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Nudging at Scale: Experimental Evidence from FAFSA Completion Campaigns
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ABSTRACT

Do nudge interventions that have generated positive impacts at a local level maintain efficacy when scaled state or nationwide? What specific mechanisms explain the positive impacts of promising smaller-scale nudges? We investigate, through two randomized controlled trials, the impact of a national and state-level campaign to encourage students to apply for financial aid for college. The campaigns collectively reached over 800,000 students, with multiple treatment arms to investigate different potential mechanisms. We find no impacts on financial aid receipt or college enrollment overall or for any student subgroups. We find no evidence that different approaches to message framing, delivery, or timing, or access to one-on-one advising affected campaign efficacy. We discuss why nudge strategies that work locally may be hard to scale effectively.

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I. Introduction

Some of the most promising evidence on the potential efficacy of nudges in public policy comes from efforts to improve low-income students' access to financial aid in the U.S. postsecondary system. Researchers have long recognized that complexities associated with the Free Application for Federal Student Aid (FAFSA) can deter college-ready students from enrolling or succeeding in higher education (Dynarski and Scott-Clayton, 2006; King, 2004). Among the largest school districts, 35-50 percent of high school seniors do not complete the FAFSA prior to graduation.[1] In 2012, for every nine college students who received a Pell grant, there was one student who would have received such funding but who did not submit a FAFSA (Bird, 2016).

Numerous recent studies suggest that nudge campaigns are a low cost but effective way to increase FAFSA completion rates by helping students navigate the complex submission process. Table 1 presents a comprehensive overview of nudge and informational intervention studies in higher education, including their sample, location, and partner organization.[2] Nudges in these studies frequently consist of one-way communication, sending information or connecting students to resources to help them navigate college and financial aid processes. In some cases, nudge campaigns also offer two-way communication, providing students with "light touch" counseling as they make college application, enrollment, and financing decisions. These interventions appeal to behavioral economic models in which students may be inattentive to relevant information or deadlines. They also appeal to neoclassical models of economic behavior because they provide students information about potential higher education investments.

Nearly all existing evidence of positive impacts from nudges in higher education comes from relatively small-scale studies done in partnership with a local organization serving hundreds or thousands of students. These include individual school districts, colleges, and local non-profits in various locations across the United States and internationally. The median study in Table 1 has 6,233 students. Many view these types of interventions as promising because it seems possible to

[1] Authors' calculations based on school-level FAFSA completion data available from Federal Student Aid.
[2] We include studies of nudge and informational interventions that focus on helping high school and current college students navigate college decisions. With few exceptions, all studies are randomized controlled trials. We exclude studies that involve informational interventions in earlier grades that focus on shaping students' college expectations and/or persistence in primary and secondary education. We also exclude studies that involve more intensive college coaching or mentoring interventions given the higher cost typically associated with these types of outreach, though we acknowledge the distinction between nudge interventions that offer two-way communication with an advisor and coaching interventions is not always clear.

scale them at low cost. Such scaling could involve partnership with a single statewide or nationwide organization connected to many students, which we refer to here as “global” scale up. Alternatively, “local” scale up could occur through coordinating efforts of multiple smaller organizations that are more local in nature than statewide or nationwide ones. We currently have, however, limited evidence on whether such interventions maintain such efficacy at scale and what effective pathways to scale might be (i.e., global versus local scale up). Additionally, we have little evidence regarding the specific mechanisms that explains these interventions’ efficacy.

In this paper, we provide some of the first evidence on these issues through a global scale up of FAFSA nudges studied in previous research with much smaller samples. We run the largest FAFSA nudge campaigns to date by partnering with one nationwide organization and one statewide organization to send information about FAFSA filing to high school and college students. The experimental sample comprises 800,000 students across both partners. The first intervention targeted all lower-income and first-generation high school seniors who had registered with the Common Application, a national portal through which students can apply to up to 20 of 800+ participating colleges with a single application. Treated students received messages encouraging them to complete the FAFSA early to maximize the financial aid they received. We randomly varied the messages along multiple dimensions, including their: behavioral framing; delivery channel (mail, e-mail, or text message); offer of one-on-one advising assistance; and a social nudge to encourage peers to complete the FAFSA as well.

The second intervention targeted all students in a Large State (“Large State”) who had applied to college through a state-sponsored portal that allows applications to all the state’s public four-year colleges, as well as to some private institutions and community colleges. Treated students received text messages informing them of two important changes in federal financial aid policy: the ability to file the FAFSA starting October (rather than January) of the year prior to college enrollment and the ability to use income tax returns their families had already filed in the financial aid application process. These messages also varied along multiple dimensions, including their timing and the use of infographics to increase visual salience. We experimentally varied many aspects of the intervention and targeted different populations.

We consistently find no effect these messages on student enrollment or financial aid outcomes. This null finding is consistent across samples, content, timing, visual presentation, and offers of personalized help. Large sample sizes allow us to rule out very small effects of these interventions. We believe the design of our experiments and these precise null results contribute in three ways to the existing research literature.

First, and most broadly, our work shows that recent concerns about scaling up randomized controlled trials in developing countries are also relevant in the context of developed countries. Development economists have recently highlighted promising pilot programs that fail to replicate at scale. This can occur for a number of reasons including poor fidelity to the treatment, site selection bias, and context dependence (Allcott, 2015; Bold et al., 2018; Banerjee et al., 2008; Banerjee et al., 2017). Global scale up of nudges in higher education may also face substantial barriers to success, for reasons we discuss further below.

Second, nearly all prior financial aid nudge campaign studies have reached students by partnering with individual high schools, colleges, or local college-focused non-profits. Many of those smaller scale studies demonstrated that messages sent to students at critical financial aid junctures can at low cost generate substantial improvements in college enrollment (Castleman and Page, 2015; Castleman and Page, 2016a; ideas42, 2016; Page, Castleman, and Meyer, 2018).[3] Very few studies have done scaled up outreach using a global scale approach. Those that have looked at a large scale such as states or national organizations have, consistent with our results, found little or no effects. When effects are detected, they are often much smaller than those found in early work or apply only to a narrow set of students (Hyman, 2018; Gurantz et al., 2019; Bergman et al., 2019).

While our research takes a similar global approach to scale as these recent studies, we extend our understanding of the efficacy of nudge interventions at scale in several ways. Like Bergman et al. 2019, we focus on the impact of financial aid nudges for a broader segment of the academic distribution of potential college students. Other recent studies pursuing a global approach to scale focus on a more narrow, higher-achieving student population. Our study more closely adheres to the behaviorally-informed design—both in terms of content framing and delivery—of prior evidence-based studies. Other recent large-scale studies rely more heavily on delivery methods (e.g., mail or email) that may be less effective at reaching students or they contained dense information that may have required substantially greater cognitive load for students to

[3] Other similar studies have considered nudges designed to increase academic performance through studying with mixed success (Oreopoulos et al. (2018a), Oreopoulos et al. (2018b) Dobronyi et al. (2018)). One on one coaching seems to be the most effective but least scalable method to improve success (Oreopoulos & Petronijevic 2018)

understand. Finally, our intervention focused on a discrete and consequential action: applying for financial aid. As a result, the intervention focused on concrete, near-term calls to action for students, whereas other recent studies rely on students completing numerous college planning tasks or depend on students to be motivated by benefits (in the case of tax credits for higher education) that they would not realize for numerous months.

Third, our results help answer important open questions about the mechanisms that drive prior campaigns' efficacy. Many of these prior campaigns provided one-on-one advising available to all students who responded to messages they received, or use some form of financial incentive to encourage student engagement with informational materials and/or with advising (Carrell and Sacerdote, 2017; Castleman and Page, 2015; Castleman and Page, 2016a; Hoxby and Turner, 2013). Therefore, it is difficult to disentangle how much of the impact observed in past research stems from simplified information, ongoing reminders, reduced barriers to professional advising, or from eliminating near-term costs (e.g., application fee waivers), or how much the positive results were a function of some other study-specific context or population. We designed our experiment to mimic many of these intervention features. We offered college advising to students, varied the content and delivery method of information, and sent numerous reminders. We find that none of these variations ultimately mattered for student outcomes, suggesting that some other distinguishing feature(s) between prior studies and our interventions accounts for positive effects in the more localized interventions in this area.

We note three hypotheses that might explain our null results given the past successes of very similar, smaller scale campaigns. First, unlike this paper, most prior work involved a local partner with closer connections to and knowledge of treated students. Local partners may know something important about their students and such students may react differently to messages from partners they feel are specifically invested in them or their communities. Second and relatedly, the global scale-up in this study implied messaging content was more generic and less personalized to students than in prior interventions, perhaps resulting in lower salience for students. Third, current cohorts of students may have better information about FAFSA completion than did previous cohorts, so that there exist fewer students for whom nudge campaigns would make a difference on the margin.

Though we cannot distinguish between these hypotheses, our work suggests fruitful areas for further research on the difficulties of scaling nudge campaigns and demonstrates the potential pitfall for “global” approaches to scaling nudge campaigns. At the very least, our work suggests that future attempts to scale up such campaigns should proceed cautiously and with attention paid to these hypotheses.

