statistics on all of the first-stage regressions are below 6), and 2) the campus resource data is from college 2 where we have reason to believe that estimated effects on academic outcomes are spurious. We therefore do not present results of this analysis, although they are available upon request.

received text messages accessed 1.2 additional hours of support in fall 2016 off a baseline of 2.5 hours for control group students. Approximately half of this increase is attributed to greater use of tutoring services. Treated students received 35 minutes of tutoring support on average during the semester, which represents a fourfold increase over the control group mean.

3.4. Data and Outcome 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.[16]

We use this data in three ways. First, as discussed above, we used the historical data provided by each institution to develop school-specific dropout prediction models. 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 and performance during the 2016-17 school year. We report on five outcome measures over the duration of the message campaign: whether students re-enrolled to spring 2017, the number of credits attempted in spring 2017, the number of credits completed in 2016-17, students' GPA in 2016-17, and whether students graduated in 2016-17.

3.5. Randomization Procedure and Baseline Equivalence

[16] Degree receipt at this time is determined by the records our institutional/system partners maintain. In the future, we will also be able to observe enrollment at non-partner institutions from National Student Clearinghouse records which our partners routinely collect.

To investigate whether impacts of message outreach varied with 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. We implemented this procedure by predicting the probability of dropout for currently enrolled students using the dropout models we developed. 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.

To inform future research, we also tested out different message variants as part of this study. As a result, at most institutions we randomly assigned students to one of three treatment arms: a control condition and two variants of the treatment group which received slightly different messages.[17] We grouped students into triads for this reason and then randomly assigned students within risk groups to one of the three treatment arms. Of the 3,804 students in the full experimental sample, we randomly assigned 2,526 students to a treatment arm and 1,278 students to the control condition. In all analyses we aggregate treated students into a pooled treatment group, as we do not observe evidence of differential effects by message type.

In column 6 of Table 1, we show that random assignment appears to have created equivalent groups of students in the treatment and control conditions. Analogous results for the restricted samples are similar and reported in Appendix Table A3. Although treated students were 2.5 percentage points (6 percent) more likely to transfer into their current institution than the control group, this is the only significant difference we detect among the thirteen covariate balance tests reported. We also examined whether the full set of student covariates jointly predict whether students are assigned to treatment. The p-value associated with the F-test for joint

[17] For example, at three campuses we randomly assigned one treatment group to receive only messages intended to address informational and procedural obstacles to completion. The other treatment group received only messages intended to address psychosocial barriers to completion.

significance is 0.814. We conclude that randomization achieved baseline equivalence, although we examine the stability of effects by estimating models with and without the inclusion of pre-treatment covariates.

3.6. Empirical Strategy

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

$$Y_{ib} = \alpha + \beta T_{ib} + \gamma(T_{ib} * D_{ib}) + \tau D_{ib} + \delta_b + \zeta X_{ijb} + \varepsilon_{ijb}, \tag{2}$$

where Y_{ib} is one of the five 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. D_{ib} is the mean-centered risk rating assigned to each student from the dropout prediction models we developed. To facilitate interpretation of the estimates, we scale the risk rating by a factor of 10 when estimating equation (2). The coefficients of interest in equation (2) are β and γ. β captures the estimated effect of being assigned to receive message outreach for students with average risk of dropout and γ represents the marginal treatment effect per 10-point increment in risk above the mean. δ_b denotes randomization block fixed effects. We do not include campus fixed effects in the model, as time-invariant differences across campuses are already controlled for through the block dummies. To increase the precision of our estimates, X_{ib} is a vector of student-level covariates comprised of 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 age, cumulative credits completed, cumulative GPA, and the fraction of total credits attempted that students earned at the start of fall 2016. ε_{ib} is a student-specific random error term; in all results we report robust standard errors that allow for heteroskedasticity in the error term.

4. Results

4.1 Impacts on Degree Completion

We present impacts of the intervention on degree completion in fall 2016, spring 2017, and throughout the 2016-17 school year in Table 4. In columns 1-3, we report effect estimates for all students in each sample who completed at least one-half of the credits typically required for graduation. In columns 4-6, we report effect estimates for the subset of students in each sample in proximity to degree completion in 2016-17, which we define as students with expected cumulative credit totals at the end of spring 2017 that fulfill the minimum published credit requirements for degree completion at each institution.[18] In Appendix Table A4, we report analogous results from models that exclude pre-treatment covariates. The point estimates across all samples and outcomes are nearly identical from models with and without inclusion of covariates, which reinforces that randomization created balanced treatment and control groups when treatment assignments were made.

Across all four samples, we find no evidence of average treatment effects on degree attainment one year following intervention for all students and for students in proximity to completion. For example, the point estimates in columns 1-3 are all near-zero and none are statistically significant. However, these null effects may be due to the fact that inframarginal students would be expected to progress towards graduation regardless of whether or not they

[18] We constructed expected cumulative credit totals for each student according to the number of cumulative credits they had earned at the start of fall 2016 and the number of credits they attempted in fall 2016. The latter should not have been influenced by the intervention (and we find no evidence that it was) because message outreach began after classes started. Specifically, the measure equals: [cumulative credits earned at the start of fall 2016 + 2*(number of credits attempted in fall 2016)] for institutions on the semester system and [cumulative credits earned at the start of fall 2016 + 3*(number of credits attempted in fall 2016)] for institutions on the trimester system.

received message outreach and the risk of dropout was low for many students in the study sample.

In the first row of each panel in Table 4, we therefore report estimates of effect heterogeneity by predicted risk of dropout. In columns 1 and 4, we report impacts on completion in fall 2016. The coefficients on the interaction term are negative but not significant in the full sample (panel A) and when only college 1 is excluded (panel B). However, the inflated fall completion rate among control group students at college 2 obscures positive impacts on fall completion for high-risk students across all other campuses, and as a result, the estimates are positive when college 2 is excluded from the sample in panels C and D. In our preferred estimates in panel D, the coefficient on the interaction term is significant at the 0.05 percent level and implies that the probability of graduation in fall 2016 increased by 0.9 percentage points per 10-point increase in dropout risk above the mean.

In columns 2 and 5, we also find evidence across all samples that higher risk students were more likely to graduate in spring 2017. The coefficients on the interaction term imply that for every 10-point increase in dropout risk above the mean, the probability of graduation in spring 2017 increased by 1.1-1.4 percentage points among all students (column 2) and by 2.4-3.1 percentage points among students in proximity to graduation (column 5). All of the estimates in column 2 and column 5 are significant at the 0.10 and 0.05 percent level, respectively.

