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Affirmation on the go: A proof-of-concept for text message delivery of values affirmation in education

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Abstract

The present study offers a proof-of-concept for the delivery of values affirmation via text message. In two studies, we tested whether we could distill the typical 15-minute pen-and-paper values affirmation exercise into a brief (~4 minute) text-message based exercise. In Study 1 ($N = 42$), we asked students to identify an upcoming academic stressor. In Study 2, we targeted students ($N = 121$) who reported that they would be starting a summer internship they expected to be stressful. In both studies, students completed a brief exercise (affirmation or control) via text message the night before their stressor. Across the studies, we found consistent benefits of this mobile affirmation on students' belonging, inconsistent effects on their perceptions of stress, and no effects on their evaluations of their stressor when measured shortly after (Study 1) or during (Study 2) the stressor. Together, these studies offer initial evidence for a novel, promising, and scalable method of delivering values affirmation at the "right time and place" using mobile technology. We also discuss lessons we learned and offer recommendations to researchers interested in administering affirmation via text message.

KEYWORDS
education, field research/interventions, self/identity

College students text—a lot. Can we leverage text messaging to help them better cope with the stressors they face? In particular, mobile phones—which students carry with them almost everywhere—might allow social psychological interventions to be delivered at the "right time and place," as has been argued to be crucial to their efficacy (see Cohen & Sherman, 2014; Ferrer & Cohen, 2019; Yeager & Walton, 2011). This prospect could increase the scalability of effective interventions, allowing them to be delivered to a broad population of students as they pursue their education. Despite this promise, much work remains to both tailor existing interventions to mobile formats and to test their effectiveness empirically. In the present research, we examined the potential to use mobile technology to deliver an affirmation intervention timed to unique stressors in students' lives.

Stress and self-affirmation theory

College students face numerous stressors in their academic lives. Encountering challenging or adverse experiences in domains we view as important to our sense of identity can interfere with performance, exacerbate feelings of stress, and undermine well-being (Sherman & Cohen, 2006). Given that we are unlikely to remove stress from the lives of college students, self-affirmation theory (Steele, 1988; see also Cohen & Sherman, 2014; Sherman & Cohen, 2006) offers a novel and highly useful perspective on how individuals might be buffered against the negative effects of stress. Self-affirmation theory posits that, when faced with a stressor in one domain, people can shore up their overall sense of self-integrity by bolstering another, non-threatened aspect of the self. These acts of self-affirmation expand the working self-concept (Critcher & Dunning, 2015) and allow people to construe a stressor in a broader context (Schmeichel & Vohs, 2009; Wakslak & Trope, 2009).

Leveraging these insights, a large and growing area of research aims to use self-affirmation interventions to improve people's outcomes in the face of stress (Cohen & Sherman, 2014). Participants in these studies typically complete a writing exercise designed to remind them of unthreatened aspects of the self before a stressful experience in the laboratory or their everyday lives. Of particular relevance to the present study, affirmation interventions have been shown to improve performance (Creswell et al., 2013) and reduce stress (Creswell et al., 2005; Morgan & Harris, 2015; Sherman et al., 2009). Recently, researchers have also begun to examine affirmation's potential to buffer or increase feelings of well-being (Armitage, 2016; Brady et al., 2020; Morgan & Atkin, 2016; Nelson et al., 2014), in the wake of threatening experiences.

Affirmation in educational contexts

Of note, a substantial number of studies have shown that students facing academic-related stressors—both experimentally manipulated in the laboratory (e.g., Trier Social Stress Task presentations, difficult problem sets under time pressure) and naturalistic (e.g., midterm exams)—can be buffered by self-affirmation exercises. For example, in the laboratory, Creswell and colleagues (2005) found that self-affirmed participants showed decreased cortisol reactivity in response to a stressful, evaluative speech task in front of unfriendly evaluators. In another laboratory study (Creswell et al., 2013), problem-solving under time pressure improved when

undergraduates who reported high levels of chronic stress completed a self-affirmation. Sherman and colleagues (2009) showed that self-affirmation decreased physiological and emotional stress responses around students' midterm exams.

Many notable examples of this research have been field studies where researchers have delivered *values affirmations* in the classroom to improve student outcomes (Cohen et al., 2009; Harackiewicz et al., 2014; Jordt et al., 2017). Although not the only approach to affirmation (Armitage et al., 2011; McQueen & Klein, 2006), values affirmation is a commonly used method to offer self-affirmation to students, in part because of its similarity to standard classroom activities (i.e., reading and writing exercises). In a typical values affirmation, students are presented with a list of values (e.g., relationships with friends and family, independence, creativity). They are asked to select their most important values and to spend 15 minutes writing about why these values are important to them. By reflecting on their core values in this way, students' selves are bolstered against situational threat. Relative to control conditions where students write about neutral topics such as less important values, affirmed students display benefits consistent with lowered psychological threat and stress. In past studies, students affirmed before a major exam or toward the beginning of the year have earned higher grades (e.g., Cohen et al., 2009; Harackiewicz et al., 2014) and reported greater well-being, in particular, greater belonging (Brady et al., 2016; Cook et al., 2012). Further, their sense of belonging has been less contingent on day-to-day events (Brady et al., 2016; Cook et al., 2012; Sherman et al., 2013), a phenomenon known as untethering.

The right time and place

A key methodological feature of affirmation intervention studies is that the opportunity to affirm needs to be *timely*. Affirmations offer the greatest benefit when they occur soon before a stressor, before a defensive response has been generated (Critcher et al., 2010)—or, if after, soon after, before the defensive processes have taken root (Tang & Schmeichel, 2015; see also Cohen & Sherman, 2014). Timeliness is inherent in the laboratory research on self-affirmation, where affirmations are delivered proximal to an experimentally-induced stressor (Creswell et al., 2005; Koole et al., 1999). Critcher et al. (2010) demonstrated that even these laboratory effects are susceptible to small differences in timing, finding that affirmation before threatening feedback decreased defensive responding, while affirmation after threatening feedback did not. Likewise, Cook and colleagues (2012) showed that an affirmation intervention at the beginning of an academic year benefited students' grades and belonging more than the same intervention even 4 weeks later.