II. Intervention Design

Common Application Experiment. Our outreach consisted of two campaigns: a fall campaign, in which students received two email messages encouraging them to consider college affordability when deciding where to apply, and a multi-modal (email, text message, and postal) winter campaign encouraging students to complete the Free Application for Federal Student Aid (FAFSA) as early in the calendar year as possible to maximize their financial aid. The fall email campaign reached students who had registered with the Common Application by October 12, 2015 (“Fall cohort”). The winter campaign targeted the Fall cohort as well as the additional students who registered with the Common Application by December 18, 2016 (“Winter cohort”). Students in the Fall cohort received their first email message between October 26 and October 30, 2015. The Common Application sent a follow-up e-mail message between November 11 and November 16, 2015. The Common Application sent the first winter email during the week of January 11, 2016 and a second email the week of February 1, 2016. For students who entered in the Fall cohort, these were in practice their third or fourth emails.

The Common Application sent a generic introductory text message the week of January 10, 2016 to all students assigned to a texting condition, simply informing them that they would receive a set of messages over the following weeks and encouraging them to save the number associated with the text so that it would be recognized by their phone. Text messages tailored to assigned experimental treatments were then sent the weeks of January 17, January 24, January 31, February 7, and February 14. Roughly 87 percent of our experimental samples were assigned to receive text messages—this includes all students assigned to the experimental variations, as well as 50 percent of students in the control condition who received basic text messages.[4] Roughly one-third of students also received a three-page postal mailer in mid-January.

[4] Not all text messages were delivered, for one of two reasons: (1) the number the student provided was not a valid cell-phone number; and (2) an unanticipated issue with cell-phone carrier “spam” filters blocking text message delivery, which resulted in approximately 28 percent of scheduled messages not being delivered. We discuss this further in Appendix B.

Large State Experiment. The Large State intervention targeted several cohorts of students, including rising freshmen, current college students, students who had previously applied but not enrolled, and students who left college with substantial credits but no degree. We focus on the rising freshman sample for comparability to the Common Application experiment. The other samples yield similar results with no effect on student outcomes in any of the samples, and the message variations were generally similar across groups. The rising freshman sample consists of students who graduated from high school in summer 2016 and applied to college using the state application portal for fall 2016.[5]

The state agency we partnered with sent an introductory text message, similar to the one used in the Common Application experiment, on October 11, 2016 to all students assigned to a texting condition. Text messages for each treatment condition were then sent on the following dates: October 18, October 25, November 1, November 8, and November 22, 2016.[6] The agency sent a final text message on January 10, 2017 reminding students about steps they could take to apply for financial aid.

Experimental variations

In the Common Application and Large State experiments we tested multiple mechanisms that could affect campaign efficacy (we describe the specific randomization procedure to assign students to different experimental conditions in more detail below).

<u>Common Application</u>

- *Variations in access to one-on-one advising.* Several prior studies demonstrate that providing students with one-on-one college or financial advising can lead to substantially improved postsecondary outcomes (Avery, 2013; Barr and Castleman, 2017; Carrell and Sacerdote, 2017; Castleman and Goodman, 2018; Castleman, Page, and Schooley, 2015). By contrast, information-only interventions to improve access to financial aid or college

[5] The application portal also sent text messages to students who applied prior Fall 2016 but who had not enrolled in school in the previous three years—this is the "Applied Not Enroll" Group in the tables. The Applied Not Enroll Group had 110,000 treated students and 207,193 control students. Currently enrolled students in college who had previously used the application portal are referred to as "Enrolled" in the table. The currently enrolled group had 149,736 treated students and 367,006 control students. The last group, "Leavers" were students who had attempted 60 credits at a university or 30 credits at a community college who left without obtaining a degree. For leavers there were 20,671 treated students and 10,727 students in the control group.

[6] For current students, we also varied treatment timing with some students receiving messages beginning in mid-September 2016 ("early start"), just prior to the start of FAFSA filing, and others receiving messages starting October 11, 2016 ("on time start"), after FAFSA filing began.

have generally had limited or no effect (Bergman, et al., 2019; Bettinger et al., 2016; Hyman 2018). We therefore randomly varied whether students received information-only outreach or nudges that invited students to connect to an advisor for support with FAFSA completion.

- *Variations in content framing.* Behavioral science theory and empirical evidence demonstrate that the way content is framed can influence how salient it is to individuals and whether the information drives behavior change. For instance, foundational research in behavioral economics demonstrated that people tend to prefer certain benefits over potential gains (Kahneman and Tversky, 1979). Students from low-income families tend to overestimate their net cost of college, suggesting they may not have a precise understanding of the financial aid for which they are eligible (Avery and Kane, 2004; Grodsky and Jones, 2007). This suggests that making the financial benefits associated with FAFSA completion more salient—particularly relative to the amount of time necessary to complete the FAFSA—may motivate some students on the margin of FAFSA completion to invest the time necessary to apply for financial aid. Based on this hypothesis, we developed a "Financial Benefits" content variation. Other behavioral science research shows that individuals' identities have strong effects on their attitudes and behaviors (Baumeister 1987). Individuals have a vested interest in preserving positive perceptions of themselves, and thus, are likely to behave in a manner consistent with positive past behaviors (Swann, Jr. and Ely 1984,) showing a robust "self-consistency bias." If students embrace an externally-activated identity (such as a nudge that reinforces the motivation they have shown by starting their college applications), they are more likely to take actions that are framed as identity-consistent. Based on this hypothesis, we developed an "Identity/Norms" content variation. Finally, researchers demonstrate that providing people with concrete planning prompts and guidance can increase follow through on various actions, from voting to getting a flu vaccination (Nickerson and Rogers, 2010; Milkman et al., 2012). Especially for adolescents, who are more likely to struggle with organization, planning, and time management (Casey and Somerville, 2011), guiding students to form concrete implementation intentions for when, how, and with whom they will complete FAFSA may further contribute to increased filing rates. Based on this hypothesis, we developed a "Planning" content variation.

- *Variations in message delivery.* Foundational behavioral science research also demonstrated that the channels through which people receive information can affect how they respond to it (Leventhal, 1963). We varied whether students received outreach through postal letters, emails, and/or text messages to investigate whether delivery channel affected campaign impacts.[7]

Large state

- *Variation in information presentation (infographics versus text).* Visual imagery can more effectively capture attention and make information more salient. We varied whether students received infographics with information about how to apply for financial aid or received texts containing the same information but without the infographics to investigate whether visual images can prompt students to take action.
- *Variation in timing of messages.* For some of the student samples in the Large State experiment, we varied the timing of messages, with some students receiving messages several weeks before the FAFSA could be filed and others receiving messages around the time FAFSA filing became available (FAFSA filing began October 1). Students in the "early" treatment condition received messages beginning in mid-September (FAFSA could be filed starting October 1); students in the "on-time" treatment condition received messages beginning in early to mid-October. This variation allowed us to examine whether students benefit from having additional time to prepare to undertake important tasks or if "just in time" messaging is more effective. We do not, however, have this source of variation for the rising freshman sample.
- *Variations in motivational framing.* We developed a motivational framing for the current college student sample similar to the one described above for the Common Application sample.

We provide additional detail on intervention content for both experiments in Appendix B and intervention materials can be found in Appendix C.

[7] We also tested a school-level experimental variation in which a subset of students were randomly assigned to encourage other students at their school to complete FAFSA. As with the other intervention arms we find no impact of the social nudge on students' postsecondary outcomes. Because of the different structure of the randomization and given the breadth of variations we already cover in the paper we do not present results from the social nudge in this paper but can provide them upon request.

III. Research Design

Randomization Procedure

Common Application

We randomly assigned students to one of the experimental conditions described above in two phases. In October 2015, we identified students who had by that time registered with the Common Application and who met at least one of the three following "low socio-economic status" (low-SES) criteria[8]:

1. Indicated on their application that they qualified for a need-based application fee waiver
2. Indicated that they were the first in their family to go to college
3. Indicated that they intended to apply for need-based financial aid AND attended a high school where at least 40 percent of students qualified for free or reduced price lunch[9]

For the Fall cohort, we excluded students with a reported SAT score of at least 1230 for the math and verbal sections or a reported ACT score of at least 28 (n=36,632) because these students were receiving a different and concurrent intervention from the Common Application focused on college applications. The resulting sample size for the Fall cohort was 187,482 students. To perform the randomization for the Fall cohort, we first randomly selected 2,000 students to receive the additional offer of one-on-one advising.[10] We did so by randomly selecting 2,000 schools to designate as "advising schools", and then randomly selecting one student per advising school to receive advising. To ensure that students selected for the advising condition were representative of this population of low-SES Common Applicants, we set the probability for whether a high school was randomly selected directly proportional to the proportion of low-SES Common Applicants attending that high school. That is, a high school with 100 low-SES Common Applicants would be more likely to be chosen as an advising school, compared to a high school with 50 low-SES Common Applicants. For each student assigned to advising, we then randomly assigned one of three content variations to pair with the offer of advising (i.e., Advising + Financial

[8] We excluded from our sample 18,602 low-SES students attending high schools participating in a similar messaging intervention.
[9] Using high-school level data from NCES's Common Core of Data (CCD), we calculated percent of students who qualified for free or reduced priced lunch during the 2013-14 academic year. We were able to match this information from CCD to the Common Application data for 95.3% of public school students (90.4% of public schools).
[10] This relatively small sample size for the Advising experimental condition was due to the high resource nature of the one-on-one advising offered.

