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

Zachary Mabel*
Harvard Graduate School of Education

Benjamin L. Castleman
University of Virginia

Eric P. Bettinger
Stanford University School of Education and NBER

September 2017

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 no evidence of effects on academic performance or attainment in the full sample, although implementation challenges at two institutions make results from this sample difficult to interpret and potentially downward-biased. In our preferred sample which 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 David Deming, Bridget Terry Long, 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.

*Please address all correspondence to Zachary Mabel at zam340@mail.harvard.edu.

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 disadvantaged populations 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 suggests that one in three dropouts complete at least three-quarters of the credits that are typically required to graduate before they withdraw (Mabel & Britton, 2017). Across the country this translates into approximately 2 million individuals 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. We conducted the pilot phase of the Nudges to the Finish Line (N2FL) project during the 2016-17 academic year in partnership with nine broad-access, public higher education institutions in New York City, Virginia, Texas, Ohio, and Washington State. In collaboration with Persistence Plus, a Boston-based provider of mobile supports to college students, we designed 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 college advising. In two other sites, students

[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).

received the automated messages and also had access to a dedicated 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 effects on academic performance or attainment in the full sample, implementation challenges at two institutions make results from this sample difficult to interpret and potentially downward-biased. These challenges arose in part because we randomized students in late summer before fall enrollments finalized. Figure 1 illustrates the timing of randomization relative to the start of the academic year and our intervention. On average, we relied on preliminary fall 2016 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.

Unfortunately, the timing of randomization produced initial enrollment imbalances at two institutions. At one campus we observe a large fall enrollment difference between treated and control students. At another campus we observe a large fall enrollment imbalance among students with high risk of dropout who presumably stand to benefit most from message outreach. These imbalances are not a function of message outreach, which began after classes started, but they may bias estimates of downstream intervention impacts. We therefore present estimates from the full sample as well as for samples in which we exclude these institutions.

Results from our preferred sample, which excludes the two institutions where initial enrollment imbalance is observed, are indicative of positive impacts on attainment. The intervention decreased fall-to-spring dropout by 2.5 percentage points, representing a 14 percent decrease over the control group mean. The findings also suggest a large impact on degree completion 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. 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. Over the next two years, we will expand the study to include 25,000 students at approximately 20 institutions to examine effects on persistence and completion over a longer time horizon and at scale for this student population.

The remainder of this paper is structured as follows. In Section 2, we present a simple economic model of human capital investment to understand what factors may affect the decision to drop out late into college. In Section 3, 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 4, we present details on the research design, including the participating schools, intervention components, study sample, randomization procedure, and empirical strategy. We present results from the pilot year in Section 5. In Section 6, we summarize the findings and conclude with a brief discussion of our work in progress.

2. An Economic Model of Dynamic Schooling Decisions

Standard models of human capital assume that individuals choose whether or not to attend college by evaluating the expected lifetime benefits of the investment against the expected lifetime costs. We extend this simple framework to a multi-period model, in which individuals can have time-inconsistent preferences, in order to examine potential determinants of late dropout. Consider for simplicity the case of bachelor's degree-seeking students choosing between re-enrolling for a third year of college or leaving school after two years to enter the labor market. At the end of year two, students weigh the decision to return to college based on their earnings potential with a degree versus their earnings potential from having completed two years of college. Assuming that students would graduate in four years, the net present value of completing college conditional on having finished two years of school is:

$$\sum_{t=2}^{T} \frac{[w(BA)-w_1]}{\beta\delta^t} - [w_0 - c_1] - \frac{[w_1-c_2]}{\beta\delta}. \tag{1}$$

The first term in equation (1) is the earnings return to students over the $(T-2)$ years after completing college, discounted by $\beta\delta^t$, where δ is the standard discount factor and β captures the fact that individuals tend to over-value immediate payoffs at the expense of their long-term intentions (i.e., they exhibit present-biased preferences). The next two terms are the continuation costs incurred from re-enrolling in school. At the end of year two, the cost to attending year three is forgone wages from immediate entry to the labor market (w_0) net of the tuition, effort, and

psychic costs to attending year three (c_1). Likewise, the perceived cost of persisting to year four at the end of year two is the present discounted difference between one's earnings potential after three years of college (w_1) and the cost of attendance in year four (c_2). It follows that students will decide to dropout before graduation if:

(2) $$\sum_{t=2}^{T} \frac{[w(BA)-w_1]}{\beta\delta^t} < [w_0 - c_1] + \frac{[w_1-c_2]}{\beta\delta}$$

This simple model highlights two factors that can induce students to drop out after making substantial investments in college. First, the real or perceived costs of continuation may increase over time (i.e., $c_2 > c_1 > c_0$) and exceed future benefits to completion. While this could affect the schooling decisions of students with and without strong present-biased preferences, individuals who discount future benefits more (i.e., $\beta < 1$) will be more likely to drop out if costs rise. Second, even if costs remain constant and the benefits to completion large, individuals with strong present-biased preferences may decide to withdraw if costs are sufficiently high. The theoretical insights from this model therefore predict that interventions which lower continuation costs and encourage students to place more weight on the future benefits of completion could have significant impacts on attainment for students at risk of late dropout.

3. Literature Review: 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, 2013). Resource constraints at broad-access public institutions where the majority of postsecondary students in the United States 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).[3]

Despite indications that these obstacles are particularly salient to students late into college, the causes of late dropout 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 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 informational and behavioral interventions may have little impact on academic progress and motivate the need for more resource-intensive strategies to lower rates of late departure.[4] In this study, 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] To inform our intervention design, Persistence Plus also conducted 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 when challenges arose.

[4] 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.

4. Research Design

We partnered with a diverse array of non-selective, public two- and four-year institutions across the country to implement N2Fl during the 2016-17 school year. All of our partner institutions accept 75 percent or more of the applicants that apply. 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.

4.1. 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, and 2) completed at least 50 percent of the credits typically required for degree completion through summer 2016.[5] 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 for two reasons. First, it was unclear ex ante which profile(s) of student would be most responsive to the intervention. Second, we anticipated needing to exclude many potentially-eligible students because having an active cell phone number and providing consent to receive text messages were prerequisites to participation in the study. The broad criteria therefore ensured we were able to recruit an adequate sample from each institution to detect reasonably-sized impacts of the intervention.

Based on the eligibility criteria above and the size of enrollments at our partner institutions, we recruited 3,804 students to participate in the pilot year intervention. In columns 2-4 of Table 1, we present summary statistics by treatment status for the students in the analytic sample. To

[5] 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.

examine the extent to which the sample reflects the population of undergraduates attending public colleges and universities nationally, in column 1 we show analogous statistics for a nationally representative sample using data from the National Postsecondary Student Aid Study of 2012 (NPSAS:12). 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. Students in the study sample on average had a 29 percent chance of dropout according to the prediction models we developed using historical data from partner institutions (see section 4.3 for more details on these models), but students at community colleges had a 17.5 percentage point higher risk of dropout on average compared to four-year students in the study sample (40.4 percent versus 22.9 percent).