In previous studies of affirmation in education, affirmations were usually timed to specific evaluative moments known to be stressful to groups of students; for example, students likely to be experiencing stereotype threat before a classroom exam, such as Black and Latinx students or women in science and math domains (Cohen et al., 2009; Steele & Aronson, 1995). Thus, many affirmation studies have found effects that were moderated by demographic characteristics. In principle, however, it should be possible to identify times of high stress in any student's individual life and time affirmations accordingly. Rather than selecting events that are stressful to a particular group of students, identifying such events for each student might extend affirmation's impact to students from groups not targeted by academic stereotypes. However, a key limitation involves delivery: students' idiographic stressors rarely coincide with access to the in-person delivery of a pen-and-paper affirmation. How do we deliver values affirmation to students facing diverse stressors when and where they need it most?

Mobile affirmation

Fortuitously, modern technology may provide just such an opportunity. Globally, mobile phones are becoming more and more ubiquitous—the median ownership of mobile phones for people living in advanced economies is greater than 90%, and nearly 80% for people living in emerging economies (Pew Research Center, 2019). With regard to students specifically, text messaging is the most used function on college students' phones and their preferred means of mobile communication (Skierkowski & Wood, 2012). On average, college students spend more than 90 minutes texting each day (Roberts et al., 2014) and, while awake, usually respond within minutes to an incoming text (Battestini et al., 2010). This everyday use suggests text messaging is a convenient, normal, and effective form of communication.

Delivering affirmation via mobile means is novel within education. However, there have been related efforts in the domain of health, with mixed success. Taber and colleagues (2016) embedded affirmation-informed messages in a text messaging program for people intending to quit smoking. The affirmation texts encouraged people to reflect broadly on their life (e.g. "…focus on your values!…") but did not require responses from participants. Retention in the study was low, with only 6.4% of participants completing outcome measures. However, among those who did, participants who had been affirmed were more likely to report smoking cessation at six weeks post-intervention. Springer and colleagues (2018) examined whether affirmation delivered in a mobile app might increase people's adherence to a healthy eating goal and reduce attrition from app use. In the study, the affirmation activity consisted of two questions per week in which participants reflected on past times in which they had been kind (another common form of affirmation; Reed & Aspinwall, 1998). Affirmation increased adherence to the health goal but did not affect attrition, which was greater than 40%. Finally, in another study, Taber and colleagues (2019) developed an affirmation activity that consisted of sending a link to an external page with a 10-item version of the kindness questionnaire. Despite the researchers' goal to develop a mobile-friendly affirmation exercise, they found that participants spent less than 3 minutes completing the affirmation and wrote few high-quality responses to the questionnaire prompts. Further, with regard to health outcomes, the affirmation backfired: it lowered participants' perceptions of smoking as risky and reduced their intentions to quit. Ultimately, the researchers concluded that their approach was "neither effective nor feasible" and that the work of translating interventions for diverse ''real-world" settings is "necessary but challenging" (Taber et al., 2019, p. 1059).

Two elements stand out about this past research. First, none of the studies used values affirmation as their means of mobile affirmation. In fact, one study explicitly noted that the "extensive nature of the values essay makes it a poor fit for delivery through mobile phones" (Springer et al., 2018). Second, two of the three studies either embedded affirmations into a mobile app (Springer et al., 2018) or provided participants with a link to a web browser page (Taber et al., 2019). Compared with text messages, both of these approaches require participants to take an extra step—opening an app or following a link to an external page—that might dissuade people from engaging with the affirmation.

The present study

Informed by this work, we asked two questions: can we distill a values affirmation into an activity that can be delivered via text message? If so, can this text-based values affirmation help students

cope with the diverse, idiographic academic stressors they face in their everyday lives? The successful implementation of such a method would expand the opportunity for intervening at scale to a large and diverse group of students with the wide-ranging stressors they face in the context of their education. It would also advance theory on self-affirmation by showing just how distilled an affirmation intervention might be.

Thus, in the present research, we distilled the standard 15-minute values affirmation activity (completed via pen-and-paper or online) into a brief (approximately 4-minute) text message activity. Our intervention aimed to maintain the key components of a longer pen-and-paper values affirmation but was conducted via a short exchange of simple text messages. At the beginning of the study, students ranked their values and identified the date and nature of an upcoming educational stressor. Then, the night before their stressor, the research team texted students and asked them to spend a couple minutes reflecting on their most important value. Three minutes later, we asked them to reply with a single sentence about their reflection. In Study 1, we first tested whether college students would have better outcomes if affirmed before a self-identified academic stressor. In Study 2, we then tested whether college students affirmed before starting a summer internship would also have better outcomes. In both studies, we leveraged timeliness (Cohen et al., 2017): by letting students identify their own stressor, and then timing an affirmation intervention to that specific individual's designated stressor, we expected to find benefits across diverse stressors.

Given our interest in heterogeneous stressors, we assessed three outcomes that would be applicable to all identified stressors and that have been core outcomes in numerous previous affirmation studies: performance, stress, and well-being (see Cohen & Sherman, 2014). Self-affirmation theory states that when affirmed, people's psychological threat response is tamped down; in turn, they feel less threatened and more comfortable in their environment. This often yields greater performance and lower stress (e.g., Cohen et al., 2009; Creswell et al., 2005). But it should also foster a general sense of psychological well-being (Emanuel et al., 2018; Howell et al., 2016; Nelson et al., 2014). Because of the diverse nature of students' stressors, we were not able to measure students' performance directly, but instead relied on self-reported performance. For well-being, we focused on life satisfaction (Study 1) and feelings about belonging (Studies 1 and 2).