Excluding college 2 from the sample therefore reveals evidence of effects on completion in both the fall and spring terms for high-risk students. In our preferred results in panel D, the estimate in column 3 indicates that degree attainment one year following intervention increased by 2.3 points per 10-point increment in dropout risk among students who received message outreach. For students in proximity to completion at the start of the intervention, message outreach increased the probability of completion by 3.4 percentage points per 10-point increment in dropout risk. These estimates translate into large impacts for high-risk students. For example, completion rates for students in the top tercile of dropout risk–i.e., students predicted to have at least a 33 percent chance of dropping out–increased by 6.1 percentage points, from 16.2 percent to 22.3 percent, after one year of intervention. This represents a 38 percent increase in completion relative to the control group.

4.2 Impacts on Intermediate Outcomes

In Table 5, we present estimates of intervention impacts on re-enrollment in spring 2017, the number of credits attempted in spring 2017, the number of credits completed in 2016-17, and GPA in 2016-17 in columns 1-4, respectively.[19] Unlike the impacts on completion, we find suggestive evidence of small, but positive average treatment effects on re-enrollment to spring 2017 and credit attainment throughout the 2016-17 academic year. In the full sample, all of the coefficients in row 2 of panel A are positive and the estimate on credit attainment (0.53) is significant at the 0.10 level. Furthermore, when we exclude college 1 from the sample in panel B, the results suggest that message outreach increased the likelihood of re-enrollment in spring 2017 by 2.9 percentage points (4 percent over the control group mean). We also find suggestive evidence of small, but positive average treatment effects in our preferred estimates in panel D. The estimates on re-enrollment to spring 2017 (2.5 percentage points), credits attempted in spring 2017 (0.5 credits), and credits completed throughout the 2016-17 academic year (0.7 credits) are all significant at the 10 percent level.[20]

[19] We report analogous estimates from models that exclude pre-treatment covariates in Appendix Table A5.
[20] It is noteworthy that the estimate on re-enrollment in Panel D is similar to the one in Panel B. Because the impact on re-enrollment is robust to the exclusion of college 2, the average impact on fall-to-spring enrollment in Panel B appears to capture real gains in academic progress and not a mechanical effect arising from higher rates of non-completion among treated students in fall 2016.

However, we find no evidence in Table 5 that higher-risk students benefited more from message outreach on these intermediate outcome measures.[21] The coefficients on the interaction terms in columns 1-4 are small in magnitude and none are positive and statistically significant at conventional levels, including in our preferred estimates in panel D. If the goals of the message campaign were limited to increasing short-term persistence and credit completion, then our risk ratings would not identify students most likely to benefit from additional outreach and support. We interpret this as evidence that the targeting value of predictive modeling can be sensitive to the degree of alignment between the outcome(s) used to determine student risk status and the outcomes(s) used to measure intervention efficacy.

5. Discussion

Pilot year results of the N2FL intervention indicate that many students who are within reach of college graduation stand to benefit from more outreach. Although we find little evidence of impacts on academic performance or degree attainment in the overall sample, the intervention increased completion substantially for high-risk students according to the dropout models we developed. Across all samples, the probability of graduation in spring 2017 increased by 1.1-1.4 percentage points per 10-point increase in dropout risk above the mean. In our preferred sample, we find effects on degree completion for high-risk students in both the fall and spring terms that translate into large impacts on degree completion one year following intervention. For students in the top-tercile risk group with at least a 33 percent chance of dropout according to our prediction models, the intervention increased degree completion after one year by 6.1 percentage points, or 38 percent over the control group mean.

[21] In Appendix Table A5, we report estimates of heterogeneous effects on intermediate outcomes by unidimensional student attributes analogous to Table 5. Of the 20 hypothesis tests we conducted (five subgroups by four outcomes), only one group difference is statistically significant. The average treatment effect on re-enrollment to spring 2017 is driven entirely by students with cumulative GPAs below 3.0 at the start of intervention. However, we interpret this result cautiously since one in 20 statistical tests at the $\alpha = 0.05$ level is expected to be a false positive.

Our results compare favorably to interventions that serve students at earlier points in the college process. For example, in our preferred sample, we find that text message outreach increased the probability of fall-to-spring re-enrollment by 2.5 percentage points on average, which is similar to the 3 percentage point increase in enrollment that Castleman and Page (2015) find from messaging college-intending high school graduates during the summer between high school and college. The degree effects for high-risk students are also similar to impacts found from a randomized evaluation of InsideTrack, a more intensive student coaching program that serves mostly non-traditional students (Bettinger & Baker, 2014). As a result, the return on investment to providing high-risk students within a few semesters of graduating with low-cost support may be large, although it remains unclear whether the impacts on attainment we find after one year represent brief or lasting gains. We intend to examine this question in future research by tracking students over multiple years.

A second question that emerges is what components of the messaging campaign increased student attainment. Our findings indicate that the intervention substantially increased the use of campus resources, but it remains unclear whether this was a channel through which the impacts on persistence and attainment were realized. To further investigate possible mechanisms, we are conducting a follow-up study at 20 broad-access, public institutions. The results will provide experimental evidence on the impacts of message outreach on persistence and completion over a longer time horizon and at scale for upper-division students at risk of late dropout. By differentiating the message topics that treated students receive in this next phase of research, we also intend to examine whether the barriers to completion for upper-division students are primarily academic or financial in nature. Through this ongoing research agenda, we hope to offer empirical evidence on the barriers most obstructive to upper-division students so that institutions can develop scalable interventions that increase completion rates among late-state, at-risk students.

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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 institutions

	(1)	(2)	(3)	(4)	(5)	(6)
			Experimental Sample			
	NPSAS Sample	All N2FL Students w/ ≥ 50% of credits earned	All Students	Treated Students	Control Students	T-C Difference
Male	0.438	0.409	0.396	0.390	0.408	-0.013
Black	0.175	0.147	0.185	0.194	0.167	-0.003
Hispanic	0.220	0.198	0.244	0.257	0.219	0.002
White	0.507	0.436	0.389	0.369	0.429	0.015
Race other	0.098	0.136	0.121	0.117	0.128	-0.013
Race missing	0.000	0.084	0.062	0.065	0.058	-0.002
Age	27.1	23.7	24.8	25.2	24.1	0.218
Cumulative GPA	2.85	3.09	2.96	2.97	2.93	0.023
Attended urban institution	1.000	0.675	0.788			
Enrolled in public 2-year institution	0.781	0.260	0.343			
Enrolled in public 4-year institution	0.219	0.740	0.657			
Transferred into current school		0.403	0.473	0.498	0.423	0.025**
Share of total attempted credits earned through spring 2016		0.943	0.937	0.938	0.936	0.004
Cumulative credits earned through spring 2016		82.8	80.2	79.9	80.9	-0.433
Predicted risk of dropout		0.223	0.289	0.290	0.288	-0.000
Enrolled in fall 2016			0.963	0.964	0.962	0.006
Number of students	58,410	19,942	3,804	2,526	1,278	3,804

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

Notes: The data in column 1 is from the National Postsecondary Student Aid Study of 2012 (NPSAS:12) and is restricted to students attending public, urban two- and non-selective four-year institutions. Summary statistics in column 1 are calculated using survey sampling weights. The data in columns 2-6 are from N2FL partner institution administrative records. Means are reported in columns 1-5. Estimates of post-randomization balance are reported in column 6 from OLS/LPM models that include randomization block fixed effects.