Our experimental sample resembles the national student population at public colleges with respect to sex (40 percent male versus 43 percent, respectively) and academic achievement (average GPAs are 2.96 and 2.88 among students in the study and nationwide, respectively). However, students in our sample are slightly younger than the typical college enrollee on average (24.8 years versus 25.9 years nationally). Students of color are also overrepresented in our sample (55 percent versus 42 percent) because seven of the nine institutions that participated in the pilot year intervention operate in urban areas. Our results generalize most to students attending public, urban institutions in the United States for this reason.

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, the timing of randomization produced unintended design effects at two institutions. As shown in panel A of Figure 2, at one site (college 1) treated students were 8

percentage points more likely to enroll in fall 2016. This initial enrollment imbalance is not a function of message outreach, which did not begin until after classes started, but it may contribute 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 (downwardly) biasing our treatment estimates on degree completion (re-enrollment).

At a second site (college 2 in Figure 2), 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. First, high-risk students at this institution were substantially less likely to enroll in the fall (a similar issue as with college 1, though in the opposite direction). In 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.[6] Second, we find no supporting evidence of a negative impact of the intervention for students at college 2. As shown in Figure 2 and Appendix Figure A1, we find no evidence of negative impacts on other outcomes we would expect to be associated with degree completion at college 2, such as credits completed or GPA. Similarly, we show that at all other sites where treated and control students enrolled at similar rates in fall 2016, there is no evidence of negative impacts on academic outcomes. Later this fall we expect to receive student-level interaction data from the messaging campaign. This data will us to further examine whether students responded negatively to message outreach, thus providing an additional means of exploring the origins of the negative fall degree effect at college 2.

The implementation challenges at these two sites influence the estimated treatment effects. For example, across all nine sites, the effect on fall-to-spring re-enrollment is only negative at

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

college 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. Likewise, because: 1) college 2 is a two-year institution, 2) community college students in the study sample were at greater risk of late dropout than four-year students on average, and 3) the negative effect on fall completion at college 2 is more than twice as large as the degree impacts across all other sites in absolute magnitude, college 2 will have an outsize influence on the estimates of degree impacts by dropout risk. 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; restricting both yields a sample of 2,804 students. Our preferred estimates are from the latter sample that is free of all unintended implementation challenges.

4.2. Intervention Design

Students randomly assigned to treatment received automated text messages during the 2016-17 academic year. From our review of existing literature and the student 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 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.

We 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.[7]

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

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

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”.

Non-interactive messages were also customized to each institution and according to students’ background characteristics and prior academic record.[8] In addition to receiving automated outreach, half of students assigned to treatment at two institutions had the ability to engage in two-way text interactions with advisors on campus. The intervention at these colleges offered students more opportunity to receive real-time guidance and support from an advisor at their institution. Across all campuses we observed a relatively high level of student engagement. Sixty-nine percent of treated students responded to an interactive message, 35 percent replied 5 or more times, and on average students responded to messages on 7 occasions during the school year.[9]

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

[8] For example, messages that reminded students to refile their FAFSA were customized to the deadlines and financial aid resources available at each institution, and messages about receiving credit for courses taken at other institutions were delivered to transfer students only.

[9] The student-level interaction data we will receive later this fall will also allow us to examine the types of messages students responded to and examine variation in effects by level of engagement. We will update the paper with those results once the interaction data becomes available.

upper-division students, is scant at most 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.

4.3. 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.[10]

We use this data in three ways. First, we used the historical data provided by each institution to develop school-specific logistic regression dropout prediction models. 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 such as gender and race/ethnicity, measures of students' enrollment experiences and performance in college, including whether they had previously stopped out, transferred schools, switched majors, and their cumulative GPA through spring 2016, and measures of students' financial need and aid receipt. We then assigned risk ratings to students in the experimental sample using the dropout prediction models.[11] Students at greatest risk of dropout exhibited increasing rates of course failure and erratic

[10] 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.

[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

credit loads as they progressed in school and were more likely to have transferred into their current institution. They were also more likely to be older, male, and students of color. Descriptive statistics of the sample by dropout risk are presented in Table A2. Additional details pertaining to model construction are provided in Appendix C.

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 rely on it 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: 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.[12]

In addition to institutional academic and financial data, at one college we also observe records of the on-campus resources students utilized in fall 2016 (e.g., whether students met with a financial aid advisor, accessed tutoring services, etc.). This data is available because students at the college 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, and if so, whether this appears to be a mechanism through which message outreach affected academic progress and performance.

4.4. Randomization Procedure and Baseline Equivalence

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

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).

[12] We expect to receive fall 2017 registration records this fall to also report impacts on spring-to-fall re-enrollment.

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.

During the pilot year we also tested out different message variants to inform our intervention design in future years. As a result, at most institutions students were randomly assigned to one of three treatment arms: a control condition and two variants of the treatment group which received slightly different messages.[13] 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, 2,526 students were randomly assigned to a treatment arm and 1,278 were assigned 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 variant of message outreach.

In column 5 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 conditioned samples are similar and reported in 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 conclude that randomization achieved baseline equivalence, although we

[13] 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.

additionally examine the stability of effect estimates by presenting models that do and do not include pre-treatment covariates.

4.5. 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:

$$Y_{ib} = \alpha + \beta T_{ib} + \delta_b + \zeta X_{ib} + \varepsilon_{ib}, \tag{3}$$

where Y_{ib} is one of the five academic outcomes (i.e., term credits completed, degree completion, etc.) 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. The coefficient of interest in this model is β, which represents the causal estimate of being assigned to receive text-based outreach during the 2016-17 school year. As discussed above, we estimate models with and without the inclusion of student-level covariates (X_{ib}). This set of covariates is 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. 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.

We examine heterogeneity of treatment effects by dropout risk by estimating models of the following form:[14]

$$Y_{ib} = \alpha' + \beta' T_{ib} + \gamma(T_{ib} * D_{ib}) + \tau D_{ib} + \delta'_b + \zeta' X_{ijb} + \varepsilon'_{ijb}, \tag{4}$$

where, as before, i and b respectively index students and blocks, and D_{ib} denotes 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). β' therefore 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.

5. Results

5.1 Overall Impacts on Academic Achievement and Attainment

We present impacts of the intervention on academic outcomes for the full sample in Table 2. In odd-numbered columns, we report results from models of equation (3) that include only the treatment indicator and randomization block fixed effects. In even-numbered columns, we show results from models that also include pre-treatment covariates. The point estimates and standard errors are nearly identical from the two models across all samples and outcomes, which reinforce that randomization created balanced treatment and control groups. For parsimony, we only report estimates from models that include baseline controls in the remainder of the paper.

