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Becoming Happier Takes Both a Will and a Proper Way: An Experimental Longitudinal Intervention To Boost Well-Being

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An 8-month-long experimental study examined the immediate and longer term effects of regularly practicing two assigned positive activities (expressing optimism and gratitude) on well-being. More important, this intervention allowed us to explore the impact of two metafactors that are likely to influence the success of any positive activity: whether one self-selects into the study knowing that it is about increasing happiness and whether one invests effort into the activity over time. Our results indicate that initial self-selection makes a difference, but only in the two positive activity conditions, not the control, and that continued effort also makes a difference, but, again, only in the treatment conditions. We conclude that happiness interventions are more than just placebos, but that they are most successful when participants know about, endorse, and commit to the intervention.

Keywords: happiness, intervention, gratitude, optimism, effort, motivation

The pursuit of happiness and fulfillment is a goal shared by the majority of people in the West and, increasingly, around the world (Diener, 2000; Diener, Suh, Smith, & Shao, 1995). Historically speaking, because of psychology's prevailing focus on the alleviation of psychopathology and weakness, relatively few studies (e.g., see Boehm & Lyubomirsky, 2009; Lyubomirsky, 2008; Lyubomirsky, Sheldon, & Schkade, 2005, for reviews) have actually attempted to increase individuals' well-being (defined as high life satisfaction, frequent positive affect, and infrequent negative affect; Diener, Suh, Lucas, & Smith, 1999). The aim of the current research was to test predictions from a model of well-being change (Lyubomirsky, Sheldon, et al., 2005; Sheldon & Lyubomirsky, 2004), which argues that the intentional pursuit of greater happiness—when done under optimal conditions—can be successful.

We began by testing two activities that have previously been found to be effective in enhancing happiness—practicing optimistic thinking by visualizing one's best possible future selves (cf. King, 2001) and expressing gratitude through writing (cf. Emmons & McCullough, 2003). More important, however, we examined

two contextual or boundary conditions predicted by our model to impact the efficacy of these activities—namely, (a) the intention to use the intervention to become happier, as operationalized by a self-selection factor, and (b) ongoing effort exerted toward the activities specified by the intervention, as operationalized by objective ratings. For a framework to undergird these factors, we turn to a model of the architecture of well-being change.

A Model of Well-Being Change

In their model of happiness, Lyubomirsky, Sheldon, and Schkade (2005) proposed that three major factors contribute to people's levels of well-being: (a) their happiness set point (i.e., the genetically determined stable level of happiness, which has been shown to account for approximately 50% of the variance in individual differences in well-being), (b) their life circumstances (e.g., factors such as income, marital status, or religiosity, which are typically found to account for roughly 10% of individual differences in well-being), and (c) positive cognitive, behavioral, and goal-based activities (which have the potential to account for a significant portion, up to 40%, of individual differences in well-being) (see also Sheldon & Lyubomirsky, 2004, 2006a, 2007). This last factor, positive activity, has been the linchpin of recent research efforts to bolster people's happiness levels and served as the focus of the current studies. Thus, we focused on positive activities in the current research, examining factors that may moderate the impact of such activities on well-being.

Although research on the heritability of well-being (e.g., Lykken & Tellegen, 1996), the stability of personality (e.g., McCrae & Costa, 1990), and the phenomenon of hedonic adaptation (Fredrick & Loewenstein, 1999) suggests that it should be very difficult, if not impossible, to become a lastingly happier person (see Lyubomirsky, 2011; Lyubomirsky, Sheldon et al., 2005; Sheldon & Lyubomirsky, 2004, 2006a, for reviews), evidence from earlier and more recent experimental intervention studies suggests that well-being can indeed be increased, at least in the short term (e.g.,

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Fordyce, 1977, 1983), and possibly over longer periods of time (e.g., Seligman, Steen, Park, & Peterson, 2005). In other words, although genes and personality traits may operate to hold happiness levels relatively constant over time, and although individuals may be predisposed to adapt to positive life experiences relatively swiftly, engaging in happiness-increasing activities (such as committing to important goals, meditating, acting kindly toward others, thinking optimistically, or expressing gratitude) has the potential to improve levels of happiness for significant periods of time (Lyubomirsky, Sheldon, et al., 2005; cf. Heller, Watson, & Ilies, 2004). Further, unlike many life circumstances and experiences to which one adapts rather quickly (e.g., moving to a desirable new home, office, or city), the practice of volitional positive activities may serve as a natural antidote to the process of hedonic adaptation (Lyubomirsky, 2011). That is, these activities, when practiced using optimal timing and variation (see Sheldon, Boehm, & Lyubomirsky, in press), may provide lasting boosts in well-being that do not wear off entirely with the passage of time. To this end, the aim of the current studies was to examine whether, when, and how two such positive activities (expressing optimism and gratitude) lead to improvements in well-being over time.