Benefit, Advising + Identity/Norms, and Advising + Planning). Finally, we randomly assigned the remaining 185,482 students to the other four experimental conditions (Control, Financial Benefit, Identity/Norms, Planning) in equal proportions. We performed this randomization *within high school* in order to increase the precision of our estimates by controlling for school level differences in student outcomes.

We conducted the second phase of the randomization in mid-December 2015 for winter emails, text messages, and postal mailers. We included all students from the October cohort in the winter campaign, with the exception of 259 students who terminated their accounts with Common Application prior to the December randomization. Also included in the winter campaign were low-SES students who registered with Common Application between October and December, and the low-SES high-achievers previously excluded from the October cohort. This process resulted in an additional 267,020 students for the winter campaign. All students in the October cohort maintained their student-level content variation assignment. For the December cohort, we randomly assigned students to one of four content variations (Control, Financial Benefit, Identity/Norms, or Planning) in equal proportions. Again, we performed this student-level randomization *within high-school*. All students in the winter campaign received email messages. All treated students were also eligible to receive text messages; over 99% of students provided cell phone numbers. Roughly one-third of students were randomly assigned (within high-school and treatment variation) to receive postal mailers.

Large State

We randomized students who had completed high school and applied for college admission in Fall 2016 into treatment and control groups. We will refer to this group as "rising freshman." In total we assigned 70,000 students to receive text messages with 115,793 students not receiving any text messages who constitute our control group. We stratified on whether students had applied to four-year schools, two-year schools, or both. Due to data timing issues, we could not confirm whether students enrolled in the fall prior to sending messages. Hence, some students who applied to college and did not enroll are included in our treatment group. Randomization for the other groups including Apply Not Enroll, Enrolled, and Leavers was similar. For enrolled students we also stratified on student classification (e.g. freshman, sophomore, etc.).

Empirical strategy

Our evaluation of the impact of the nudge campaign on college and financial aid outcomes relies on college enrollment data provided by the National Student Clearinghouse (NSC) as well as administrative data from the Large State. The exact variables available differ slightly from each source.

Common Application

For the Common Application intervention, we use the following regression models to estimate the impact of our treatments, either overall or by experimental conditions:

$$Y_{isw} = \beta_0 + \beta_1 AnyTreatment_{isw} + \delta_{sw} + \epsilon_{isw} \quad (1)$$

$$Y_{isw} = \beta_0 + \beta_1 Advising_{isw} + \beta_2 Plan_{isw} + \beta_3 Norms_{isw} + \beta_4 Financial_{isw} + \delta_{sw} + \epsilon_{isw} \quad (2)$$

Here, Y is a given outcome for student i in high school s first treated in wave w (fall or winter of senior year). In equation (1), *AnyTreatment* indicates whether the student was in any of the experimental (non-control) conditions. In equation (2), *Advising* indicates whether the student was assigned to receive one-on-one advising via text message. *Plan*, *Norms* and *Financial* indicate which content treatment arm to which the student was assigned. Note that, because students in the advising conditions were also assigned to one of the three content variations, *Advising* is not mutually exclusive from *Plan*, *Norms*, or *Financial*. The omitted category is the control condition. All regressions include high school by treatment wave fixed effects given, δ_{sw}, that the randomization was conducted within such strata. We cluster standard errors by high school to account for potentially unobserved correlations in the error terms across high school classmates. We show separately that controlling for further demographics has little impact on our estimates due to the randomization.

Common Application outcome sample construction

Due to the cost of NSC matches, we were not able to receive college enrollment outcome data for all students in our sample. We selected a subset of the full sample to receive NSC matches with which to perform our outcome evaluation in January 2018, which allow us to observe enrollment and college choice outcomes for the three semesters (Fall 2016, Spring 2017, and Fall

2017) following the students' expected year of high school graduation. We selected 271,365 students for the NSC sample by the following method:

1. In order to take full advantage of the within high school randomization procedure, we first identified all high schools at which all the following experimental variations were represented by at least one student:
    a. Control condition
    b. Control condition + basic text messages
    c. Financial Benefit, no postal mailer
    d. Financial Benefit + postal mailer
    e. Identity/Norms, no postal mailer
    f. Identify/Norms + postal mailer
    g. Planning, no postal mailer
    h. Planning + postal mailer
    i. Advising, any content variation (if an advising school)
2. Among these high schools where there was at least one student in each of the nine variations, we first selected all advising high schools to contribute to the NSC sample. We gave preference to advising schools when constructing our NSC sample in order to maximize the statistical power to detect effects of the advising intervention, which was offered to only one student per advising high school. This process led to a total of 1,714 advising high schools, and a total of 130,151 individual students, for the NSC sample.
3. Among non-advising high schools, we then randomly selected individual high schools to contribute to the NSC sample until the full 271,365 students were identified. This made a total of 3,681 non-advising high schools, and a total of 141,214 students for the NSC sample. Only roughly half of students from the last high school selected entered the NSC sample, due to space constraints.

This sampling procedure does give preference to high schools with more students in the full experimental sample—that is, high schools with some combination of larger enrollment counts, a larger proportion of lower-SES students, and a larger proportion of students who are Common Application users. However, when we compare the full experimental sample and the NSC sample on observable student characteristics, the two samples are quite similar (see Table 2). For instance, both samples are approximately 60 percent female, 65 percent first in their family to go to college, and 43 percent used a means-tested fee waiver to submit applications through the Common Application.

Large State

Our empirical strategy is very similar for the Large State intervention. We run regressions of the form:

$$Y_i = \beta_0 + \beta_1 Treatment_i + \boldsymbol{X_i}\boldsymbol{\Gamma} + \varepsilon_i \tag{3}$$

Where Y_i is an outcome for student i, $Treatment_i$ is an indicator for a student being assigned any treatment, $\boldsymbol{X_i}$ is a vector of student controls including indicators for parent education, self-reported family income, age at application, gender, and race. We sometimes separate $Treatment_i$ into indicators to test if messages that contained infographics had a different effect from text-based content or early versus on-time information. All regressions also control for the level of stratification which is what school types a student applied to. Robust standard errors are presented. We use administrative data from Large State to measure financial aid and enrollment outcomes. These administrative data contain data from the universe of public higher education institutions in the state. As we show in Table 3, the sample of rising first year students in Large State (the primary sample we focus on in our analyses) is over 50 percent female, just over 30 percent of students' mothers have a high school diploma or less, and 28 percent of the sample reports annual family income below $80,000.[11]

Results

We first present evidence of baseline equivalence and then present results on college enrollment and persistence as well as financial aid receipt for the overall samples. Next, we discuss whether the effects vary by student characteristics or variations in treatment. Consistently, we find no effect of treatment.

Baseline equivalence

[11] We report slightly different measures of parental education level in the Large State than Common Application based on the data available from each partner. Family income is missing for 47 percent of the Large State sample.

We assess baseline equivalence by regressing student-level characteristics on indicators of treatment; in the case of the Common Application, we also include high school by cohort fixed-effects in these regression models, as this was the level of randomization. Tables 4 and 5 display the results for the Common Application and Large State interventions, respectively. For the Common Application, we present the results of these baseline equivalence tests for both the full experimental sample and the NSC evaluation sample. Overall, we find that the sample is well balanced on baseline covariates. There is one marginally significant difference in the Large State sample in terms of age at application, which is to be expected given the number of tests. However, the magnitude of this difference is extremely small. We also test baseline equivalence across treatment variations, and find similar evidence of equivalence.[12]

Financial aid, college enrollment, and persistence

In Tables 6 and 7 we present evidence of the Common Application (Table 6) and Large State financial aid nudge campaign (Table 7) impacts on students' college enrollment, enrollment quality, and persistence. In the Large State intervention, we are also able to report on the impact of the intervention on a measure of students' FAFSA filing and financial aid receipt. These are particularly useful because the intervention was specifically targeted to affect student's FAFSA filing and financial aid packages.

As we show in Table 6, students in the Common Application sample enroll in college at high rates overall. Eighty-two percent of the control group enrolled in college in Fall 2016 and 73 percent enrolled at four-year institutions. The financial aid nudges had a precisely estimated zero impact on overall enrollment or enrollment by institution level. Given the size of our sample we can rule out impacts greater than half a percentage point. Persistence rates are similarly high among the sample; 74 percent of students remained continuously enrolled into their second year of college. We estimate precise zero impacts on continuous enrollment and can again rule out impacts greater than half a percentage point. We also find little evidence that treatment of Common Application students affected college application patterns or other measures of quality of college students enrolled in (Appendix Table A2).[13]

[12] Results available upon request

[13] In Appendix Table A1 we show that the Common Application nudge campaign also did not affect a range of application outcomes, such as whether students applied to any colleges, the number of applications they submitted, and the quality of institutions to which students applied. In Appendix Table A2 we consider a broader set of

In Table 7 we present impacts on college enrollment for the Large State intervention. We do not find impacts on our proxy for filing a FAFSA, Pell receipt, or loan amounts. We observe enrollment outcomes in the spring semester following the intervention and then in the following fall (2017-2018 academic year). We also observe a measure for whether students filed a FAFSA[14] for the academic year following the intervention, how much students received in Pell Grant assistance, and how much students borrowed in loans. Relative to the Common Application sample, overall enrollment rates for the Large State sample are lower. Fifty-four percent of students enrolled in college in the first spring after the intervention and 49 percent enrolled in the first fall after the intervention. We find no effect on enrollment overall in spring 2017 or at four-year institutions, for which we can rule out impacts larger than one percentage point. The intervention led to a modest 1.1 percentage point increase in enrollment at two-year institutions that is statistically significant at the 10 percent level in the first spring. The effect on two-year enrollment disappears in the next semester.