In panel A of Table 2, we find little evidence of intervention impacts in the full sample. With the exception of the point estimates on graduation, the coefficients are generally positive but not significant at conventional levels. We find suggestive evidence of a positive impact on credit

[14] We also investigated whether the intervention produced variation in effects by race, sex, credit attainment at the start of the intervention, and institutional level and setting (i.e., urban versus non-urban). We find no evidence of differential effects on these dimensions. These results are available upon request.

completion, although the magnitude of the estimate (0.53 credits) is substantively small and it is only marginally significant at the 10 percent level. However, when we exclude college 1 from the sample (panel B), the results indicate that message outreach increased the likelihood of re-enrollment from fall to spring by 3 percentage points (4 percent over the control group mean). In panel C we report estimates from the sample that includes college 1 but excludes college 2. The magnitude of the coefficient on re-enrollment attenuates and is again not significant due to the negative impact on spring re-enrollment at college 1, but the impact on credit completion (0.7 credits) re-emerges and is precisely estimated.

Panel D of Table 2 presents the results from our preferred sample that excludes both institutions where we faced implementation challenges, as described above. Although the point estimates are estimated less precisely in this smaller sample, we find suggestive evidence of positive impacts 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). All of those estimates are marginally significant at the 10 percent level.[15] The point estimates on GPA are also positive (0.05-0.06 points) but only marginally significant in the model without baseline controls. It is particularly 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 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. Despite indications that message outreach decreased fall-to-spring

[15] The restricted sample is underpowered to detect effects of the magnitude we estimate. For example, the minimum detectable effect on fall-to-spring re-enrollment is 0.039 percentage points in this sample. The marginally significant estimates may thus reflect true impacts of message outreach on academic outcomes that will become more precisely estimated when we recruit additional cohorts to the study in future years.

dropout by 14 percent, we find no evidence of impacts on overall degree attainment across all four samples.

5.2 Impacts by Predicted Risk of Dropout

The null effects on completion may be due to the fact that the risk of dropout was low for many students in the study sample. These inframarginal students would be expected to make steady progress towards graduation regardless of whether or not they received low-touch nudges to set goals, connect to resources, and remember deadlines. In Table 3, we therefore report results from models of equation (4) which examine whether the intervention produced differential effects by predicted risk of dropout. In our preferred results in panel D, we find evidence of positive impacts on degree completion for students at higher risk of dropout. The estimate in column 5 of panel D indicates that degree attainment in 2016-17 for students who received messages increased by 2.2 points for every 10-point increment in dropout risk. In Appendix Table A4, we report estimates of degree effects by risk tercile to simplify interpretation of the variation in effects. 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 6.1 percentage points, from 16.2 percent to 22.3 percent, after one year of intervention. The represents a 38 percent increase in completion relative to the control group. We find no evidence that dropout risk moderated the impacts of message outreach on other outcomes. The coefficients on the interaction term in columns 1-4 of panel D are substantively small and not significant.

To further investigate whether the fall degree effect at college 2 conceals positive impacts on degree completion broadly experienced among high-risk students across all sites, we examined effects by dropout risk separately by term. These results are presented in Table 4. In column 2, we find consistent evidence that higher risk students were more likely to graduate in spring 2017

across all samples. 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.[16] The results in Table 4 therefore provide additional evidence that high-risk students who received message outreach were more likely to graduate after one year of intervention.

5.3 Did Message Outreach Lower Barriers to Accessing On-Campus Resources?

The text message campaign was designed to make it easier for students to engage with staff and access supports on campus. We now turn to investigating whether message outreach achieved this goal. As a reminder, these results are limited to a single institution that systematically collects student-level data on campus resource usage, and as a result, they may not reflect how treated students responded to the text message campaign at other campuses. It is also unfortunate that this campus happens to be college 2, where the unintended implementation challenges make it difficult to interpret impacts on academic outcomes.[17] We therefore focus on the less ambiguous first-stage effects of message outreach on resource take-up, but we report results of whether removing barriers to resource use appears to be a channel through which attainment gains were realized in Appendix A.

In Table 5, we present effects on whether students utilized various types of campus resources in fall 2016 and how much support they received. The evidence suggests that treated

[16] In column 1 of Table 4, which reports impacts on completion in fall 2016, the coefficients on the interaction term are also positive when college 2 is excluded from the sample (panels C and D). Furthermore, all of the estimates in panels A-C of column 2 are marginally significant at the 10 percent level, while in our preferred sample (panel D), they are significant in both the fall and spring terms (0.009 and 0.014, respectively).

[17] Because 2SLS estimates are proportional to reduced-form regressions of academic outcomes on treatment assignment, the design effects at college 2 may lead to spurious results because the reduced-form estimates are negative for some outcomes. Therefore, even if message outreach increased resource use and resource use is positively correlated with academic performance and attainment, 2SLS estimates of resource effects on academic outcomes may be negative and potentially misleading. The exclusion restriction imposed by 2SLS models – namely that all of the treatment effect operates through resource use – is also likely to be violated and another reason for focusing primarily on the first-stage results.

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 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 more intensive 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.

In Appendix Table A5, we examine 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. In addition to the fact that the implementation issues make inference difficult, the F-statistics on all of the first-stage regressions are below 6. Treatment assignment is therefore a weak instrument for resource use and may lead to biased estimation. In summary, we do not find evidence that message outreach produced impacts on attainment by connecting students to more on-campus supports, but the results are inconclusive due to the implementation and weak instrument limitations.

6. 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, in our preferred

sample that excludes two sites where we find evidence of unintended design effects, interactive text messages designed to alleviate informational, procedural, and psychosocial barriers to completion decreased fall-to-spring dropout by 14 percent, from 17.5 to 15 percent. This result is noteworthy for two reasons. First, message outreach was not targeted exclusively to students at risk of dropping out late into college, and our risk models predicted that many students in the study faced minimal risk of non-completion. Furthermore, it is plausible that impacts on spring-to-fall dropout will be larger given that dropout from college occurs more frequently between rather than within school years (Long & Mabel, 2012). We expect to be able to examine effects on spring-to-fall dropout soon after the fall 2018 term begins at each participating institution.

We also find suggestive evidence that message outreach increased degree attainment for students at high-risk of dropout. 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.

These findings compare favorably to interventions that target students at earlier points in the college process and that are higher touch. For example, Castleman and Page (2015) find that providing college-intending high school graduates with similar types of text-based support to reduce “summer melt” between high school completion and college matriculation increased rates of enrollment at two-year colleges by 3 percentage points. 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). Furthermore, N2FL cost only $100 per student including development and technology start-up costs, and $20 per student when start-up costs are excluded. N2FL therefore produced one-year degree effects for high-risk students roughly one-fifth the size and at less than 5 percent of the cost of impacts from ASAP, a comprehensive multi-year student support initiative at CUNY's community colleges (Scrivener et al., 2015).[18] Given evidence of the positive return on investment to ASAP (Levin & Garcia, 2017), our preliminary findings suggest the return on investment to providing high-risk students within a few semesters of graduating with low-cost support may also be large, although it remains unclear whether the impacts on attainment we find after one year represent brief or lasting gains. We will examine this question in the future by tracking students over multiple years.