Interventions to Increase Well-Being via Positive Activities

The first investigations to demonstrate that well-being can be sustainably increased by engaging in happiness-increasing activities were conducted as far back as the 1970s and 1980s (Fordyce, 1977, 1983). In a series of studies, Fordyce sought to improve well-being by teaching students in his classes what happy people do and then have these students practice those behaviors in their everyday lives. Results showed that students assigned to practice 14 different happiness-enhancing activities (e.g., spend more time socializing, become more active, and develop optimistic thinking) once daily over a 6-week period reported improved well-being for more than 2 months, relative to a comparison group.

To directly test the hypothesis that intentional positive activity can boost happiness, Lyubomirsky, Sheldon, and colleagues (2005) conducted two 6-week-long interventions that isolated two specific activities (one behavioral and one cognitive) thought to enhance well-being, committing acts of kindness and “counting one’s blessings.” On the basis of research showing a positive association between self-reported helping and self-reported happiness (Dunn, Aknin, & Norton, 2008; Lyubomirsky, King, & Diener, 2005; Sheldon et al., in press), the first experiment prompted participants to engage in five acts of kindness on a particular weekday (e.g., donating blood, feeding a friend’s pet). Results showed that well-being increased for these participants, compared to a control group (see also Sheldon et al., in press). The second 6-week intervention was based on previous research demonstrating that “counting your blessings” can bolster feelings of satisfaction and well-being (Emmons & McCullough, 2003; cf. Koo, Algoe, Wilson, & Gilbert, 2008). In this study, participants were instructed to regularly think about things for which they were grateful (e.g., “a healthy body,” “my mother,” and “having a Valentine”). Relative to controls, participants who expressed gratitude once a week (but not three times a week) indicated greater well-being.

Seligman and colleagues (2005) tested the ability of five additional positive exercises (practiced over 1 week) to increase happiness among an online sample of self-selected community-residing participants. The results of this study showed that participants who used personal strengths in novel ways each day and those who considered three good things that happened to them each day showed increases in their happiness and declines in their depressive symptoms over a 6-month period. Indeed, these activities seemed to create ever-increasing happiness gains across nearly all follow-up assessments. Participants who wrote and delivered a letter of gratitude also reported increased happiness and reduced depression.

Besides such work with nonclinical samples, researchers have also shown that positive psychological practices can relieve symptoms in mild to moderately depressed individuals, as well as in outpatients with major depressive disorder and other affective disorders (Fava, Rafanelli, Cazzaro, Conti, & Grandi, 1998; Fava et al., 2005; Seligman, Rashid, & Parks, 2006).

Under What Conditions Do Positive Activities Work Best?

In sum, a review of the small, but growing, well-being intervention literature suggests that a number of positive activities can increase well-being over both short and, possibly, longer term periods of time. It is worth noting that well-being is conceptualized here, and in previous work, in the hedonic (vs. eudaimonic) sense, although the distinction has not been shown to be well-specified or empirically supported (Kashdan, Biswas-Diener, & King, 2008). However, our primary aim was not to test the efficacy of particular positive activities or exercises per se, but rather to examine two boundary conditions that might enhance or limit any activity’s utility. The model of well-being change (Lyubomirsky, Sheldon, et al., 2005; Sheldon & Lyubomirsky, 2004) proposes a number of variables that should impact the efficacy of any positive activity, not just the two activities investigated here.