We find no impact of the Large State intervention on enrollment in the first fall after the intervention and we can rule out overall enrollment impacts greater than 1.3 percentage points. It is not surprising that there are no effects on student enrollment given that the outreach was designed to affect financial aid outcomes but did not actually change student's FAFSA filing or financial aid packages. In Appendix Table A3 we show, similar to the Common Application experiment, that the Large State intervention did not affect the quality of the institution attended.[15] A sizeable share of students engaged with the content—over 40 percent of the treated sample responded to the text messages. This suggests that a lack of information about FAFSA completion does not explain the null effects we observe—many students interacted with the information they received.

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enrollment quality measures for the Common Application sample. We find evidence that the intervention led to a very small positive and significant increase, in the range of 0.5 percentage points, in the share of treated students attending public institutions and institutions with net prices below $15,000/year. However, these impacts are too small to be practically significant and we do not find evidence of impact on other quality measures, such as the mean graduation rate, admission rate, or median SAT scores at institutions students attended.

[14] We do not observe FAFSA filing directly. Instead we know if students filed the FAFSA, received merit aid, or filed the state-specific form for financial aid if they were enrolled. Because we find no effect on enrollment, interpretation of this estimate is that among enrolled students, there was no change in the probability of filing the FAFSA/state-specific form or receipt of merit aid.

[15] We report different institutional quality measures in the Common App and Large State experiments based on data available from each partner.

Subgroup impacts

While we find no effects overall, this could mask important heterogeneity in which students are affected by treatment. For instance, students in the fall and winter versions of the Common Application experiment could vary in their probability of enrolling due to differences in their underlying characteristics or the timing of the information. In Appendix Table A4 we show that the effect of the Common Application intervention did not vary by whether we enrolled students in the outreach in the fall or winter of their senior year in high school.

These types of interventions could also have heterogenous effects by measures of student background. We consider heterogeneity by whether a student was a first-generation college student, applied using a fee waiver, was female, had a high SAT score, or came from a high school with high or low fractions of free and reduced-price lunch. In Appendix Table A5 (Common Application) we largely estimate null impacts for each subgroup, and for the few subgroups for whom we obtain significant (yet small in terms of magnitude) results, we do not find consistent patterns of impacts across outcomes. In no case do the subgroup estimates rise above one percentage point in magnitude. Appendix Table A6 similarly shows no difference in the impact of the Large State intervention across subgroups including first generation in college, gender, and low-income students.

In Table 8 we show results of the Large State interventions for the other cohorts of students with whom we intervened—current college students, students who had applied but not enrolled in college, and students who earned substantial credits but withdrew prior to a degree. We find no impacts of the intervention for any of these students.

One advantage of our study is that we can look at many different subgroups, many of which include groups of students who have been found to benefit in previous interventions. We find that irrespective of time in the college application/enrollment process and student characteristics, our interventions had no impact.

Treatment variations

In Tables 9, 10, and 11 we present estimates of whether the impact of the Common Application and Large State interventions varied based on the treatment variations we tested. In Table 9 we investigate whether impacts of the Common Application intervention on college enrollment and enrollment quality varied by how we framed the financial aid nudge content.

Overall, we do not find meaningful differences in outcomes across content frames. There are some outcomes for which the financial benefit or planning content frame estimates are statistically significant, but in almost all cases the magnitude of these impacts relative to the control mean is less than one percent. Relative to the information-only content frame treatment arms, the point estimates on the advising intervention are slightly larger and because this intervention arm only had ~2,000 students, the confidence intervals are wider. For instance, we cannot rule out impacts in the 2-3 percentage point range for advising on whether students enrolled in college in the first fall after the intervention. Even these impacts would still be modest, but in line with the impacts of other text-supported advising interventions (e.g., Castleman and Page, 2017).

In Table 10 we present evidence that compares receiving the information as text or as an infographic relative to no information. We find no difference in whether students enrolled or the sector that they enrolled in. We also find no effects on financial aid outcomes based on how we present information to students. For students who were enrolled at four-year institutions, we varied whether students received the information early or late; these results are presented in Panel B of Table 10. There was no effect on overall enrollment but perhaps a shift from two-year enrollment to four-year enrollment. It also appears that early text messages may have had a very modest effect on our proxy for filing a FAFSA but this result is marginally statistically significant.

In Table 11 we test whether impacts of the Common Application intervention varied based on how we delivered content to students. We do not find consistent differences in impact across delivery method, and in no case do the impacts associated with a particular delivery method or combination of methods exceed a percentage point in magnitude.

Discussion

Despite substantial and growing interest in behavioral science interventions in education at various levels of government, we currently have little evidence about whether nudge interventions that have generated positive impacts on postsecondary outcomes at a local level can be scaled and maintain efficacy. We also have little evidence about the specific mechanisms underlying the positive impacts of promising smaller-scale nudges. Organizations, policymakers, and researchers seeking to scale nudge interventions can do so via two primary pathways: one is a global approach, partnering with a state or national organization to reach out to large numbers of students, while another is a local approach that consists of partnering with many local organizations.

Leveraging both statewide and nationwide large-scale experiments with a collective sample of over 800,000 students, we find no effect of a global approach to scaling financial aid nudges on students' financial aid receipt, college enrollment, or college persistence. Our study contributes to a small body of recent research that demonstrates that large-scale information and nudge interventions have little effect on students' college or financial aid outcomes (Bergman, et al. 2019; Bettinger et al., 2012; Gurantz et al. 2019; Hyman 2018).

A natural question is why these interventions failed to improve postsecondary outcomes when smaller scale interventions led to increased college enrollment and persistence? Given the nature of our experiments we can rule out several hypotheses. First, our lack of positive impacts was not due to the delivery method. Students were randomly assigned to different combinations of physical mailers, text messages, and emails, and none of the combinations of channels had any effect. Second, we do not find evidence that nudges at scale are effective for a wide range of specific student populations who are at various stages of the college-going and completion process. Many previous nudge interventions, including large-scale campaigns, have focused on college-intending or high-achieving students (e.g., Castleman and Page, 2015, 2016a; Gurantz et al., 2019; Hoxby & Turner, 2013; Hyman, Forthcoming), as shown in the descriptions of sample populations from prior studies in Table 1. Our experiments included students with high baseline college-going rates (Common Application) and low baseline college-going rates (Large State intervention). Prior experiments have also demonstrated that low-touch interventions can lead to improved postsecondary outcomes for students earlier in the college application process (e.g., Hoxby and Turner, 2013) as well as for students much later in the process (e.g., Carrell and Sacerdote, 2017; Castleman and Page, 2015). In the Large State intervention we targeted groups of students at various stages of college-going and completion: rising freshmen, currently enrolled students, students who applied but did not enroll in college, and students who completed substantial credits but who withdrew prior to a degree. We find no impact for any of these populations.

Third, we find little evidence to support that the way content is framed affects whether financial aid nudges are effective. We did not test every possible version of behaviorally informed content, but we find no difference in aid receipt or enrollment based on several different approaches to frame intervention content. One channel we cannot rule out as definitively is the importance of making one-on-one advising available to students. While we do not find significant positive effects of offering students in the Common Application sample the opportunity to connect via text with an advisor, our confidence intervals include impacts (2-3 pp) of a similar magnitude that more local interventions have found from text-based nudge approaches. Additionally, the take-up rate of the offer of advising was much smaller in our Common Application intervention compared with prior efforts, with only 11.6 percent of students ever responding to a text message (Table A6). Take up could be lower for a number of reasons but it seems likely that the more distant relationship between the Common Application and students as compared to earlier studies may have played a role. The possibility that large-scale nudging with the option of two-way advising could have a positive impact is broadly consistent with the body of research on nudge interventions in postsecondary education summarized in Table 1: campaigns that offer two-way interaction tend to be more likely to influence students' college decisions than those that facilitate one-on-one connections to a counselor, advisor, or mentor. Yet from a feasibility perspective, it would be challenging for a large organization like the Common Application to staff a sufficient number of advisors to provide meaningful two-way advising to hundreds of thousands of students.

Having ruled out several potential explanations for why the large-scale nudges were not effective, we offer several possibilities for why interventions that worked at a local level may not effectively scale up. First, if participants infer that an intervention is delivered broadly, the salience and value of the campaign for any one recipient may be diluted. In the context of our study, students presumably had a weaker connection to the Common Application and the Large State partner than they presumably did to smaller, community-based organizations that were the ostensible message sender in many prior interventions. Second, the messages were primarily generic and one-way, one of the limitations of a global approach to scale, so students may very quickly have concluded that they were receiving the same outreach as many other students. The lack of a more direct relationship with the sender and the generic nature of outreach could both explain our lack of impacts. A parallel large-scale texting campaign conducted by Avery et al. (in progress) should lend interesting insight into this hypothesis, since their project features both a national level intervention administered by an organization with whom students had a tenuous connection and a school-level intervention administered by specific counselors at students' high school.