A second question that emerges is what components of the intervention increased student attainment. Our findings indicate that message outreach substantially increased the use of campus resources, but it remains unclear whether this was a channel through which the impacts on academic progress were realized. Although the data currently in our disposal do not allow us to test alternative mechanisms, the messages alone may have increased attainment for a number of reasons. By reminding students to think about their future and what their degree can accomplish for themselves and others, the messages may have helped to refocus students' attention on the long-term benefits of finishing school instead of the incremental time, effort, and financial costs to attendance. Some messages were also designed to help students make specific plans about when and where to study and complete important tasks, and prior research finds that students with poor

[18] The per-student cost estimate for N2FL is an upper bound, as it includes content development and technology start-up costs Persistence Plus incurred. When start-up costs are excluded, the per-student cost is approximately $20 per student.

time management and procrastination tendencies are more likely to dropout from college (Beattie, Laliberte, Michaud-Leclerc, & Oreopoulos, 2017). Evidence also suggests that prompting at-risk students to identify concrete steps to achieving long-term goals can improve academic progress and performance in college (Morisano, Hirsh, Peterson, Pihl, & Shore, 2010). Finally, some of the messages were intended to build and reinforce students' perceptions of themselves as successful college students, which has also been shown to increase academic performance and early persistence among undergraduates (Alter, Aronson, Darley, Rodriguez, & Ruble, 2010; Dee, 2014; Ramirez & Beilock, 2011; Yeager et al., 2016).

We will continue to investigate possible mechanisms as new cohorts enter the study. Over the next two years we will expand the study to include 25,000 students at approximately 20 institutions. This will allow us to examine effects on persistence and completion over a longer time horizon and at scale for this student population. We also plan to examine the barriers to completion for late dropouts in greater detail by refining the intervention design. In the next research phase, one group of treated students on each campus will receive messages specifically designed to address academic planning barriers to completion (e.g., by providing students with course recommendations to complete their remaining requirements). Another group of students will receive messages intended to address financial barriers to completion (e.g., by notifying students that are at-risk of exhausting eligibility for financial aid). Through this work we hope to provide valuable insights into the barriers that are most obstructive to students approaching completion and offer policymakers and higher education leaders scalable solutions to address those obstacles.

REFERENCES

Alter, A. L., Aronson, J., Darley, J. M., Rodriguez, C., & Ruble, D. N. (2010). Rising to the threat: Reducing stereotype threat by reframing the threat as a challenge. Journal of *Experimental Social Psychology, 46*(1), 166–171.

Angrist, J., Lang, D., & Oreopoulos, P. (2009). Incentives and Services for College Achievement: Evidence from a Randomized Trial. *American Economic Journal: Applied Economics, 1*(1), 136–163.

Autor, D. H. (2014). Skills, education, and the rise of earnings inequality among the "other 99 percent." *Science, 344*(6186), 843–51.

Avery, C., & Turner, S. (2012). Student loans: Do college students borrow too much—or not enough? *Journal of Economic Perspectives, 26*(1), 165–192.

Bailey, M. J., & Dynarski, S. M. (2011). Inequality in postsecondary education. In G. J. Duncan & R. J. Murnane (Eds.), *Whither Opportunity* (pp. 117–132). New York: Russell Sage Foundation.

Beattie, G., Laliberte, J.-W. P., Michaud-Leclerc, C., & Oreopoulos, P. (2017). *What sets college thrivers and divers apart? A contrast in study habits, attitudes, and mental health* (NBER Working Paper No. 23588). Cambridge: National Bureau of Economic Research.

Bettinger, E. P., & Baker, R. B. (2014). The Effects of Student Coaching: An Evaluation of a Randomized Experiment in Student Advising. *Educational Evaluation and Policy Analysis, 36*(1), 3–19.

Bettinger, E. P., Boatman, A., & Long, B. T. (2013). Student Supports: Developmental Education and Other Academic Programs. *The Future of Children, 23*(1), 93–115.

Bettinger, E. P., & Long, B. T. (2009). Addressing the needs of underprepared students in higher education: Does college remediation work? *Journal of Human Resources, 44*(3), 736–771.

Bound, J., Lovenheim, M. F., & Turner, S. (2010). Why Have College Completion Rates Declined? An Analysis of Changing Student Preparation and Collegiate Resources. *American Economic Journal: Applied Economics, 2*(3), 129–157.

Bowen, W. G., Chingos, M. M., & McPherson, M. S. (2009). *Crossing the finish line: Completing college at America's public universities*. Princeton, NJ: Princeton University Press.

Cadena, B. C., & Keys, B. J. (2015). Human Capital and the Lifetime Costs of Impatience. *American Economic Journal: Economic Policy, 7*(3), 126–53.

Carnevale, A. P., Jayasundera, T., & Gulish, A. (2016). *America's divided recovery: College have and have-nots*. Washington, DC: Georgetown University, Center on Education and the Workforce.

Castleman, B. L., & Page, L. C. (2015). Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? *Journal of Economic Behavior and Organization, 115*(9), 144–160.

Castleman, B. L., & Page, L. C. (2016). Freshman year financial aid nudges: An experiment to

increase FAFSA renewal and college persistence. *Journal of Human Resources, 51*(2), 389–415.

Castleman, B. L., Schwartz, S., & Baum, S. (Eds.). (2015). *Decision making for student success: Behavioral insights to improve college access and persistence*. New York: Routledge.

Center for Community College Student Engagement. (2015). *Community college survey of student engagement*. Austin: University of Texas at Austin, Center for Community College Student Engagement.

Chetty, R., Friedman, J. N., Saez, E., Turner, N., & Yagan, D. (2017). *Mobility report cards: The role of colleges in intergenerational mobility*. Unpublished manuscript.

Clark, D., Gill, D., Prowse, V., & Rush, M. (2017). *Using goals to motivate college students: Theory and evidence from field experiments* (NBER Working Paper No. 23638). Cambridge: National Bureau of Economic Research.

Clotfelter, C. T., Hemelt, S. W., & Ladd, H. F. (2016). *Multifaceted aid for low-income students and college outcomes: Evidence from North Carolina* (NBER Working Paper No. 22217). Cambridge: National Bureau of Economic Research.

Dee, T. S. (2014). Stereotype threat and the student athlete. *Economic Inquiry, 52*(1), 173–182.

Deming, D. J., & Walters, C. R. (2017). *The impact of price caps and spending cuts on U.S. postsecondary attainment* (NBER No. 23736). Cambridge: National Bureau of Economic Research.

Erisman, W., & Steele, P. (2015). *Adult college completion in the 21st century: What we know and what we don't*. Washington, DC: Higher Ed Insight.

Grubb, W. N. (2006). Like, what do I do now? In T. Bailey & V. S. Morset (Eds.), *Defending the community college equity agenda* (pp. 195–222). Baltimore: Johns Hopkins University Press.

Gurantz, O. (2015). Who loses out? Registration order, course availability, and student behaviors in community college. *The Journal of Higher Education, 86*(4), 524–563.