Table 2. Pre-treatment characteristics of experimental sample by tercile of predicted dropout risk

	(1) Bottom Tercile	(2) Middle Tercile	(3) Top Tercile
Male	0.378	0.045**	0.052**
Black	0.145	-0.014	0.031*
Hispanic	0.248	0.022	0.020
White	0.433	-0.009	-0.039**
Race other	0.123	0.002	0.000
Age	23.43	1.670***	3.041***
Cumulative GPA	3.113	-0.103***	-0.497***
Transferred into current school	0.509	0.036**	0.066***
Share of total attempted credits earned through spring 2016	0.953	-0.011**	-0.060***
Cumulative credits earned through spring 2016	93.61	-5.654***	-5.940***
Attended public, two-year institution	0.110	0.205***	0.496***
Number of students	1,268	1,268	1,268

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

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 in rows 1-10 include school fixed effects. Estimates in row 11 exclude school fixed effects. The range of risk ratings in the bottom, middle, and top tercile are: 0-0.18; 0.18-0.32; and 0.32-0.97, respectively. The respective means by tercile are 0.13, 0.24, and 0.50.

Table 3. Estimates of intervention effects on campus resource use in fall 2016 at college 2

	(1)	(2)	(3)	(4)
	Used Resource		Amount Used (in Minutes)	
	Effect Estimate	Control Mean	Effect Estimate	Control Mean
All Resources	-0.034	0.778	71.205**	151.80
	(0.040)		(32.088)	
Tutoring	0.037	0.054	35.125**	8.53
	(0.025)		(17.391)	
Academic Advising	-0.014	0.455	4.195	39.19
	(0.046)		(7.586)	
Financial Advising	0.043	0.317	2.200	21.34
	(0.046)		(5.112)	
Counseling/Health Services	0.019	0.012	10.969	1.16
	(0.014)		(7.802)	
Career Advising	0.009	0.054	3.485	4.69
	(0.022)		(3.632)	
Other	-0.027	0.521	15.114	77.38
	(0.047)		(16.924)	

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

Notes: The sample is limited to 500 students at one partner institution (college 2) where on-campus resource use is observed. Effect 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 age, cumulative credits completed, and the fraction of total credits attempted that were earned at the start of fall 2016. Robust standard errors are reported in parentheses.

Table 4. Estimates of intervention effects on degree completion by predicted risk of dropout

	(1)	(2)	(3)	(4)	(5)	(6)
	All Students			Students in Proximity to Graduation		
	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17
A. Full Sample						
Treatment x Predicted Dropout Risk	-0.005	0.011*	0.005	-0.004	0.024**	0.007
	(0.005)	(0.006)	(0.007)	(0.034)	(0.010)	(0.012)
Average Treatment Effect	-0.009	0.006	-0.003	-0.052	-0.000	-0.014
	(0.010)	(0.013)	(0.015)	(0.056)	(0.021)	(0.023)
Control Group Mean (at avg. risk = 0.288)	0.111	0.211	0.322	0.313	0.292	0.446
Observations	3,804	3,804	3,804	1,038	2,567	2,567
B. Excluding College 1						
Treatment x Predicted Dropout Risk	-0.004	0.012*	0.008	-0.012	0.025**	0.009
	(0.006)	(0.006)	(0.008)	(0.038)	(0.011)	(0.013)
Average Treatment Effect	-0.017	0.003	-0.014	-0.054	-0.002	-0.022
	(0.011)	(0.014)	(0.016)	(0.067)	(0.023)	(0.025)
Control Group Mean (at avg. risk = 0.293)	0.116	0.221	0.337	0.337	0.305	0.466
Observations	3,304	3,304	3,304	849	2,211	2,211
C. Excluding College 2						
Treatment x Predicted Dropout Risk	0.007	0.013*	0.019***	0.028	0.028**	0.030**
	(0.004)	(0.007)	(0.007)	(0.038)	(0.012)	(0.013)
Average Treatment Effect	0.010	0.001	0.011	0.033	-0.008	0.003
	(0.009)	(0.014)	(0.016)	(0.058)	(0.023)	(0.025)
Control Group Mean (at avg. risk = 0.276)	0.071	0.223	0.294	0.207	0.322	0.420
Observations	3,304	3,304	3,304	830	2,142	2,142

Table 4, Continued. Estimates of intervention effects on degree completion by predicted risk of dropout

	(1)	(2)	(3)	(4)	(5)	(6)
	All Students			Students in Proximity to Graduation		
	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17
D. Excluding Both						
Treatment x Predicted Dropout Risk	0.009**	0.014**	0.023***	0.025	0.031**	0.034**
	(0.005)	(0.007)	(0.008)	(0.043)	(0.013)	(0.014)
Average Treatment Effect	0.003	-0.003	-0.001	0.041	-0.012	-0.006
	(0.010)	(0.016)	(0.017)	(0.072)	(0.026)	(0.028)
Control Group Mean (at avg. risk = 0.280)	0.072	0.236	0.308	0.219	0.342	0.440
Observations	2,804	2,804	2,804	641	1,786	1,786

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

Notes: Panel B excludes one college where fall 2016 enrollment imbalance is observed. Panel C excludes one college where a negative effect on fall graduation is observed for reasons plausibly unrelated to intervention efficacy. Panel D excludes both colleges where unintended design effects are observed. Columns 4-6 are restricted to students whose expected cumulative credit totals (based on pre-intervention cumulative credit totals and term credits attempted in fall 2016) fulfill the minimum published credit requirements for degree completion at each institution. Effect estimates are from linear probability models that include risk ratings, 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 age, cumulative credits completed, and the fraction of total credits attempted that were earned at the start of fall 2016. Risk ratings are centered and multiplied by 10 so that the coefficient on the main treatment term reports the average treatment effect and the coefficient on the interaction term reports the marginal effect per 10-point increase in predicted dropout risk. Robust standard errors are reported in parentheses.

