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What triggers prosocial effort? A positive feedback loop between positive activities, kindness, and well-being

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ABSTRACT

Across two studies, we found evidence supporting a positive feedback loop between positive activities, kindness and well-being. In Study 1, participants were randomly assigned to one of four distinct positive activities (versus a neutral writing activity) before spending three weeks engaging in kind acts. We found that the positive activities served as triggers – that is, they predicted greater prosocial effort, which in turn predicted greater well-being immediately following the intervention and at a two-week follow-up. In Study 2, we explored the specific effects of a gratitude trigger, and extended the intervention period to six weeks. Although, we did not replicate the direct effect of the gratitude trigger on prosocial effort, people who wrote gratitude letters (versus writing about their week) reported relatively greater elevation, which predicted greater prosocial effort during the six weeks. In turn, replicating Study 1, greater effort predicted higher well-being immediately following the study.

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Well-being; happiness;
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Remember there's no such thing as a small act of kindness. Every act creates a ripple with no logical end. (Scott Adams)

What moves people to put effort into helping others? For one, happy people are more prosocial than their less happy peers (Aknin, Dunn, & Norton, 2012; Krueger, Hicks, & McGue, 2001). Multiple experiments have explored the causal direction of this link and found that, not only does experimentally induced positive emotion lead to increased helping behavior (e.g. Carlson, Charlin, & Miller, 1988), but also that performing kind acts leads to greater happiness (e.g. Dunn, Aknin, & Norton, 2008; Lyubomirsky, Sheldon, & Schkade, 2005). Thus, as one paper aptly noted, it seems that 'happiness runs in a circular motion' with prosociality – in other words, one promotes the other in a positive feedback loop (Aknin et al., 2012). Building on previous work, across two studies, we use experimental methodology and a longer time course to explore how a reciprocal process between positive emotion and prosociality may develop in a naturalistic setting. Furthermore, in Study 2, we go beyond testing the general effect of positivity on helping behavior and focus on one specific emotional state that may be especially motivating – namely, elevation.

Positive emotion drives prosocial behavior

When people feel good, they do good. Although extensive evidence now supports this truism, the vast majority comes from short-term studies in highly controlled laboratory settings (e.g. Aknin et al., 2012; Carlson et al., 1988). Indeed, in most such studies, the opportunity to perform prosocial behavior is prescribed by an experimenter in a lab (e.g. participants can assist the experimenter with a task or help a confederate pick up papers) and arise immediately following a brief positive emotion manipulation (e.g. finding a dime, hearing [false] success feedback). In the current studies, we explore whether a particular kind of positive emotion elicitation – a positive activity that we will call a positive trigger – could prompt greater prosocial effort in self-chosen acts of kindness in our participants' daily lives during the weeks following the trigger.

We have multiple reasons to expect that positive emotions will stimulate prosocial effort over a longer, more naturalistic time course. First, positive emotions facilitate effort toward the task at hand, whatever the task might be. For example, people who received a positive emotion manipulation (versus no manipulation) before engaging in an anagram-solving task successfully completed more

anagrams, persisted at the task longer, tried more combinations for all anagrams (including unsolvable ones) and reported more motivation toward the task (Erez & Isen, 2002; see also Kavanagh, 1987). Similarly, in a health context, individuals induced into a positive mood reported higher self-efficacy in managing a hypothetical illness, as well as greater intention to put effort into fighting the illness, than those induced into a negative mood (Schuettler & Kiviniemi, 2006). Thus, positive emotions appear to signal to people that they are capable of mustering effort into challenging tasks, and that their effort will pay off. Conceivably, if positive emotions can stimulate effort toward mundane tasks like anagram-solving or stressful tasks like managing illness, they could also stimulate effort toward prosocial behavior. Furthermore, positive emotions that are other-praising, such as elevation, may be especially likely to stimulate kind acts toward others (Algoe & Haidt, 2009).

Second, theory suggests that the maintenance of any behavior depends on people's satisfaction with the actual outcomes (e.g. how well they think their kind act was received; Rothman, 2000). People in a positive state are relatively more likely to construe the events in their lives optimistically (Dickerhoof, 2007; Lyubomirsky, 2001), thus helping them interpret the outcomes of their behavior in a positive light. For example, after a week of making thoughtful gestures for their coworkers, those in a positive state may be more likely to focus on the grateful responses rather than the confused or apathetic responses. Consequently, those in a positive state might evaluate the outcomes of performing kind acts optimistically and maintain their efforts. Furthermore, as discussed earlier, people typically receive a boost in happiness after performing kind acts (e.g. Dunn et al., 2008; Lyubomirsky et al., 2005), which may in and of itself reinforce the kind behavior in a positive feedback loop.

Third, and perhaps most important, even a momentary surge of positive emotion can promote durable relational outcomes by stimulating the urge to play, explore and act (Fredrickson, 1998, 2013). For example, when trying to engage in kind acts, someone in a receptive, energized state might feel comfortable giving a compliment to a stranger who then becomes a friend or might think of a creative way to surprise his or her romantic partner, thereby strengthening the relationship. Thus, even transient positive emotions can help individuals take specific actions in their lives that stimulate durable relationship resources, which can feed back into even more prosocial behavior as they seek to help and support those closest to them. Importantly, such positive chains of everyday life events are not likely to be set in motion in a controlled laboratory setting.

Positive activities increase positive affect

Multiple randomized controlled studies have shown that simple, brief and self-administered positive activities boost positive emotion (Lyubomirsky & Layous, 2013; see also Bolger et al., 2013; Sin & Lyubomirsky, 2009). In one study, college students who were asked to write optimistically about their 'best possible future self' increased in positive affect more than students who wrote about what they did during the past week (Layous, Nelson, & Lyubomirsky, 2013). In another study, people who wrote about their most 'intensely positive experience' reported greater positive affect than people who wrote about neutral topics such as the layout of their bedroom (Burton & King, 2004). And finally, individuals who expressed gratitude showed greater boosts in positive affect than those who did not (Emmons & McCullough, 2003; Lyubomirsky, Dickerhoof, Boehm, & Sheldon, 2011). Thus, empirical evidence suggests that positive activities reliably increase positive affect. In the current studies, we strategically place one positive activity (e.g. expressing gratitude) before instructing people to perform kind acts (another positive activity) to test whether such placement increases prosocial effort. Because of this strategic positioning, we call the first positive activity a positive trigger. We propose that this positive trigger will render the participants particularly receptive to the subsequent instructions to perform kind acts – ready and even eager to put effort into prosociality.