Horn, L., & Paslov, J. (2014*). Data point: Out-of-pocket net price for college* (NCES 2014-902). Washington, DC: U.S. Department of Education, National Center for Education Statistics, Institute of Education Sciences.

Lavecchia, A. M., Liu, H., & Oreopoulos, P. (2014). *Behavioral Economics of Education: Progress and Possibilities* (NBER Working Paper No. 20609). Cambridge: National Bureau of Economic Research.

Levin, H. M., & Garcia, E. (2017). Accelerating community college graduation rates: A benefit-cost analysis. *The Journal of Higher Education*, 1–27.

Long, B. T., & Mabel, Z. (2012). *Barriers to college success: Pipeline progress to degree completion*. Cambridge: Harvard Graduate School of Education. Unpublished manuscript.

Mabel, Z., & Britton, T. (2017). *Leaving late: Understanding the extent and predictors of college late departure*. Cambridge: Harvard Graduate School of Education. Unpublished manuscript.

.

Martorell, P., & McFarlin, I. (2011). Help or Hindrance? The Effects of College Remediation on Academic and Labor Market Outcomes. *Review of Economics and Statistics, 93*(2), 436–454.

Milkman, K. L., Beshears, J., Choi, J. J., Laibson, D., & Madrian, B. C. (2011). Using implementation intentions prompts to enhance influenza vaccination rates. *Proceedings of the National Academy of Sciences of the United States of America, 108*(26), 10415–20.

Morisano, D., Hirsh, J. B., Peterson, J. B., Pihl, R. O., & Shore, B. M. (2010). Setting, elaborating, and reflecting on personal goals improves academic performance. *Journal of Applied Psychoogy, 95*(2), 255–264.

Nickerson, D. W., & Rogers, T. (2010). Do You Have a Voting Plan? Implementation Intentions, Voter Turnout, and Organic Plan Making. *Psychological Science, 21*(2), 194–199.

Nodine, T., Jaeger, L., Venezia, A., & Bracco, K. R. (2012). *Connection by design: Students' perceptions of their community college experience*. San Francisco: WestEd.

Ramirez, G., & Beilock, S. L. (2011). Writing about testing worries boosts exam performance in the classroom. *Science, 331*(6014), 211–213.

Rosenbaum, J. E., Deil-Amen, R., & Person, A. E. (2006). *After admission: From college access to college success*. New York: Russell Sage Foundation.

Rutschow, E. Z., Cullinan, D., & Welbeck, R. (2012). *Keeping students on course: An impact study of a student success course at Guilford Technical Community College*. New York: MDRC.

Schneider, M. (2010). *Finishing the first lap: The cost of first-year student attrition in America's four-year colleges and universities*. Washington, DC: American Institutes for Research.

Schneider, M., & Yin, L. (2011). *The hidden costs of community colleges*. Washington, DC: American Institutes for Research.

Scott-Clayton, J. (2015). The shapeless river: Does a lack of structure inhibit students' progress at community college? In B. L. Castleman, S. Schwartz, & S. Baum (Eds.), *Decision making for student success: Behavioral insights to improve college access and persistence*. New York: Routledge.

Scott-Clayton, J., Crosta, P. M., & Belfield, C. R. (2014). Improving the targeting of treatment: Evidence from college remediation. *Educational Evaluation and Policy Analysis, 36*(3), 371–393.

Scrivener, S., Weiss, M. J., Ratledge, A., Rudd, T., Sommo, C., & Fresques, H. (2015). *Doubling graduation rates: Three-year effects of CUNY's Accelerated Study in Associate Programs (ASAP) for Developmental Education Students*. New York: MDRC.

Shapiro, D., Dundar, A., Wakhungu, P. K., Yuan, X., Nathan, A., & Hwang, Y. (2016). *Completing College: A National View of Student Attainment Rates - Fall 2010 Cohort* (Signature Report No. 12). Herndon: National Student Clearinghouse Research Center.

Shapiro, D., Dundar, A., Yuan, X., Harrell, A., Wild, J., & Ziskin, M. (2014). *Some College, No Degree: A National View of Students with Some College Enrollment, but No Completion*. Herndon: National Student Clearinghouse Research Center.

U.S. Department of Education. (2015). Digest of Education Statistics 2014, Table 104.92. Retrieved July 28, 2017, from https://nces.ed.gov/programs/digest/d14/tables/dt14_104.92.asp

U.S. Department of Education. (2017). Digest of Education Statistics 2016, Table 503.40. Retrieved from https://nces.ed.gov/programs/digest/d16/tables/dt16_503.40.asp

Visher, M. G., Weiss, M. J., Weissman, E., Rudd, T., & Wathington, H. D. (2012). *The Effects of Learning Communities for Students in Developmental Education: A Synthesis of Findings from Six Community Colleges*. New York: MDRC.

Yeager, D. S., Walton, G. M., Brady, S. T., Akcinar, E. N., Paunesku, D., Keane, L., … Dweck, C. S. (2016). Teaching a lay theory before college narrows achievement gaps at scale. *Proceedings of the National Academy of Sciences of the United States, 113*(24), E3341–E3348.

Table 1. Pre-treatment characteristics of the experimental sample by treatment condition and summary statistics of a nationally representative sample of undergraduates attending public institutions

	(1)	(2)	(3)	(4)	(5)
		Experimental Sample			
	NPSAS:12 Sample	All Students	Treated Students	Control Students	T-C Difference
Male	0.430	0.396	0.390	0.408	-0.013
Black	0.161	0.185	0.194	0.167	-0.003
Hispanic	0.160	0.244	0.257	0.219	0.002
White	0.579	0.389	0.369	0.429	0.015
Race other	0.100	0.121	0.117	0.128	-0.013
Race missing		0.062	0.065	0.058	-0.002
Age	25.9	24.8	25.2	24.1	0.218
Cumulative GPA	2.88	2.96	2.97	2.93	0.023
Attended urban institution	0.543	0.788			
Enrolled in public 2-year institution	0.427	0.343			
Enrolled in public 4-year institution	0.573	0.657			
Transferred into current school		0.473	0.498	0.423	0.025**
Share of total attempted credits earned through spring 2016		0.937	0.938	0.936	0.004
Cumulative credits earned through spring 2016		80.2	79.9	80.9	-0.433
Predicted risk of dropout		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	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). The NPSAS:12 sample is restricted to students attending public two- or four-year institutions and summary statistics are calculated using survey sampling weights. The data in columns 2-5 are from N2FL partner institution administrative records. Means are reported in columns 1-4. Estimates of post-randomization balance are reported in column 5 from OLS/LPM models that include randomization block fixed effects.