STUDY 1: PILOT

Our first study asked two basic questions: first, could the typical values affirmation activity be distilled into a brief text message activity? Second, could this text message affirmation buffer students against an idiographic stressor? To this end, in Study 1, we examined whether a text-based affirmation delivered the night before an upcoming academic stressor, which participating students identified themselves, improved their assessment of how well the stressor went and reduced their perception of how stressful it was. We also examined whether affirmation might protect or enhance their sense of well-being—in particular, their life satisfaction and belonging at school. We assessed these outcomes the day after the stressor. In line with previous work (Cook et al., 2012; Sherman et al., 2013), we also examined the phenomenon of decoupling, the tendency for affirmation to untether the link between adversity and well-being (Sherman, 2013). This has been suggested to occur because affirmation, by making the stressor less threatening, makes it less likely to impact well-being. To this end, we assessed whether the relationship between students' perception of the stressor and their sense of well-being was weaker in the affirmation than the control condition.

Method

Participants and design

Fifty-six undergraduate students at a selective private university were recruited from the general university population to participate in a study related to student experiences and smartphones.[1] Sample size was determined by recruiting as many participants as possible during the study window. Five students did not respond to the initial text message and were thus never randomized to condition. Nine students did not complete outcome measures. Survey completion did not differ by condition.

The final sample consisted of 42 students randomized to one of two conditions: affirmation ($n = 20$) or control ($n = 22$). The majority were women (71%), and the rest were men. On average, students were 19.7 years old ($SD = .96$, range: 18–22). Most were second-year college students (62%), with the remainder being third-year (24%) or fourth-year (14%) students. Students could select multiple racial-ethnic identities, and 26% did. Overall, most students identified as White (52%), Asian (36%), and/or Latinx (19%), while fewer identified as Black (7%), Native American (10%), and/or in another way (7%). Students were paid for participation.

Procedure

Baseline survey. Midway through the academic term, students came to a computer lab on campus to complete a baseline survey. In the survey, students identified an upcoming academic-related stressor, provided the date of that stressor, and noted times when they would be available the night before the stressor. For illustrative examples of the stressors that students reported, see Supplemental Online Material (SOM), Table S2.

Next, following typical values affirmation procedures (e.g., Logel & Cohen, 2012), students reviewed a list of values (e.g., relationship with family and friends, creativity) and ranked the values from most important to least important. Finally, they completed baseline measures and provided their cell phone number.

Experimental manipulation. The night before the stressor that students identified in the baseline survey, during a time at which they had said they would be available, the research team manually texted participants using our own phones to deliver the experimental manipulation. This occurred 3 to 12 days after the baseline survey ($M = 6.31$, $SD = 2.47$).

The first text message asked the student if they were available. If the student responded positively, they received two condition-specific messages containing the experimental manipulation. If a student responded negatively, they were asked when they would be available later that evening and texted at that time. If students did not respond, the initial message was resent every 20 minutes for up to 2 hours. After they completed the manipulation (discussed below), students were thanked for their time and told that they would receive the next portion of the study via email.

Post-stressor survey. The day after their stressor (2 days after the text message manipulation), students received an email asking them to complete an online survey. The survey assessed how well students' stressors went, how stressful the stressors were for students, and students' well-being.

Materials

In addition to the primary measures discussed below, we assessed a number of secondary and exploratory measures (see SOM). Study materials are publicly available on the Open Science Framework (https://osf.io/8y34k/).

Baseline measures. At baseline, we measured students' life satisfaction with two items ("I am satisfied with my life" and "In general, my life is very close to my ideal"; scale: 1 = *strongly disagree* to 7 = *strongly agree*; Diener et al., 1985; $\rho = .86$). We also assessed their belonging at college with two items ("I feel like I belong at [university]" and "I feel similar to the kind of people who succeed at [university]"; scale: 1 = *strongly disagree* to 7 = *strongly agree*; Walton & Cohen, 2007; $\rho = .79$). We used these measures as covariates in the corresponding analyses of post-stressor outcomes.

Experimental manipulation. The experimental manipulation was embedded in two condition-specific text messages students received the night before their stressor. In the affirmation condition, the text prompted students to take a few minutes and reflect on the value they had identified in the baseline survey as most important to them and to consider why it was important to them. Then, they were asked to respond with a one-sentence text about why the value is important to them. In the control condition, the text prompted students to take a few minutes and reflect on the value they had identified in the baseline survey as least important to them and why it could be important to someone else. Then, they were asked to respond with a one-sentence text about why the value could be important to someone else. See Table 1 for the text of the prompts students received. In both conditions, the relevant value students had selected at baseline was piped into the second text message.

Dependent measures. Beyond the primary measures discussed below, we assessed a number of secondary and exploratory measures (see SOM).

Evaluation. In the post-stressor survey, students reported how the stressor went on a face-valid, single-item measure (item: "Overall, how well or poorly did the experience go?"; scale: 1 = *very poorly* to 7 = *very well*).

Stress. In the post-stressor survey, students reported how stressful the stressor was (two items: "In general, how stressful was the experience?" and "How overwhelmed did you feel by the experience?"; scale: 1 = *not at all*, 5 = *extremely*; Muraven et al., 2005; $\rho = .85$).

Life satisfaction. In the post-stressor survey, students completed the same two life satisfaction items that they had answered at baseline ($\rho = .81$).

Belonging. In the post-stressor survey, students completed the same two belonging items that they had answered at baseline, as well as a third item ("I feel like people at [university] accept me"; from Walton & Cohen, 2007; $\alpha = .84$).

Analytic approach. Condition was dummy-coded (control = 0, affirmation = 1) and used as the predictor in four separate linear models, one for each of the dependent measures. To increase statistical power and precision (Darlington & Hayes, 2017; Kahan et al., 2014; Turner et al., 2012), we included baseline life satisfaction and belonging in the respective analyses of post-stressor outcomes (see also Armitage, 2016). As expected, baseline life satisfaction was highly predictive of post-stressor life satisfaction, $r = .76$, $p<.001$, and baseline belonging was highly predictive of post-stressor belonging, $r = .67$, $p<.001$.