Table 5. Estimates of intervention effects on re-enrollment, credit completion, and GPA one year following random assignment by predicted risk of dropout

	(1)	(2)	(3)	(4)
	Re-enrolled Spring 2017	Credits Attempted Spring 2017	Credits Completed AY 2016-17	GPA AY 2016-17
A. Full Sample				
Treatment x Predicted Dropout Risk	-0.002	-0.181*	-0.258	0.002
	(0.007)	(0.107)	(0.160)	(0.018)
Average Treatment Effect	0.020	0.293	0.531*	0.032
	(0.013)	(0.216)	(0.307)	(0.030)
Control Group Mean (at average risk = 0.288)	0.808	10.77	20.89	2.90
Observations	3,804	3,804	3,804	3,693
B. Excluding College 1				
Treatment x Predicted Dropout Risk	-0.004	-0.215*	-0.295*	-0.005
	(0.008)	(0.111)	(0.164)	(0.018)
Average Treatment Effect	0.029**	0.418*	0.486	0.039
	(0.014)	(0.234)	(0.328)	(0.030)
Control Group Mean (at average risk = 0.293)	0.801	11.01	21.33	2.89
Observations	3,304	3,304	3,304	3,206
C. Excluding College 2				
Treatment x Predicted Dropout Risk	-0.000	-0.125	-0.073	0.019
	(0.008)	(0.117)	(0.176)	(0.020)
Average Treatment Effect	0.014	0.314	0.695**	0.039
	(0.014)	(0.230)	(0.334)	(0.033)
Control Group Mean (at average risk = 0.276)	0.831	11.39	21.66	2.896
Observations	3,304	3,304	3,304	3,209

Table 5, Continued. Estimates of intervention effects on re-enrollment, credit completion, and GPA one year following random assignment by predicted risk of dropout

	(1)	(2)	(3)	(4)
	Re-enrolled Spring 2017	Credits Attempted Spring 2017	Credits Completed AY 2016-17	GPA AY 2016-17
D. Excluding Both				
Treatment x Predicted Dropout Risk	-0.001	-0.150	-0.100	0.015
	(0.008)	(0.122)	(0.182)	(0.021)
Treatment	0.025*	0.482*	0.686*	0.050
	(0.015)	(0.255)	(0.363)	(0.033)
Control Group Mean (at average risk = 0.280)	0.826	11.75	22.28	2.88
Observations	2,804	2,804	2,804	2,722

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

Notes: Panel B excludes one college where fall 2016 enrollment imbalance is observed. Panel C excludes one college where a negative effect on fall graduation is observed for reasons plausibly unrelated to intervention efficacy. Panel D excludes both colleges where unintended design effects are observed. Estimates are from OLS/LPM models that include risk ratings, 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 age, cumulative credits completed, and the fraction of total credits attempted that were earned at the start of fall 2016. Risk ratings are centered on the mean and multiplied by 10 so that the coefficient on the main treatment term reports the average treatment effect and the coefficient on the interaction term reports the marginal effect per 10-point increase in predicted dropout risk. Robust standard errors are reported in parentheses.

Figure 1. Timeline of randomization relative to the start of the fall 2016 term and message outreach