Table 2. Estimates of N2FL intervention effects on re-enrollment, credit completion, GPA, and degree completion one year following random assignment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Re-enrolled Spring 2017		Credits Attempted Spring 2017		Credits Completed AY 2016-17		GPA AY 2016-17		Graduated AY 2016-17	
A. Full Sample	0.019	0.020	0.261	0.290	0.490	0.527*	0.039	0.032	-0.005	-0.003
	(0.013)	(0.013)	(0.220)	(0.215)	(0.315)	(0.307)	(0.030)	(0.030)	(0.016)	(0.015)
Control Group Mean	0.808		10.77		20.89		2.899		0.322	
Observations	3,804		3,804		3,804		3,693		3,804	
B. Excluding College 1	0.028**	0.029**	0.387	0.416*	0.440	0.483	0.047	0.039	-0.015	-0.014
	(0.014)	(0.014)	(0.239)	(0.234)	(0.337)	(0.328)	(0.032)	(0.030)	(0.017)	(0.016)
Control Group Mean	0.801		11.01		21.33		2.889		0.337	
Observations	3,304		3,304		3,304		3,206		3,304	
C. Excluding College 2	0.014	0.014	0.299	0.328	0.664*	0.704**	0.048	0.037	0.008	0.009
	(0.014)	(0.013)	(0.238)	(0.232)	(0.345)	(0.335)	(0.032)	(0.032)	(0.016)	(0.016)
Control Group Mean	0.831		11.39		21.66		2.896		0.294	
Observations	3,304		3,304		3,304		3,209		3,304	
D. Excluding Both	0.024	0.025*	0.453*	0.490*	0.632*	0.693*	0.060*	0.048	-0.003	-0.003
	(0.015)	(0.014)	(0.263)	(0.256)	(0.374)	(0.364)	(0.034)	(0.033)	(0.018)	(0.017)
Control Group Mean	0.826		11.75		22.28		2.884		0.308	
Observations	2,804		2,804		2,804		2,722		2,804	
Student-Level Covariates	N	Y	N	Y	N	Y	N	Y	N	Y

*** 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 randomization block fixed effects. Models with covariates also include the following controls: 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 3. Estimates of N2FL intervention effects on re-enrollment, credit completion, GPA, and degree completion one year following random assignment by predicted risk of dropout

	(1)	(2)	(3)	(4)	(5)
	Re-enrolled Spring 2017	Credits Attempted Spring 2017	Credits Completed AY 2016-17	GPA AY 2016-17	Graduated AY 2016-17
A. Full Sample					
Treatment x Predicted Dropout Risk	-0.002	-0.181*	-0.258	0.002	0.005
	(0.007)	(0.107)	(0.160)	(0.018)	(0.007)
Treatment	0.020	0.293	0.531*	0.032	-0.003
	(0.013)	(0.216)	(0.307)	(0.030)	(0.015)
Control Group Mean (at average risk = 0.288)	0.808	10.77	20.89	2.90	0.322
Observations	3,804	3,804	3,804	3,693	3,804
B. Excluding College 1					
Treatment x Predicted Dropout Risk	-0.004	-0.215*	-0.295*	-0.005	0.007
	(0.008)	(0.111)	(0.164)	(0.018)	(0.008)
Treatment	0.029**	0.418*	0.486	0.039	-0.015
	(0.014)	(0.234)	(0.328)	(0.030)	(0.016)
Control Group Mean (at average risk = 0.293)	0.801	11.01	21.33	2.89	0.337
Observations	3,304	3,304	3,304	3,206	3,304
C. Excluding College 2					
Treatment x Predicted Dropout Risk	-0.000	-0.125	-0.073	0.019	0.019***
	(0.008)	(0.117)	(0.176)	(0.020)	(0.007)
Treatment	0.014	0.314	0.695**	0.039	0.011
	(0.014)	(0.230)	(0.334)	(0.033)	(0.016)
Control Group Mean (at average risk = 0.276)	0.831	11.39	21.66	2.896	0.294
Observations	3,304	3,304	3,304	3,209	3,304

Table 3. Estimates of N2FL intervention effects on re-enrollment, credit completion, GPA, and degree completion one year following random assignment by predicted risk of dropout

D. Excluding Both					
Treatment x Predicted Dropout Risk	-0.001	-0.150	-0.100	0.015	0.022***
	(0.008)	(0.122)	(0.182)	(0.021)	(0.008)
Treatment	0.025*	0.482*	0.686*	0.050	-0.002
	(0.015)	(0.255)	(0.363)	(0.033)	(0.017)
Control Group Mean (at average risk = 0.280)	0.826	11.75	22.28	2.88	0.308
Observations	2,804	2,804	2,804	2,722	2,804

*** 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 randomization block fixed effects and pre-treatment covariates. See table 2 for details. 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.

Table 4. Estimates of N2FL intervention effects on degree completion in fall 2016, spring 2017, and combined by predicted risk of dropout

	(1)	(2)	(3)
	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17
A. Full Sample (N = 3,804)			
Treatment x Predicted Dropout Risk	-0.005	0.011*	0.005
	(0.005)	(0.006)	(0.007)
Treatment	-0.009	0.006	-0.003
	(0.010)	(0.013)	(0.015)
Control Group Mean	0.111	0.211	0.322
B. Excluding College 1 (N = 3,304)			
Treatment x Predicted Dropout Risk	-0.004	0.011*	0.007
	(0.006)	(0.006)	(0.008)
Treatment	-0.017	0.003	-0.015
	(0.011)	(0.014)	(0.016)
Control Group Mean	0.116	0.221	0.337
C. Excluding College 2 (N = 3,304)			
Treatment x Predicted Dropout Risk	0.007	0.013*	0.019***
	(0.004)	(0.007)	(0.007)
Treatment	0.010	0.001	0.011
	(0.009)	(0.014)	(0.016)
Control Group Mean	0.071	0.223	0.294
D. Excluding Both (N = 2,804)			
Treatment x Predicted Dropout Risk	0.009**	0.014**	0.022***
	(0.005)	(0.007)	(0.008)
Treatment	0.003	-0.003	-0.000
	(0.010)	(0.016)	(0.017)
Control Group Mean	0.072	0.236	0.308

*** 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 linear probability models that include randomization block fixed effects and pre-treatment covariates. See table 2 for details. 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 N2FL intervention effects on campus resource use in fall 2016 at one site (college 2) where on-campus resource use is observed

	(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. Estimates are from OLS/LPM models that include randomization block fixed effects and pre-treatment covariates. See table 2 for details. Robust standard errors are reported in parentheses.

