Financial Aid Nudges: A National Experiment With Informational Interventions

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Despite high prices, many college students do not re-file the Free Application for Federal Student Aid (FAFSA) or file late, making college less affordable. Low-cost technological interventions delivering personalized information and/or advising may improve refiling and academic outcomes, but questions remain regarding the efficacy of this approach at scale. This multi-pronged randomized experiment tested informational and framing text message interventions for a national sample of approximately 10,000 undergraduates. The text outreach caused earlier FAFSA re-filing for some students. However, gains in re-filing during the active intervention period were not sustained after the intervention concluded and did not translate into additional federal financial aid or improved postsecondary persistence or attainment. Implications for the scaling and targeting of nudging are discussed.

Keywords: *colleges, economics of education, finance, experimental research*

College list prices are rising rapidly and are a substantial barrier to college completion for both middle- and lower-income students. As a result, financial aid is the main mechanism for increasing college affordability for students, but it is distributed using a complex system that includes a lengthy application (the Free Application for Federal Student Aid [FAFSA]) and has numerous rules for continued receipt (Bettinger et al., 2012; Castleman & Page, 2014, 2016; Goldrick-Rab et al., 2016; Kelly & Goldrick-Rab, 2014; Page & Scott-Clayton, 2016; St John et al., 2000).

This administrative complexity reduces the effectiveness of financial aid programs (Bird et al., 2021; Castleman & Page, 2016; Dynarski & Wiederspan, 2012; Page et al., 2020). In particular, challenges with completing the application, filing, and verification cause eligible students with financial need to leave at least $5 billion dollars on the table each year. Evidence from the nationally representative Beginning Postsecondary Students study of 2004/2009 suggested that 15% to 20% of rising second-year students who were Pell-eligible in the previous year do not refile a FAFSA (Bird &

Castleman, 2016). These students are much less likely to persist than peers of similar backgrounds who re-file their FAFSA (Bird & Castleman, 2016; Novak & McKinney, 2011). This differential is not surprising. Without filing the FAFSA, students do not have access to all federal, state, and institutional aid, including grants, loans, and work-study dollars.

Money is also foregone when students do not refile the FAFSA on-time. State and institutional aid is limited and often runs out; filing early is the only way to ensure that eligible students receive those funds (McKinney & Novak, 2015). March 1 is the priority-filing deadline in many states, but only 33% of undergraduates who are Black and 37% of undergraduates from low-income backgrounds file FAFSA by that date (Castleman & Page, 2015).

Why do so many economically vulnerable students attend colleges without the financial aid for which they qualify? How might colleges and universities help students to retain their financial aid and, in turn, improve degree completion rates? A better understanding of this challenge and the identification of an effective, low-cost intervention might help to improve college attainment.

Background

Nudging

A growing body of research suggests that timely, personalized reminders hold promise for increasing the rate of people successfully following through on their intentions and pursuing programs and opportunities that lead to improved outcomes for them or their families (Thaler & Sunstein, 2009). In educational settings, nudges appear to be a cost-effective strategy for improving certain outcomes. More specifically, some scholars have incorporated nudges on FAFSA filing into randomized interventions intended to increase college retention (Cannon & Goldrick-Rab, 2016; Castleman & Page, 2016; Page, Lee, & Gehlbach, 2020). The first study, which took place at a 4-year public university, identified positive effects of phone-based outreach from a call center on rates of on-time FAFSA re-filing for continuing students, somewhat increasing the amount of aid (primarily state aid) that students received. But the increase in aid did not result in

increased retention to the next year of college (Cannon & Goldrick-Rab, 2016). In contrast, the second study found positive effects of text-message nudges on persistence to the second year of college among community college students, but no impacts among first-year students at 4-year institutions where rates of persistence into the second year were already high (Castleman & Page, 2016).[1] The third study, focusing on outreach via an artificially intelligent chatbot at one 4-year university campus, found that chatbot outreach targeted to students who had not yet completed their FAFSA improved timely filing as well as registration for the following academic year (Page, Lee, & Gehlbach, 2020). However, a recent, much larger study found that nudging at scale may be less effective than these prior studies would suggest. Specifically, Bird and colleagues (2021) examined national- and state-level FAFSA completion messaging campaigns that reached nearly 800,000 students and estimated precise null impacts both overall and across a variety of treatment variations.

Given the vast size and complexity of the American higher education system, and constrained institutional resources, it is important to continue to explore whether, and under what conditions, text-message based nudging can be an effective strategy for supporting student success. These findings will serve to increase efficiency in the existing financial aid system and also help students retain financial aid. This study leverages a first-of-its-kind opportunity provided by the U.S. Department of Education's National Center for Education Statistics, in which a nationally representative sample from the National Postsecondary Student Aid Study of 2016 (NPSAS:2016) was made available for intervention and experimentation under the guidelines of a special call for proposals from the Institute for Education Sciences. This created the opportunity to examine the impacts of informational interventions aimed at increasing continued receipt of financial aid and continued enrollment in college in a national sample of students. Through the experiment, we examined the relative effectiveness of information presented with different types of framing as well as the effectiveness of nudges coupled with the offer of individual assistance.

Contributions to the Literature

The current study builds on Bird and colleagues' (2021) evaluation of large-scale FAFSA completion efforts in several ways, and thus makes important contributions to the research literature. First, the intervention with the national sample in the Bird et al. (2021) study focused on initial FAFSA completion for high school seniors, whereas our study focused on FAFSA renewal among current college students.[2] Moreover, the national sample in their study consisted of students who had started the college application process with the Common Application. Students in that sample have high rates of college-going. Light-touch interventions like text-based nudging may have less of an impact among a population already positively inclined for college enrollment. By comparison, the current study focuses on a nationally representative sample of college students attending both 2- and 4-year institutions. Finally, the Bird et al. (2021) study observes neither FAFSA filing nor financial aid receipt in the national sample and only financial aid receipt in the state sample. Our study contributes additional evidence on the impact of FAFSA renewal campaigns highlighting whether and when students refiled FAFSA.

Another contribution of our paper is further evidence of the efficacy of U.S. government's efforts to leverage existing data and communication channels to increase use of public benefits. For instance, researchers have investigated government efforts to increase usage of the Earned Income Tax Credit (EITC; Bhargava & Manoli, 2015), Supplemental Nutrition Assistance Program (SNAP; Finkelstein & Notowidigdo, 2019), income-based loan repayment (White House Social and Behavioral Sciences Team, 2015), supplemental security income (Hemmeter et al., 2020), and federal financial aid among low-income, unemployed adults (Barr & Turner, 2018). Our study was funded by the Institute for Education Sciences with a particular focus on whether the NPSAS survey process could be leveraged as a channel to improve postsecondary outcomes for NPSAS participants. The results of our study speak to large-scale, government-engaged efforts to connect U.S. residents to public benefits.

Methodology

Intervention

With the goal of helping college students continue to receive financial aid and remain enrolled, we planned and distributed two types of informational interventions supported by Signal Vine's text messaging platform. The two types of interventions were as follows: (a) simplified information and prompts delivered via text message, and (b) informational text messages coupled with the offer to interact (via text) with a remote advisor. For the first type of intervention, we tested three variants. Specifically, we differentiated language in the text messages to investigate mechanisms by which this approach might increase FAFSA filing and college persistence (Castleman & Page, 2016). We considered two prominent behavioral theories—social pressure and commitment device. Thus, the text message outreach came in three "flavors": (a) a basic reminder and information version, (b) a social-pressure version that added to the basic version cues about average peer behavior in accomplishing the task, and (c) a commitment-device version that instead prompted recipients to commit to a particular day to accomplish the task.

The social pressure approach operates under the theory that individual behavior can be influenced (both positively and negatively) by knowledge or perceptions of peer behavior. Many scholars have documented that students will adopt positive behaviors if they see or believe their peers are engaged in the behavior. For example, Sacerdote (2001) and Stinebrickner and Stinebrickner (2006) found that being assigned dorm-mates or roommates who are academically inclined improves students' own grades and study habits. Many researchers have attempted to curb binge drinking among college students by seeking to reduce students' perception of how much their peers drink in a typical weekend evening (Johnston & White, 2003; Perkins, 2002). Similarly, Hoxby and Turner (2013) improved the range and number of college applications that low-income, high-achieving students submitted by including information about peer submissions as a component of their Expanding College Opportunity intervention.