Despite the null effects in the aggregate, several implications emerge from our work. First, our results suggest that a more effective path to scale may depend on increasing the number of local institutions implementing evidence-based campaigns. While scaling interventions locally is a costlier and more labor-intensive approach to scale, by maintaining a stronger connection to students as recipients, the sustenance of positive impacts could justify greater costs. Advances in data science and technology also hold promise to improve large-scale outreach efforts, in either global or local scale up approaches. Chiefly, supplemental data could be leveraged to develop microtargeted nudges, following practices used in other social marketing contexts (Metcalf et al. 2018). Similarly, data could be leveraged to provide higher degrees of personalization and ensure that recipients recognize the relevance of the outreach they receive.

Nudge interventions in higher education have the promise of affordable scalability. We test whether the effects scale across a range of samples, settings, information framings, delivery method, and one-on-one help using a global approach by partnering with statewide and nationwide organizations to reach more than 800,000 students. We find no impact in any of these settings despite having similar content and even similar researchers working on these projects. We cannot isolate exactly why our interventions did not work, but it seems like the relationship between the student and the source of the nudge matters. Future work may be able to test this explicitly and advance our understanding of when nudges work.

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Table 1. Nudge and informational intervention studies in postsecondary education

Authors	Stage in college-going process	Sample	Sample size	Geographic location	Partner organization	Intervention	One-way or two-way interaction	Key findings
Oreopoulos and Dunn (2013)	Forming college expectations	High school students from disadvantaged high schools	975	Toronto, Ontario	Five public high schools in Toronto, Ontario	3-minute video highlighting benefits of postsecondary education and invitation to use a financial aid calculator to approximate aid eligibility	One-way	Students in treatment group who were unsure about their educational attainment intentions reported lower expected returns from leaving education after high school, were less likely to report that costs prevented some from going to college, and were less likely to be unsure about educational expectations after treatment
Carrell and Sacerdote (2013)	Applying for college/Applying for financial aid	High school seniors who had taken no or few steps to apply to college with 10th grade test score at or above 40th percentile	949	New Hampshire	Dartmouth College and 12 high schools in New Hampshire	Personalized in-person, phone, and email mentoring and financial assistance associated with applying for college and financial aid during the spring of the senior year	Two-way	Women in the treatment group were more likely to go to college and more likely to enroll in a four-year college; increases also seen for recent immigrants
Hoxby and Turner (2013)	Applying to college	High-achieving, low-income high school seniors	12,000	United States	Expanding College Opportunities	Semi-customized mailings with information about college application process and net costs along with application fee waiver	One-way	Students who were sent mailing applied to more colleges, were more likely to apply to a selective college, and were more likely to enroll in a selective college
Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012)	Enrolling in college/Applying for financial aid	High school seniors, recent high school graduates, and independent adults with no college degree with adjusted gross income <$45,000	16,742	156 tax preparation offices in Ohio and Charlotte (NC)	H&R Block	1) Professional assistance completing FAFSA and information about aid eligibility (assistance + information), or 2) information about aid eligibility (information only)	Two-way	Assistance + information treatment: increased college enrollment among dependent students and by a smaller amount among independent students with no prior college experience; Information treatment: no change in enrollment

Bergman, Denning, and Manoli (forthcoming)	Enrolling in college	Rising high school seniors, college students, students who applied to but did not enroll in college	1,042,303	Applicants to Texas public colleges through ApplyTexas application portal	Texas Higher Education Coordinating Board and ApplyTexas	Emails and letters describing tax benefits for college	One-way	No change in enrollment or reenrollment
Hyman (forthcoming)	Enrolling in college	High-achieving high school seniors	50,000	Michigan	Michigan Department of Education	Mailed letter with information encouraging students to consider college and directing them to website with more information; letter varied by either highlighting 1) college affordability, 2) college choice, or 3) college application information	One-way	Low-income students more likely to enroll in college (mostly driven by four-year college enrollment), results attenuate for second-year persistence and disappear for third-year persistence. Low-income and non-white students had highest rates of website engagement. Letters focused on college affordability led to largest rates of website engagement
Gurantz, Howell, Hurwitz, Larson, Pender, and White (2019)	Enrolling in college	Low- and middle-income students in top 50% of PSAT/SAT distributions	785,000	United States	College Board	1) Mailed brochure with information about key elements of college application process, 2) mailed brochure with direct outreach via text message, virtual advising, or small financial incentive, and 3) Email with link to information about application process	One-way and two-way	No change in enrollment
Barr and Turner (2018)*	Enrolling in college	Unemployment insurance recipients	United States	United States	None (Survey of Income and Program Participation data used for college enrollment outcome)	Letter with information about college costs and benefits and steps to apply for college and financial aid	One-way	Recipients of the letter were more likely to enroll in college within 6 months
Page and Gehlbach (2017)	Enrolling in college	Admitted college students	7,489	Atlanta (GA)	Georgia State University	Text-based intervention using conversational artificial intelligence to provide personalized messages to students regarding pre-matriculation tasks	Two-way	Students assigned to treatment group were more likely to enroll on-time

Castleman and Page (2016b)	Enrolling in college	College-intending high school seniors and their parents	4,754	Boston, Fall River, Lawrence, and Springfield (MA), Miami (FL)	uAspire	14 text messages encouraging students or students and their parents to complete summer tasks for college enrollment	Two-way	Assignment to treatment increased on-time college enrollment for both students and students and parent groups
Castleman, Owen, and Page (2015)	Enrolling in college	College-intending high school graduates	1,602	Albuquerque Public School graduates who intended to enroll at the University of New Mexico	Albuquerque Public Schools (APS) and University of New Mexico (UNM)	Personalized outreach via phone, email, and text messages from either a counselor at a high school or at the University of New Mexico encouraging students to complete summer tasks for college enrollment	Two-way	Summer outreach increased enrollment among Hispanic males; Hispanic males were particularly responsive to outreach from UNM relative to outreach from APS
Castleman and Page (2015)	Enrolling in college	College-intending high school graduates and their parents	6,196	TX, MA, and PA	Dallas Independent School District (TX), uAspire (MA), and Mastery Charter Schools (PA)	1) 10 personalized text message reminders of college-related tasks required for matriculation, or 2) information and encouragement from a peer mentor	Two-way	Text messages increased likelihood of enrolling in a two-year college; Peer mentor outreach increase four-year college enrollment
Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2014)	Enrolling in college	Soon to graduate high school students	21,194	Finland	97 high schools in Finland	Class session and supplemental material containing information about earnings distribution, employment rates, and most common occupations associated with specific postsecondary degrees	One-way	No change in the likelihood of enrolling in postsecondary education or the type of program in which students enrolled; within the treatment group, those who reported that the labor market outcomes for their intended program was worse than they thought were more likely to change fields
Castleman, Page, and Schooley (2014)	Enrolling in college	College-intending high school graduates	927 in Boston (MA); 1,446 in Fulton County Schools (GA)	Two large urban public school districts: Boston (MA) and Fulton County Schools (GA)	uAspire	Phone, email, text, and Facebook messages and in-person meetings with advisors to encourage students to complete summer tasks for college enrollment	Two-way	Offer of counseling increased immediate college enrollment (larger increase among low-income students); students who received a counseling offer were more likely to enroll in their intended college, remain enrolled through first year, and persist into sophomore year
Avery (2013)	Enrolling in college	Low-income high school juniors and seniors	238	Minneapolis and St. Paul metro regions (MN)	College Possible	Two-year program offering tutoring and application assistance	Two-way	Participating in the College Possible program increased the likelihood of applying to and attending a four-year college

Castleman, Arnold, and Wartman (2012)	Enrolling in college	College-intending high school graduates	162	Providence (RI)	Seven urban high schools in Big Picture network of schools	Proactive outreach from high school counselors via phone, email, instant messaging, and Facebook encouraging students to complete summer tasks for college enrollment	Two-way	Students assigned to receive proactive outreach from high school counselors were more likely to enroll in college on-time, enroll full-time, enroll in a four-year college, and keep college plans developed in high school
Carrell and Sacerdote (2017)	Enrolling in college/persisting in college	High school seniors on the verge of not applying to college	2,624	New Hampshire	20 high schools	Mentoring related to college application and financial aid processes, application fees paid, and $100 cash bonus for participation, 2) cash bonus but no mentoring, and 3) information and encouragement via letters, emails, and phone calls	Two-way	Increase in college-going through at least first two years for women assigned to mentoring group; being assigned to mentoring shifted enrollment for some women from no college to a two- or four-year college and from a two-year college to a four-year college; Smaller, less robust, and non-significant findings for men assigned to mentoring
Oreopoulos and Petronijevic (2019)	Persisting in college	First-year college students	24,772	Toronto, Canada	University of Toronto	1) Goal setting prompts, 2) mindset prompts, 3) online coaching, 4) online coaching with one-way text messaging, 5) two-way text messaging, or 6) face to face coaching	One-way and two-way	Coaching interventions improved study habits and subjective well-being but did not change grades or persistence
Oreopoulos and Petronijevic (2018)	Persisting in college	First-year college students	5,179	Toronto, Canada	University of Toronto	1) One-time online exercise designed to affirm goals, 2) exercise plus text and email outreach with weekly messages containing academic advice and motivation content, or 3) exercise plus proactive one-on-one coaching	One way and two way	Increase in GPA among students assigned to coaching intervention, no change for other treatment groups
Oreopoulos, Petronijevic, Logel, and Beattie (2018)	Persisting in college	College students	3,395	Toronto, Canada	University of Toronto	1) Psychologically informed, personalized online module, or 2) module plus text message coaching outreach	One way and two-way	No change in grades or credit accumulation but treatment improved non-academic outcomes with students in both treatment groups experiencing a greater sense of belonging and more likely to seek help