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

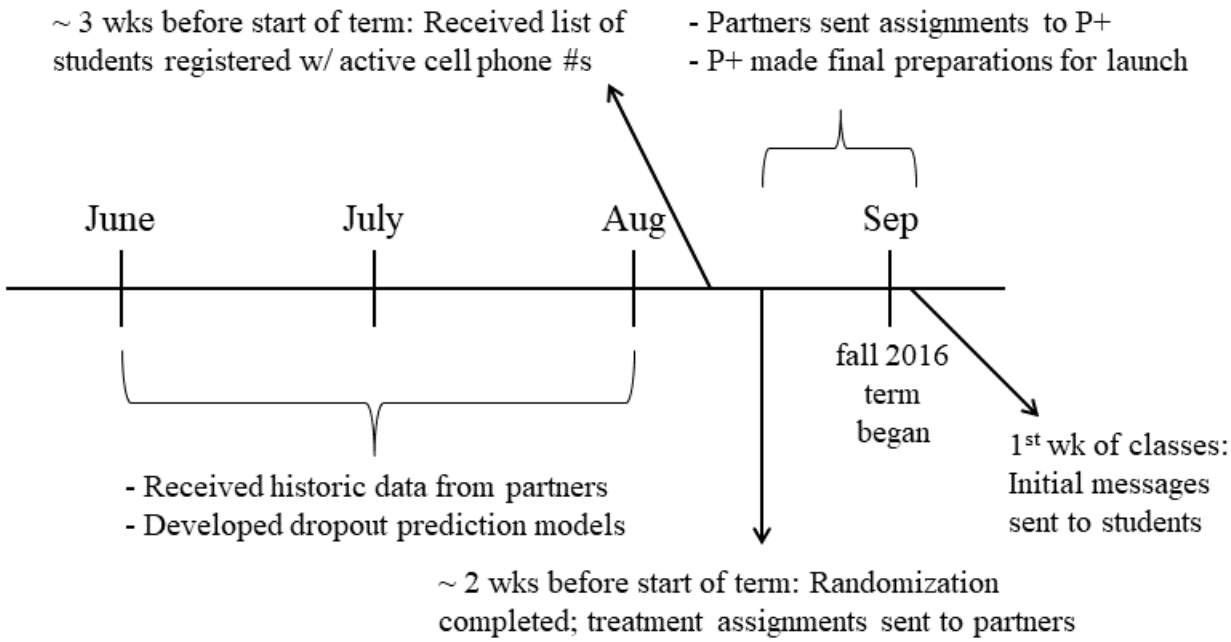
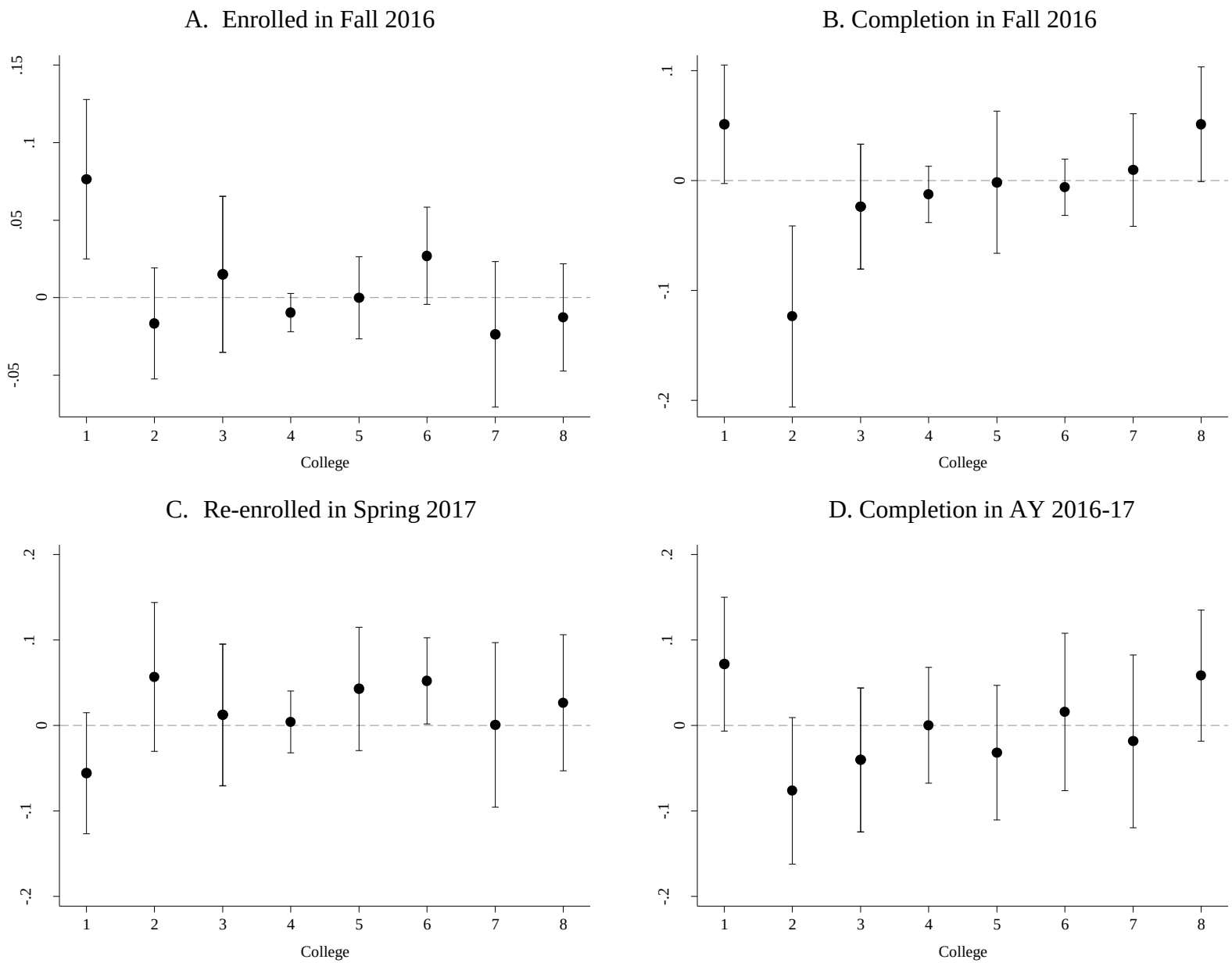


Figure 2. Estimates of N2FL intervention effects on fall enrollment, spring re-enrollment, and degree completion by campus



Notes: Effects for two institutions in the same system are pooled to increase estimation precision.

Appendix A – Supplemental Tables and Figures

Table A1. Estimates of N2FL 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)	(2)	(3)	(4)
	Enrolled Fall 2016	Graduated Fall 2016	Graduated Spring 2017	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. 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***
Number of students	1,268	1,268	1,268

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

Notes: Means are reported in columns 1. Differences relative to bottom-tercile students are reported in columns 2 and 3 from OLS/LPM models that include school fixed effects. The range of risk ratings in the bottom, middle, and top terciles are: 0-0.18; 0.18-0.32; and 0.32-0.97, respective. The respective means by tercile are 0.13, 0.24, and 0.50.