For consistency throughout the paper, effect sizes are reported in terms of Hedges' *g*. It is a standardized mean difference like Cohen's *d*, but it corrects for bias to yield a more accurate

TABLE 1 Text messages students received the night before their stressor

	Control	Affirmation
Confirming availability	Hi. This is the Student Experiences study you participated in. You listed now as a good time to contact you. This activity will take just 5 minutes of your time. Can you do that right now?	
Experimental manipulation, Text #1	Once a participant responded affirmatively that they were available*:*	
	In the last session of the study, you ranked [value] as the least important of a list of personal values. Please spend the next few minutes, right now, thinking about reasons why this value may be important to someone else. We will text you again in a couple minutes.	In the last session of the study, you ranked [value] as the most important of a list of personal values. Please spend the next few minutes, right now, thinking about reasons why this value is important to you. We will text you again in a couple minutes.
Experimental manipulation, Text #2	*Three minutes after Text #1 was sent:*	
	Now that you've had a few minutes to think, please reply with one sentence about why this value may be important to someone else.	Now that you've had a few minutes to think, please reply with one sentence about why this value is important to you.

Note. Text is underlined to highlight the differences between the control and affirmation experimental materials. Underlining was not present in the messages that students received. Similar messages were used in Study 2, with minor differences in phrasing to account for differences between the two studies.

estimate of the population effect size (Cumming, 2012), especially in small samples (Lakens, 2013). To calculate Hedges' *g*, we used the R package compute.es (Del Re, 2015).

Results

Preliminary analyses

To assess baseline equivalence of the two conditions, we examined whether the conditions differed from each other across 12 baseline and demographic factors. There was one significant difference, such that participants in the affirmation condition reported lower baseline self-integrity than their control counterparts. See SOM.

We assessed intervention fidelity by the percentage of participants who responded to the experimental text messages (88.1%), how many words they wrote in their response ($M = 21.50$; $SD = 9.13$ words), and their self-reported focus on the task ($M = 3.00$ on a 5-point scale; $SD = 1.01$). Across these metrics, fidelity was high and did not differ by condition. See SOM.

For illustrative examples of students' values reflections in response to the affirmation and control text messages, see Table 2.

TABLE 2 Illustrative student responses to the experimental manipulation (sent via text message)

Control	Affirmation
"Someone's ideal career might depend on having athletic ability or being good at sports might be a source of self-esteem for them." (Study 1)	"My family and friends are the reason why I am where I am today, and they'll be the reason why I get wherever I end up going in life; I owe them everything." (Study 1) "Independence is important to me because I think that it is a necessary trait to make substantial contributions to society." (Study 1)
"Music and art are creative outlets for people who like to express themselves." (Study 2)	"[Humor] is important to me because I have a unique sense of humor and appreciate those who share that with me." (Study 2)
"Family and friends can be a good source of advice." (Study 2)	"When you grow up somewhere where everything outside whiteness is devalued, being part of community where difference is celebrated is very liberating" (Study 2)

Note. Minor grammatical corrections have been made to a few responses to improve readability.

Primary analyses

There was no difference between affirmed ($M = 4.85$, $SD = 1.57$) and control students ($M = 4.73$, $SD = 1.75$) in their evaluation of how well the stressor went, $B = .12$ [−.92, 1.16], $SE = .51$, $t = .24$, $p = .81$, $g = .07$ [−.53, .68]. Similarly, there was no difference between affirmed ($M = 2.75$, $SD = .90$) and control students ($M = 2.55$, $SD = .69$) in their report of how stressful their stressor was, $B = .20$ [−.29, .70], $SE = .25$, $t = .83$, $p = .41$, $g = .25$ [−.34, .85].

With regard to well-being, there was no difference in life satisfaction between affirmed ($M = 5.45$, $M_{adj} = 5.36$, $SD = 1.05$) and control students ($M = 5.23$, $M_{adj} = 5.31$, $SD = 1.15$), $B = .06$ [−.40, .52], $SE = .23$, $t = .26$, $p = .80$, $g = .05$ [−.34, .44]. However, there was a significant effect of affirmation on students' belonging. The day after their personal stressor, affirmed students ($M = 5.78$, $M_{adj} = 5.77$, $SD = .59$) reported greater belonging at school than their control counterparts ($M = 5.21$, $M_{adj} = 5.22$, $SD = 1.08$), $B = .55$ [.15, .95], $SE = .20$, $t = 2.79$, $p = .008$, $g = .63$ [.17, 1.09]. See Figure 1.

Decoupling

We also examined whether affirmation decoupled students' evaluation of their stressor from their belonging. Separately by condition, we examined partial correlations between how well the stressor went and post-stressor belonging, controlling for baseline belonging. The partial correlation among control participants was $r = .26$, $p = .25$, while the partial correlation among affirmed participants was $r = -.04$, $p = .88$. We formally tested the difference in these correlations by conducting a regression analysis with condition, how well the stressor went, the interaction of these two variables, and baseline belonging as predictors and post-stressor belonging as the outcome. The interaction was not significant, $B = -.14$ [−.40, .12], $SE = .13$, $t = -1.10$, $p = .28$.

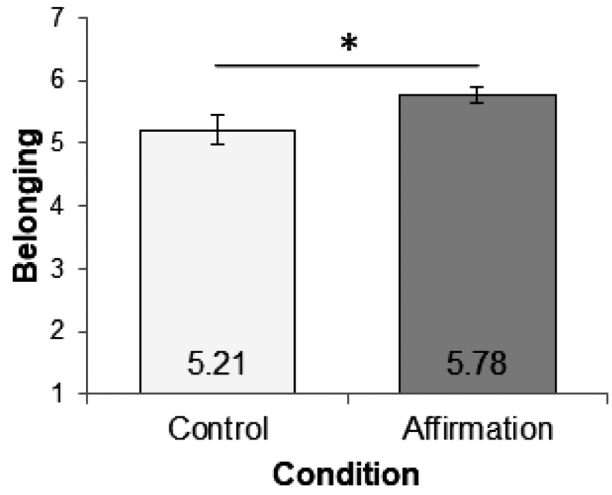

FIGURE 1 Students affirmed via text message before an idiographic stressor report greater belonging
Note: Values are raw; error bars represent +/- 1 SE

Discussion

In Study 1, we distilled the standard 15-minute values affirmation activity into a 4-minute text message activity. Furthermore, we found that this affirmation text message activity, delivered before self-identified academic stressors, improved students' subsequent belonging. However, we found no evidence that it affected their life satisfaction or their evaluations of how well the stressors went or how stressful the stressors were.