Self-Selection

To illustrate the first boundary condition tested in the present research, consider that the strongest happiness intervention results have been reported by Seligman et al. (2005), who demonstrated that several happiness-enhancing activities can maintain well-being improvements for up to 6 months. In these studies, participants were fully aware of the study’s aim and had volunteered with the hope (or expectation) that their well-being would increase. In other words, their experiment used a self-selected sample of participants who sought out a Web site about happiness and *chose* to participate in an online study to improve well-being. Similarly, all of Fordyce’s (1977) participants were self-selected and aware of the purpose of the activities. In contrast, all the happiness-enhancing interventions conducted by Lyubomirsky, Sheldon, and colleagues to date (Lyubomirsky, Sheldon et al., 2005; Sheldon & Lyubomirsky, 2006b; Sheldon et al., in press) have used designs in which participants are unaware of the true purpose of the study. That is, participants practice “cognitive exercises” and were not informed that changes in happiness were our major focus. All of these studies have yielded somewhat weaker or less durable effects than the Seligman and Fordyce studies. This raises the possibility

that a potentially important moderator of a happiness intervention's effectiveness is conscious knowledge of the intervention's purpose and motivation to help achieve that purpose.

When people purchase a self-help book, they are acting knowingly—choosing an experience that they hope will help them to become a happier or better person. One might say that they are “motivated” to engage in the experience, with some expectations regarding the positive results that it might bring. Because of such expectancies, later outcomes of the experience may be due to motivated cognition—that is, demand effects, self-fulfilling prophecy effects, or simple positive response biases (Kunda, 1990). For example, when somebody praises the “Seven Steps to Happiness” book that she just read, avowing that it really did make her happier, how can one be sure she is not just caught in a positive illusion, or trying to save face, or attempting to reduce dissonance? Obviously, it would help to track her actual gains in happiness over time, as observing tangible gains would at least be consistent with her belief. However, we suggest that it would also help to compare individuals like her with two other groups: (a) those assigned to read the same happiness book who did not seek it out themselves and (b) those assigned to read a different happiness book that contains no effective advice (i.e., essentially a placebo). This design would allow us to disentangle the effects of self-selection (yes vs. no) from the effects of the substantive content of the activity (treatment vs. control).

Considering activity *content* leads us to the notion that not all happiness interventions will work, just as not all self-help books will be helpful. The ideal self-help book would contain advice supported by data, so that not only would participants be motivated to act on it, but that they would also be doing the “right” thing (i.e., an activity that should work if practiced diligently and correctly). Similarly, the ideal happiness intervention would contain practices supported by empirical research. In sum, just believing one will become happier is probably not enough: One also needs to be doing something that is objectively effective at raising happiness.

In short, we argue that people need both a “will” and a “proper way” to gain maximal benefits from a happiness intervention. Analogously, medical patients need to know what their treatment is and be motivated to follow it, but also must have been prescribed the “right” treatment, not just a placebo. To be sure, a procedure involving self-selection is usually avoided in experimental research, because those who select themselves into a study expecting benefits may systematically differ from those who engage in the same study without such expectations. However, we believe it is appropriate to examine the role of self-selection in the context of a longitudinal happiness intervention, as this mirrors the goals and experience of any consumer of positive psychology programs (or self-help books).

Continued Effortful Practice of the Activity

Also consistent with the medical model, participants assigned to a happiness intervention should gain the greatest benefit if they try hard to carry it out over time. In other words, it is not enough to knowingly choose the activity initially; one must also put objective sustained effort into its practice. Consistent with this idea, Seligman et al. (2005) provide evidence that effortful pursuit of happiness-enhancing activities is essential to their success. Specifically, their study showed that continued adherence to the

happiness-enhancing activities during the course of the intervention impacted the activities' beneficial effects. Similarly, Sheldon and Lyubomirsky (2006b) found that continued effortful performance of an “optimistic thinking” activity predicted greater maintained change in subjective well-being, compared to a control condition. Accordingly, we predicted that participants who exert more effort during the weeks of practicing their assigned activities would accrue greater benefit from them. However, the benefit should only be observed if the activity is an efficacious one, which provides the actual means for increases in well-being to accrue. Based on this reasoning, we expected the biggest boosts in well-being to be shown by participants who keep putting effort into the activity and who are assigned to an “active” treatment condition, as compared to an “inert” control condition.

The Present Study

Our intervention focused on two activities that have previously been shown to bolster well-being for up to a minimum of 2 months—practicing optimistic thinking by visualizing one's best possible future selves (cf. King, 2001; Sheldon & Lyubomirsky, 2006b) and expressing appreciation toward others via gratitude letters (cf. Emmons & McCullough, 2003; Lyubomirsky, Sheldon, et al., 2005; Seligman et al., 2005). Accordingly, our study randomly assigned participants to express optimism, express gratitude, or generate a list of the previous week's activities (i.e., the comparison control group; Lumley & Provenzano, 2003) once a week over the course of an 8-week period. Measures of well-being were administered immediately before, immediately after, and 6 months subsequent to the completion of the intervention period.