In this study, the social pressure texts emphasized the rates at which other students file the FAFSA and take advantage of campus resources. In addition to informing students of campus resources like the financial aid office, the texts also added social cues, such as "Many of your peers rely on campus resources to help them succeed. Have you found helpful supports to turn to for any academic challenges?" Other texts provided more concrete social references to motivate students to act. For instance, one message said, "Just checking in about FAFSA. About 70 percent of college students have completed their FAFSA by this time of year."

Alternatively, students may benefit from a commitment device. Completing the FAFSA is a time consuming, challenging, and relatively unpleasant task. Faced with a distant deadline and an even farther off reward, students may continue to procrastinate and never actually complete the application (Ashraf et al., 2006; DellaVigna & Malmendier, 2006). Commitment devices aim to increase the probability individuals will complete a future action by tying completion of that action to a reward or penalty if the individual follows through (or fails to do so). For a review of theoretical and empirical research on commitment devices, see Bryan et al. (2010). Commitment devices have been leveraged to promote increased rates of savings and better health, among other outcomes in other policy domains (Ashraf et al., 2006; Rogers et al., 2014). In the context of education, commitment devices have been leveraged to increase student success in online learning and to improve school-based conduct among middle school students (Patterson, 2018; Robinson et al., 2018).

The commitment device texts asked students to pick a particular day (e.g., Monday, Wednesday, or Friday of the following week) to work on their FAFSA or complete another task. When that day arrived, the system sent a follow-up message to remind students of their commitment.

In addition to the framing of the messaging, the study considered whether there is additional benefit to providing students with the opportunity to connect with an advisor who is knowledgeable about FAFSA filing as well as other college-going processes. College Possible, a non-profit organization headquartered in Saint Paul, Minnesota, and focused on mitigating barriers to college access and success, offered this support during our study.[3] Specifically, College Possible identified, trained, and supervised advisors to monitor and respond to incoming messages from treatment students invited to engage with the organization via text.

We compared each of the active intervention groups to each other as well as to a control group of students who received no intervention. To summarize, through the structure of an experimental study, we compared outcomes for students assigned at random to one of the following five groups: (a) Control condition—no intervention, (b) texting with basic language, (c) texting with social pressure language, (d) texting with commitment device language, and (e) college possible—texting with basic language plus the offer to interact with and receive follow-up support from an advisor.

The intervention was meant to begin in January 2017 and last 15 weeks. However, several external factors delayed the start date, including challenges with obtaining required Office of Management and Budget clearances for the work during a period of presidential transition. Instead, the active intervention period began on February 7, 2017, and continued through May 16, 2017.

In addition to the delayed timeline, two other external factors may have affected this study. First, starting in fall 2016 the Department of Education implemented "prior-prior year," allowing students to file FAFSA starting in October rather than in January by using their tax filings from 2 years rather than 1 year earlier. The prior-prior year policy change may have led a greater-than-expected share of this study's sample already to have completed the FAFSA by the start of the intervention, consistent with trends nationally (Murphy, 2017). In addition, during spring 2017, the Internal Revenue Service (IRS) Data Retrieval Tool that allows students to import IRS tax data into the FAFSA, thus simplifying the application process, went down.[4] The effect of this unexpected occurrence is unknown (Murphy, 2017).

In Table 1, we present the schedule and topical focus of each set of messages distributed. To increase the odds that the text messages would offer useful information to students, perhaps even to those who already had filed their FAFSA by the time the intervention began, we added

TABLE 1

Text Message Distribution Schedule and Topic

Date		Content
Tuesday	February 7, 2017	Introduction
Thursday	February 9, 2017	FAFSA completion
Monday	February 13, 2017	FAFSA completion
Thursday	February 16, 2017	FAFSA completion
Tuesday	February 21, 2017	Staying on track academically and campus resources to support student success
Thursday	February 23, 2017	FAFSA completion
Monday	February 27, 2017	FAFSA completion
Monday	March 13, 2017	FAFSA completion
Tuesday	March 14, 2017	Supplemental forms (e.g., CSS Profile) that the student's campus also requires for financial aid
Thursday	March 16, 2017	Extra message: Information on the IRS data retrieval tool being unavailable
Tuesday	March 28, 2017	Satisfactory Academic Progress requirements for maintaining financial aid
Tuesday	April 11, 2017	Earned Income Tax Credit
Thursday	April 20, 2017	Supplemental Nutrition Assistance Program
Tuesday	May 2, 2017	Preparing for final exams
Tuesday	May 09, 2017	FAFSA completion
Thursday	May 11, 2017	Supplemental Nutrition Assistance Program
Tuesday	May 16, 2017	Summer jobs and internships

Note. CSS = The College Scholarship Service profile offered by the College Board; FAFSA = Free Application for Federal Student Aid; IRS = Internal Revenue Service.

supplemental information to the message content. Additional messages provided information on (a) academic requirements for retaining financial aid, (b) what to do while the IRS Data Retrieval Tool was down, and (c) eligibility for two other social benefits for which our study participants may have qualified, the SNAP and the EITC.

Sample

The full study sample included 9,881 students. The research firm RTI assisted with sampling, as the sample was a subset of those included in the NPSAS:2016. To be included in the study sample, students had to agree when they completed the NPSAS survey (between January and November 2016) to future communication via text message (information about the specific communication was not provided). Prior to randomization, we stratified the sample by three student characteristics: (a) intensity of enrollment (full-time, part-time, unknown), (b) institutional level (4-year, 2-year, less than 2-year), and (c) whether the student had filed the FAFSA for the 2016 to 2017 academic year (yes, no). This resulted in 18 distinct groups (strata) within which students were randomized to experimental conditions. The probability of assignment to treatment versus control varied somewhat across these strata. Specifically, to accommodate sample sharing between this project and another NPSAS-related experimental study being conducted simultaneously (Bettinger & Long, 2017), some students in our control group also served as controls for this separate experimental study.

To handle the variation in treatment assignment probabilities across strata, we assigned weights to each observation according to the inverse probability of assignment to the given experimental condition within each stratum. We then interacted NPSAS sample weights (designed to create national representativeness) with the randomization weights and ran all regressions with these interacted weights. In practice, these weights made little difference in our estimates, although the experimental results that we present are based on models that incorporate the weights.

TABLE 2

Sample and Subsample Definitions and Sizes

			Sample				
	1	2	3	4	5	6	7
			FAFSA not filed and enrolled in college pre-treatment				
			Two-year enrollees		Four-year enrollees		
Treatment group	Full sample	FAFSA not filed pre-treatment	All		All	Public	Private
Control	2,152	1,746	860	258	602	407	195
College possible	2,898	2,341	1,188	280	908	603	305
Texting, basic	1,601	1,291	654	177	477	346	131
Texting, social pressure	1,590	1,300	627	139	488	316	172
Texting, commitment device	1,640	1,326	685	157	528	330	198
Total	9,881	8,004	4,014	1,011	3,003	2,002	1,001

Note. FAFSA = Free Application for Federal Student Aid.

Table 2 displays information on the study sample and all analytic subsamples.[5] In the full sample, 19% of students filed the FAFSA for the upcoming (2017–2018) academic year *prior* to the start of the intervention. In addition, approximately half of the students in the full sample were not enrolled in college during the term prior to intervention. Thus, the remainder of this paper estimates treatment impacts for three groups: (a) Sample 1: the full sample ($N = 9{,}881$), (b) Sample 2: students who had not filed a FAFSA prior to the start of the intervention ($N = 8{,}004$), and (c) Sample 3: students who were enrolled but who had not filed a FAFSA prior to the start of the intervention ($N = 4{,}014$).[6] In addition, we estimate heterogeneous impacts by institution type (2-year vs. 4-year) and sector (public vs. private; Samples 4–7).

Baseline Equivalence

Table 3 demonstrates how we assessed baseline equivalence for the full sample (Supplementary Appendix Table A0 in the online version of the journal contains the relevant *t*-statistics for the baseline equivalence tests while Supplementary Appendix Tables A1 and A2 in the online version

of the journal assess this for samples 2 and 3). The instances of statistically significant differences in baseline measures are scattered and occur slightly below chance. At the 5% level and given randomization, we would have expected about four *t*-statistics above 1.96 but we have only one. In sum, randomization was successful in producing a sample that is well-balanced on a host of baseline covariates at both the institutional and student levels. Note that institutional characteristics considered here are associated with the institution each student attended at the time of taking the NPSAS survey (i.e., the 2015–2016 academic year).