We believe using positive activities to elicit positive emotion (as opposed to laboratory prompts) significantly extends the helping literature. Past experiments often relied on positive emotion elicitation that are somewhat superficial and hard to replicate in daily life, like finding a dime in a pay phone (e.g. Cunningham, Steinberg, & Grev, 1980; Isen & Levin, 1972) or receiving a gift of stationery (Isen, Clark, & Schwartz, 1976). In contrast, positive activities are usually self-initiated and comprise repeated thoughts or actions (e.g. self-reflection on self and close others) that are likely to provide longer lasting or self-reinforcing positive emotions, as well as stronger motivation to help others.

More prosocial effort predicts greater happiness

Although the main focus of our research is exploring the processes by which positive triggers prompt prosocial effort, we also anticipate that relatively greater prosocial effort will be associated with positive downstream consequences for the giver. In past experiments, performing kind acts has been shown to increase personal happiness (Dunn et al., 2008; Layous, Lee, Choi, & Lyubomirsky, 2013; Lyubomirsky et al., 2005; Nelson, Layous, Cole, &

Lyubomirsky, *in press*; Nelson et al., 2015; Sheldon, Boehm, & Lyubomirsky, 2012) relative to control tasks. We propose that an initial positive activity (such as one designed to generate gratitude, optimism or joy) will stimulate prosocial effort, which then will feed back into personal well-being. In turn, this increase in happiness is expected to promote even more frequent positive emotions, which can continue to fuel ongoing prosocial behavior, creating a positive feedback loop or upward spiral (see also Aknin et al., 2012). Indeed, recent theory suggests that a single, well-timed psychological intervention can trigger a cascade of positive and durable outcomes by creating one positive event upon which others can build through recursive processes (Walton, 2014).

The current studies

In two studies, we explored whether and how brief positive activities (the triggers) motivate prosocial effort. In Study 1, we tested the effect of engaging in one of four positive writing activities (versus a neutral one) prior to doing acts of kindness over the course of 3 weeks. Specifically, participants were randomly assigned to write a general gratitude letter to anyone they wish (general gratitude condition), write a specific gratitude letter toward a person for a particular kind act (specific gratitude condition), write about an intensely positive experience (joy condition; Burton & King, 2004), write about their best possible self (optimism condition; King, 2001) or list what they did over the past 7 days (control condition).¹ Throughout the three weeks, all participants, regardless of condition, were prompted (by online instructions) to perform kind acts (for whomever they want) and then to report their kind acts anonymously the following week on the study website. We predicted that participants who engaged in any of the positive activities will perform more effortful kind acts (self-reported) than participants who engaged in the control task (Hypothesis 1). We also expected that participants who engaged in relatively more effortful kind acts will show relatively larger increases in happiness (Hypothesis 2).

In our second study, we aimed to replicate the general findings from Study 1, as well as test the specific mechanisms by which one trigger of interest to many researchers – expressing gratitude – elicits effort toward prosocial behavior.

Study 1: The effect of four positive triggers on prosocial effort

Method

Participants

Two-hundred and thirty-three participants (69.6% female; $M_{AGE} = 20.02$, $SD = 2.92$) from the University of California,

Riverside were granted course credit in exchange for participation. The plurality of participants identified as Asian (40.1%) or Hispanic/Latino(a) (29.3%), with the rest identifying as White (13.8%), 'more than one' (6.5%), 'other' (5.2%), Black/African American (3.9%), Hawaiian/Pacific Islander (0.9%) or American Indian/Alaskan Native (0.4%).²

Design and procedure

The present study took place entirely over the Internet, using a website available only to registered participants. The study consisted of a three-week intervention period and a follow-up assessment two weeks later, for a total duration of five weeks. Upon logging in to the study website for the first time, participants were randomly assigned to one of 5 possible conditions that varied only with respect to the 8-min writing activity performed at baseline: (1) writing a general gratitude letter ($n = 45$), (2) writing a specific gratitude letter ($n = 47$), (3) writing about one's best possible future self ($n = 48$), (4) writing about an intensely positive experience ($n = 47$) and (5) writing about what one did last week ($n = 46$).³ To minimize potential demand effects, all participants were told that they would engage in positive practices.

After the writing activity at baseline, all participants were asked to perform several acts of kindness during one day each week over 3 weeks (Lyubomirsky et al., 2005; see top of Figure 1 for Study 1 timeline). Each Monday, they logged in to the study website to complete measures and report on their kind acts from the previous week. The first assessment (T_1) contained a consent form, demographics and measures of the outcome variables (i.e. life satisfaction, positive emotions, and negative emotions). Right after completing these questionnaires, students performed their initial assigned writing exercise. Next, all participants received instructions to perform acts of kindness during the week.

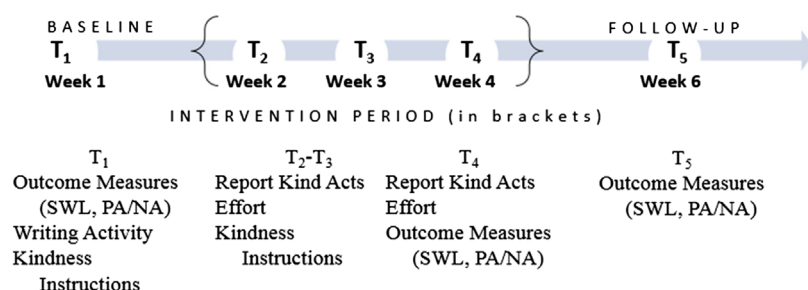
At the second (T_2) and third (T_3) assessments, participants described the acts of kindness they performed upon logging in to the website, as well as the effort they expended on performing kind acts during the past week and the positive and negative emotions they experienced throughout the week. Next, participants were again asked to complete acts of kindness during the week. At post-test (T_4), participants reported their acts of kindness for the last time and completed the effort and outcome measures. Two weeks later (T_5), participants again completed the outcome measures to assess the durability of changes.

Experimental manipulations

Participants were exposed to one of five experimental conditions at baseline.

General gratitude. In this condition, students were asked to spend approximately 8 min remembering and

Study 1



Study 2

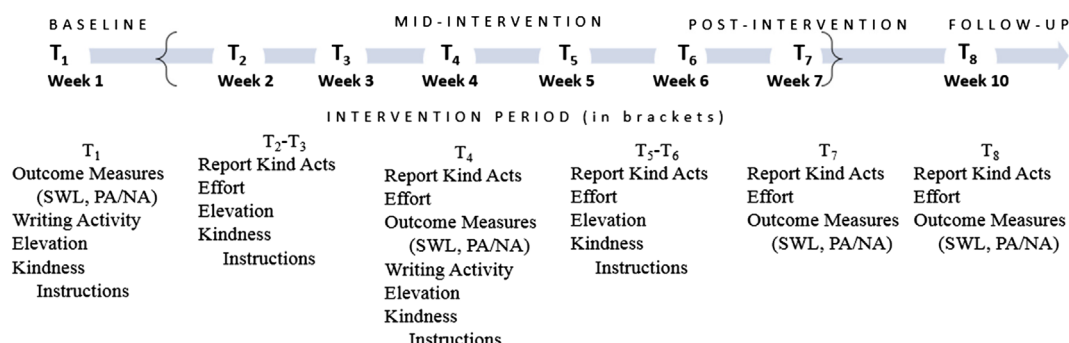


Figure 1. Study 1 (Top) and Study 2 (bottom) timeline. Activities and measures are listed in order of completion.

writing about a person to whom they are extremely grateful (e.g. teacher, parent and friend; see Lyubomirsky et al., 2011). Participants were told to write as if they are directly thanking that person.