Our first hypothesis in this study concerned the overall efficacy (i.e., main effect) of our two positive exercises. We predicted that, relative to the control task, practicing either optimism or gratitude would lead to improved well-being outcomes by the end of the intervention, and that these improvements might even be maintained at the 6-month follow-up—although likely to a much lesser degree. Notably, this main effect hypothesis was not essential to the primary aims of this article, as we expected any main effects to be qualified by the higher order boundary conditions that were the true focus of this research.

Our second hypothesis concerned whether motivation to become happier plays a role in well-being change. We predicted that students who self-selected themselves into a so-called “happiness intervention” would report greater gains in well-being relative to students who self-selected into a “cognitive exercises” study. This pattern was expected to be observed at both postintervention assessment periods, although, again, we expected this main effect hypothesis to be qualified, as gains may require both “a will” and “a proper way.”

To address this essential issue, we also tested self-selection (i.e., motivation to become happier) as a moderator of the effects of our two happiness-enhancing activities on increased well-being (Hypothesis 3). Specifically, we predicted that the greatest improvement in well-being (both postintervention and 6 months later) would occur for “motivated” students instructed to express optimism and gratitude, followed by “nonmotivated” students who practiced these two activities, and, last, by students who wrote about their past 7 days (i.e., both motivated and nonmotivated individuals in the control condition). In other words, having the

motivation and intention to pursue happiness is not enough; the positive activity has to be appropriate and worthwhile. This would be an important finding, indicating that happiness-intervention effects are not mere self-selection effects; if control participants who believe and hope that the activity will make them happier do not actually become happier, then this would indicate that the content of the activity matters, and that researchers are justified in identifying and comparing happiness-enhancing practices.

Our last two hypotheses concerned the main and moderating effects of continued effort on the impact of our two happiness-enhancing activities on well-being. Similar to Hypothesis 2, concerning the main effect of initial self-selection into the study, Hypothesis 4 addressed the main effect of continued effort once the study began. That is, those participants who exerted objectively more effort in their assigned activity were expected to demonstrate greater boosts in their well-being, compared with those participants who exerted less effort. More important, Hypothesis 5 predicted that the effort effect would be strongest in the two experimental conditions and weakest or nonexistent in the control condition.

Method

Participants

Undergraduate students ($N = 355$; 248 female, 107 male) at a public university took part in this study. All students participated in exchange for course credit, as well as up to \$40 additional compensation for the extra time the study required, including completing it up to Time 2 (T_2 ; immediate postintervention) and Time 3 (T_3 ; 6-month follow-up).¹ It should be noted that the follow-up occurred when students had completed the relevant course or graduated.

Twenty-three individuals who failed to complete at least four of the eight weekly intervention assignments were removed from the sample. In addition, two students were excluded as outliers whose baseline well-being was more than 3 *SDs* from the sample mean, leaving a final sample of 330 individuals (235 female, 95 male). Of the 330 participants used in the final analysis, 13 failed to complete T_2 (3.9%) and 120 failed to complete T_3 (36%). The T_3 attrition rate is comparable to the average attrition rate (42%) found in a meta-analytic review of 152 longitudinal studies, including 48 that used college samples (Roberts & DelVecchio, 2000).

Over one-third of the sample was of Asian descent (40%), 20% were Hispanic, 17% were Caucasian, 5% were African American, 5% were Hawaiian/Pacific Islander, 6% indicated "more than one ethnicity," and 7% identified themselves as "other." Students ranged in age from 18 to 46 years ($M = 19.66$, $SD = 2.91$).

Design

A 2 (Self-Selection: Yes vs. No) \times 3 (Activity: Optimism, Gratitude, Control) factorial design was used. Potential participants were offered the option to take part in one of two posted studies: one advertised as a happiness intervention and the other as a study involving cognitive exercises. Ample slots were available for each "study" at all times. In this way, students self-selected into either a "low motivation" group (those who signed up for the cognitive exercises study and who, presumably, were not as in-

vested in becoming happier; $n = 99$) and a "high motivation" group (those who signed up for the happiness intervention and, presumably, desired to be happier; $n = 231$). All participants—regardless of the study they signed up for—were then randomly assigned to express optimism ($n = 112$), convey gratitude ($n = 108$), or generate a list of their experiences over the past week ($n = 110$; i.e., control).