Models for assessing treatment impacts all take the following general form. Here, the equation is expressed for the impact of any treatment (e.g., with all treatments pooled together):

$$Y_{ij} = \alpha_j + \beta \, TREAT_{ij} + X\gamma + \epsilon_{ij}. \qquad (1)$$

In Equation 1, *i* indexes student and *j* indexes the strata within which we randomized students to interventions as described above. To handle the structure of randomization, fixed effects, α_j,

TABLE 3

Assessment of Baseline Equivalence: Full Sample

Variable	Control	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	*t*-test (Any Intervention-Control)
Pre-treatment						
Filed FAFSA 16/17	0.58	0.62	0.61	0.60	0.61	n.a.
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	
Outcome measure						
Filed FAFSA 17/18	0.4191	0.4317	0.4447	0.4214	0.4226	0.0407
	(0.49)	(0.50)	(0.50)	(0.49)	(0.49)	
Institutional characteristics						
75th Pctl verbal SAT	585.5	587.01	588.16	587.33	585.5	0.16
	(61.59)	(59.96)	(65.27)	(64.38)	(61.59)	
75th Pctl ACT	26.34	26.33	26.35	26.5	26.34	−0.57
	(3.72)	(3.66)	(3.90)	(3.93)	(3.72)	
Admission rate	0.67	0.67	0.66	0.66	0.67	1.14
	(0.21)	(0.20)	(0.19)	(0.20)	(0.21)	
Public	0.55	0.57	0.55	0.54	0.55	−0.36
	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	
Non-profit	0.16	0.13	0.16	0.16	0.16	−0.68
	(0.36)	(0.34)	(0.36)	(0.37)	(0.36)	
For-profit	0.3	0.29	0.3	0.29	0.3	0.95
	(0.46)	(0.45)	(0.46)	(0.46)	(0.46)	
Student characteristics						
Age	27.11	26.39	26.09	26.26	26.1	0.47
	(9.46)	(9.23)	(8.97)	(9.02)	(8.75)	
Female	0.59	0.6	0.6	0.6	0.59	0.53
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	
Class year	2.22	2.22	2.23	2.3	2.31	0.77
	(1.19)	(1.19)	(1.20)	(1.24)	(1.24)	
First generation	0.44	0.45	0.43	0.45	0.42	0.79
	(0.50)	(0.50)	(0.49)	(0.50)	(0.49)	

(continued)

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TABLE 3 (CONTINUED)

Variable	Control	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	t-test (Any Intervention-Control)
White	0.46	0.47	0.47	0.44	0.46	−0.39
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	
Black	0.21	0.22	0.2	0.23	0.21	−0.05
	(0.41)	(0.41)	(0.40)	(0.42)	(0.41)	
Hispanic	0.22	0.21	0.22	0.21	0.22	0.74
	(0.41)	(0.41)	(0.41)	(0.41)	(0.41)	
Asian	0.04	0.03	0.04	0.05	0.05	0.07
	(0.20)	(0.18)	(0.20)	(0.23)	(0.21)	
Multiracial	0.06	0.06	0.06	0.06	0.06	−0.05
	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	
SAT verbal	532.96	539.63	535.59	534.14	536.35	−0.42
	(109.37)	(106.31)	(98.26)	(105.17)	(102.02)	(109.37)
SAT math	537.63	536.00	544.22	530.26	539.60	537.63
	(111.79)	(104.66)	(102.01)	(106.99)	(108.37)	(111.79)
ACT	22.66	23.21	22.95	22.72	23.93	22.66
	(4.81)	(5.12)	(4.63)	(4.77)	(4.63)	(4.81)
HS GPA	2.55	2.50	2.46	2.54	2.51	2.55
	(1.24)	(1.22)	(1.19)	(1.23)	(1.20)	(1.24)
GPA	2.77	2.87	2.76	2.78	2.80	2.77
	(1.00)	(0.95)	(1.04)	(0.98)	(1.02)	(1.00)
EFC ($)	13,638.41	14,523.83	13,761.15	12,205.97	14,409.37	13,638.41
	(40,311.52)	(38,374.62)	(34,618.15)	(18,439.55)	(26,538.16)	(40,311.52)
N prior FAFSAs	3.5	3.3	3.2	3.3	3.3	3.5
	(3.03)	(2.82)	(2.79)	(2.89)	(2.81)	(3.03)
Observations	2,152	2,898	1,601	1,590	1,640	2,152
F-statistic	0.57	1.46	1.00	0.93	0.87	0.57
P(F)	0.98	0.04	0.48	0.59	0.69	0.98

Notes. Columns 1 to 5 present means for each treatment arm. Column 6 shows *t*-test for all treatment arms minus control when we include the sample weights and dummies for assignment strata. The FAFSA 16/17 filing is one of the stratification groups and is perfectly predicted by the strata dummies. Supplementary Appendix Table A0 in the online version of the journal shows further *t*-tests comparing each treatment arm to control. The final two rows show the *F* test and associated *p*-value from an OLS regression of assignment to each treatment arm on all baseline covariates. ACT = American College Testing; FAFSA = Free Application for Federal Student Aid; HS GPA = high school grade point average; EFC = expected family contribution; SAT = Scholastics Aptitude Test.

are included for each of these groups. $TREAT_{ij}$ is an indicator equal to one if the student is assigned to receive text outreach and otherwise equal to zero. For models that consider effects for the specific interventions, this term is replaced by a set of four dummy variables with corresponding regression coefficients. X represents baseline student- and institution-level covariates included to improve the precision of estimates. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school grade point average (GPA), college GPA, expected family contribution (EFC), and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status. Robust standard errors are reported.[7]

In Equation 1, the coefficient β represents the effect of being assigned to receive the text-message outreach (i.e., the intent-to-treat effect) rather than the effect of receiving the outreach. A substantial share of students opted out at some point during the intervention, with the majority (approximately 20% of all treatment-assigned students) opting out immediately upon receiving the introductory text. Students who opted out immediately did not receive any meaningful component of the intervention and could reasonably be assumed to have a treatment effect of zero. As such, some analyses employ an instrumental variables (IVs) approach to estimate the effect of intervention participation on FAFSA filing outcomes by using treatment assignment as an instrument for participation. In these analyses, all students who did not opt out immediately are considered to have participated in the intervention, and participation is measured with a binary indicator equal to one for all treatment-assigned students who did not opt out immediately after the introductory message. Because some students opted out during the intervention period, the IV estimates likely provide a lower-bound estimate of the effect of full participation.

Results

Program Implementation

Did students receive the informational interventions as intended? In Table 4, we assess the level of student engagement in the intervention. The top panel describes engagement results for the overall sample, and the next two panels describe results for the two main subsamples (see Supplementary Appendix Table A3 in the online version of the journal for all remaining subsamples). Nearly all students assigned to one of the treatment arms received text outreach. The average number of text messages received was higher in the text-only groups compared to the College Possible group. In the College Possible group, students needed to respond to an initial prompt to receive follow-up from a College Possible advisor and therefore had lower rates of receipt. The average student in the College Possible group received nearly 15 messages during the intervention, whereas the average number of messages in the text-only groups ranged from nearly 18 to just over 19.

As shown in Table 4, more than one-quarter of students opted out of messaging at some point during the intervention. This opt-out rate was similar irrespective of whether a College Possible advisor was offered. Most students (around 20%) opted out at the very start of the intervention. These rates of initial and overall opt out are substantially higher than in prior, comparable text-based interventions. For example, Castleman and Page (2015, 2016) reported opt-out rates of 5% to 6% in text interventions targeting recent high school graduates who are transitioning to college for the first time, as well as interventions targeting such students during their first year of college. Bird and colleagues (2021) similarly reported very low opt-out rates (under 5%) in their national FAFSA completion campaign, conducted in partnership with the Common Application. The higher opt-out rate in the present study may indicate students' lack of desire for ongoing communication from NPSAS after completing the survey in which they agreed to participate.