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 N2FL intervention effects on degree completion in 2016-17 by tercile of predicted dropout risk

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample			Excluding Colleges 1 and 2		
	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17	Graduated Fall 2016	Graduated Spring 2017	Graduated AY 2016-17
Treatment x Bottom Tercile	-0.000	-0.013	-0.013	-0.017	-0.027	-0.043
	(0.017)	(0.025)	(0.027)	(0.018)	(0.029)	(0.030)
Treatment x Middle Tercile	-0.009	0.007	-0.002	0.006	-0.022	-0.016
	(0.019)	(0.024)	(0.027)	(0.020)	(0.029)	(0.032)
Treatment x Top Tercile	-0.017	0.027	0.008	0.022	0.042*	0.061**
	(0.017)	(0.020)	(0.024)	(0.014)	(0.023)	(0.026)
P-value on F-test of Equal Effects	0.775	0.462	0.844	0.247	0.092	0.021
Control Group Means						
Bottom Tercile	0.090	0.275	0.366	0.090	0.307	0.397
Middle Tercile	0.131	0.214	0.345	0.084	0.257	0.341
Top Tercile	0.113	0.142	0.255	0.037	0.125	0.162
Observations	3,804	3,804	3,804	2,804	2,804	2,804

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

Notes: The upper bounds of the bottom and middle tercile of predicted dropout risk are 0.17 and 0.33, respectively. In columns 4-6, the sample excludes two colleges where unintended design effects are detected. Effect 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.

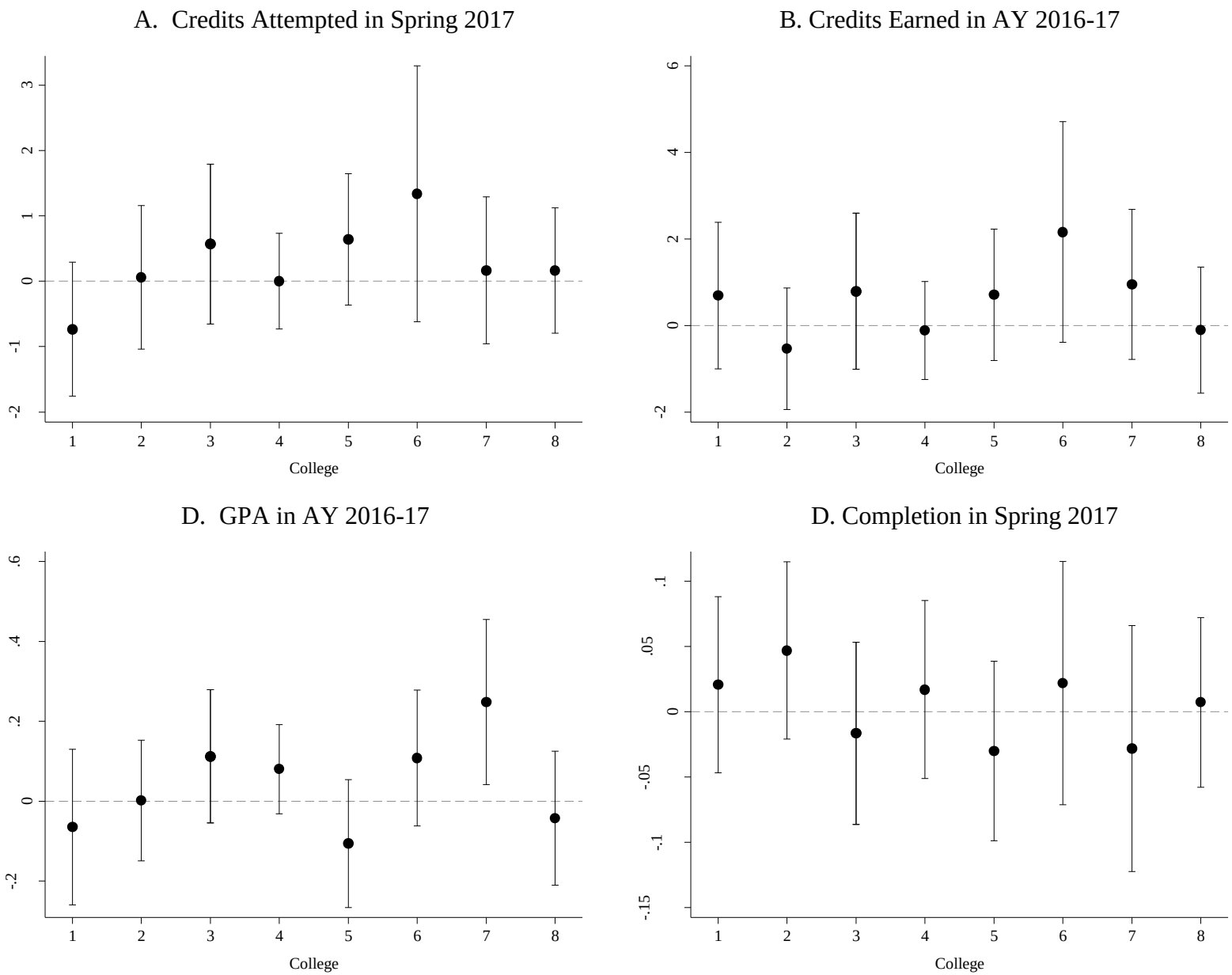
Table A5. OLS and 2SLS estimates of the effect of campus resource use (in minutes) on re-enrollment, credit completion, GPA, and degree completion

	(1)	(2)	(3)	(4)
	OLS Models		2SLS Model	
Outcome	No Controls	+ Covariates	Point Estimate	F-statistic
Re-enrolled Spring 2017	0.000	0.000	0.006	4.895
	(0.001)	(0.001)	(0.007)	
Credits attempted Spring 2017	0.018*	0.015	-0.028	4.895
	(0.011)	(0.011)	(0.084)	
Credits completed AY 2016-17	0.050***	0.044***	-0.120	4.895
	(0.015)	(0.014)	(0.125)	
GPA AY 2016-17	0.003**	0.002**	0.000	5.425
	(0.001)	(0.001)	(0.010)	
Graduated AY 2016-17	0.001	0.000	-0.009	4.895
	(0.001)	(0.001)	(0.008)	

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

Notes: The sample is limited to 500 students at one partner institution where on-campus resource use is observed. For 2SLS estimates, assignment to treatment is instrumented for total minutes of resource use in fall 2016. Coefficients report effects per 10 minutes of additional resource use. 2SLS models include pre-treatment covariates and randomization block fixed effects as exogenous predictors. See table X for the list of pre-treatment covariates.

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



Notes: Effects for two institutions in the same system are pooled to increase estimation precision.

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].

Appendix C – Construction of school-specific predictive dropout models

At each institution, we evaluated the performance of candidate prediction models to identify one that best distinguished between students who dropped out late and students who graduated or were still enrolled in the historical data. The specific models we developed differed slightly across institutions based on data availability. However, in general we compared the performance of models that only included students' fixed attributes, baseline achievement measures, enrollment experiences, and financial aid receipt up to the term they completed one-half of the credits typically required for graduation to models that also included measures of their enrollment history and aid receipt after surpassing this credit threshold.

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:

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

In this model, $Pre50_i$ is a vector of fixed student attributes and time-variant measures prior to attaining the one-half credit completion threshold. 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 influence risk of dropout, $Pre50_i$ 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 over the same period: 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 students' enrollment experiences and financial aid receipt 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. Across all institutions we observed similar model performance in both the development and validation samples.