Procedure

Introductory appointment. Except for an introductory face-to-face appointment that took place in a laboratory, this study was conducted entirely over the Internet, using a Web site accessible only to our participants.² At this appointment, groups of 5–10 participants received a verbal description of the study. Notably, regardless of the "study" they signed up for (i.e., both low- and high-motivation groups) all participants were told that the aim of the study was to improve well-being. This information was presented at this time in accord with procedures of standard placebo-controlled designs, in which all participants are informed about the "treatment" that is being tested, which is said to have a reasonable chance of working (Rosenthal, 1976). Thus, we could test whether the experimental conditions would differ from the control condition, which should provide evidence for a placebo-type effect (if any). It is also noteworthy that this design isolates the effects of the self-selection factor, because all participants' expectancies of the study's aim are equated once they arrive at the lab, and all that differs is the experience they initially thought they would receive.

Baseline assessments. After receiving their instructions, students accessed the study's Web site and answered the first set of questionnaires at their convenience. This first baseline assessment consisted of a consent form, demographic questions, and measures of the students' well-being. Approximately 1 week later, participants completed a second baseline assessment by again accessing the study's Web site at their own discretion and responding to identical questions assessing the same outcomes. To establish a relatively more stable baseline for these measures, we combined ratings from the first and second baseline assessments of each of the four primary outcome variables—pleasant affect, unpleasant affect, life satisfaction, and happiness. Test-retest correlations for these variables between the two assessments ranged from .55 for unpleasant affect to .84 for happiness. This stable baseline (referred to as T_1) was used to minimize measurement error that can occur with single-administration measures of well-being (e.g., the transient effects of weather and/or recent events; Schwarz & Clore,

¹ Due to space constraints, we do not present well-being data from an additional 9-month follow-up. The pattern of results was similar to previous time periods, albeit weaker and sometimes marginally significant or nonsignificant. Also, in the interest of space, data on several other variables collected in this study are not described; they are available from the first author.

² Online data collection has been shown to reduce the likelihood that participants will misrepresent their actual behaviors and to increase the likelihood that they will reveal sensitive and personal information (Turner et al., 1998; Velicer, Prochaska, Rossi, & Snow, 1992). This methodology was thought to be particularly beneficial in the current study, in which all dependent variables are self-report.

1983). Finally, immediately after completing the second set of baseline questionnaires, students began the intervention.

Experimental manipulation. Participants were randomly assigned to one of three conditions for an 8-week period.

In the *expressing optimism* condition, students were asked to spend 15 min per week writing about an imagined future self—that is, to visualize living a life consistent with their ideal self. Borrowing from King's (2001) "best possible selves" paradigm, the instructions for the first week of the intervention prompted participants to "think about [their] romantic life in the future (say in 10 years)," to "imagine that everything has gone as well as it possibly could," and then to "write about what [they] imagined." In addition to writing about their best possible future romantic life, students were prompted during weeks 2 through 8, respectively, to write about their best possible future educational attainment, hobbies or personal interests, family life, career situation, social life, community involvement, and physical/mental health.

In the *expressing gratitude* condition, students were instructed to spend 15 min per week remembering times in their lives when they were grateful for something that another person did for them and then writing a letter about those experiences directly to that person (but not sending it). Participants' instructions each week, adapted from Seligman et al. (2005), encouraged them to describe "in specific terms why [they] are grateful to this individual and how the individual's behavior affected [their] life" and "what [they] are doing now and how [they] often remember their efforts." They could write to a new person each week or continue their letter to the same person. Students randomly assigned to compose such "gratitude letters" generally chose to address their letters to a parent, friend, teacher, or other close relative.

In the *comparison control* condition, participants spent 15 min per week listing what they did over the past 7 days. To maintain the cover story that *all* participants (including controls) were expected to increase their happiness levels, this condition was described as a task that would hone one's organization skills. That is, participants were prompted to "create a mental outline of what [they] did during the past 7 days" and to "write out these activities in a list format."