Several additional factors may contribute to the high opt-out rate. First, the intervention may not have been salient for students who had already filed FAFSA or were no longer enrolled. However, opt-out rates were similar regardless of students' FAFSA completion or enrollment status. Second, students may not be interested in text support in general and/or in text support from an individual they do not recognize or from

TABLE 4

Fidelity of Implementation

Treatment type	Messages received (Y/N)	Opt out (Y/N)	Opt out immediately (Y/N)	Restart (Y/N)	N messages total	N messages sent by student	N messages received by student
Sample 1: Full sample (N = 9,881)							
College possible	1.00	0.29	0.22	0.00	14.88	2.01	12.87
Texting, basic	1.00	0.27	0.20	0.00	17.90	0.87	17.03
Texting, social pressure	1.00	0.26	0.17	0.00	19.12	0.80	18.32
Texting, commitment device	1.00	0.30	0.21	0.00	18.24	0.83	17.41
Control	—	—	—	—	—	—	—
R^2	.997	.174	.122	.001	.676	.16	.696
Sample 2: FAFSA not filed prior to intervention (N = 8,004)							
College possible	1.00	0.30	0.23	0.00	14.44	1.83	12.601
Texting, basic	1.00	0.28	0.21	0.00	17.57	0.80	16.79
Texting, social pressure	1.00	0.27	0.18	0.00	18.84	0.72	18.13
Texting, commitment device	1.00	0.30	0.21	0.00	18.16	0.80	17.36
Control	—	—	—	—	—	—	—
R^2	.997	.181	.126	.001	.673	.152	.688
Sample 3: Enrolled and FAFSA not filed prior to intervention (N = 4,014)							
College possible	1.00	0.29	0.22	0.00	15.07	2.08	13.00
Texting, basic	1.00	0.30	0.24	0.00	17.27	0.82	16.45
Texting, social pressure	1.00	0.28	0.18	0.00	19.01	0.83	18.18
Texting, commitment device	1.00	0.31	0.22	0.00	18.14	0.82	17.30
Control	—	—	—	—	—	—	—
R^2	.998	.189	.137	.002	.667	.171	.688

Notes. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016 to 2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Results presented are from a regression of the outcome on a set of indicators for treatment group assignment. All implementation measures are zero for students in the control group by construction.

an organization with which they have no affiliation. The initial intervention message began, "Hi this is ____ from College Possible. When you did the NPSAS survey, you signed up for text messages about college-related topics." Students may not have recognized the NPSAS acronym or the College Possible name or may have forgotten that they previously consented to contact. NPSAS16 survey administration began in January 2016. Therefore, for some students the intervention began more than a year after they first consented.

Relatedly, students may have decided that they did not need the messaging outreach, and therefore that it was not relevant for them. When we considered the characteristics of treatment group students by whether they opted out immediately, we observed that those who opted out immediately exhibit several indicators to suggest that they are generally more advantaged. Those who opted out immediately, on average, are less likely to be first-generation college-goers, are less likely to be Black or Hispanic, and are more likely to be white. Those who opt out immediately also had higher GPAs, higher expected family contributions to the cost of college, and attended institutions with higher SAT scores overall. There were no differences by opt out status on characteristics such as gender, age, number of prior FAFSAs filed as well as institutional characteristics such as sector (public/private) and non-profit status. One possibility, in sum, is that those who opted out were comparatively more confident in their ability to complete the FAFSA and/or to access resources to support doing so, if needed. Finally, in the time that elapsed between survey administration and the start of the intervention, some students' cell phone numbers could have changed. Indeed, we saw some evidence of students reporting wrong numbers in the message content.

Although participants had the opportunity to restart messaging after opting out, very few did. Most students who received text messages did not respond (i.e., they were not highly likely to send messages to trigger automated, follow-up content), though students who were offered a College Possible advisor sent an average of two messages responding to the texts.

Impacts on Re-Filing FAFSA

Next, we examine impacts of assignment to text outreach on whether and when students filed the FAFSA to receive financial aid in the 2017 to 2018 academic year. We report week-by-week filing across 34 weeks after the start of the intervention (approximately through the end of September 2017). We reason that this is the latest possible timeframe for students to file FAFSA to receive financial aid to start the fall of the 2017 to 2018 academic year. We estimate impacts on FASFA re-filing two ways. First, we pool treatment groups to consider the impact of *any version* of the intervention week-by-week as the intervention period progressed. Second, we examine the intervention groups separately to see whether specific types of texting or texting plus advising outperformed others.

Tables 5 through 7 report results for the main sample and two subsamples. There are no clear impacts on FAFSA filing either during or after the period of active intervention for the full sample (Table 5). For example, for the full sample (Table 5), by the end of week 1, 20% of those in the control group had filed a FAFSA, with treatment effects ranging from 0.1 to 0.9 percentage points (i.e., not statistically significant). By week 14, 31.9% of students in the control group had refiled the FAFSA. The FASFA refiling rate was only 0.7 percentage points higher (i.e., not statistically significant) among students in any of the four treatment groups. However, when the sample is restricted to students who had not already filed FAFSA pre-treatment, there are modest, statistically significant impacts on FAFSA filing during the timeframe of active intervention. During weeks 1 to 12 (the intervention ended in week 14), effects are about 1 to 2 percentage points (Table 6) and are somewhat larger (i.e., 2–4 percentage points) for students who had not yet done the FAFSA and were enrolled pre-treatment (Table 7). Text-based outreach might be most relevant to this sample. However, even these effects quickly attenuated after the active intervention period.[8] This attenuation of effects was similar across all analytic subsamples. Thirty-four weeks after the intervention began, approximately 43% of students had filed the FAFSA regardless of experimental condition (Table 5).

TABLE 5

Impact on FAFSA Filing by Week: Full Sample

Week	Control mean	ITT					IV
		Any treatment	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	Any treatment
1	0.200	0.003 (0.010)	0.001 (0.012)	0.009 (0.013)	0.002 (0.013)	0.005 (0.013)	0.004 (0.010)
2	0.208	0.005 (0.010)	0.002 (0.012)	0.012 (0.013)	0.003 (0.013)	0.008 (0.013)	0.006 (0.010)
3	0.223	0.005 (0.010)	0.001 (0.012)	0.010 (0.014)	0.004 (0.013)	0.008 (0.013)	0.006 (0.010)
4	0.239	0.007 (0.010)	−0.002 (0.012)	0.017 (0.014)	0.006 (0.014)	0.012 (0.014)	0.008 (0.010)
5	0.244	0.009 (0.010)	0.002 (0.012)	0.019 (0.014)	0.006 (0.014)	0.015 (0.014)	0.012 (0.010)
6	0.251	0.009 (0.011)	0.005 (0.012)	0.016 (0.014)	0.006 (0.014)	0.013 (0.014)	0.011 (0.010)
7	0.259	0.008 (0.011)	0.004 (0.012)	0.014 (0.014)	0.003 (0.014)	0.012 (0.014)	0.010 (0.010)
8	0.266	0.013 (0.011)	0.006 (0.012)	0.023 (0.014)	0.010 (0.014)	0.019 (0.014)	0.016 (0.010)
9	0.274	0.013 (0.011)	0.007 (0.012)	0.024* (0.014)	0.008 (0.014)	0.016 (0.014)	0.016 (0.010)
10	0.285	0.011 (0.011)	0.006 (0.012)	0.021 (0.014)	0.006 (0.014)	0.014 (0.014)	0.014 (0.010)
11	0.290	0.015 (0.011)	0.012 (0.012)	0.023 (0.014)	0.013 (0.014)	0.014 (0.014)	0.019* (0.010)
12	0.299	0.014 (0.011)	0.011 (0.013)	0.020 (0.014)	0.013 (0.014)	0.013 (0.014)	0.017* (0.010)
13	0.310	0.010 (0.011)	0.007 (0.013)	0.019 (0.014)	0.009 (0.014)	0.008 (0.014)	0.013 (0.010)
14	0.319	0.007 (0.011)	0.003 (0.013)	0.016 (0.014)	0.005 (0.014)	0.005 (0.014)	0.009 (0.010)
15	0.326	0.006 (0.011)	0.004 (0.013)	0.018 (0.015)	0.002 (0.014)	0.004 (0.014)	0.008 (0.010)
16	0.332	0.006 (0.011)	0.005 (0.013)	0.016 (0.015)	0.001 (0.014)	0.004 (0.014)	0.008 (0.011)
17	0.342	0.003 (0.011)	0.003 (0.013)	0.014 (0.015)	−0.003 (0.014)	−0.001 (0.014)	0.004 (0.011)
18	0.347	0.004 (0.011)	0.002 (0.013)	0.013 (0.015)	0.001 (0.014)	0.003 (0.014)	0.005 (0.011)
19	0.353	0.004 (0.011)	0.002 (0.013)	0.013 (0.015)	0.001 (0.014)	0.001 (0.014)	0.005 (0.011)