Huntington-Klein and Gill (2019)	Completing college	College students	6,047	unknown (United States)	Regional four-year public university	Informational fliers encouraging students to take a full course load each semester	One-way	No change in credits taken, whether students took a full course load, or pass rate
Goldrick-Rab, Page, Sacerdote, Castleman, and Seftor (2019)	Applying for financial aid	College students	7,996	National (United States)	National Postsecondary Student Aid Study; advising offered through College Possible	1) Text messages with simplified information and prompts, and 2) text messages paired with offer to connect 1:1 with advisor; also varied framing of text messages (basic information, basic information + cues about average peer behavior, and basic information + prompt to commit to a day to complete task)	Two-way	Students who received text messages filed FAFSA in a shorter timeframe than control group students but no change in filing rates by the start of the next academic year; no difference in treatment effects across framing variations or offer of 1:1 advising
Page, Castleman, and Meyer (2016)	Applying for financial aid	High school seniors	17,731	Austin and Houston (TX)	66 high schools in eight school districts	Personalized, data-driven text messages with updates on FAFSA submission and completion process and encouragement to seek out local supports	Two-way	Students in high schools that received treatment were more likely to submit and complete FAFSA
Castleman and Page (2016a)	Applying for financial aid	First-year college students	808	Students who had worked with uAspire in Springfield and Boston (MA) offices while in high school	uAspire	12 personalized text message reminders to re-file FAFSA	Two-way	Community college students were more likely to persist through second year of college; no change in persistence among students at four-year colleges
ideas42 (2015)	Applying for financial aid	College students	63,000	mainly metro Phoenix (AZ)	Arizona State University	8 weekly emails encouraging students to file FAFSA early	One-way	Increased priority FAFSA submission
Evans, Boatman, and Soliz (2019)	Paying for college	High school seniors, community college students, and adults without a college degree	1,657 in high school; 3,770 in community college; 843 adults without a college degree	High schools in TX, KY, TN, and MA; community colleges in IL, TN, MI, and TX; unknown for adults	None (survey data collected from students)	Hypothetical survey that varied the framing (income-based repayment versus income share agreement) and labeling (loan versus income share agreement) of loan offers	One-way	High school and community college students who received a borrowing offer labeled "loan" were less likely to accept the offer, effects were twice as large for black and Hispanic high school students

Marx and Turner (forthcoming)	Paying for college	Community college students	19,724	Unknown (United States)	Anonymous community college	Nonzero loan offer listed in financial aid award letter	One-way	Students who received a nonzero loan offer were more likely to borrow and borrowed more on average. Students who were induced to borrow completed more credits, increased GPA, and were more likely to transfer to a four-year public college within one year
Rosinger (forthcoming)	Paying for college	Admitted and enrolled students at a four-year university	3,476	unknown (United States)	Anonymous four-year college	Standardized financial aid award letter intended to simplify informtion about college costs, loan options, and college outcomes	One-way	No impact on whether a student enrolled, borrowed, or amount borrowed
Darolia and Harper (2018)	Paying for college	Current college students	9,802	Columbia (MO)	University of Missouri	Personlized letter with summary of borrowing to date, estimate of future month payments, and information about borrowing among peers	One-way	No change in whether students borrowed or amount they borrowed
Barr, Bird, and Castleman (2019)	Paying for college	Community college students	2,876	Baltimore County (MD) and surrounding area	Community College of Baltimore County	8 text messages sent over a month providing information about borrowing choices and connecting students with financial aid counselors	Two-way	Reduced unsubsidized, led to worse academic outcomes, and increased loan default rates three years after entering repayment
Booij, Leuven, and Oosterbeek (2012)	Paying for college	College students	3,812	The Netherlands	None (survey data collected from students)	Information about loan options imbedded in an online survey	One-way	Treatment increased students' accurate knowledge of loan options but had no impact on borrowing

Notes: We include studies of nudge and informational interventions that focus on helping high school and current college students navigate college decisions. Unless indicted by an asterisk, all studies are randomized controlled trials. We exclude studies that involve informational interventions in earlier grades that focus on shaping students' college expectations and/or persistence in primary and secondary education (e.g., Dinkleman and Martinez, 2014; Jensen, 2010; McGuigan, McNally, & Wyness, 2016; Nguyen, 2008). Studies are ordered by stage in the college-going process (earliest to latest) and then with most recent studies listed first. See Castleman (2017), Castleman et al. (2017), French and Oreopoulos (2017), Lavecchia, Liu, and Oreopoulos (2014), and Meyer and Rosinger (2019) for additional reviews and discussions of these and other studies).

Table 2: Summary statistics for the Common Application full, experimental, and evaluation samples

	All Common Applicants	Full Experimental Sample	NSC Match
	(1)	(2)	(3)
Student variables			
Female	57.7%	60.7%	60.1%
First generation	37.8%	65.9%	65.1%
Fee Waiver	25.3%	43.1%	43.0%
Intent to apply for aid	61.1%	73.1%	74.7%
SAT score	1154	1104	1107
ACT score	26.5	25.2	25.2
No SAT/ACT score	37.5%	39.0%	39.0%
High School GPA (%)	89.5%	87.8%	87.3%
Missing GPA	31.5%	34.5%	34.8%
High school variables			
Number of Common Applicants	234	136	176
Number in experimental sample	74	87	115
12th grade enrollment	369	345	383
Percent Free/reduced lunch	32.4%	44.0%	43.2%
Percent white	58.8%	49.3%	46.9%
N	836,269	454,243	271,365

Notes: Student-level information is self-reported by students on the Common App, and is based on student responses as of October 12th (for the Fall cohort) or December 18th (for the Winter cohort). The Number of Common Applicants and the Number in experimental sample variables are based on total counts of Common Applicants in each high school (reported using CEEB code) as of December 18th, 2015. The final three High school variables are merged in from the Common Core of Data.

Table 3: Summary statistics for the Large State Rising Freshman sample

	Rising Freshman
	(1)
Student variables	
Female	54.2%
Mother's education: Bachelor's Degree	22.6%
Income 0-39K	15.7%
Income 40-79K	12.5%
Income 80K+	24.4%
Income Missing	47.2%
In State Resident	89.2%
White	62.4%
Black	16.7%
Hispanic	41.4%
Asian	7.7%
Age at Application	18.6
	(0.59)
N	185,749

Notes: Student characteristics are self-reports from college application. See text for a description of the Rising Freshman sample for Large State. Source is administrative records from Large State.

Table 4: Covariate Balance for Common Application, separately for full experimental and evaluation samples

Panel A: Full Experimental Sample

	Female (1)	First Generation (2)	Fee Waiver (3)	Intent to apply for aid (4)	SAT (5)	ACT (6)	No Score (7)	GPA (%) (8)	Missing GPA (9)
Any treatment	0.003	0.001	0.002	-0.001	0.494	-0.019	0.002	0.000	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(1.019)	(0.031)	(0.002)	(0.001)	(0.002)
N	454,243	454,243	454,243	454,243	176,809	158,267	454,243	297,394	454,243
Control mean	0.605	0.657	0.429	0.732	1104	25.21	0.388	0.877	0.346

Panel B: NSC match evaluation sample

	Female (1)	First Generation (2)	Fee Waiver (3)	Intent to apply for aid (4)	SAT (5)	ACT (6)	No Score (7)	GPA (%) (8)	Missing GPA (9)
Any treatment	0.003	0.002	0.002	-0.001	0.816	-0.014	0.003	0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(1.249)	(0.038)	(0.002)	(0.001)	(0.002)
N	271,365	271,365	271,365	271,365	109,656	90,623	271,365	176,756	271,365
Control mean	0.599	0.649	0.428	0.748	1107	25.27	0.388	0.873	0.349

Notes: Each column show the results from a separate student-level regression of the specified dependent variable (student characteristics) on treatment indicators and high school x cohort (Fall or Winter) fixed effects. See notes in Table 2 for more information about these student characteristics. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Table 5: Covariate Balance for Large State

	Male	Mother's Bachelor's Degree	Father's Bachelor's Degree	Family Income 80k+	Age at Application	White	Black	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treatment	0.008	-0.001	-0.002	-0.004	0.009*	0.006	-0.003	-0.006
	(0.006)	(0.004)	(0.006)	(0.003)	(0.006)	(0.006)	(0.005)	(0.006)
Control Mean	0.511	0.242	0.196	0.322	18.65	0.661	0.114	0.333
N	185,793	185,793	185,793	185,793	185,793	185,793	185,793	185,793

Notes: Each column show the results from a separate student-level regression of the specified dependent variable (student characteristics) on treatment indicators and indicators for level of stratification. All treatments are grouped into a single treatment variable. See notes in Table 3 for more information about these student characteristics. Heteroskedasticity robust standard errors are in parenthesis. * p < 0.1 ** p < 0.05. *** p < 0.01