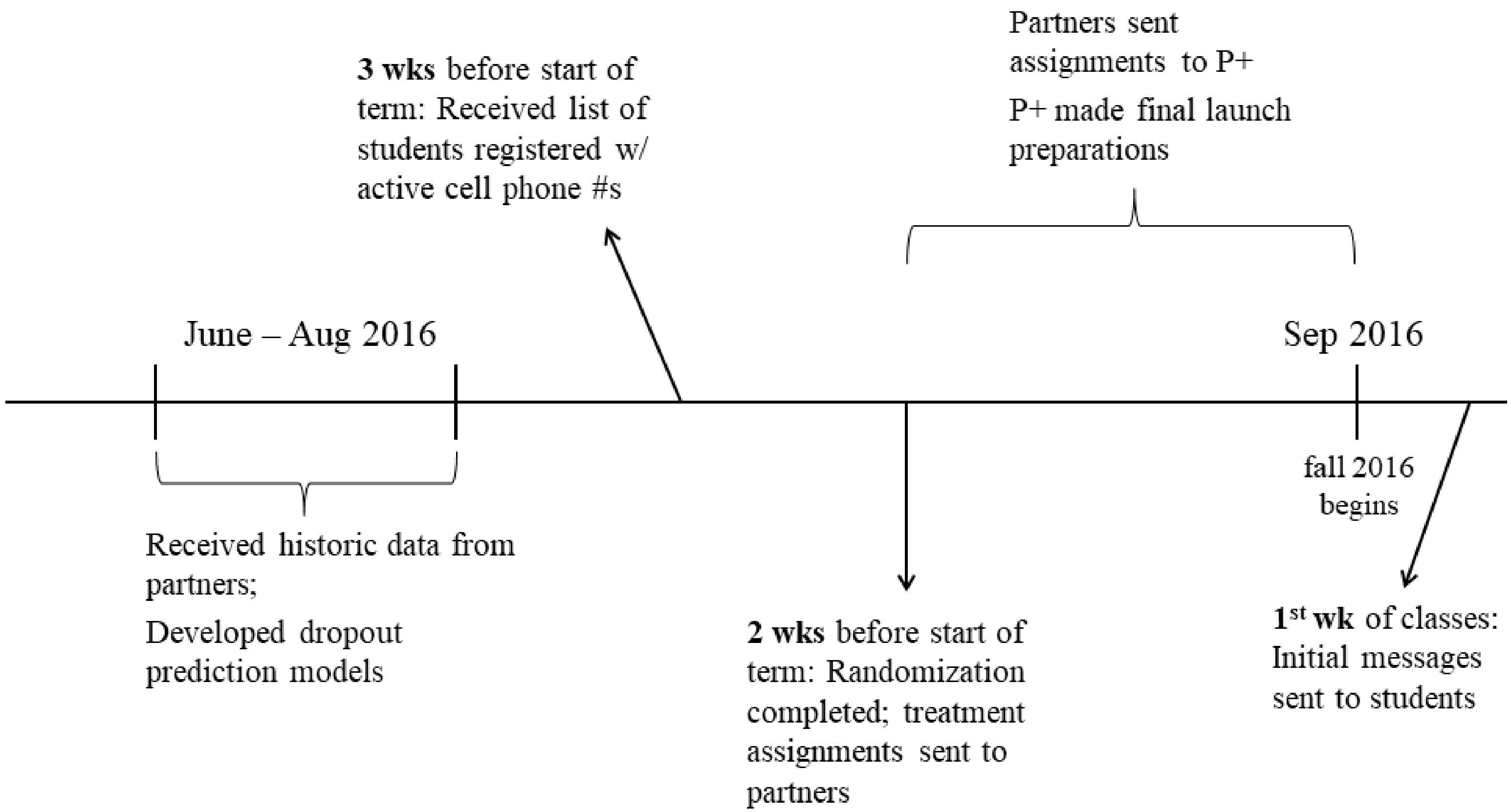
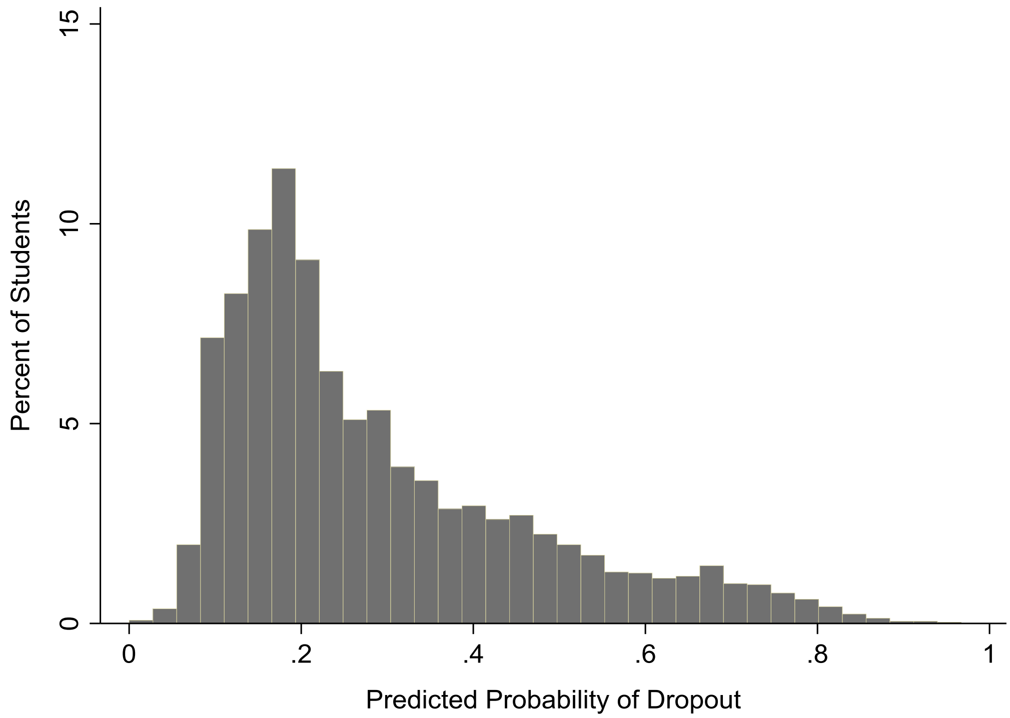
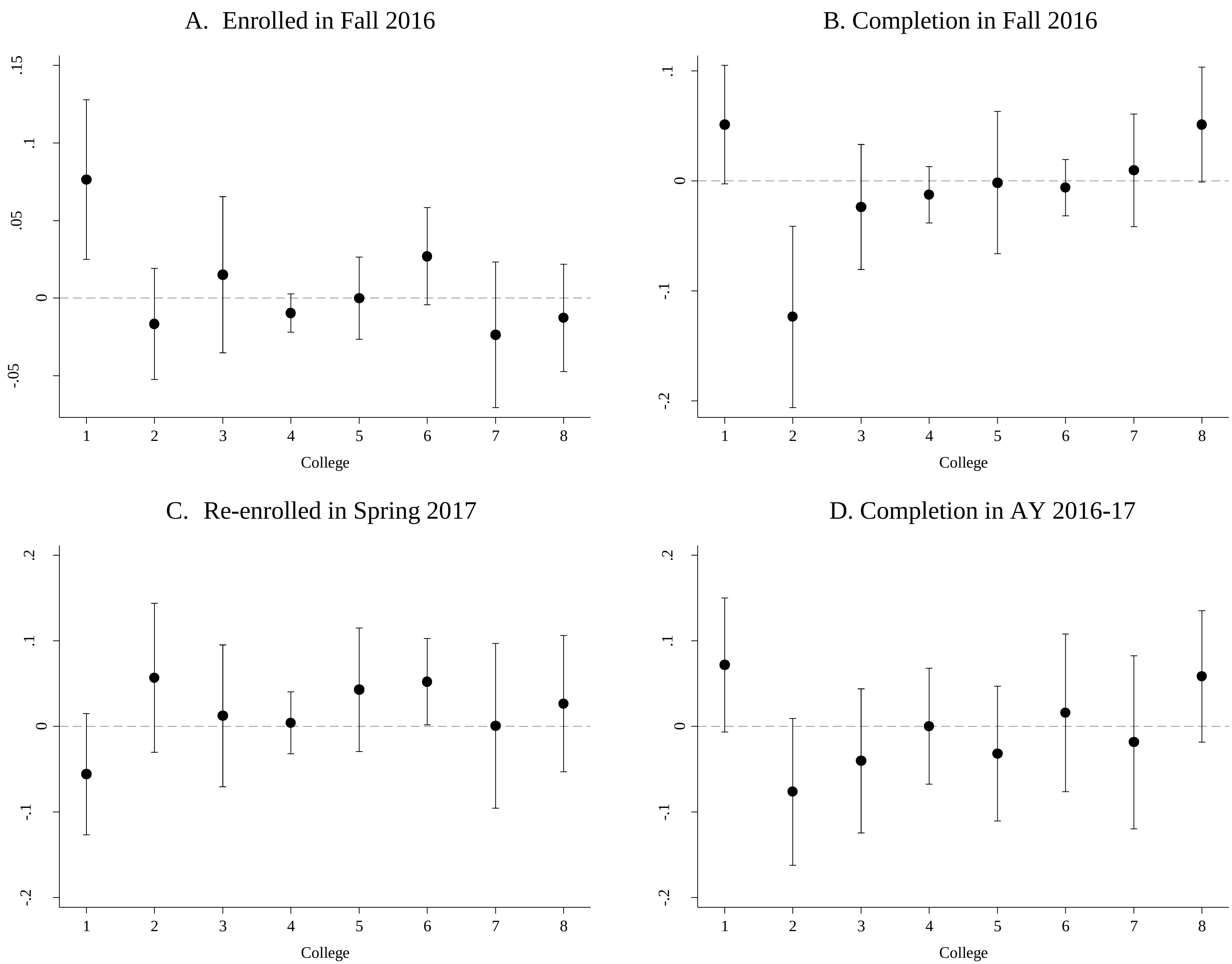


Figure 2. Distribution of predicted dropout probabilities in the full experimental sample



Notes: Predicted dropout probabilities are generated from school-specific logistic regression models developed using background information and the enrollment experiences of historical cohorts of students at each partner institution. See section 3.2 of the text for model detail

Figure 3. Estimates of N2FL intervention effects on fall enrollment, spring re-enrollment, and degree completion by campus



Appendix A – Supplemental Tables and Figures

Table A1. Estimates of intervention effects on fall enrollment and degree completion by tercile of predicted dropout risk at college where negative graduation effect in fall 2016 is observed