It is worth noting explicitly that what students wrote in their responses to the text message prompts (Table 2) was strikingly similar to what students write about in longer values affirmation essays in other values affirmation studies (see Cohen & Sherman, 2014). Combined with the effects on belonging, this suggests that students were able to experience the key psychological components of the values affirmation exercise in a brief text message activity.

STUDY 2

In Study 2, we asked whether text-based affirmation could improve outcomes for students facing another important educational stressor: beginning a summer internship or job. Internships are associated with better subsequent academic performance (Binder et al., 2015) and may improve students' awareness of their work-related abilities and values (Taylor, 1988). Further, experimental and quasi-experimental evidence finds that they improve students' subsequent job prospects and increase their earnings (Margaryan et al., 2020; Nunley et al., 2016; Taylor, 1988), and thus are a potentially valuable part of students' academic training.

The principal differences between Study 1 and Study 2 were the nature of the stressor and the use of an automated text-messaging service to both deliver the text message affirmation and collect outcome measures. We recruited college students who indicated that they had an upcoming summer internship or job (hereafter, "internship") that they expected to be stressful, and delivered the affirmation via text message the night before the first day of their internship. Over the course of the summer, we collected students' contemporaneous assessments of their internship and their uncertainty about their belonging via text message. At the end of the summer, we also collected participants' retrospective assessments of these same outcomes in an online survey.

Methods

Participants and design

We recruited participants by emailing the undergraduate student body at the same university as in Study 1, again recruiting as many students as possible during the recruitment window. Students were invited to complete a brief survey to qualify for a longer study. More than 900 students started this qualification survey, qualifying for the study if they (a) had an upcoming summer internship starting on a Monday, Tuesday, or Wednesday at the end of June, (b) had access to a cell phone and provided a cell phone number, (c) said they had not previously completed the values ranking activity or were "unsure" whether they had done so,[2] (d) did not choose the response "not at all stressful" when asked, "Overall, how stressful do you think your internship will be for you?", and (e) agreed to participate.

The final sample consisted of 121 students who were randomized to one of two conditions: affirmation ($n = 60$) or control ($n = 61$). One student later withdrew from the study and six did not answer any of the dependent measures, leaving an effective sample of 114 students for analysis ($n_{control} = 56$, $n_{affirmation} = 58$), although not all participants completed all measures. The sample was equally split between women (49%) and men (51%) and well-distributed across class years (35% first-year college students, 27% second-year, 26% third-year, 12% fourth-year or beyond). Students were permitted to select multiple racial-ethnic identities, and 18% did. Overall, most students identified as White (54%) and/or Asian (35%), while fewer identified as Latinx (17%), Black (12%), Native American (1%), or in another way (1%). Students were paid for participation.

Procedure

In the qualification survey, participants were asked what their primary summer plans were (e.g., internship, job, travel, other). Students who said that they had an internship or job were asked to report which day it would begin and how stressful they expected their first day to be. They provided demographic information including their phone number, answered baseline questions, and completed the values ranking procedure used in Study 1. Finally, students read a brief description of the study and consented. For illustrative examples of the summer internships students reported, see SOM, Table S1.

Materials

In addition to the primary measures discussed below, we assessed a number of secondary and exploratory measures (see SOM). Study materials are publicly available on the Open Science Framework (https://osf.io/8y34k/).

Qualification survey (baseline). The qualification survey included one question to assess students' concern about fitting in at their internship ("How concerned are you about fitting in at your internship?"; scale: 1 = *not at all concerned*, 5 = *very concerned*). We conceptualized this question as an anticipatory analogue of the belonging uncertainty outcome (discussed below); therefore, it was included as a covariate in analyses with belonging uncertainty as the outcome.

Experimental manipulation. The main difference from Study 1 was that we used a text messaging service called Signal Vine to automate the delivery of our affirmation or control text messages, as opposed to the research team sending them individually from our own mobile phones. Although there were slight changes in the phrasing (e.g., a different study name, asking participants to reply "ready" when they were ready for the activity), the core prompts for the values affirmation and control text messages were the same. Students' internships began on different dates, and the experimental text messages were timed to be delivered the night before each participant's self-identified start date. Students received the experimental text messages 11 to 23 days after having completed the baseline survey ($M = 15.90$ days, $SD = 3.57$).

First few days. Three days after the intervention text messages and thus after the second day of their internship, students were asked to complete a survey about how the internship was going so far. These measures were of secondary interest given that the stressor had just begun (and would continue). They are discussed in greater depth in the SOM. Of note, students in the affirmation condition were more likely to complete this survey than students in the control condition.

Contemporaneous measures. Throughout the summer, we used the automated text messaging service to collect ongoing measures from students about how their internship was going. We sent these text messages every 2 weeks for a total of four times spanning 6 weeks. The first assessment took place at the end of students' first week of their internship. To simplify our measures for text message-based data collection, we asked three questions, each a single-item analogue of one of the dependent measures from Study 1. Students replied to each question with a numeric response which was captured by the text messaging service.

How well the internship was going was assessed with the question, "On a scale from 1 (*not at all well*) to 5 (*extremely well*), how well is your internship going overall?" The stressfulness of students' internship was assessed with the question, "On a scale from 1 (*not at all stressful*) to 5 (*extremely stressful*), how stressful is your internship for you?" Belonging uncertainty was assessed with a single-item measure, "On a scale from 1 (*never*) to 5 (*always*), when you think about your internship how often, if ever, do you wonder: 'Maybe I don't belong here'?" (Yeager et al., 2013).

The vast majority of students completed all four (83%) or three of the four (12%) assessments; only three students did not complete any of the assessments. Given that our interest was in students' contemporaneous responses in general rather than in longitudinal trends, we averaged students' ratings across the summer to create a single measure for each outcome. Reliability of these assessments was adequate (how well the internship was going: $\alpha = .78$, how stressful the internship was: $\alpha = .75$, belonging uncertainty at the internship: $\alpha = .82$).