Table 6: Overall treatment impacts of Common Application intervention on enrollment and persistence outcomes

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.003	0.000	0.002	0.002	0.001
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Control mean of outcome	0.824	0.728	0.100	0.791	0.737
N	271,365	271,365	271,365	271,365	271,365

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and high school x cohort (Fall or Winter) fixed effects and the student-level characteristics shown in Table 2. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. We construct the enrollment and persistence outcomes using National Student Clearinghouse matches. Enrollment in Fall immediately after intervention (Fall 2016) or enrollment in following Fall (Fall 2017) enrollment is defined as whether the student was enrolled as of October 1st, 2016 (2017). Continuous enrollment is defined as enrollment in Fall 2016, Spring 2017, and Fall 2017. Spring 2017 enrollment is defined as whether the student was enrolled as of March 1st, 2017. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Table 7: Overall treatment impacts of Large State intervention on enrollment, persistence, and financial aid outcomes for Rising Freshman

	1st Spring after intervention			First Fall after intervention – Enrollment			First Fall after intervention – Financial Aid		
	Enrolled	Enrolled at 4-year	Enrolled at 2-year	Enrolled	Enrolled at 4-year	Enrolled at 2-year	Filed FAFSA	Pell Grant Amount	Loan Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any treatment	0.009	-0.001	0.011*	0.001	0.000	0.003	-0.006	-26.75	-20.05
	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(23.8)	(28.75)
Control mean of outcome	0.542	0.316	0.238	0.490	0.294	0.208	0.436	1217.7	1065.3
N	185,793	185,793	185,793	185,793	185,793	185,793	185,793	185,793	185,793

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and indicators for parent education, self-reported family income, gender, type of school applied to, and race as well as age at application. Heteroskedasticity robust standard errors are in parenthesis. We construct the enrollment and financial aid outcomes using administrative data from Large State. Messages were sent in Fall 2016, some outcomes are measured then, "1st Spring after intervention ". "First Fall after intervention" refers to the Fall of 2017 which was the first semester after the messages were sent. * p < 0.1 ** p < 0.05. *** p < 0.01

Table 8: Impact of the Large State intervention for other student populations

	First fall after intervention				
	Enrolled	Enrolled at 4-year	Enrolled at 2-year	Filed FAFSA	Pell Grant Amount
	(1)	(2)	(3)	(4)	(5)
Applied but did not enroll					
Any treatment	-0.001	-0.002**	0.001*	0.000	-0.888
	(0.001)	(0.001)	(0.000)	(0.001)	(2.93)
Control mean of outcome	0.061	0.050	0.012	0.081	152.3623
N	317,193	317,193	317,193	317,193	317,193
Currently enrolled					
Any treatment	-0.003	-0.004*	0.000	0.004	17.01
	(0.004)	(0.002)	(0.003)	(0.004)	(16.26)
Control mean of outcome	0.533	0.154	0.393	0.435	886.21
N	516,739	516,739	516,739	516,739	516,739
Leavers (2-year institutions)					
Any treatment	0.000	0.001	0.001	0.002	8.471
	(0.003)	(0.004)	(0.004)	(0.004)	(12.81)
Control mean of outcome	0.145	0.109	0.039	0.132	242.33
N	23,248	23,248	23,248	23,248	23,248

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and indicators for available controls. Heteroskedasticity robust standard errors are in parenthesis We construct the enrollment and financial aid outcomes using administrative data from Large State. Messages were sent in Fall 2016, some outcomes are measured then, "1st Spring after intervention ". "First Fall after intervention" refers to the Fall of 2017 which was the first semester after the messages were sent. These are relative to the control group which did not receive messages. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Table 9: Common Application intervention impacts by content variation

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Advising	0.007	-0.004	0.010	0.014	0.006
	(0.009)	(0.010)	(0.007)	(0.009)	(0.010)
Financial	0.004*	0.003	0.000	0.004*	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Identity	0.001	-0.001	0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Planning	0.003	-0.000	0.003*	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Control mean of outcome	0.824	0.728	0.100	0.791	0.737
N	271,365	271,365	271,365	271,365	271,365

Notes: Each column show the results from a separate student-level regression of the specified outcome on the content and advising indicators and high school x cohort (Fall or Winter) fixed effects and the student-level characteristics shown in Table 2. The excluded category is the control group. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. Note that the advising indicator is not mutually exclusive from the three content variation indicators. See notes in Table 6 for information on the construction of the outcome variables. * p < 0.1 ** p < 0.05. *** p < 0.01

Table 10: Large State intervention impacts by content variation and timing

	Enrolled	Enrolled at 4-year	Enrolled at 2-year	Filed FAFSA	Pell Grant Amount
	(1)	(2)	(3)	(4)	(5)
		Rising Freshman			
Content	0.002	0.000	0.004	-0.00639	-30.08
	(0.006)	(0.005)	(0.006)	(0.00620)	(24.69)
Media	0.000	-0.001	0.003	-0.00588	-23.41
	(0.006)	(0.005)	(0.006)	(0.00620)	(24.61)
N	185,793	185,793	185,793	185,793	185,793
		Currently enrolled four-year students			
Early Treatment	-0.002	0.005	-0.009***	0.0113*	18.3
	(0.007)	(0.006)	(0.003)	(0.006)	(21.97)
Late Treatment	0.001	0.006	-0.007**	0.007	22.22
	(0.007)	(0.006)	(0.003)	(0.007)	(20.33)
N	252,543	252,543	252,543	252,543	252,543

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for treatment and indicators for parent education, self-reported family income, gender, type of school applied to, and race as well as age at application. Heteroskedasticity robust standard errors are in parenthesis Messages were sent in Fall 2016, some outcomes are measured then, "1st Spring after intervention ". "First Fall after intervention" refers to the Fall of 2017 which was the first semester after the messages were sent. Different treatment arms are represented including "content" which was text-based information and "media" which conveyed the same information using graphics. "Early Treatment" was receiving the messages before students could file the FAFSA while "Late Treatment" received messages just after the filing window. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.1$

Table 11: Common Application intervention impacts by delivery method

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Control, plus text	0.003	0.001	0.002	0.007**	0.004
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Content variation, email and text	0.004*	0.001	0.003	0.006**	0.004
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Content variation, plus postal	0.004	0.001	0.003	0.004	0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Control mean of outcome	0.823	0.727	0.0990	0.788	0.735
N	271,365	271,365	271,365	271,365	271,365

Notes: Each column show the results from a separate student-level regression of the specified outcome on the content and advising indicators and high school x cohort (Fall or Winter) fixed effects and the student-level characteristics shown in Table 2. The excluded category is Control, no text. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. See notes in Table 6 for information on the construction of the outcome variables. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Appendix Tables

Table A1: Impacts of the Common Application intervention on college application outcomes

			Characteristics of colleges applied to						
	Any application	Number of applications	Public	Private	Net price < $15k	Graduation rate > 70%	Admission Rate < 30%	Median SAT > 1200	Instructional Exp > $10k
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any treatment	0.000	0.007	0.000	0.000	0.000	0.000	0.001	0.001	-0.001
	(0.001)	(0.011)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Control mean of outcome	0.777	3.756	0.623	0.806	0.371	0.559	0.369	0.654	0.525
N	454,243	454,243	454,243	454,243	454,243	454,243	454,243	454,243	454,243

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and high school x cohort (Fall or Winter) fixed effects and the student-level characteristics shown in Table 2. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. We construct college application outcomes using student x application level data provided by the Common App, which includes all applications submitted as of approximately June 2016. We merge in IPEDS data on net price, graduation rate, admission rate, and median SAT score of incoming course to construct these measures. We also tested other cut-points for the college quality measures in columns (5)-(8) (e.g. net price below $20,000); we also tested other college quality measures such as 3-year cohort default rate, instructional expenditures above certain amounts, net-price for low-income students. We find the same pattern of results for these alternative measures. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.1$

Table A2: Impacts of the Common Application intervention on enrollment quality (1st Fall)

	Public	Private	Net price < \$15k	Graduation rate > 70%	Admission Rate < 30%	Median SAT > 1200	Instructional Exp. > \$10k
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any treatment	0.005**	-0.002	0.005**	-0.002	0.000	0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Control mean of outcome	0.524	0.297	0.292	0.125	0.0560	0.196	0.470
N	271,365	271,365	271,365	271,365	271,365	271,365	271,365

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and high school x cohort (Fall or Winter) fixed effects and the student-level characteristics shown in Table 2. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. We college quality measures of college attended in Fall 2016 using NSC matches and IPEDS data on net price, graduation rate, admission rate, and median SAT score of incoming course to construct these measures. We also tested other cut-points for the college quality measures in columns (5)-(8) (e.g. net price below \$20,000). We also tested other college quality measures such as 3-year cohort default rate, instructional expenditures above certain amounts, net-price for low-income students. We also constructed these measures for Fall 2017 enrollment. We find the same pattern of results for these alternative measures. * p < 0.1 ** p < 0.05. *** p < 0.01

Table A3: Impacts of the Large State intervention on enrollment quality (1st Fall)