	(1) Enrolled Fall 2016	(2) Graduated Fall 2016	(3) Graduated Spring 2017	(4) Graduated AY 2016-17
Treatment x Bottom Tercile	0.002	-0.053	0.090*	0.038
	(0.039)	(0.062)	(0.051)	(0.070)
Treatment x Middle Tercile	0.025	-0.094	-0.004	-0.097
	(0.025)	(0.073)	(0.074)	(0.079)
Treatment x Top Tercile	-0.067**	-0.164**	0.037	-0.127*
	(0.029)	(0.072)	(0.062)	(0.077)
P-value on F-test of Equal Effects	0.050	0.504	0.558	0.229
R^2	0.360	0.488	0.352	0.471
Control Group Means				
Bottom Tercile	0.946	0.304	0.071	0.375
Middle Tercile	0.964	0.411	0.214	0.625
Top Tercile	1.000	0.418	0.109	0.527
Observations	500	500	500	500

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

Notes: The upper bounds of the bottom and middle tercile of predicted dropout risk are 0.24 and 0.42, respectively. Estimates are from linear probability models that include randomization block fixed effects and pre-treatment covariates. See table 2 for details. Robust standard errors are reported in parentheses.

Table A2. Estimates of enrollment and completion imbalances in fall 2016 by campus

	(1)	(2)	(3)	(4)
	Enrolled in Fall 2016		Graduated in Fall 2016	
	T-C Difference at All Other Colleges	DID at College X	T-C Difference at All Other Colleges	DID at College X
College 1	-0.002	0.079***	-0.017	0.068**
	(0.007)	(0.027)	(0.011)	(0.030)
College 2	0.010	-0.027	0.009	-0.133***
	(0.007)	(0.020)	(0.010)	(0.043)
College 3	0.006	0.009	-0.007	-0.017
	(0.007)	(0.026)	(0.011)	(0.031)
College 4	0.009	-0.019*	-0.008	-0.004
	(0.008)	(0.010)	(0.012)	(0.017)
College 5	0.008	-0.008	-0.010	0.008
	(0.007)	(0.015)	(0.010)	(0.035)
College 6	0.004	0.023	-0.009	0.003
	(0.007)	(0.018)	(0.011)	(0.017)
College 7	0.010	-0.033	-0.011	0.020
	(0.007)	(0.025)	(0.011)	(0.028)
College 8	0.010	-0.023	-0.018*	0.070**
	(0.007)	(0.019)	(0.011)	(0.029)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Effects for two four-year institutions in NYC are pooled to increase estimation precision. Effect estimates are from OLS/LPM models that include randomization block fixed effects and controls for the following pre-treatment covariates: indicators for sex, race/ethnicity (Black, Hispanic, Other, and Missing Race), transfer status, age, cumulative credits completed, and the fraction of total credits attempted that were earned at the start of fall 2016. Robust standard errors are reported in parentheses.

Table A3. Pre-treatment characteristics by treatment condition, excluding campuses where unintended design effects are observed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Excluding College 1			Excluding College 2			Excluding Both		
	Treated Students	Control Students	T-C Difference	Treated Students	Control Students	T-C Difference	Treated Students	Control Students	T-C Difference
Male	0.404	0.421	-0.017	0.386	0.415	-0.023	0.402	0.430	-0.029
Black	0.159	0.137	0.004	0.197	0.168	-0.005	0.157	0.133	0.003
Hispanic	0.229	0.203	-0.004	0.250	0.205	0.004	0.216	0.185	-0.003
White	0.410	0.461	0.018	0.407	0.483	0.010	0.464	0.527	0.012
Race other	0.127	0.136	-0.016	0.102	0.108	-0.009	0.111	0.116	-0.012
Race missing	0.073	0.061	-0.001	0.045	0.036	-0.000	0.053	0.040	-0.000
Age	24.2	23.4	0.244	25.4	24.1	0.368	24.3	23.2	0.420
Cumulative GPA	2.97	2.93	0.023	2.98	2.93	0.025	2.98	2.93	0.025
Transferred into current school	0.429	0.373	0.027*	0.534	0.459	0.016	0.458	0.405	0.017
Share of total attempted credits earned through spring 2016	0.946	0.944	0.002	0.936	0.935	0.005	0.946	0.943	0.002
Cumulative credits earned through spring 2016	75.46	78.26	-0.537	85.46	86.29	-0.253	81.37	83.93	-0.349
Predicted risk of dropout	0.298	0.293	-0.000	0.278	0.276	-0.000	0.285	0.280	-0.000
Enrolled in fall 2016	0.960	0.968	-0.002	0.965	0.961	0.010	0.962	0.969	-0.000
Number of students	2,151	1,153	3,304	2,193	1,111	3,304	1,818	986	2,804

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

Notes: Columns 1-3 exclude one college where fall 2016 enrollment imbalance is observed. Columns 4-6 exclude one college where a negative effect on fall graduation is observed for reasons plausibly unrelated to intervention efficacy. Columns 7-9 exclude both colleges where unintended design effects are observed. See text for details. Means are reported in columns 1-2, 4-5, and 7-8. Estimates of post-randomization balance are reported in columns 3, 6, and 9 from OLS/LPM models that include randomization block fixed effects.

Table A4. Estimates of intervention effects on degree completion by predicted risk of dropout, excluding covariates

	(1)	(2)	(3)	(4)	(5)	(6)
	All Students			Students in Proximity to Graduation		
	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17
A. Full Sample						
Treatment x Predicted Dropout Risk	-0.005	0.011*	0.006	-0.006	0.026**	0.011
	(0.006)	(0.006)	(0.008)	(0.034)	(0.010)	(0.013)
Average Treatment Effect	-0.010	0.005	-0.005	-0.048	-0.003	-0.020
	(0.010)	(0.014)	(0.016)	(0.056)	(0.021)	(0.024)
Control Group Mean	0.111	0.211	0.322	0.313	0.292	0.446
Observations	3,804	3,804	3,804	1,038	2,567	2,567
B. Excluding College 1						
Treatment x Predicted Dropout Risk	-0.003	0.013**	0.009	-0.008	0.027**	0.014
	(0.006)	(0.006)	(0.008)	(0.037)	(0.011)	(0.013)
Average Treatment Effect	-0.018	0.002	-0.016	-0.053	-0.005	-0.029
	(0.011)	(0.015)	(0.017)	(0.066)	(0.023)	(0.026)
Control Group Mean	0.116	0.221	0.337	0.337	0.305	0.466
Observations	3,304	3,304	3,304	849	2,211	2,211
C. Excluding College 2						
Treatment x Predicted Dropout Risk	0.007	0.013*	0.020**	0.027	0.030**	0.035***
	(0.005)	(0.007)	(0.008)	(0.038)	(0.012)	(0.013)
Average Treatment Effect	0.009	0.001	0.010	0.031	-0.011	-0.000
	(0.010)	(0.014)	(0.016)	(0.056)	(0.023)	(0.026)
Control Group Mean	0.0711	0.223	0.294	0.207	0.322	0.420
Observations	3,304	3,304	3,304	830	2,142	2,142