Specific gratitude. Similar to the general gratitude condition, students in this condition were asked to spend approximately 8 min remembering and writing about their gratitude. However, instead of writing generally about a person, participants in this condition were asked to focus on a specific kind act bestowed upon them (e.g. grateful to a sibling for caring for them when they were sick).

Optimism. Borrowing from King's (2001) 'best possible selves' paradigm, participants in this condition, were asked to spend 8 min writing about an imagined ideal future self (see Boehm, Lyubomirsky, & Sheldon, 2011).

Joy. In this condition, participants spent 8 min writing about an 'intensely positive experience' from their past modeled after the concept of peak experiences (see Burton & King, 2004).

Control. In the comparison condition, participants spent 8 min listing what they did over the past 7 days (Lyubomirsky et al., 2011). To maintain the cover story that all activities (including the control) should increase happiness levels, this condition was described as an organization task.

Acts of kindness

Regardless of condition, participants were instructed to perform acts of kindness for others each week. They were instructed to do as many acts of kindness as they wanted, but to make sure to do them all during one day of the week (Lyubomirsky et al., 2005). When participants logged on to the website to complete the effort measures each week ($T_2 - T_4$), they were first asked to list all the acts of kindness they completed during the previous week.

Materials

Consent and demographic information. When participants logged in to the study website for the first time, they read a consent form that informed them of their rights as a participant. After consenting to participate, they were asked to provide general demographic information such as their sex, age and ethnicity.

Life satisfaction. The Satisfaction With Life Scale (SWLS; Diener, Emmons, Larsen, & Griffin, 1985) assesses respondents' current satisfaction with their life in general. The SWLS consists of five questions (e.g. 'In most ways my life is close to my ideal', 'I am satisfied with my life'), which are rated on seven-point Likert-type scales (1 = *strongly disagree*, 7 = *strongly agree*). Thus, higher scores on this measure indicate greater life satisfaction. Validation studies have shown that the SWLS comprises a single factor and possesses high internal consistency ($\alpha = 0.87$) and high test-retest reliability ($r = 0.82$; Diener et al., 1985). In the current study, satisfaction with life was assessed at T_1 (Cronbach's $\alpha = 0.86$), T_4 (Cronbach's $\alpha = 0.90$), and T_5 (Cronbach's $\alpha = 0.90$).

Affect. Positive and negative emotions were assessed using Diener and Emmons (1985) Affect-Adjective Scale. This measure taps a range of positive (i.e. happy, pleased, joyful, enjoyment/fun) and negative (i.e. frustrated, depressed/blue, angry/hostile, worried/anxious) emotions. Participants rated the extent to which they have experienced each of these emotions in the past week on a seven-point Likert scale (0 = *not at all*, 1 = *slightly*, 2 = *somewhat*, 3 = *moderately*, 4 = *much*, 5 = *very much*, 6 = *extremely much*). Positive and negative emotions were assessed at T_1 through T_5 and averaged separately at each time point (Cronbach's α for positive emotions across time points > 0.87 ; Cronbach's α for negative emotions across time points > 0.84).

Well-being composite. Because well-being is typically conceived of as having both a cognitive component (e.g. life satisfaction) and an affective component (e.g. frequent positive emotions and infrequent negative emotions; Diener, Suh, Lucas, & Smith, 1999), we averaged participants' scores on life satisfaction and positive and negative (reverse-scored) emotions to form a well-being composite at T_1 (baseline), T_4 (post-intervention) and T_5 (follow-up). All three components were already on seven-point scales, but we adjusted the life satisfaction scale to 0–6 instead of 1–7 to match the affect component before averaging. This well-being composite was used as the main outcome variable in all analyses of indirect effects (Cronbach's α for composites across time points > 0.74).

Self-reported effort. At T_2 , T_3 and T_4 , participants indicated the amount of effort they put into performing kind acts during the previous week by responding to two questions, each on nine-point scales: 'How much effort did you put into performing last week's acts of kindness?' (1 = *no effort at all*, 9 = *a great deal of effort*) and 'How hard did you try when performing last week's acts of kindness' (1 = *not hard at all*, 9 = *extremely hard*).

The two effort items were averaged at each time point (Cronbach's $\alpha > 0.77$ across time points) and then we averaged across the three intervention weeks (T_2 through T_4 ; Cronbach's $\alpha = 0.83$) to create a measure of the average effort a participant expended across the intervention. We included all participants who completed the self-reported effort questions on at least two out of the three possible weeks.

Results and discussion

Baseline analyses

Analyses of baseline well-being revealed no significant differences by condition, $F(4, 228) = 0.33$, $p = 0.86$, sex, $t(230) = -0.65$, $p = 0.52$ (males dummy-coded as '1'), or ethnicity, $F(7, 224) = 0.95$, $p = 0.47$. Participants who failed to complete the post-intervention time point (T_4 ; $n = 62$) did not show significant differences in well-being at baseline from those who did complete it, $t(231) = -0.79$, $p = 0.43$. However, participants who failed to complete the follow-up time point (T_5 ; $n = 78$) reported lower well-being at baseline, $t(231) = -2.04$, $p = 0.04$, than those who did. As mentioned before, this attrition was evenly spread across conditions.

Effort

Importantly, our four positive trigger conditions did not prompt different levels of effort, $F(3, 154) = 0.41$, $p = 0.75$, so all subsequent analyses collapse across positive triggers. Supporting our first hypothesis, a contrast analysis comparing all four trigger conditions (+1) to the control condition (−4) revealed that people who engaged in positive activities before performing kind acts reported more effort towards those acts than those who engaged in the control activity, $t(192) = 2.08$, $p = 0.04$, $r = 0.15$.⁴

Analyses of indirect effects

We also examined whether greater effort toward kind acts predicted larger increases in well-being over time (Hypothesis 2). Although we found no direct effect of the positive activities (versus control) on post-intervention or follow-up well-being (see below for discussion of this finding), we anticipated an indirect effect of the positive triggers on well-being via effort. Although old practices specified that a direct effect needs to be present to test for mediation, the new convention is that exploring indirect effects in the absence of a direct effect is acceptable (see Preacher & Hayes, 2004; Shrout & Bolger, 2002; for support for this approach). Importantly, because we did not find a direct effect of the positive trigger on well-being, we can

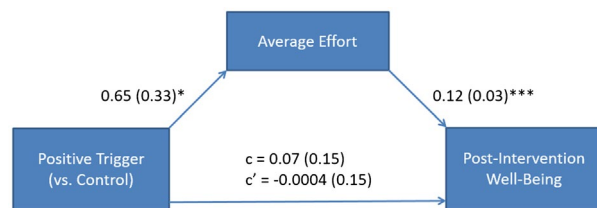


Figure 2. Unstandardized regression coefficients (standard errors) for the effect of trigger at baseline (versus control group) on post-intervention well-being via average effort, controlling for baseline well-being (Study 1).