(continued)

TABLE 5 (CONTINUED)

Week	Control mean	ITT					IV
		Any treatment	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	Any treatment
20	0.358	0.007 (0.011)	0.006 (0.013)	0.016 (0.015)	0.001 (0.014)	0.004 (0.014)	0.008 (0.011)
21	0.369	0.002 (0.011)	0.002 (0.013)	0.012 (0.015)	−0.006 (0.014)	−0.001 (0.014)	0.002 (0.011)
22	0.380	−0.003 (0.011)	−0.004 (0.013)	0.006 (0.015)	−0.010 (0.014)	−0.007 (0.014)	−0.004 (0.011)
23	0.390	−0.006 (0.011)	−0.008 (0.013)	0.003 (0.015)	−0.010 (0.014)	−0.009 (0.014)	−0.008 (0.011)
24	0.394	−0.004 (0.011)	−0.007 (0.013)	0.006 (0.015)	−0.008 (0.014)	−0.006 (0.014)	−0.005 (0.011)
25	0.400	−0.001 (0.011)	−0.004 (0.013)	0.011 (0.015)	−0.005 (0.014)	−0.006 (0.015)	−0.002 (0.011)
26	0.408	−0.002 (0.011)	−0.005 (0.013)	0.013 (0.015)	−0.006 (0.015)	−0.006 (0.015)	−0.002 (0.011)
27	0.413	0.000 (0.011)	−0.004 (0.013)	0.013 (0.015)	−0.002 (0.015)	−0.003 (0.015)	0.000 (0.011)
28	0.417	0.000 (0.011)	−0.005 (0.013)	0.011 (0.015)	−0.001 (0.014)	−0.001 (0.015)	0.000 (0.011)
29	0.422	−0.000 (0.011)	−0.006 (0.013)	0.009 (0.015)	0.001 (0.015)	−0.002 (0.015)	−0.000 (0.011)
30	0.424	0.000 (0.011)	−0.006 (0.013)	0.011 (0.015)	−0.000 (0.014)	0.001 (0.015)	0.000 (0.011)
31	0.426	0.000 (0.011)	−0.006 (0.013)	0.012 (0.015)	−0.000 (0.014)	−0.001 (0.015)	0.000 (0.011)
32	0.428	0.000 (0.011)	−0.005 (0.013)	0.013 (0.015)	−0.000 (0.014)	−0.001 (0.015)	0.000 (0.011)
33	0.428	0.003 (0.011)	−0.003 (0.013)	0.015 (0.015)	0.002 (0.015)	0.001 (0.015)	0.003 (0.011)
34	0.428	0.003 (0.011)	−0.002 (0.013)	0.015 (0.015)	0.002 (0.015)	0.001 (0.015)	0.004 (0.011)

Note. Standard errors in parentheses. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016 to 2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status. Final FAFSA message in week 13. Horizontal line after 14 weeks demarcates the end of the active intervention period. ITT = intent-to-treat; IV = instrumental variables.
*$p < .10$. **$p < .05$. ***$p < .01$.

TABLE 6
Impact on FAFSA Filing by Week: Sample 2

| Week | Control mean | ITT | | | | | IV |
		Any treatment	College Possible	Texting, basic	Texting, social pressure	Texting, commitment device	Any treatment
1	0.010	0.006**	0.005	0.010**	0.006	0.006	0.008**
		(0.003)	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)
2	0.021	0.009**	0.006	0.014**	0.008	0.009	0.011**
		(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.004)
3	0.038	0.008	0.005	0.011	0.009	0.009	0.010*
		(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.006)
4	0.058	0.011	0.002	0.021**	0.012	0.015	0.014**
		(0.007)	(0.008)	(0.010)	(0.009)	(0.009)	(0.007)
5	0.065	0.014**	0.006	0.024**	0.012	0.020**	0.018**
		(0.007)	(0.008)	(0.010)	(0.009)	(0.010)	(0.007)
6	0.073	0.014*	0.010	0.020**	0.012	0.017*	0.018**
		(0.007)	(0.009)	(0.010)	(0.010)	(0.010)	(0.007)
7	0.084	0.012	0.010	0.018*	0.009	0.016	0.016**
		(0.008)	(0.009)	(0.011)	(0.010)	(0.011)	(0.008)
8	0.091	0.019**	0.011	0.028**	0.018	0.024**	0.024***
		(0.008)	(0.010)	(0.011)	(0.011)	(0.011)	(0.008)
9	0.102	0.018**	0.013	0.030**	0.014	0.021*	0.023***
		(0.009)	(0.010)	(0.012)	(0.011)	(0.011)	(0.008)
10	0.115	0.016*	0.011	0.027**	0.012	0.019	0.020**
		(0.009)	(0.010)	(0.012)	(0.012)	(0.012)	(0.009)
11	0.122	0.021**	0.019*	0.029**	0.020*	0.019	0.027***
		(0.009)	(0.011)	(0.013)	(0.012)	(0.012)	(0.009)
12	0.132	0.020**	0.016	0.025**	0.021*	0.018	0.025***
		(0.009)	(0.011)	(0.013)	(0.012)	(0.012)	(0.009)
13	0.146	0.016	0.013	0.024*	0.016	0.013	0.020**
		(0.010)	(0.011)	(0.013)	(0.013)	(0.013)	(0.010)
14	0.157	0.011	0.008	0.021	0.011	0.009	0.014
		(0.010)	(0.012)	(0.013)	(0.013)	(0.013)	(0.010)
15	0.166	0.011	0.008	0.023*	0.007	0.007	0.014
		(0.010)	(0.012)	(0.014)	(0.013)	(0.013)	(0.010)
16	0.173	0.010	0.009	0.021	0.006	0.007	0.013
		(0.010)	(0.012)	(0.014)	(0.013)	(0.013)	(0.010)
17	0.186	0.007	0.007	0.018	0.001	0.001	0.009
		(0.010)	(0.012)	(0.014)	(0.014)	(0.014)	(0.010)
18	0.192	0.008	0.005	0.016	0.006	0.007	0.010
		(0.011)	(0.012)	(0.014)	(0.014)	(0.014)	(0.010)
19	0.200	0.007	0.005	0.017	0.006	0.004	0.009
		(0.011)	(0.012)	(0.014)	(0.014)	(0.014)	(0.010)

(continued)

TABLE 6 (CONTINUED)

| Week | Control mean | ITT | | | | | IV |
		Any treatment	College Possible	Texting, basic	Texting, social pressure	Texting, commitment device	Any treatment
20	0.206	0.011 (0.011)	0.011 (0.013)	0.020 (0.015)	0.007 (0.014)	0.008 (0.014)	0.014 (0.011)
21	0.220	0.005 (0.011)	0.005 (0.013)	0.016 (0.015)	-0.001 (0.014)	0.002 (0.014)	0.007 (0.011)
22	0.234	-0.001 (0.011)	-0.002 (0.013)	0.009 (0.015)	-0.006 (0.015)	-0.005 (0.015)	-0.001 (0.011)
23	0.245	-0.004 (0.011)	-0.007 (0.013)	0.004 (0.015)	-0.006 (0.015)	-0.008 (0.015)	-0.006 (0.011)
24	0.250	-0.002 (0.011)	-0.006 (0.013)	0.008 (0.015)	-0.004 (0.015)	-0.004 (0.015)	-0.003 (0.011)
25	0.258	0.001 (0.011)	-0.003 (0.013)	0.015 (0.015)	0.000 (0.015)	-0.004 (0.015)	0.001 (0.011)
26	0.267	0.001 (0.012)	-0.004 (0.013)	0.016 (0.015)	-0.001 (0.015)	-0.003 (0.015)	0.001 (0.011)
27	0.274	0.003 (0.012)	-0.003 (0.013)	0.016 (0.015)	0.004 (0.015)	-0.000 (0.015)	0.004 (0.011)
28	0.279	0.003 (0.012)	-0.004 (0.013)	0.014 (0.015)	0.004 (0.015)	0.003 (0.015)	0.004 (0.011)
29	0.285	0.002 (0.012)	-0.005 (0.013)	0.012 (0.015)	0.007 (0.015)	0.002 (0.015)	0.003 (0.011)
30	0.288	0.003 (0.012)	-0.005 (0.013)	0.014 (0.015)	0.006 (0.015)	0.005 (0.015)	0.004 (0.011)
31	0.291	0.003 (0.012)	-0.005 (0.013)	0.016 (0.016)	0.006 (0.015)	0.003 (0.015)	0.004 (0.011)
32	0.292	0.004 (0.012)	-0.005 (0.013)	0.016 (0.016)	0.006 (0.015)	0.003 (0.016)	0.005 (0.011)
33	0.292	0.007 (0.012)	-0.001 (0.013)	0.019 (0.016)	0.009 (0.015)	0.006 (0.016)	0.008 (0.011)
34	0.292	0.007 (0.012)	-0.000 (0.013)	0.019 (0.016)	0.009 (0.015)	0.006 (0.016)	0.009 (0.011)