Time 1, time 2, and time 3 assessments. At the close of the 8th week of the intervention, students again completed all of the key well-being measures. This T_2 assessment allowed us to track changes in these outcomes from immediately before to immediately after the completion of the intervention. To test the long-term durability of the well-being increases, T_3 assessed change at 6 months postintervention. A graphic of the study timeline is presented in the Appendix.

Materials

Consent and background information. When students logged into the study Web site for the first time, they read a consent form that informed them of their rights as participants. After agreeing to take part, they were asked to provide general background information, such as their gender, ethnicity, and age.

Affect. Mood was assessed using two types of affect—Unpleasant and Pleasant—described by Feldman Barrett and Russell (1998). These measures consisted of three adjectives each—*miserable*, *unhappy*, and *troubled* for Unpleasant Affect and *content*, *happy*, and *pleased* for Pleasant Affect. These items were

chosen over the commonly administered adjectives that comprise the Positive Activation and Negative Activation Scale (PANAS; Watson, Clark, & Tellegen, 1988), because the unpleasant adjectives (such as *miserable*) and the pleasant adjectives (such as *pleased*) better represent the kind of mood the current study was focused on improving. That is, we were interested in *unpleasant* and *pleasant* mood, rather than the highly "activated" adjectives found on the Negative Affect and Positive Affect subscales of the PANAS (e.g., *jittery*, *alert*, *nervous*, *interested*, *scared*, and *active*; see Feldman Barrett & Russell, 1998, for further discussion of this issue). As in the original PANAS, participants indicated the degree to which they experienced each of the three unpleasant and three pleasant emotions over the past week on 5-point Likert-type scales (1 = *very slightly or not at all*, 5 = *extremely*). Across the measurement time periods in the current study (T_1 through T_3), good internal consistency was observed for both Unpleasant Affect (α s ranging from .84 to .89) and Pleasant Affect (α s from .82 to .87).

Life satisfaction. The Satisfaction With Life Scale (SWLS; Diener, Emmons, Larsen, & Griffin, 1985) assessed respondents' current satisfaction with their lives 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 7-point Likert-type scales (1 = *strongly disagree*, 7 = *strongly agree*). Thus, higher scores on this measure indicate greater satisfaction with life. Validation studies have shown that the SWLS comprises a single factor and possesses high internal consistency (α = .87) and high test-retest reliability (r = .82; Diener et al., 1985). Across the measurements in this study, α s ranged from .85 to .92.

Happiness. Participants evaluated their current happiness with the Subjective Happiness Scale (SHS; Lyubomirsky & Lepper, 1999). The SHS is a 4-item measure that asks respondents first to rate on 7-point Likert-type scales how generally happy they are (1 = *not a very happy person*, 7 = *a very happy person*) and how happy they are relative to their peers (1 = *less happy*, 7 = *more happy*). The remaining two questions require participants to indicate the extent to which a description of a "very happy" and a "very unhappy" person, respectively, characterizes them (1 = *not at all*, 7 = *a great deal*). After reverse-scoring the fourth item, higher scores on this measure indicate greater subjective happiness. Prior studies have reported α coefficients for the SHS from .79 to .94 (Lyubomirsky & Lepper, 1999), and, in this study, they ranged from .83 to .89 across all of our assessments.

Effort. The degree of effort and energy that students put into their writing exercises every week was assessed by naïve coders who read what participants wrote, then responded to the question, "How much effort is this participant putting into the exercise" (1 = *not very much*, 7 = *very much*); in other words, assessing how hard participants were working at implementing their assigned activity. Notably, coders were not asked to judge the quality or writing skill demonstrated in the participants' narratives. We focused on observer-rated effort because we wanted to measure participants' ongoing engagement in the activities as objectively as possible, independently of any response bias potentially induced by the self-selection manipulation. The interrater agreement for these eight codings (over 8 weeks) ranged from .68 to .93 across two judges. Further, it is clear that coders were not just using the length of participants' narratives as a proxy for effort; the correlations between word count and effort ratings ranged from $-.04$ to $.11$

across the 8 weeks ($M = .02$, all ns). Coded effort across all eight writing assignments was then averaged to create an overall “objective” index of the effort students appeared to be investing in the intervention activities.