* $p \leq 0.05$; *** $p < 0.001$.

only speak to the ‘indirect effect’ of prosocial effort and cannot call prosocial effort a mediator (Preacher & Hayes, 2004). Using Preacher and Hayes (2008) recommended procedures, we estimated the path coefficients in a mediational model, as well as a bootstrap bias-corrected confidence interval (with 5000 bootstrapped samples) for the specific indirect effect of condition (positive triggers vs. control) on post-intervention and follow-up well-being through average level of effort.⁵ For these analyses, we dummy-coded condition (any positive trigger = 1, control = 0) and predicted paths from condition (positive trigger vs. control) to average level of effort, $b = 0.65$, $SE = 0.33$, $p = 0.05$, and from average level of effort to post-intervention well-being, $b = 0.12$, $SE = 0.03$, $p = 0.0009$, controlling for baseline well-being ($N = 169$; see Figure 2 for unstandardized regression coefficients). The bootstrap analyses supported our prediction of a positive indirect effect of condition through effort [0.003, 0.20], suggesting that people who engaged in positive activities put relatively more effort into performing kind acts than control participants, and that greater effort was associated with greater gains in well-being.

Using the same bootstrapping techniques, we also tested whether the effect of effort extended to well-being at follow-up. We again found that condition (trigger vs. control) predicted average effort, $b = 0.83$, $SE = 0.36$, $p = 0.02$, and average effort predicted well-being at follow-up, $b = 0.08$, $SE = 0.04$, $p = 0.03$, controlling for baseline well-being ($N = 153$). The bootstrap confidence interval again supported our prediction of a positive indirect effect of condition through effort [0.005, 0.19].

Positive affect as a mechanism?

Implicit in our model is the assumption that positive activities promote more positive emotions, which, in turn, drive their greater effort. Indeed, a planned contrast (positive trigger conditions = +1, control condition = -4) on the difference score between positive affect at T_2 and T_1 revealed that students who engaged in a positive writing activity increased in positive affect more than students

who engaged in the control writing activity, $t(196) = 1.96$, $p = 0.05$, $r = 0.14$; however, this T_2 boost in positive affect did not relate to average effort, $r(188) = 0.09$, $p = 0.23$. Hence, gains in positive affect engendered by positive activities did not explain the additional levels of effort toward kind acts mustered by those who engaged in such activities (versus controls). Thus, in Study 2, we will take a more specific approach – testing one particular positive emotion that we predict will mediate the relationship between the positive trigger of gratitude and prosocial effort.

Changes in well-being over time?

Notably, because all of our conditions required doing kind acts each week – an activity shown by prior research to make people happier (e.g. Lyubomirsky et al., 2005; Nelson et al., in press) – we did not have a neutral comparison group. Hence, we expected to see gains in well-being over time across the entire sample. However, one-sample t -tests on the difference scores between post-intervention or follow-up well-being and baseline well-being indicated no change in well-being over time across conditions, $t(171) = -0.02$, $p = 0.98$, and $t(156) = 0.39$, $p = 0.70$, respectively. We were not necessarily surprised by this result. Past studies have found that the well-being of undergraduates decreases over the course of a quarter or semester as the stresses of school begin to compound (e.g. Sheldon & Lyubomirsky, 2006). A neutral comparison group, had it been included, would likely have revealed a downward trajectory in well-being across the intervention, differentiating itself from the other conditions in this study (Nelson, Fuller, Choi, & Lyubomirsky, 2014).

In addition, although the positive triggers indirectly predicted post-intervention and follow-up well-being via greater average effort (supporting Hypotheses 1 and 2), we did not find significant group differences on changes in well-being over time to post-intervention or follow-up, $F(4, 167) = 0.10$, $p = 0.98$, and $F(4, 152) = 1.56$, $p = 0.19$, respectively (see Table 1 for well-being composite means and standard deviations by condition and time point). Thus, although the positive triggers elicited greater effort, and that effort was related to higher well-being, there were likely other factors that contributed to well-being over time (e.g. people’s receptivity to positive activities in general).

Study 2: How does gratitude motivate prosocial effort?

Supporting our first hypothesis, Study 1 found that engaging in a positive trigger right before receiving instructions to perform kind acts boosts effort to perform those acts. Also, consistent with Hypothesis 2, greater prosocial effort throughout the intervention predicted relatively higher

Table 1. Well-being means (standard deviation) by condition and time point (Study 1).

	Baseline (T_1)		Post-intervention (T_4)		Follow-up (T_5)	
	<i>M</i> (SD)	<i>n</i>	<i>M</i> (SD)	<i>n</i>	<i>M</i> (SD)	<i>n</i>
General gratitude	3.78 (1.10)	45	4.00 (0.91)	32	3.98 (1.01)	25
Specific gratitude	3.97 (1.00)	47	4.12 (1.09)	38	4.35 (0.79)	33
Optimism	3.97 (0.91)	48	3.83 (1.05)	35	3.84 (1.01)	34
Joy	3.91 (1.04)	47	3.90 (1.01)	32	3.95 (0.93)	30
Control	3.92 (0.91)	46	3.84 (1.09)	35	4.02 (1.12)	35

Note: All conditions performed acts of kindness for 3 weeks. Baseline took place before any manipulation, post-intervention took place immediately following the three-week intervention, and follow-up took place 2 weeks after post-intervention.

well-being immediately following it and at the two-week follow-up. Somewhat counterintuitively, however, increases in a composite of positive emotions in the week following the positive triggers did not predict greater effort. Two possibilities are that the positive trigger was working via a mechanism other than positive affect (e.g. reciprocity toward the experimenter, increased agreeableness or increased liking of others) or via discrete positive emotions that were not revealed by the composite. For example, recalling an intensely positive experience is likely to stimulate joy, which prompts energy and effort toward prosocial behavior, whereas the optimism writing task may stimulate hope, which, in turn, prompts one to pay it forward. Multiple experiments would be needed to test each positive trigger and its potential underlying mechanisms. In Study 2, we chose to focus on gratitude. Specifically, we aimed to test the mechanism by which expressing general gratitude could stimulate effort toward prosocial behavior – namely, via elevation.