Table A4, Continued. Estimates of intervention effects on degree completion by predicted risk of dropout, excluding covariates

	(1)	(2)	(3)	(4)	(5)	(6)
	All Students			Students in Proximity to Graduation		
	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17
D. Excluding Both						
Treatment x Predicted Dropout Risk	0.009**	0.015**	0.024***	0.025	0.032**	0.039***
	(0.005)	(0.007)	(0.008)	(0.043)	(0.013)	(0.014)
Average Treatment Effect	0.002	-0.003	-0.002	0.043	-0.015	-0.010
	(0.010)	(0.016)	(0.018)	(0.067)	(0.026)	(0.028)
Control Group Mean	0.072	0.236	0.308	0.219	0.342	0.440
Observations	2,804	2,804	2,804	641	1,786	1,786

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

Notes: Panel B excludes one college where fall 2016 enrollment imbalance is observed. Panel C excludes one college where a negative effect on fall graduation is observed for reasons plausibly unrelated to intervention efficacy. Panel D excludes both colleges where unintended design effects are observed. Columns 4-6 are restricted to students whose expected cumulative credit totals (based on pre-intervention cumulative credit totals and term credits attempted in fall 2016) fulfill the minimum published credit requirements for degree completion at each institution. Effect estimates are from linear probability models that include risk ratings and randomization block fixed effects. Risk ratings are centered and multiplied by 10 so that the coefficient on the main treatment term reports the average treatment effect and the coefficient on the interaction term reports the marginal effect per 10-point increase in predicted dropout risk. Robust standard errors are reported in parentheses.

Table A5. Estimates of intervention effects on re-enrollment, credit completion, and GPA one year following random assignment by predicted risk of dropout, excluding covariates

	(1)	(2)	(3)	(4)
	Re-enrolled Spring 2017	Credits Attempted Spring 2017	Credits Completed AY 2016-17	GPA AY 2016-17
A. Full Sample				
Treatment x Predicted Dropout Risk	-0.001	-0.171	-0.229	0.006
	(0.008)	(0.109)	(0.164)	(0.018)
Average Treatment Effect	0.019	0.264	0.494	0.039
	(0.013)	(0.220)	(0.315)	(0.030)
Control Group Mean (at average risk = 0.288)	0.808	10.77	20.89	2.90
Observations	3,804	3,804	3,804	3,693
B. Excluding College 1				
Treatment x Predicted Dropout Risk	-0.003	-0.199*	-0.253	0.005
	(0.008)	(0.113)	(0.168)	(0.019)
Average Treatment Effect	0.028**	0.402*	0.459	0.047
	(0.014)	(0.241)	(0.338)	(0.032)
Control Group Mean (at average risk = 0.293)	0.801	11.01	21.33	2.89
Observations	3,304	3,304	3,304	3,206
C. Excluding College 2				
Treatment x Predicted Dropout Risk	-0.000	-0.127	-0.066	0.024
	(0.008)	(0.120)	(0.181)	(0.021)
Average Treatment Effect	0.014	0.285	0.656*	0.051
	(0.014)	(0.236)	(0.344)	(0.033)
Control Group Mean (at average risk = 0.276)	0.831	11.39	21.66	2.90
Observations	3,304	3,304	3,304	3,209

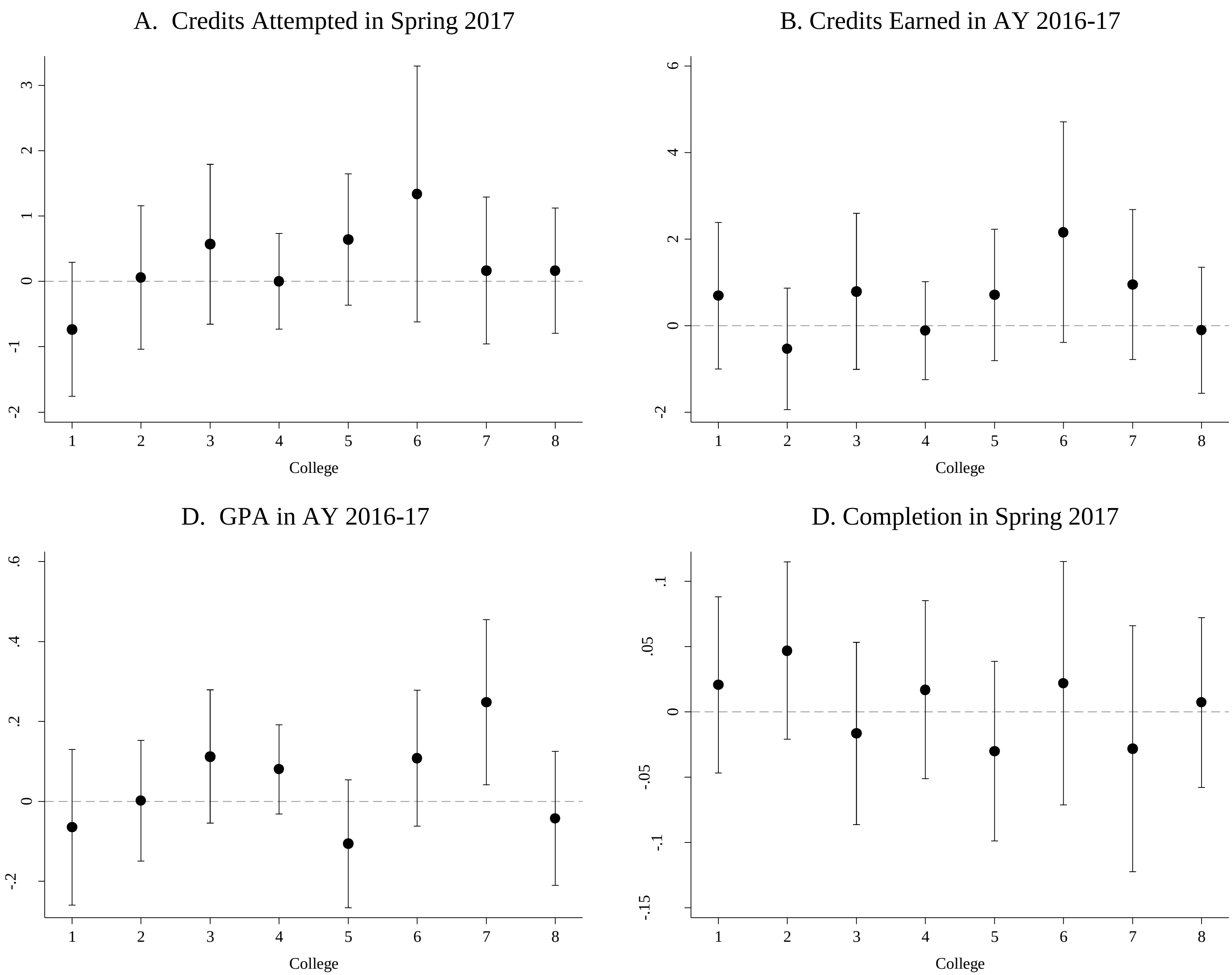
Table A5, Continued. Estimates of intervention effects on re-enrollment, credit completion, and GPA one year following random assignment by predicted risk of dropout, excluding covariates