Retrospective measures. At the end of the summer (6–8 weeks after the last contemporaneous measures were collected), we asked students to complete a final survey. It included retrospective analogues of the measures they completed throughout the summer. All but seven students completed these measures.

How well the internship had gone was assessed with the question, "Overall, how well did your internship go?" (scale: *1 = not at all well*, *5 = extremely well*). The stressfulness of the internship was assessed with the question, "Overall, how stressful was your internship for you?" (scale: *1 = not at all well*, *5 = extremely well*). Belonging uncertainty was assessed with the question, "During your internship, how often, if ever, did you wonder: 'Maybe I don't belong here'?" (scale: *1 = never*, *5 = always*).

Analytic approach

Consistent with the analytic approach for Study 1, condition was dummy-coded (control = 0, affirmation = 1) and used as the predictor in separate linear models, one for each of the dependent measures. To increase statistical power and precision (Darlington & Hayes, 2017; Kahan et al., 2014; Turner et al., 2012), we included baseline concern about fitting in as a covariate in analyses with belonging uncertainty as the outcome (as discussed above, we considered this a measure of *anticipated* belonging uncertainty). As expected, baseline concern about fitting in was quite predictive of belonging uncertainty at students' internship both contemporaneously, $r = .49$, $p < .001$, and retrospectively, $r = .42$, $p < .001$.

Results

Preliminary analyses

To assess baseline equivalence of the two conditions, we examined whether participants in the conditions significantly differed from each other across nineteen baseline and demographic factors. We found no significant differences. See SOM.

We assessed intervention fidelity by the percentage of participants who responded to the experimental text messages (93.9%), how many words they wrote in their response ($M = 21.58$; $SD = 12.00$), and their self-reported focus on the task ($M = 3.23$ on a 5-point scale; $SD = .91$). Across these metrics, fidelity was high and did not differ significantly by condition. See SOM.

For illustrative examples of students' values reflections in response to the affirmation and control text messages, see Table 2.

Primary analyses

Contemporaneous outcomes. Mirroring the results from Study 1, students in the affirmation condition ($M = 3.98$, $SD = .61$) did not differ significantly from those in the control condition ($M = 4.08$, $SD = .66$) in their assessment of how well their internship was going, $B = -.10$ [−.34, .14], $SE = .12$, $t = -.80$, $p = .43$, $g = .15$ [−.22, .52].

However, in contrast to Study 1, affirmation did affect students' stress over the summer. Affirmed students ($M = 2.39$, $SD = .62$) reported that their internship was significantly less stressful for them than control students did ($M = 2.72$, $SD = .79$), $B = -.33$ [−.60, −.07], $SE = .13$, $t = -2.48$, $p = .01$, $g = .47$ [.09, .84]. See Figure 2, Left Panel.

Affirmation also affected students' belonging uncertainty. Affirmed students ($M = 1.94$, $M_{adj} = 1.94$, $SD = .76$) reported questioning their belonging at their internship less often than their control counterparts ($M = 2.23$, $M_{adj} = 2.23$, $SD = .96$), $B = -.29$ [−.58, −.01], $SE = .14$, $t = -2.02$, $p = .05$, $g = .34$ [.01, .66]. See Figure 2, Right Panel.

Retrospective outcomes. Consistent with the contemporaneous findings, students in the affirmation condition ($M = 4.07$, $SD = .84$) did not differ significantly from students in the control condition ($M = 4.29$, $SD = .78$) in their retrospective assessment of how well their internship had gone, $B = -.22$ [−.53, .09], $SE = .16$, $t = -1.38$, $p = .17$, $g = .27$ [−.11, .65].

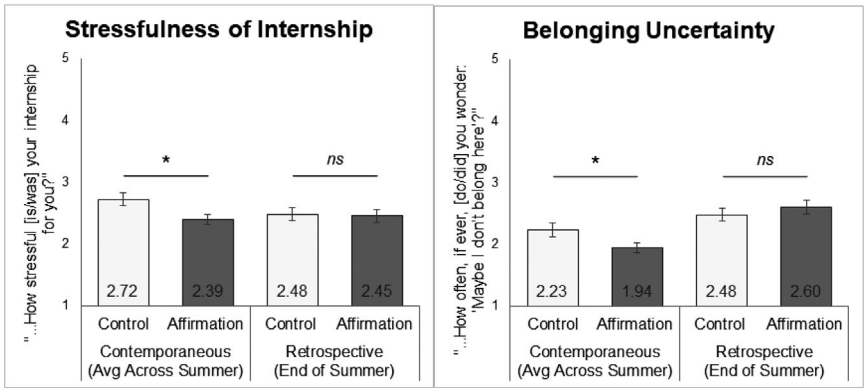

FIGURE 2 Students affirmed via text message report lower stress and belonging uncertainty during, but not retrospectively after, a summer internship

Note: Values are raw; error bars represent +/- 1 SE

Contrary to the contemporaneous findings, however, students in the affirmation condition ($M = 2.45$, $SD = .79$) did not differ significantly from students in the control condition ($M = 2.48$, $SD = .80$) in their retrospective assessment of how stressful their internship had been, $B = -.03$ [−.33, .28], $SE = .15$, $t = -.17$, $p = .87$, $g = .03$ [−.34, .41]. See Figure 2, Left Panel.

Also contrary to the contemporaneous findings, students in the affirmation condition ($M = 2.60$, $M_{adj} = 2.58$, $SD = .87$) were non-significantly but directionally *higher* in their retrospective assessment of their belonging uncertainty at their internship compared with their control counterparts ($M = 2.48$, $M_{adj} = 2.50$, $SD = 1.02$), $B = .08$ [−.25, .42], $SE = .17$, $t = .51$, $p = .61$, $g = .09$ [−.25, .43]. See Figure 2, Right Panel.