Expressing gratitude motivates kindness via elevation

We propose that expressing gratitude promotes more effort toward kind acts because it elicits the ‘other-praising’ emotion of elevation (Algoe & Haidt, 2009; Haidt, 2003). Haidt (2003) uses the term elevation to describe the feelings, thoughts and physiological responses one has after witnessing an act of moral virtue – specifically, a feeling of being moved and uplifted, a warm feeling in the chest, a sense of optimism about humanity and a desire to become a better person and give back to others. Although elevation is usually described as resulting from witnessing a non-self-relevant act of virtue, we propose that reflecting on past acts of virtue – even if self-relevant – can stimulate elevation. Indeed, gratitude has been proposed as a ‘moral barometer’ by helping people recognize the good deeds happening around them and also as a ‘moral motivator’ prompting people to pay-it-forward (McCullough, Kilpatrick, Emmons, & Larson, 2001).

Past research has shown that people who wrote about times in which they felt grateful were relatively more

elevated than people who wrote about times in which they felt relieved (i.e. a positive emotion control comparison) or listed what they did throughout the week (i.e. a neutral control comparison; Layous et al., 2016). Notably, across three studies, expressing or recalling gratitude actually had as strong of an effect on elevation and indebtedness as it did on gratitude. Because often the deeds people feel most grateful for are ones that can never be repaid, they might be left with a feeling of indebtedness that cannot be relieved by giving back to the benefactor. Thus, the person might feel moved and uplifted that someone has invested in her, but simultaneously motivated to help others and become a better person to relieve some of this indebtedness (Layous et al., 2016).

The complexity of the experience of gratefulness – that it includes feeling grateful, but also indebted and elevated – is an important element of our hypothesis that gratitude letters trigger prosocial effort. Specifically, recent theory suggests that gratitude is low in ‘approach motivational intensity’ because it occurs after a goal has been accomplished (i.e. one has already received a gift; Harmon-Jones, Gable, & Price, 2013). However, because expressing gratitude also stimulates states like indebtedness and elevation, which are presumably higher on approach motivational intensity, people who consider a person for whom they are especially grateful may be more likely to put effort into being generally more generous and prosocial than people who are contentedly sipping a cup of tea (i.e. a state that involves low motivational intensity in Harmon-Jones and colleagues’ framework).

An accumulating body of evidence supports our contention that elevation might be particularly powerful in motivating prosocial efforts. Across two experiments, people who watched an elevating video clip (versus a neutral nature documentary) were more likely to offer the researcher help in a subsequent task and devoted more time to helping (Schnall, Roper, & Fessler, 2010). Notably, in the second experiment, people who watched the elevating clip helped even longer than those who watched a humorous clip, indicating that elevation promoted helping behavior above and beyond simply being in a positive state (Schnall et al., 2010). Also supporting the link between elevation and kindness,

one study found that a trait tendency toward experiencing elevation was related to prosocial behavior even after controlling for the Big 5 and spiritual transcendence (Landis et al., 2009). Finally, as preliminary evidence for the far-reaching influence of elevation, preschool teachers who reported feeling elevated by their school principals showed more positive organizational behaviors and more commitment to the school than those who simply reported feeling happy or serene (Vianello, Galliani, & Haidt, 2010). Thus, we expect that expressing gratitude will promote elevation, which, in turn, will predict greater expended effort toward performing kind acts.

In sum, we expected that, because expressing gratitude reminds people that someone has done something good for them, gratitude letter writers will feel moved, uplifted and ready to pay their favor forward (Hypothesis 3; i.e. gratitude trigger will predict elevation), which will predict greater levels of effort toward prosocial behavior (Hypothesis 4; i.e. elevation will predict prosocial effort). In turn, mirroring results from Study 1, prosocial effort will predict greater post-test and follow-up well-being (Hypothesis 2). Like in Study 1, we will also test whether the gratitude trigger leads directly to greater prosocial effort (Hypothesis 1; i.e. in the absence of elevation). Lastly, this experiment also explored the duration of the gratitude trigger's benefit, extending the practice of kind acts to 6 weeks and the follow-up to one month (versus the three-week experiment and two-week follow-up in Study 1).

Thus, in Study 2, we randomly assigned participants to write general gratitude letters, do nothing, or write about their weekly activities before receiving instructions to perform kind acts. Because participants practiced kind acts for twice as long (6 vs. 3 weeks), we also tested the additional effect of expressing gratitude halfway through the intervention (versus just at the beginning).

Method

Participants

Participants were 139 undergraduate students (75.5% female; $M_{AGE} = 19.60$, $SD = 3.10$) from James Madison University who were granted course credit in exchange for participation in the study. The majority of participants identified as White (84.2%), with the remaining participants identifying as 'More than one' ethnicity (6.5%), Asian (5.8%), Hispanic/Latino (2.2%), Black/African-American (0.7%), or 'Other' (0.7%).⁶

Design and procedure

Participants logged into the study website once a week for 7 weeks (including at baseline and during the six-week intervention period) to complete the intervention activities, and

then at a one-month follow-up. To explore whether a trigger deployed at the midpoint of the intervention could provide participants an extra boost in prosocial effort, we varied when and how frequently the gratitude trigger was administered in three experimental conditions. Specifically, one condition included the gratitude trigger at baseline and only at baseline ($n = 34$); one condition included the gratitude trigger at mid-intervention and only at mid-intervention ($n = 34$); and one condition included the gratitude trigger at baseline and mid-intervention ($n = 36$). Finally, one condition included a control writing activity, just at baseline ($n = 35$). We found no effect of the gratitude trigger at midpoint on any of our intermediary or dependent variables (see Layous, 2014), so we collapsed across conditions to compare the two conditions that expressed gratitude at baseline ('gratitude trigger'; $n = 70$) to the conditions that did not ('no trigger'; $n = 69$). As in Study 1, all participants were instructed to perform kind acts.⁷

Materials

In Study 2, we used the general gratitude positive trigger and control prompts described in Study 1, as well as all of the same outcome and effort measures. Life satisfaction, positive affect and negative affect were assessed at baseline (T_1 ; Cronbach's α s > 0.79), mid-intervention (T_4 ; α s > 0.83), post-intervention (T_7 ; α s > 0.89), and follow-up (T_8 ; α s > 0.86; see bottom of Figure 1 for timeline). Cronbach's alphas for the well-being composite were > 0.72 for all time points. Self-reported effort was again measured after each week of the intervention ($T_2 - T_7$; Cronbach's α s > 0.86) and then averaged across the intervention ($T_2 - T_7$; $\alpha = 0.90$). The following measure was added to Study 2.