Note. Sample 2 includes those who had not filed the 2017—2018 FAFSA by the start of the intervention period and who were enrolled in fall 2016. Standard errors in parentheses. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016—2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status. Final FAFSA message in week 13. Horizontal line after 14 weeks demarcates the end of the active intervention period. ITT = intent-to-treat; IV = instrumental variables.

*$p < .10$. **$p < .05$. ***$p < .01$.

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TABLE 7
Impact on FAFSA Filing by Week: Sample 3

| Week | Control mean | ITT | | | | | IV |
		Any treatment	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	Any treatment
1	0.016	0.007 (0.006)	0.005 (0.006)	0.012 (0.008)	0.010 (0.008)	0.004 (0.007)	0.009* (0.006)
2	0.034	0.007 (0.008)	0.005 (0.009)	0.010 (0.011)	0.004 (0.010)	0.009 (0.010)	0.009 (0.008)
3	0.060	0.007 (0.010)	0.005 (0.011)	0.011 (0.013)	0.003 (0.013)	0.011 (0.013)	0.009 (0.010)
4	0.089	0.015 (0.012)	0.003 (0.013)	0.030* (0.016)	0.013 (0.016)	0.025 (0.016)	0.020* (0.012)
5	0.101	0.018 (0.012)	0.009 (0.014)	0.030* (0.017)	0.009 (0.016)	0.032** (0.016)	0.023* (0.012)
6	0.112	0.021* (0.013)	0.018 (0.015)	0.028 (0.018)	0.012 (0.017)	0.028* (0.017)	0.027** (0.013)
7	0.125	0.021 (0.013)	0.021 (0.016)	0.024 (0.018)	0.010 (0.018)	0.027 (0.018)	0.027** (0.013)
8	0.134	0.035** (0.014)	0.029* (0.016)	0.042** (0.019)	0.025 (0.019)	0.047** (0.019)	0.044*** (0.014)
9	0.149	0.033** (0.014)	0.028* (0.017)	0.044** (0.020)	0.024 (0.019)	0.039** (0.019)	0.042*** (0.014)
10	0.169	0.028* (0.015)	0.026 (0.018)	0.037* (0.020)	0.022 (0.020)	0.030 (0.020)	0.036** (0.015)
11	0.176	0.037** (0.015)	0.040** (0.018)	0.041** (0.021)	0.030 (0.021)	0.034* (0.020)	0.047*** (0.015)
12	0.193	0.034** (0.016)	0.035* (0.018)	0.031 (0.021)	0.033 (0.021)	0.035* (0.021)	0.043*** (0.015)
13	0.214	0.028* (0.016)	0.028 (0.019)	0.029 (0.022)	0.025 (0.022)	0.028 (0.021)	0.035** (0.016)
14	0.232	0.020 (0.017)	0.018 (0.019)	0.024 (0.022)	0.017 (0.022)	0.021 (0.022)	0.025 (0.016)
15	0.243	0.020 (0.017)	0.021 (0.020)	0.022 (0.022)	0.011 (0.022)	0.023 (0.022)	0.025 (0.016)
16	0.252	0.022 (0.017)	0.025 (0.020)	0.022 (0.022)	0.014 (0.023)	0.024 (0.022)	0.028* (0.016)
17	0.272	0.014 (0.017)	0.021 (0.020)	0.014 (0.023)	0.001 (0.023)	0.013 (0.022)	0.018 (0.017)
18	0.277	0.019 (0.017)	0.023 (0.020)	0.020 (0.023)	0.010 (0.023)	0.022 (0.023)	0.025 (0.017)

(continued)

TABLE 7 (CONTINUED)

Week	Control mean	ITT					IV
		Any treatment	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	Any treatment
19	0.291	0.016 (0.017)	0.020 (0.020)	0.016 (0.023)	0.013 (0.023)	0.013 (0.023)	0.021 (0.017)
20	0.298	0.021 (0.017)	0.029 (0.020)	0.020 (0.023)	0.012 (0.023)	0.017 (0.023)	0.027 (0.017)
21	0.321	0.009 (0.018)	0.020 (0.020)	0.005 (0.023)	−0.004 (0.024)	0.003 (0.023)	0.011 (0.017)
22	0.334	0.005 (0.018)	0.017 (0.020)	−0.005 (0.023)	−0.007 (0.024)	0.002 (0.023)	0.006 (0.017)
23	0.353	−0.002 (0.018)	0.008 (0.020)	−0.010 (0.023)	−0.009 (0.024)	−0.004 (0.023)	−0.002 (0.017)
24	0.359	0.003 (0.017)	0.009 (0.020)	−0.001 (0.023)	−0.003 (0.024)	−0.001 (0.023)	0.003 (0.017)
25	0.369	0.007 (0.018)	0.011 (0.020)	0.006 (0.023)	0.008 (0.024)	0.002 (0.023)	0.010 (0.017)
26	0.385	0.003 (0.018)	0.004 (0.020)	0.005 (0.024)	0.002 (0.024)	−0.000 (0.023)	0.004 (0.017)
27	0.392	0.006 (0.018)	0.007 (0.020)	0.005 (0.024)	0.014 (0.024)	0.001 (0.023)	0.008 (0.017)
28	0.398	0.007 (0.018)	0.006 (0.020)	0.003 (0.023)	0.017 (0.024)	0.003 (0.023)	0.009 (0.017)
29	0.406	0.006 (0.018)	0.004 (0.020)	0.001 (0.024)	0.018 (0.024)	0.002 (0.023)	0.008 (0.017)
30	0.411	0.003 (0.018)	0.001 (0.020)	−0.001 (0.023)	0.012 (0.024)	−0.001 (0.023)	0.003 (0.017)
31	0.414	0.003 (0.018)	0.002 (0.020)	−0.000 (0.023)	0.016 (0.024)	−0.002 (0.023)	0.004 (0.017)
32	0.416	0.003 (0.018)	0.002 (0.020)	−0.003 (0.024)	0.016 (0.024)	−0.003 (0.023)	0.004 (0.017)
33	0.416	0.005 (0.018)	0.004 (0.020)	−0.002 (0.024)	0.022 (0.024)	−0.001 (0.024)	0.007 (0.017)
34	0.416	0.006 (0.018)	0.005 (0.020)	−0.000 (0.024)	0.022 (0.024)	−0.001 (0.024)	0.008 (0.017)

Note. Sample 3 includes those who had not filed the 2017 to 2018 FAFSA by the start of the intervention period and who were enrolled in fall 2016. Standard errors in parentheses. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016 to 2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status. Final FAFSA message in week 13. Horizontal line after 14 weeks demarcates the end of the active intervention period. ITT = intent-to-treat; IV = instrumental variables.

*$p < .10$. **$p < .05$. ***$p < .01$.

TABLE 8

Impact of Outreach on Receipt of Any Federal Financial Aid, by Subsample

Sample	Control mean	Any treatment	College possible	Texting, basic	Texting, social pressure	Texting, commitment device	N
Full sample	0.35	0.01	0.00	0.01	0.02	0.00	9,881
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Sample 2	0.26	0.01	0.00	0.01	0.02	0.00	8,004
		(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	
Sample 3 (all)	0.38	0.00	−0.01	0.01	0.01	−0.01	4,014
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Sample 4 (2-year)	0.33	−0.01	−0.02	0.01	−0.01	−0.01	1,011
		(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	
Sample 5 (4-year)	0.39	0.00	−0.01	0.01	0.02	−0.02	3,003
		(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	
Sample 6 (4-year public)	0.38	0.00	0.00	0.00	0.01	−0.01	2,002
		(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	
Sample 7 (4-year private)	0.40	0.00	−0.01	0.05	0.03	−0.05	1,001
		(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	

Note. Sample 2 did not file a FAFSA prior to intervention, and Sample 3 was enrolled but did not file FAFSA prior to intervention. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016 to 2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Robust standard errors, clustered at the student level, reported in parentheses. Baseline covariates at the individual level include age, sex, class year, first-generation status, race ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status.
*$p < .10$. **$p < .05$. ***$p < .01$.