	Graduation rate > 60%	Graduation rate > 70%	Cohort Default Rate <5%	Cohort Default Rate <10%	Instructional Exp. > $8k	Instructional Exp. > $10k
	(1)	(2)	(3)	(4)	(5)	(6)
Any treatment	0.00162	0.00256	-0.00325	-0.000688	0.00281	0.00194
	(0.00217)	(0.00210)	(0.00332)	(0.00473)	(0.00273)	(0.00240)
N	185,793	185,793	185,793	185,793	185,793	185,793

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and indicators for parent education, self-reported family income, gender, type of school applied to, and race as well as age at application. Heteroskedasticity robust standard errors are in parenthesis. We construct the institution level characteristics using IPEDS. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Table A4: Overall treatment impacts of Common Application intervention on enrollment and persistence outcomes, by experimental cohort

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.003	0.000	0.002	0.003	0.004
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Any Treatment * Fall Cohort	-0.001	0.001	-0.001	-0.003	-0.006
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Control mean of outcome	0.826	0.253	0.0354	0.793	0.257
N	271,365	271,365	271,365	271,365	271,365

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment; an interaction between the treatment indicator and an indicator for Fall cohort; high school x cohort (Fall or Winter) fixed effects; and the student-level characteristics shown in Table 2. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. See notes in Table 6 for more information about the construction of the outcomes. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Table A5: Heterogeneous treatment impacts of the Common Application intervention

Panel A: Estimated treatment impacts by First Generation status

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.005**	0.001	0.004**	0.003	0.003
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Any Treatment * First Generation	-0.005	-0.001	-0.004	-0.001	-0.002
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Control mean of outcome	0.824	0.728	0.100	0.791	0.737
Observations	271,365	271,365	271,365	271,365	271,365

Panel B: Estimated treatment impacts by Fee waiver status

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.0029	0.0018	0.0007	0.0018	0.0007
	-0.0021	-0.0024	-0.0015	-0.0022	-0.0024
Any Treatment * Fee Waiver	-0.0005	-0.0031	0.0023	0	0.0009
	-0.0035	-0.0039	-0.0027	-0.0037	-0.0039

Control mean of outcome	0.824	0.728	0.1	0.791	0.737
Observations	271,365	271,365	271,365	271,365	271,365

Panel C: Estimated treatment impacts by intent to apply for financial aid

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.003	0.004	-0.001	0.004	0.003
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Any Treatment * Intent to apply for aid	-0.001	-0.004	0.003	-0.003	-0.002
	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)
Control mean of outcome	0.824	0.728	0.1	0.791	0.737
Observations	271,365	271,365	271,365	271,365	271,365

Panel D: Estimated treatment impacts by gender

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.005*	0.002	0.002	0.006*	0.003
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Any Treatment * Female	-0.003	-0.003	-0.000	-0.006*	-0.004

	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Control mean of outcome	0.824	0.728	0.1	0.791	0.737
Observations	271,365	271,365	271,365	271,365	271,365

Panel E: Estimated treatment impacts by SAT score

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.003	0.000	0.002	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Any Treatment * High SAT	0.001	0.005	-0.004	0.000	0.003
	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)
Control mean of outcome	0.824	0.728	0.1	0.791	0.737
Observations	271,365	271,365	271,365	271,365	271,365

Panel F: Estimated treatment impacts by high school Free/Reduced Price Lunch

	Enrolled 1st Fall after intervention	Enrolled at 4-year 1st Fall after intervention	Enrolled at 2-year 1st Fall after intervention	Enrolled 2nd Fall after intervention	Enrolled Continuously
	(1)	(2)	(3)	(4)	(5)
Any Treatment	0.001	-0.001	0.002	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)

Any Treatment * FRPL > 40%	0.003	0.004	-0.002	0.000	0.001
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Control mean of outcome	0.824	0.728	0.1	0.791	0.737
Observations	271,365	271,365	271,365	271,365	271,365

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment; an interaction between the treatment indicator and an indicator for the student characteristic of interest; high school x cohort (Fall or Winter) fixed effects; and the student-level characteristics shown in Table 2. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. See notes in Table 6 for more information about the construction of the outcomes. * p < 0.1 ** p < 0.05. *** p < 0.01

Table A6: Heterogeneous treatment impacts of the Large State intervention

	First Fall after intervention				
First Gen	Enrolled	Enrolled at 4-year	Enrolled at 2-year	Filed FAFSA	Pell Grant Amount
	(1)	(2)	(3)	(4)	(5)
Any treatment	-0.001	0.000	-0.001	-0.006	-18.80
	(0.006)	(0.005)	(0.005)	(0.006)	(23.21)
Treated * First Gen	0.000	0.000	0.001	0.001	6.87
	(0.003)	(0.002)	(0.002)	(0.003)	(8.72)
Male					
Any treatment	-0.003	-0.003	0.002	-0.001	0.88
	(0.003)	(0.003)	(0.003)	(0.003)	(11.86)
Treated * Male	0.006	0.007*	-0.002	0.002	4.02
	(0.005)	(0.004)	(0.004)	(0.005)	(16.52)
Low Income					
Any treatment	-0.001	-0.002	0.001	-0.001	3.00
	(0.003)	(0.002)	(0.002)	(0.003)	(7.89)
Treated * Low Income	0.009	0.009	0.001	0.003	-1.80
	(0.006)	(0.006)	(0.005)	(0.006)	(32.43)
N	185,793	185,793	185,793	185,793	185,793

Notes: Each column show the results from a separate student-level regression of the specified outcome on an indicator for any experimental treatment and treatment interacted with a student characteristic of interest. We also control for indicators for parent education, self-reported family income, gender, type of school applied to, and race as well as age at application. Heteroskedasticity robust standard errors are in parenthesis. We construct the enrollment and financial aid outcomes using administrative data from Large State. Messages were sent in Fall 2016, some outcomes are measured then, "1st Spring after intervention ". "First Fall after intervention" refers to the Fall of 2017 which was the first semester after the messages were sent.. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$

Table A6: Distribution of Common Application text messages delivered and text interaction, overall and by treatment arm

Panel A: Distribution of text messages delivered

	All Conditions	Advising	Financial	Planning	Identity	Control
	(1)	(2)	(3)	(4)	(5)	(6)
Messages delivered						
0	12.3%	12.1%	12.3%	12.3%	12.5%	11.7%
2	7.3%	7.6%	7.3%	7.4%	7.2%	7.1%
3	33.9%	34.0%	33.8%	33.9%	33.8%	34.4%
4	7.2%	7.1%	7.2%	7.3%	7.1%	7.2%
5	1.2%	1.1%	1.2%	1.2%	1.1%	1.1%
6	38.3%	38.1%	38.3%	37.9%	38.4%	38.5%
Number of students assigned to receive messages	397205	1998	112912	112937	113146	56212

Panel B: Interaction statistics for students who received text messages

	All Conditions	Advising	Financial	Planning	Identity	Control
	(1)	(2)	(3)	(4)	(5)	(6)
Opted out	1.1%	0.3%	1.0%	1.3%	1.1%	1.0%
Responded	4.7%	11.6%	4.3%	5.1%	4.4%	5.0%
Conversation Length (messages)	0.06	0.38	0.06	0.07	0.06	0.07
Conversation Length (characters)	1.64	31.71	1.29	1.19	1.37	1.71

Notes: See Appendix A for an explanation of the issue with text delivery. Students were able to opt out of receiving subsequent messages at any time by responding with a keyword such as "STOP". The percent of students who ever responded includes students who only ever responded to opt out. Conversation length includes messages sent by both the student and the advisor (if in the Advising group), but does not include the original program messages.

Table A7: Covariate Balance for Common Application, by number of text messages received (students who were assigned to receive texts, only)

	Female	First Generation	Fee Waiver	Intent to apply for aid	SAT	ACT	No Score	GPA (%)	Missing GPA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Number of messages received									
3	-0.008*	-0.004	-0.016***	0.001	9.683***	0.161**	-0.004	0.005***	-0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(2.626)	(0.081)	(0.004)	(0.002)	(0.004)
4	-0.009*	-0.015***	-0.045***	-0.001	14.12***	0.337***	-0.014**	0.007***	-0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(3.203)	(0.101)	(0.006)	(0.002)	(0.006)
5	-0.008	-0.021**	-0.048***	0.004	24.27***	0.155	-0.012	0.011***	-0.034***
	(0.010)	(0.009)	(0.010)	(0.009)	(5.712)	(0.227)	(0.011)	(0.004)	(0.010)
6	-0.011***	-0.033***	-0.126***	0.002	26.60***	0.583***	-0.018***	0.017***	-0.027***
	(0.004)	(0.004)	(0.004)	(0.004)	(2.623)	(0.079)	(0.004)	(0.002)	(0.004)
Control mean of outcome	0.625	0.688	0.536	0.7	1075	24.44	0.385	0.869	0.35
N	210,974	210,974	210,974	210,974	87,077	71,566	210,974	139,073	210,974

Notes: Each column show the results from a separate student-level regression of the specified dependent variable (student characteristics) on indicators for the number of messages received (omitted category = 2) and high school x cohort (Fall or Winter) fixed effects. The sample for these regressions include students assigned to receive text messages and had a valid cell phone number. See notes in Table 2 for more information about these student characteristics. Heteroskedasticity robust standard errors are clustered at the high school x cohort level. * $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$