Elevation

Unlike commonly labeled emotions like joy or anger, the term 'elevation' is not likely to be familiar to laypeople. Thus, we measured elevation by asking about the emotions, thoughts, physiological responses and volitional responses theoretically associated with this construct (Haidt, 2003). Specifically, participants were asked to rate the degree to which they felt 'moved', 'uplifted', 'optimistic about humanity', 'happy', 'a warm feeling in your chest', 'a desire to become a better person' and 'a desire to help others' right now on a seven-point Likert rating scale (1 = *did not feel at all*, 4 = *felt moderately*, 7 = *felt very strongly*; Schnall et al., 2010). We averaged these items at each time point (Cronbach's α at all time points > 0.89).

Results and discussion

Baseline analyses

Analyses of baseline well-being revealed no significant differences by condition (trigger vs. no trigger; trigger coded

as '1'), $t(137)=0.36, p=0.72$; sex, $t(137)=0.67, p=0.51$ (males dummy-coded as '1'); or ethnicity, $F(5, 133)=0.43, p=0.83$. We created a dummy-coded variable to represent missingness at each time point (missing = '1'; score present = '0'), and conducted t -tests to compare the baseline well-being scores of missing participants to participants who provided a score. We found no significant differences in baseline well-being scores by missingness at mid-intervention, $t(137)=-0.74, p=0.46$, or post-intervention, $t(137)=-0.84, p=0.40$, but did find a significant difference at follow-up, T_8 : $t(137)=2.20, p=0.03$, such that people who were missing at follow-up had higher well-being at baseline ($M=4.55, SD=0.62$) than people who were not missing ($M=4.10, SD=0.84$).

Effect of the gratitude trigger

Effort

Failing to support our first hypothesis, people who completed a gratitude trigger at the beginning of the intervention (versus those who completed no trigger) did not show greater levels of effort throughout the intervention ($T_2 - T_7$), $t(119)=-0.33, p=0.74, r=0.03$. Thus, in contrast to Study 1, the gratitude trigger did not directly boost prosocial effort. However, these results do not preclude the possibility that gratitude could influence effort indirectly – in other words, that another intermediary mechanism is at work (Preacher & Hayes, 2004; Shrout & Bolger, 2002).

Elevation

Specifically, we expected the gratitude letter to spur increased elevation (Hypothesis 3), which, in turn, would stimulate relatively greater effort toward kind acts (Hypothesis 4). In support of Hypothesis 3, people who wrote a gratitude letter (versus those who did not) at baseline showed greater levels of elevation immediately following the trigger (T_1), $t(134)=4.93, p<0.0001, r=0.39$, marginally through mid-intervention ($T_1 - T_3$), $t(127)=1.92, p=0.06, r=0.17$, but not through the latter half of the intervention ($T_4 - T_6$), $t(118)=-0.24, p=0.81, r=0.02$. Perhaps once everyone (across conditions) was performing kind acts, they felt elevated from their good deeds, thus evening out the elevation score between the trigger and non-trigger groups over time.

Analyses of indirect effects: Elevation and effort

Like in Study 1, we did not find a direct effect of the trigger on increases in well-being, but proceeded to explore the indirect means by which the trigger may lead to greater well-being (Preacher & Hayes, 2004; Shrout & Bolger, 2002). To test our hypotheses regarding indirect pathways (Hypotheses 2, 3, and 4), we again used Preacher and Hayes (2008) bootstrapping procedures (with 5000

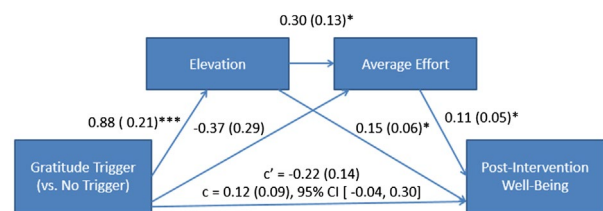


Figure 3. Unstandardized regression coefficients (standard errors) for the effect of gratitude trigger at baseline (versus no trigger) on post-intervention well-being via baseline elevation and average effort, controlling for baseline well-being (Study 2).

Note: Estimates for total effect (c path is bootstrapped). All other paths are estimated with OLS regression. * $p < 0.05$; *** $p < 0.001$.

bootstrapped samples). To simplify the presentation of results, we only included the 95% bias-corrected bootstrap confidence intervals for the indirect effects in the text of this paper; however, we will present OLS regression coefficients in Figure 3. We dummy-coded condition such that conditions that completed a gratitude writing exercise at baseline received a '1', and the other conditions received a '0' and used this variable as a dichotomous predictor in the mediation model.

Post-intervention well-being

Evidence from the bootstrap analyses supported our predicted indirect effect of condition on post-intervention well-being through baseline elevation and effort. Specifically, we found that completing a gratitude trigger at the beginning of an intervention (versus no trigger) stimulated relatively greater elevation immediately following the writing task (T_1); relatively greater elevation was associated with relatively greater effort throughout the intervention ($T_2 - T_7$); and, finally, greater effort predicted higher well-being at post-intervention ($N=108$; bootstrap analyses revealed an indirect effect of the gratitude trigger at baseline on post-intervention well-being through baseline elevation and average effort, 95% CI [0.002, 0.10]). Interestingly, we also found an indirect effect of baseline elevation alone (not predicting effort [0.005, 0.33]). Thus, elevation may directly predict post-intervention well-being, in addition to predicting post-intervention well-being via increases in effort. Lastly, effort alone (not preceded by baseline elevation [-0.15, 0.007]) included zero, thus failing to support this pathway as an indirect effect between condition and post-intervention well-being (see Figure 3).

Follow-up well-being

Our proposed indirect effect (Hypotheses 2, 3 and 4) did not, however, predict follow-up well-being as strongly as it did post-intervention well-being. Specifically, the bootstrap confidence interval for the indirect effect through baseline elevation and average effort [-0.002,

Table 2. Well-being means (standard deviation) by condition and time point (Study 2).

	Baseline (T_1)		Mid-intervention (T_4)		Post-intervention (T_7)		Follow-up (T_8)	
	<i>M</i> (SD)	<i>n</i>	<i>M</i> (SD)	<i>n</i>	<i>M</i> (SD)	<i>n</i>	<i>M</i> (SD)	<i>n</i>
<i>Trigger at baseline</i>								
Baseline	4.19 (0.79)	34	4.32 (0.92)	28	4.43 (1.03)	24	4.14 (0.96)	26
Baseline and mid-intervention	4.18 (0.80)	36	4.02 (0.95)	26	4.31 (0.82)	27	4.31 (0.85)	31
<i>No trigger at baseline</i>								
Mid-intervention	4.09 (0.93)	34	4.20 (0.80)	28	4.46 (0.88)	28	4.33 (0.86)	31
No trigger at all	4.18 (0.82)	35	4.38 (0.75)	29	4.37 (0.92)	33	4.26 (1.08)	32

Note: All conditions performed acts of kindness for 6 weeks and the above condition names represent when (if ever) participants completed the 'trigger' or gratitude letter activity. Baseline measure of well-being (T_1) took place before any manipulation, mid-intervention took place 3 weeks into the intervention (T_4), post-intervention took place at the end of the six-week intervention (T_7), and follow-up took place 4 weeks after post-intervention (T_8).