There is no evidence that any of the messaging variants—social pressure, commitment device, basic, or basic with the offer of support—was more effective than the others. If anything, point estimates were often larger for the basic form of the intervention, but these effects were not estimated precisely enough to be statistically distinguishable.

Tables 5 through 7 also report the IV (treatment on treated) impact of intervention participation on week-by-week FAFSA filing. As anticipated, these treatment effects are somewhat larger than the intent-to-treat (ITT) effects. For example, in the smaller sample of enrolled students who had not filed FAFSA pre-treatment, impacts on FAFSA filing range from 3 to 5 percentage points during the active intervention period. Nevertheless, as with the ITT effects, these treatment effects attenuated to zero following the intervention period.

Finally, there is some evidence that impacts on FAFSA filing varied by the type or sector of the college where the student was enrolled. Note

that these analyses pertain only to students who had not filed the FAFSA and were enrolled in college prior to the intervention. For those students, by the end of week 10 of the intervention FAFSA filing was nearly 7 percentage points higher for those enrolled in 2-year institutions (Supplementary Table A4 in the online version of the journal) and nearly 5 percentage points higher for those enrolled in 4-year private institutions (Supplementary Table A6 in the online version of the journal). In contrast, there are null effects for those enrolled in 4-year public institutions (Supplementary Table A5 in the online version of the journal). However, none of the groups seem to have lasting benefits of the intervention.

Impacts on Federal Financial Aid Receipt

Next, we examined impacts of nudging on receipt of federal financial aid, inclusive of Pell grants, subsidized and unsubsidized Stafford loans, Perkins loans, and Parent PLUS loans. Table 8 presents these results for the 2017 to 2018

TABLE 9

Impacts on Total Months Enrolled and Graduation: Full Sample

Treatment type	Total months enrolled since Jan 2017		Graduation	
	(1)	(2)	(3)	(4)
Any treatment	−0.08		0.02*	
	(0.32)		(0.01)	
College possible		0.10		0.02
		(0.39)		(0.01)
Texting, basic		0.11		0.01
		(0.43)		(0.02)
Texting, social pressure		−0.44		0.01
		(0.40)		(0.01)
Texting, commitment device		−0.25		0.04**
		(0.39)		(0.02)
Control M	11.20	11.20	0.35	0.35
Control SD	13.39	13.39	0.48	0.48
Observations	9,881	9,881	9,881	9,881
R^2	.176	.177	.256	.256

Note. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016 to 2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Robust standard errors, clustered at the student level, reported in parentheses. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status.
*$p < .10$. **$p < .05$. ***$p < .01$.

academic year, given that the intervention aimed to increase FAFSA filing for that year. If students filed the FAFSA somewhat earlier due to the intervention, they may have received more financial aid. We examined the possibility for all seven samples. In Supplementary Table A7 in the online version of the journal, we offer analyses of the impact on Pell receipt from 2018 to 2021 for the full sample, and in Supplementary Table A8 in the online version of the journal we do the same for loans.

Despite the text outreach creating modest boosts in FAFSA filing during the intervention period within some subsamples, it produced no statistically significant impacts on federal financial aid receipt in the main sample or in any of the subsamples considered. Given that the control group FAFSA filing rate caught up with that of the treatment groups, perhaps the lack of difference in access to federal financial aid is unsurprising. It may be that the generosity of other sources of aid (e.g., institutional aid and/or state-based aid) was affected by the differences in FAFSA timing that the outreach caused (as suggested by Page et al., 2020), but we were unable

to observe non-federal sources of aid for the purpose of this investigation.

Impacts on Persistence and Graduation

Finally, to consider impacts of the outreach on college persistence and degree attainment, RTI furnished college enrollment and graduation records obtained from the National Student Clearinghouse. These records included semester-level information on whether and where each student in the sample was enrolled and what degree, if any, they earned. We observed enrollment and degree completion outcomes through the 2019 to 2020 academic year.

Although the text-based outreach did not ultimately improve rates of FAFSA filing or federal financial aid receipt, the effects on FAFSA timing could have translated to improved levels of other aid and rates of persistence and degree attainment. Tables 9 through 11 offer estimated impacts on persistence (measured as months of enrollment) and graduation (i.e., completion of a certificate, associate's, or bachelor's degree) for

TABLE 10

Impacts on Total Months Enrolled and Graduation: Sample 2

Treatment type	Total months enrolled since Jan 2017		Graduation	
	(1)	(2)	(3)	(4)
Any treatment	−0.03		0.02	
	(0.34)		(0.01)	
College possible		0.13		0.02
		(0.39)		(0.01)
Texting, basic		0.01		0.00
		(0.45)		(0.02)
Texting, social pressure		−0.35		0.01
		(0.44)		(0.02)
Texting, commitment device		−0.06		0.04**
		(0.43)		(0.02)
Control M	9.59	9.59	0.31	0.31
Control SD	13.12	13.12	0.46	0.46
Observations	8,004	8,004	8,004	8,004
R^2	.179	.179	.241	.242

Note. Analyses include fixed effects for randomization group defined by institution type, FAFSA filing in the 2016 to 2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Robust standard errors, clustered at the student level, reported in parentheses. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status.
$p < .10$.$p < .05$.**$p < .01$.

the full sample and the secondary subsamples. For the most part, estimated treatment effects are small and statistically insignificant across samples. Overall, the results demonstrated no consistent evidence of intervention impacts on enrollment or degree attainment.

Cost Analysis

The intervention was facilitated by Signal Vine, which implemented the text messaging, and College Possible, which provided advisers to interact with students on an as-needed basis. Signal Vine hosted messaging to 7,737 students at a per-student cost of $7.13 for the period of the intervention. This reflects the costs of both one-way texting for nudge-only students ($1.00 per month), and two-way texting for nudge plus advising students ($1.50 per month). In total, the cost of texting via Signal Vine was $55,100.

College Possible served about 2,900 students at a per-student cost of $125.95 per student (this is higher than the costs for this work in other interventions). Those costs included eight College Possible advisers, each of whom had an advising load of roughly 360 students, and each of whom cost $45,000 for the 7-month period. In total, offering students advising via College Possible cost $365,000. In theory, it might be possible to find a different provider or train advisers to carry a higher advising load. For example, in another intervention (Avery et al., 2021), full-time text-based advisors carried caseloads of a few thousand students, although these students received approximately one outreach message per month rather than one per week. An advisee load of 360 students is large, but only 40% of students ever sent a message to their College Possible adviser, and only 53% received a personalized message from an advisor. It might be possible to cut the cost of such personalized advising substantially if it were conducted by university or other staff members who are already engaged in advising for the focal students. In addition, as discussed previously, the intervention might be better targeted.

TABLE 11

Impacts on Total Months Enrolled and Graduation: Sample 3

Treatment type	Total months enrolled		Graduation	
	(1)	(2)	(3)	(4)
Any treatment	−1.03*		0.01	
	(0.56)		(0.02)	
College possible		−0.54		0.013
		(0.64)		(0.02)
Texting, basic		−1.217*		−0.02
		(0.69)		(0.03)
Texting, social pressure		−1.53**		0.02
		(0.69)		(0.03)
Texting, commitment device		−1.24*		0.03
		(0.68)		(0.03)
Control *M*	15.88	15.88	0.56	0.56
Control *SD*	14.65	14.65	0.50	0.50
Observations	4,014	4,014	4,014	4,014
R^2	.107	.108	.134	.135

Note. Analyses include fixed effects for randomization group, institution type, FAFSA filing in the 2016-2017 year, and intensity of enrollment (full-time, part-time, less than part-time) at the time of baseline data collection. Robust standard errors, clustered at the student level, reported in parentheses. Baseline covariates at the individual level include age, sex, class year, first-generation status, race/ethnicity, SAT/ACT performance, high school GPA, college GPA, expected family contribution and number of FAFSAs previously filed. Baseline covariates at the institutional level include 75th percentile of SAT/ACT performance, sector, and non-profit status.