Why these discrepancies? Exploratory analyses suggested that the discrepancies may have been due to affirmed students engaging less in defensive biases and giving more "calibrated" responses at the end of the summer about what their experience had been like over the course of the summer. In contrast, control participants appear to have been looking back at their internship through "rose-colored glasses"—misreporting the stress and belonging uncertainty that they said they experienced. For example, affirmed students reported similar stress levels contemporaneously (during the internship) and retrospectively (looking back, at the end of the summer), while control students reported higher levels of stress contemporaneously than retrospectively (for further discussion of these analyses, see SOM, p. 15).

Decoupling

We again examined whether affirmation decoupled students' evaluation of their stressor from their belonging uncertainty. Separately by condition, we examined partial correlations between students' contemporaneous evaluations of how well their internship was going and their contemporaneous belonging uncertainty at the internship, controlling for baseline concern about fitting in. In the control condition, students' perceptions of how well the internship was going were quite correlated with their uncertainty about belonging at their internship, $r = -.64$, $p<.001$. In the affirmation condition, this relationship was weaker but still significant, $r = -.42$, $p = .001$. We formally tested the difference in these correlations by conducting a regression analysis with

condition, contemporaneous evaluations, the interaction of these two variables, and baseline concern about fitting in as predictors and contemporaneous belonging uncertainty as the outcome. The interaction was significant, $B = .71$ [.50, .92], $SE = .10$, $t = -6.81$, $p < .001$.

Discussion

In Study 2, we conceptually replicated the effect of affirmation on belonging from Study 1, and broadened the findings to another stressful educational situation that students face: starting a summer internship. Over the course of the summer, affirmed students reported lower stress and less worry about their belonging than their unaffirmed peers throughout the summer. In contrast to Study 1, we also found support for a decoupling effect. Affirmation significantly reduced the contingency between students' evaluations of how the internship was going and their belonging uncertainty. The effects in this context are an interesting application in education, given that internships are an educational experience outside of the classroom, where more standard classroom interventions or policies to address stress (e.g., at the instructor or curricular level) are less common or likely.

Interestingly, although we found condition differences in contemporaneous measures of stress and belonging uncertainty, we found no such differences in retrospective reports at the end of the summer. In exploratory analyses, we found suggestive evidence that affirmation students may have been less defensive and more "calibrated" in their retrospective evaluations, while control students appeared to remember their experiences more favorably than their contemporaneous evaluations would suggest.

GENERAL DISCUSSION

We capitalized on the ubiquity of mobile phones and text messaging to make two contributions to affirmation research. First, the present studies are the first to use values affirmation as a means of mobile affirmation, and the first to use mobile affirmation in education. We distilled the standard values affirmation intervention into a 4-minute text message activity, which led to improvements in students' belonging in the face of academic stressors. Though student responses to the activity were brief, they echoed many of the same core themes as longer values affirmation essays written by students in other studies (e.g., see Cook et al., 2012; Riddle et al., 2015; Tibbetts et al., 2016). Second, we leveraged mobile technology to time interventions to a broad range of idiographic academic stressors—from staging a play to submitting a paper to working an internship. To our knowledge, all other affirmation intervention studies examining academic stressors have focused on uniform stressors—in many cases, the same exact exam (e.g., Cohen et al., 2009; Harackiewicz et al., 2014), or in the most varied case, students' most stressful midterm exam (Sherman et al., 2009). When focusing on idiographic stressors, benefits of affirmation were broadly shared, not concentrated among students experiencing threat due to negative stereotypes about their group. (For moderation analyses that report null effects, see SOM.)

Though some other studies have found null or even negative effects of affirmations delivered via mobile technology (Springer et al., 2018; Taber et al., 2019), the promising results of our text message affirmation activity suggest that the medium of mobile interventions might matter. Some previous studies have embedded intervention activities in mobile apps, which offer a number of compelling features—for example, intricate interfaces and location data. However, the

comparative simplicity and directness of text messaging may be an asset: no need to download or open an app. As such, text messages may minimize barriers to receiving and completing the affirmation activity, and thus may be more likely to be seen and attended to. Text messages may also feel like an authentic means of communication, especially among high school and college students. This may have facilitated students' engagement with the activities. (The high fidelity of participants' completion of our text message exercise is consistent with this possibility.) Thus, text messages may be an authentic and engaging means for delivery of affirmations "on the go." Furthermore, while the standard pen-and-paper values affirmation is compatible with most classroom contexts, it may be less appropriate before other life stressors, like a first date or a concerning doctor's appointment. In such situations, text message affirmation may be both easier to deliver and a better fit with the context.

The present research is initial evidence for a delivery mechanism with the potential for great scalability. The proof-of-concept studies here suggest that the core of the values affirmation activity can effectively be distilled into a text message format, and that the medium of text messaging might remove the barriers to scalable implementation involved in the standard writing activity. When we refer to the scalability of interventions, we suggest that to scale is not simply to apply interventions en masse, but to allocate in ways that ensure that they get *to* the people who need it *when* they need it.

Our findings add to the emerging literature on affirmation and well-being (Brady et al., 2016; Howell et al., 2016; Nelson et al., 2014). We found that affirmation improved college students' belonging, which is considered a key index of well-being (Diener et al., 2010; Graham et al., 2016), in two different educational settings. Further, one of our studies showed evidence of a decoupling effect between perceptions of the stressors and belonging, suggesting that affirmation may help bolster belonging in the face of academic stressors. Though much work has focused on student performance, today's mental health crisis on college campuses highlights a need to consider students' emotional and social well-being as an integral part of their college experience. We encourage future affirmation research to take seriously the potential for benefits to well-being and map not only general effects, as we did here, but also more nuanced differences—for example, potentially teasing apart effects on hedonic well-being versus eudaimonic well-being versus belonging.