0.07], through baseline elevation alone $[-0.18, 0.11]$, and for average effort alone $[-0.15, 0.01]$ all included zero, suggesting that the trigger at baseline had an indirect effect on post-intervention, but not follow-up well-being ($N = 112$). Nevertheless, the path via elevation and effort trended in the predicted way.

Changes in well-being over time?

Replicating our findings from Study 1, a one-sample t -test on the difference scores between mid-intervention and baseline well-being indicated that, across conditions, well-being did not significantly increase during the first 3 weeks of the intervention, $t(110) = 0.65$, $p = 0.52$, $r = 0.06$. However, difference scores out to post-intervention and follow-up indicated significant increases in well-being over time across the sample, $t(110) = 3.09$, $p = 0.003$, $r = 0.28$, and $t(119) = 2.12$, $p = 0.04$, $r = 0.19$, respectively.

In addition, although many of our indirect pathways were supported in Study 2 (Hypotheses 2, 3, and 4, but not Hypothesis 1), we found nonsignificant between-condition difference in changes in well-being over time out to mid-intervention (trigger = '1'; no trigger = '0'), $t(109) = -1.57$, $p = 0.12$, $r = -0.15$, post-intervention, $t(109) = -0.76$, $p = 0.45$, $r = -0.07$ and follow-up, $t(118) = -0.83$, $p = 0.41$, $r = -0.08$ (see Table 2 for well-being composite means and standard deviations by condition and time point). Thus, although the trigger elicited greater elevation, which predicted greater effort, and that effort was related to higher well-being, other factors likely contributed to well-being (such as the powerful impact of doing acts of kindness) besides simply the positive trigger's indirect effect on effort.

General discussion

Building on previous research, we tested whether positive activities could trigger greater effort toward kind acts in day-to-day life. Furthermore, we explored whether this prosocial effort would feed back into happiness in a

positive feedback loop. Our data provide preliminary support for this idea – namely, that positive triggers set into motion a cycle of kindness and happiness. Specifically, in Study 1, we found that completing a positive trigger (versus a neutral writing activity) increased effort toward kind acts, which then predicted greater happiness. In Study 2, we did not find a direct effect of the gratitude trigger on prosocial effort, but we did find an indirect effect of the trigger on prosocial effort via elevation. Furthermore, replicating Study 1, we found that greater prosocial effort predicted greater post-intervention well-being. Thus, across two studies, we found initial support for our prediction that engaging in a positive emotion elicitation (i.e. a positive trigger) can promote prosociality in a naturalistic setting.

Positive activity triggers and prosocial effort

The current studies focused on whether a positive trigger could promote greater effort toward kind acts. The presumed mechanism is that such activities stimulate positive emotions – and, in the case of the gratitude trigger, elevation, in particular – which in turn predict relatively greater effort. The effect of the gratitude trigger on effort via elevation was supported in Study 2, but positive emotions were not captured optimally in Study 1 (for further discussion, see below).

We suspect too that positive activities could bolster helping behavior via pathways other than positive emotion. For example, Cialdini and colleagues' concomitance theory proposes that positive emotions may not prompt helping behaviors directly, but rather indirectly via the byproducts of positivity, such as liking others more, believing good things will happen in the future (and thus resources can be shared in the present), having an enhanced sense of self-control, and recalling the rewards of past good deeds (Cialdini, Kenrick, & Baumann, 1982; Manucia, Baumann, & Cialdini, 1984). Hence, to the extent that positive triggers enhance positive emotions, they can set off prosociality via multiple indirect mechanisms that are activated by positive emotions. Future research could

examine other potential ways that positive activities can contribute to people's prosociality.

Elevation and prosocial effort

In Study 2, we found that engaging in a gratitude trigger (versus no trigger) before receiving instructions to perform kind acts increased feelings of elevation – and that this boost in elevation predicted greater prosocial effort. For example, a person writing a letter of gratitude to her parents for all of their love and support throughout the years might feel moved by her parents' efforts and feel a warmth in her chest as she reflects upon their sacrifices. These feelings might motivate her to be generous to others (as her parents have been to her) or attempt to become a better person in other ways to prove herself deserving of her parents' efforts. Thus, when encountering an opportunity to perform kind acts, she can direct her newfound motivation and positive feelings toward acting prosocially, resulting in greater effort.

Additionally, elevation immediately following the trigger predicted higher post-intervention well-being directly, not only via greater prosocial effort. Perhaps those individuals who were receptive to the gratitude trigger, and therefore felt more elevated after completing it, were also the ones who gained the most in well-being in response to performing kind acts, regardless of their effort. We propose several reasons why prosocial effort did not completely account for the relationship between elevation and post-intervention well-being. First, elevated people who truly enjoyed performing their kind acts might have found the kind acts a breeze to perform, thus self-reporting minimal effort. Second, elevated people may have felt highly rewarded from acting generously (even when mustering objectively low effort) due to their receptivity toward positive activities in general. Finally, those who feel elevated by gratitude may simply have a greater capacity to grow in happiness. Thus, future studies could explore other mechanisms (besides prosocial effort) by which elevation promotes greater well-being while performing kind acts.

Limitations and future questions

Our studies included several limitations that point to ripe areas for future investigation. First, a surprising finding from Study 1 was that a lift in positive emotions as a result of engaging in different positive activities did not promote prosocial effort. Perhaps our emotion composite was assessed too long after the activity or diluted the effect of any one discrete positive emotion that could be driving effort; for example, the best possible selves writing task may promote relatively greater optimism, which drove effort; the intensely positive experience writing task

may promote relatively greater joy, which fueled effort. To explore this possibility, our second study found evidence for an indirect effect of expressing gratitude on well-being via one specific positive emotion – namely, elevation.

A second possibility is that, contrary to our initial theory, the positive trigger affected prosocial effort via a mechanism other than positive affect. For example, because people in the positive trigger conditions were instructed to engage in a pleasant task, they may have felt a sense of reciprocity toward the experimenter. Thus, when presented with a subsequent task (i.e. our kindness manipulation), participants in the positive trigger conditions were more amenable to the instructions and put more effort toward the prosocial task than the participants who engaged in the neutral writing task. Future research should explore the possibility that this compliance effect is not limited simply to prosocial tasks, but applies to any task subsequent to the positive trigger (e.g. Erez & Isen, 2002).

In addition, as suggested by concomitance theory, positive emotions may lead to prosocial effort not directly but indirectly – for example, by increasing liking for others. Future investigators could explore these alternative explanations for how the positive trigger acts on prosocial effort and could also incorporate real time emotion measures via experience sampling methodologies to assess the positive emotions experienced concurrently with the performance of kind acts.