*p < .10. **p < .05. ***p < .01.

Even given the high cost of College Possible advising, our calculations suggest that the intervention could be cost effective for even small yet positive impact sizes. Suppose that the return to an extra year of college is 7% and that we apply this to a very conservative baseline earnings estimate of $40,000 per year. Then an additional year of college persistence is the net present value of an additional 40-year stream of earnings (i.e., 0.07*40,000 minus lost earnings for that year of college). At a conservative (i.e., very high) discount rate of 10%, the additional stream of earnings is worth $30,119. If we estimate foregone earnings of $20,000 during the extra year of college, this implies benefits from an additional year of college of at least $10,000 in net present value terms. The Signal Vine expenses are only $7.33 per student, so an increase in 1 year of persistence for only 0.07% of students would pay for the cost of the program (0.0007*10,000 is roughly $7). College Possible plus Signal Vine costs roughly $133 per student. Hence, a boost to 1 year

persistence for 1.3% of students would justify the cost of the College Possible intervention under conservative assumptions. For reference, our study was designed with a minimum detectable effect size of 4% on college persistence. Therefore, at the minimum detectible effect (MDE), the program is well justified given its cost. These calculations are exactly why there is so much interest from policy makers and researchers in low-cost nudges.

Discussion

Do informational interventions delivered at arm's length and at-scale effectively nudge students to complete financial aid applications, improving their financial security in college, and promoting academic success? The results of this study suggest that they may not always succeed and, indeed, the results are consistent with a growing understanding of the conditions under which text-based outreach has the best chances

to shape and improve student outcomes. While the intervention somewhat accelerated the timing of FAFSA filing for some students, slightly increasing the odds that they filed during the spring when they were being nudged to do so, it did not boost overall rates of FAFSA filing. Moreover, students neither received additional federal financial aid because of the intervention nor incurred positive benefits in terms of continued enrollment or graduation. Estimated overall impacts of this intervention are consistent with another recent study on financial aid nudging (Bird et al., 2021). However, they are considerably smaller than for other interventions that provided a combination of individualized support and direct assistance with FAFSA submission (Bettinger et al., 2012; Cannon & Goldrick-Rab, 2016), or that implemented interactive, text-based nudges at a local level in partnership with non-profits or schools that had established relationships with the students targeted for outreach (Castleman & Page, 2015, 2016).

One possibility is that students could have benefited from an extended period of outreach and support. Especially given the relatively large effects on FAFSA filing for those enrolled in 2-year institutions (consistent with the findings from Castleman & Page, 2016), the intervention may have yielded better outcomes if the outreach continued through the summer of 2017 rather than ending in the spring. Also, our analyses were limited by the inability to examine whether the positive impacts of FAFSA filing during spring term impacted the amount of state and/or institutional aid students received. This might have been especially beneficial for students at private 4-year colleges and universities which provide substantial institutional aid, or those living in states with robust state financial aid programs.

More likely, however, a body of literature that has grown and coalesced over the course of this intervention points to several aspects of this study's design that may have contributed to its lack of sustained impact. First, a lack of credibility and/or name recognition on the part of the messenger may have reduced efficacy of the supports. This hypothesis is consistent with findings from Avery et al. (2021) and Bird et al. (2021). Taken together, one reason text-based interventions have not maintained their efficacy when

implemented at state or national scale is that they have been implemented by institutions with whom students lack a direct relationship. Without such a trusting relationship, text messaging may not be an appropriate form of communication. Support for this hypothesis is found in the very high opt out rates that we observe.

A second and related factor is message relevance. Avery and colleagues (2021) find that text-based outreach is more effective when messages can be customized based student-level data. For example, in one study, the authors were able to customize outreach to students according to whether they had filed a FAFSA, the FAFSA was considered complete, or the student was required to complete FAFSA verification procedures (Avery et al., 2021). In this study, we were unable to capitalize on any student-level data. Therefore, all students in a given messaging group received the same outreach whether or not it was relevant to their personal circumstances. Both lack of an existing relationship and lack of relevance of the messages themselves may have contributed to the very high opt-out rates that we observed in this study. A factor that may have contributed to a lack of relevance is the substantial amount of time (for some students, nearly a full year) from when we recruited students for the study and when texting commenced. During this time lag, students may have already solidified plans to enroll (or not) in college for the subsequent term and/or to refile the FAFSA. Indeed, by the start of our active intervention period, a large share of the sample had already filed the FAFSA for the following academic year. As a result, they were inframarginal to our text outreach.

In sum, our study contributes to the growing evidence that light-touch nudges are less effective at promoting improved college-going outcomes, especially when implemented as stand-alone interventions and without the foundation of a trusting relationship and the benefit of detailed administrative data to allow for targeting of outreach (Page et al., 2020). The lack of efficacy of broad-based nudges at scale moreover stands in contrast to the relative success of this type of intervention when implemented as an integrated complement to other college-going supports within the school or university context (Meyer et al., 2022; Page & Gehlbach, 2017; Page, Lee, & Gehlbach, 2020) as well as to the large and lasting impacts of

more intensive coaching interventions that address many of the same information and assistance barriers text-based nudges are designed to ameliorate (Barr & Castleman, 2021; Bettinger & Baker, 2014; Oreopoulos & Petronijevic, 2019; Scrivener et al., 2015).

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Notes

1. While the authors measured students' persistence in college, they did not measure impacts of the intervention on FAFSA refiling and financial aid receipt.

2. Bird and colleagues' (2021) state-level sample did focus in part on FAFSA renewal. However, recent research demonstrates that state- and national-level estimates of the impact of financial aid can differ, even when using the same identification strategy. Using the same discontinuity to estimate the impact of additional Pell Grant assistance on students' persistence in college, Denning et al. (2019) and Eng and Matsudaira (2021) obtained different estimates from state and national samples, respectively. This difference in the efficacy of Pell Grant aid at the state versus national level perhaps extends to differential efficacy of FAFSA renewal outreach efforts at the state or federal level.

3. For more information on College Possible, see www.collegepossible.org

4. The tool can be found here: https://studentaid.gov/resources/irs-drt-text

5. At the time of designing this study, our sample size targets were estimated based on prior waves of NPSAS

and assumptions regarding the share of students who would agree to participate in follow-up communication after completing the NPSAS survey. Also, we were uncertain regarding the control group rates of FAFSA filing that we might expect. We have imbalance in the sample sizes of our experimental conditions for several reasons. First, we maximized the size of our control group, subject to constraints across this project and another experimental study implemented in the context of NPSAS:2016. Both studies completed randomization in collaboration, have partially overlapping control groups, and distinct treatment groups. Second, we maximized the size of the College Possible experimental group, subject to budgetary constraints, regarding the student caseloads the College Possible staff members were able to advise. Because the College Possible intervention was the costliest of all the interventions, and given the associated staffing costs, we aimed to be as precise as possible in estimating the effects of the College Possible intervention. Considering the outcome of ever filing a FAFSA (essentially filing by the 34th week of the intervention), we estimated a minimum detectable effect (MDE) of any treatment of approximately 2.9 percentage points over the control group rate of 42.8 percentage points and an MDE of the College Possible treatment of approximately 3.4 percentage points. MDEs for the other treatment variations are somewhat larger. Nevertheless, the study is adequately powered to detect relatively modest intervention effects.

6. Many students were sampled for NPSAS:2016 during the spring term of the 2015 to 2016 academic year, but the semester prior to intervention was the fall term of the 2016 to 2017 academic year. A drawback of this sampling approach is the sizable time lag between study recruitment and intervention implementation. During this time lag, many students included in the sample appear to have stopped out of college. Nonetheless, the intervention may have been salient if they wished to return.

7. Baseline covariates at the student and institution levels were complete. Where a covariate was missing, it was coded as zero and a flag for the variable being missing was created and coded as one (i.e., zero if not missing). Then, for each variable with any missing, the variable and the missing flag were both included in the covariate vector.

8. Note that we do not correct for multiple hypothesis testing, given that the repeated FAFSA filing outcomes are highly correlated over time. Doing so, of course, could further reduce the number of significant results in Tables 6 and 7.

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