It is worth considering the null and inconsistent effects we observed. Although we found effects on belonging outcomes throughout, we did not find effects on how well the stressor went, nor consistent effects on stress or the decoupling effect. This is surprising, as the past literature on affirmation in education has focused primarily on its potential to benefit these outcomes (Cohen & Sherman, 2014; Easterbrook et al., 2021; Harackiewicz & Priniski, 2018; Sherman et al., 2009; Sherman, 2013). With regard to performance, the null effect may be due to the self-report nature of our outcomes. In past research, performance has typically been assessed with grades, but there was no single objective performance metric to assess the diversity of stressors our student sample identified. Perhaps there were, in actuality, effects on students' performance and stress, but the self-report nature of the measures obscured our ability to detect them, due to defensive biases (Sherman & Cohen, 2006). In any case, future research might profitably examine self-identified stressors where performance can be objectively measured. Relatedly, the retrospective measures of stress may have been problematic. Retrospective accounts are particularly prone to error (Kahneman & Riis, 2005). Consistent with this account, we found effects of the affirmation on stress only in Study 2 when it was measured contemporaneously; there were no effects of the affirmation when stress was measured retrospectively, including in Study 2. This highlights an important issue to consider in future work: considering not only *what* variables to measure and *how*, but also *when*.

Importantly, the effects we found were main effects of affirmation, rather than effects conditional on an underrepresented or negatively stereotyped identity as seen in many previous studies (Cohen et al., 2009; Harackiewicz et al., 2014). By design, we focused on times each student identified as likely to be stressful for them—and found general benefits, as in other work that examined personally relevant stressors (e.g., Sherman et al., 2009). These findings underscore that affirmation is not a technique to help *students from minoritized backgrounds* cope, but rather one to help *students experiencing stress* cope.

Lessons learned

While the present work offers promising preliminary results, we hope that future work will expand and improve upon this initial research. In that light, we offer a few "lessons learned." First, we quickly learned that effective scalability of this model required an automated text messaging service that could both send messages and reply to students' responses. It simply was not feasible for the research team to manually text all the participants in a study. The service we used, Signal Vine, allowed us to create scripts that automated the timed delivery of appropriate condition-specific messages with the correct value for each participant, and was able to respond in real-time as students completed the activity. Second, we benefited from streamlining components of the process as much as possible. We collected phone numbers, their values ranking, and the timing of their stressor all in the same survey in both studies, and in Study 2 were able to use the same text messaging service that delivered the intervention to collect outcome measures in situ. Third, we carefully considered other elements of the study design that might enhance (or neutralize) the benefits of our very brief affirmation. For example, in Study 2, this included limiting our sample to participants' whose internships began on Mondays, Tuesdays, or Wednesdays, so that students' completion of the experimental messages would occur on a Sunday or weeknight (a time we thought would lend itself to greater focus) and so that any recursive processes initiated might have time to "take hold" before a weekend. Finally, we strove to give students as much agency and control over the intervention as possible. We asked them in the initial survey to identify a time they would be free for a 5-minute text message activity, and then confirmed that the time was still convenient for them when we messaged, with an option to reschedule for later in the evening if they wanted to. This was intended both to decrease any inconvenience, as well as to maximize the likelihood that the students would genuinely engage with the activity.

Limitations and future directions

One clear limitation of the current research concerns the samples, both in sizes and demographic characteristics. Individually, the studies were underpowered, and all participants were from the same selective private university. The present findings need to be generalized to larger and more diverse samples and to varied contexts. Furthermore, our study focused specifically on the effect of mobile affirmation for students facing academic stressors. However, like everyone else, students also face non-academic stressors, ranging from going on a first date to taking a concerning trip to the doctor's office. Future research might evaluate the potential of mobile affirmations to improve outcomes related to other day-to-day stressors in people's lives, particularly in cases where the text message format is more compatible with people's regular daily interactions.

Another limitation, shared by many affirmation studies, is the absence of a no-task control; this allows for the possibility that the control condition was not truly neutral but actually undermined students' outcomes. Although this is possible, we think it is unlikely for several reasons. First, we took great care to give students a sense of predictability and control over the text messaging activity (e.g., allowing them to choose the time most convenient for them, asking if they were free for the activity when we texted), as a robust literature finds that these factors make tasks easier and less stressful (Carton & Aiello, 2009; Glass & Singer, 1972; Langer, 1983; Mineka & Hendersen, 1985). Second, we computed pre-post difference scores for belonging in Study 1 (post-stressor score minus baseline score, using only the two items included at both timepoints). Among students in the control condition, the difference score was not significantly different from zero ($M = -.20$, $SD = .81$, *one-sample* $t = -1.18$, $p = .25$), indicating no reliable decrease in students' belonging from before the stressor to after the stressor. (For affirmed students, the difference score was significantly positive; $M = .33$, $SD = .44$, *one-sample* $t = 3.32$, $p = .004$). Third, the few affirmation studies to include no-task control conditions have not observed negative effects of the standard control condition used here (see Cohen et al., 2006; Logel et al., 2019).

Conclusion

Our research offers some optimism for efforts to embed psychological interventions in people's everyday lives and to scale them. Our findings suggest that at least some of the benefits of values affirmation interventions can be achieved in very brief moments of reflection when embedded at the right time and place in students' lives. In particular, this research offers initial evidence for a novel, promising, and scalable method of delivering values affirmations. Scaling a psychological intervention is not merely about making it bigger or available to more people, but about offering it to people in an impactful way, when they need it.

As the proliferation of work in this area attests (Cohen & Sherman, 2014; Harackiewicz & Priniski, 2018; Smith, et al., 2021), there is great interest in how to develop, refine, and embed values affirmations in students' experiences in ways that help them manifest their potential and feel positive and satisfied about their lives. This research serves as a proof-of-concept study demonstrating the potential to affirm students ''on the go,'' in personally important stressful moments. As educators, administrators, and policy makers consider the implications of affirmation interventions and social psychological interventions more generally, it is important to re-emphasize the situational nature of these effects. This work highlights the role of educational experiences and contexts in shaping outcomes. Indeed, creating interactions and environments which provide students with the support and resources they need—material, social, and psychological—will help students thrive and succeed in their education and lives.

Notes

[1] In semesters prior to the study, students were recruited via fliers and emails to complete a prescreen survey for research studies in our lab. We invited everyone who had completed the prescreen to participate in this study.

[2] Due to an error, one participant who said they had previously completed the values ranking procedure was invited to participate in the study and did so. Results are similar whether the participant is included or not; therefore, we retain the participant in the analyses presented here.

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CONFLICT OF INTEREST

The authors have no known conflicts of interest to disclose.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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