	(1)	(2)	(3)	(4)
	Re-enrolled Spring 2017	Credits Attempted Spring 2017	Credits Completed AY 2016-17	GPA AY 2016-17
D. Excluding Both				
Treatment x Predicted Dropout Risk	-0.001	-0.154	-0.093	0.023
	(0.008)	(0.124)	(0.186)	(0.021)
Average Treatment Effect	0.024	0.444*	0.625*	0.062*
	(0.015)	(0.262)	(0.373)	(0.035)
Control Group Mean (at average risk = 0.280)	0.826	11.75	22.28	2.88
Observations	2,804	2,804	2,804	2,722

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

Notes: Panel B excludes one college where fall 2016 enrollment imbalance is observed. Panel C excludes one college where a negative effect on fall graduation is observed for reasons plausibly unrelated to intervention efficacy. Panel D excludes both colleges where unintended design effects are observed. Effect estimates are from OLS/LPM models that include risk ratings and randomization block fixed effects. Risk ratings are centered on the mean and multiplied by 10 so that the coefficient on the main treatment term reports the average treatment effect and the coefficient on the interaction term reports the marginal effect per 10-point increase in predicted dropout risk. Robust standard errors are reported in parentheses.

Figure A1. Additional estimates of N2FL intervention effects on academic outcomes by campus



Appendix B – Example Messages Delivered to Students

Purpose: Introductory Message
Content: Welcome to [INSTITUTION NAME]'s graduation support! We'll send free texts to help you complete your degree. Std msg & data rates may apply, STOP to end. More info?

Purpose: Course Enrollment Guidance
Content: If you're thinking about withdrawing from a class, ask an advisor first. Withdrawals may affect your financial aid & increase the time it takes to graduate.

Purpose: Encouraging Use of Tutoring Resources
Content: Many [INSTITUTION NAME] students seek out tutoring to enhance their learning. Do you need help with any of these [MATH, SCIENCE, WRITING, OTHER]?
Interactive Response to Students Who Replied "MATH": Many students benefit from the excellent tutors in the [CAMPUS LOCATION]. Make an appt. using [CAMPUS RESOURCE] to make the best use of your time.

Purpose: Encouraging Reverse Degree Conferral
Content: Students who transfer here from community college w/o an associate's degree can earn one while at [INSTITUTION NAME]. Interested?
Interactive Response to Transfer Students Who Replied "YES": Earning an associate's degree while working for your bachelor's can be very beneficial. Email [E-MAIL ADDRESS] or call [PHONE NUMBER] to discuss your eligibility.

Purpose: FAFSA Renewal Reminder
Content: Thanksgiving break is a great time to finish the FAFSA. Do you want us to send you a reminder?
Interactive Response to Students Who Replied "YES": No problem! Remember you can complete it online at fafsa.gov using your 2015 tax info. For free live help go to www.bit.ly/FAFSA_chat.
Interactive Response to Students Who Replied "NO": That's ok but think about when you can carve out some time to finish it soon. Remember you can complete it online at fafsa.gov using your 2015 tax info.
Interactive Response to Students Who Replied "DONE": Great job! We know the FAFSA is a lot of work and getting it done early will help maximize the $ you get for college next year.

Purpose: Goal-setting in Spring Term
Content: Setting goals is important for keeping yourself on track to graduation. What's your biggest academic goal for the Spring semester?
Interactive Response to Students Who Replied: We'll send you a reminder later in the semester about your goal and see how you're doing. Good luck!

Purpose: Clarify Remaining Academic Requirements
Content: Last semester you were unsure whether you had any math requirements left to graduate. Were you able to get that sorted out?

Interactive Response to Students Who Replied "YES": Fantastic! If you're currently taking any math courses remember that you can always visit the Math Lab in MB44 for free tutoring.
Interactive Response to Students Who Replied "NO" or "STILL UNSURE": We don't want to see any missed courses derail your plans for graduation. Talk to your advisor or the [NAME OF ADVISING CENTER] in [ON CAMUPS LOCATION] soon about this issue.

Purpose: Overcoming Procrastination
Content: Many [NAME OF INSTITUTION] students struggle with time mgmt because they procrastinate on important tasks. How much do you struggle with this?
Interactive Response to Students Who Replied "1" or "2" on 0-5 Scale: Mastering time mgmt will serve you well after you've graduated from [NAME OF INSTITUTION]. Keep up the good work.
Interactive Response to Students Who Replied "2", "3", "4" or "5": Planning your days hour-by-hour can help. Even scheduling breaks & leisure time helps you make the most of your working hours.

Purpose: Norming Students Towards a Growth Mindset
Content: [X]% of 70% of students we asked at [NAME OF INSTITUTION] believe that intelligence can be improved through practice & hard work. And you know what? They're right.

Purpose: Transportation Harship Follow-up
Content: We know that last semester you sometimes had trouble making it to [NAME OF INSTITUTION]. Have you been able to find resources to make commuting easier?
Interactive Response to Students Who Replied "YES": We're really glad to hear that. We know what a long way you've come to be this close to a degree and we want to make sure you reach graduation day.
Interactive Response to Students Who Replied "NO": If you need additional help talk to [ON-CAMUS RESOURCE] at [PHONE NUMBER]. They're ready to help connect with you with the resources you need.

Purpose: Apply to Graduate Reminder
Content: The last day to apply to graduate for this semester is [DATE]. Do you plan to graduate this semester?
Interactive Response to Students Who Replied "YES": The finish is in sight! Go to your [NAME OF ONLINE RESOURCE] to apply. If you get stuck call the [NAME OF INSTITUTION] Registrar: [PHONE NUMBER].
Interactive Response to Students Who Replied "NO" or "UNSURE": [NAME OF INSTITUTION] offers many resources to help keep you on track to graduate. Check out [ONLINE DEGREE AUDIT TOOL], talk to your advisor, or call Enrollment Services at [PHONE NUMBER].