Importantly, prosocial effort in people's daily lives could also be measured in multiple ways. Alternative approaches for future researchers include assessing effort toward kind acts performed in real time (e.g. experience sampling or daily reconstruction method), as well as assessments from peers or the actual targets of the kindness. Additionally, because it is very difficult for objective raters to code the effort expended in the types of simple and brief lists of kind acts provided by our participants (e.g. 'helped my brother with homework'), researchers could ask future participants to describe their kind acts in much greater detail. Lastly, people who reported high levels of prosocial effort may not necessarily have enjoyed performing kind acts, thus possibly diminishing the path from prosocial effort to post-intervention well-being. Thus, to explore other mechanisms, besides effort, that might drive increases in well-being, participants in the future could be asked how much they enjoyed performing their kind acts and how much they would like to perform more in the future.

Additionally, although many of our hypotheses were supported, our results were just above the threshold for significance, with the confidence intervals nearly including zero. Furthermore, our first hypothesis that positive triggers would lead directly to greater prosocial effort was supported in Study 1, but not in Study 2. Thus, the current results are not robust or conclusive and need to be replicated to ensure their reliability.

Finally, many other positive downstream consequences of being a kind person were not captured in the current studies. For example, past research has shown that children who performed kind acts (versus engaging in a mildly pleasant but not prosocial task) actually made more classroom friends by the end of the study (Layous, Nelson, Oberle, Schonert-Reichl, & Lyubomirsky, 2012). The enhanced friendships as a result of being a kind person could also trigger positive emotions that could, in turn, stimulate behavior change. Thus, boosting one's generosity could lead to improved well-being (as demonstrated in our studies), but it could also lead to other positive outcomes not explored here.

Concluding remark

Across two studies, we found evidence that a recursive cycle of happiness and kindness can be triggered experimentally by the administration of a simple, brief positive activity. Specifically, we found preliminary support for the contention that people in a positive state – namely, an elevated state – appear better equipped to put effort toward prosocial acts in the context of their daily lives and subsequently feel happier overall. Future research would do well to continue to explore the mechanisms in this positive feedback loop. In sum, to paraphrase the Scott Adams quote included in the epigraph, our findings are consistent with the notion that every act of kindness creates a ripple.

Notes

1. These four conditions allow us to experimentally test two exploratory questions – that is, are some positive activities more effective in motivating prosocial behavior than others, and does gratitude need to be general (e.g. others have contributed to our good fortune) or specific to the domain of the behavior one is about to embark on (e.g. others have been kind to me, so we are moved to be kind as well) to be most effective?
2. At the post-intervention time point, the sample size dropped considerably ($N = 170$), but the attrition was evenly spread across conditions, $\chi^2(4) = 2.33$, $p = 0.68$, and ethnic background, $\chi^2(7) = 4.89$, $p = 0.67$. However, men attrited marginally significantly more than did women, $\chi^2(1) = 3.61$, $p = 0.06$. Similarly, at the follow-up time point (T_5), sample size again dropped ($N = 154$), but attrition was again evenly spread across conditions, $\chi^2(4) = 4.65$, $p = 0.33$, and ethnicities, $\chi^2(7) = 4.73$, $p = 0.69$. The pattern of missingness among males and females seen at post-intervention did not hold for the follow-up time point, revealing that men and women showed equal levels of attrition at follow-up, $\chi^2(1) = 0.00$, $p = 0.99$.
3. All conditions from Study 1 are reported here. In Studies 1 and 2, we also assessed psychological need satisfaction weekly via the Balanced Measure of Psychological Needs (Sheldon & Hilpert, 2012). We found no evidence that

need satisfaction mediated the relationship between the positive trigger and prosocial effort. That said, past studies have found that need satisfaction mediates the relationship between positive activities and increases in well-being (Nelson et al., 2015). However, neither of the two studies in the current paper included a true control group (i.e. all participants performed kind acts), so we were unable to assess whether our studies replicated past results regarding need satisfaction.

4. Importantly, we were underpowered for pairwise comparisons between conditions (30%). Nevertheless, each individual condition at least trended toward eliciting more effort than the control condition in pairwise contrast analyses (i.e. experimental condition in question = +1; control condition = -1, other conditions, set aside). Specifically, general gratitude versus control: $t(77) = 1.70$, $p = 0.09$, $r = 0.19$; specific gratitude versus control: $t(78) = 1.59$, $p = 0.11$, $r = 0.18$; optimism versus control: $t(78) = 1.15$, $p = 0.25$, $r = 0.13$; joy versus control: $t(73) = 2.18$, $p = 0.03$, $r = 0.25$.
5. The OLS regression coefficients represent the sample-specific predictions of the true values of each pathway. These predictions are also based on the assumption that the sampling distribution for each pathway is normal. The bootstrapped analyses, however, simulate the resampling of the data over and over again (in my case, 5000 times) and provide inferential estimates based on the empirical sampling distribution (Hayes, 2013). Typically, in bootstrapped analyses, the confidence intervals represent the finding that 95% of the bootstrapped samples showed effects at or between the provided lower and upper bounds. In my results, however, we used the bias-corrected bootstrap confidence intervals, which are very similar to the percentile confidence intervals, but are adjusted as a function of the proportion of k values of the bootstrapped ab values that are less than the OLS estimate of ab calculated in the original data (Hayes, 2013).
6. The total sample size dropped mid-intervention (T_4 ; $N = 111$), post-intervention (T_7 ; $N = 112$), and follow-up (T_8 ; $N = 120$) time points, but the attrition was evenly spread across conditions at all time points, T_4 : $\chi^2(3) = 1.00$, $p = 0.80$; T_7 : $\chi^2(3) = 6.12$, $p = 0.11$; T_8 : $\chi^2(3) = 3.83$, $p = 0.28$. In addition, attrition was evenly spread across gender, T_4 : $\chi^2(1) = 2.40$, $p = 0.12$; T_7 : $\chi^2(1) = 0.83$, $p = 0.36$; T_8 : $\chi^2(1) = 0.14$, $p = 0.71$, and ethnicity, T_4 : $\chi^2(5) = 5.94$, $p = 0.31$; T_7 : $\chi^2(5) = 4.87$, $p = 0.43$; T_8 : $\chi^2(5) = 5.01$, $p = 0.42$.
7. This study included one other condition that was run simultaneously to test hypotheses beyond the scope of the current paper. Specifically, in this fifth condition, participants wrote gratitude letters every week (during the six-week intervention). For results on this weekly condition, see the first author's dissertation (Layous, 2014). Critically, in light of concerns about researcher degrees of freedom (Simmons, Nelson, & Simonsohn, 2011), all critical results presented here were the same when this condition was included in the analyses. We footnote the weekly condition here conditions for the sake of simplicity and brevity.

Disclosure statement

No potential conflict of interest was reported by the authors.

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