Results

Overview of Primary Statistical Analyses

In this study, students' satisfaction with life, happiness, pleasant affect, and unpleasant affect were assessed prior to the manipulation (i.e., by combining our two baseline assessments spaced approximately 1 week apart), immediately postintervention, and at the 6-month follow-up. Because affect, satisfaction with life, and happiness are theoretically linked (Diener, 1984), and because the intercorrelations among these measures were quite high in this study (mean $r = .59$ at baseline), we created a composite of these four outcomes—referred to as “overall well-being”—by averaging their z -scores (after reverse-coding unpleasant affect).³

To test our hypotheses, we performed planned contrasts on the change scores (i.e., $T_2 - T_1$ and $T_3 - T_1$) of our dependent variable of overall well-being (see Rosenthal, Rosnow, & Rubin, 2000, for more detailed discussion of contrast analyses).⁴ A first planned contrast compared the optimism and gratitude groups to the control group (Hypothesis 1; see top of Table 1 for contrast weights). A second planned contrast compared self-selected participants to non-self-selected participants (Hypothesis 2; see bottom of Table 1 for contrast weights). A third planned contrast compared self-selected versus non-self-selected participants across our three intervention activities (i.e., expressing optimism, expressing gratitude, or listing the past week's events; Hypothesis 3; see Table 2 for contrast weights).

To examine whether sustained effort would predict gains in overall well-being (Hypothesis 4), especially in the treatment conditions (Hypothesis 5), we conducted analyses in a regression framework. Specifically, we examined both the main effect of continued effort on changes in well-being and the interaction of effort with activity content (i.e., treatments vs. control).

Baseline Analyses

Analyses of well-being by participant sex revealed no significant main effects or interactions, and, thus, all analyses were collapsed across this variable. Also, we wanted to verify that self-selected and non-self-selected participants did not differ in well-being prior to beginning the intervention. A two-tailed t test showed that these two groups did not significantly differ on overall well-being prior to random assignment, $t(328) = .96$, ns . We also examined whether preexisting condition differences existed prior to manipulation. A one-way analysis of variance indicated that the three groups also did not differ in baseline well-being, $F(2, 327) = 2.84$, ns .

Immediate and Longer Term Changes in Well-Being

Means and standard deviations for changes in overall well-being from baseline to postintervention, as well as from baseline to follow-up, are presented in Table 1 (which compares the optimism, gratitude, and control conditions at top and self-selected vs. not

self-selected students at bottom) and Table 2 (which compares self-selection across the three conditions).

Immediate postintervention. A planned comparison contrasting the two experimental groups with the control group (testing Hypothesis 1) failed to reach statistical significance ($t < 1.30$). However, supporting Hypothesis 2, right after the completion of our 8-week intervention, self-selected students reported greater increases in well-being relative to the non-self-selected ones, $t(315) = 2.44$, $p = .02$, $r = .14$ (see bottom of Table 1). Further, consistent with Hypothesis 3, self-selected students who practiced optimism or gratitude reported greater increases in well-being relative to those who practiced these activities who were non-self-selected and relative to those in the control group, $t(311) = 2.20$, $p = .03$, $r = .12$ (see Table 2).

Six-month follow-up. Although students who had expressed optimism or gratitude displayed a trend toward greater increases in well-being relative to the control group 6 months after the intervention had ended, this difference did not reach significance, $t(210) = 1.42$, $p = .16$, $r = .10$. Similarly, as displayed in Table 1, although self-selected students were still showing a trend toward well-being gains relative to non-self-selected students, it was not significant, $t(211) = 1.60$, $p = .11$, $r = .11$.⁵ However, again supporting our most important prediction (Hypothesis 3), those who had earlier selected themselves into a happiness intervention and practiced optimism or gratitude continued to report greater boosts in well-being than did those who practiced these activities but had not self-selected, and than those in the control groups, $t(207) = 2.00$, $p = .05$, $r = .14$ (see Table 2).

³ It is worth observing that the results produced when examining mood, happiness, and satisfaction with life as separate dependent variables were similar to those produced using the composite measure. The composite measure was chosen simply to clarify and condense the overall findings.

⁴ We used analyses of difference scores as the most straightforward, natural method to test individual differences in true change over time. Because well-being was expected to remain unchanged without our intervention, the underlying assumptions of difference scores (vs. covariance analysis) seemed appropriate (Wainer, 1991). In addition, because assignment to condition was not based on initial well-being scores, covariance analysis had no advantages over difference scores (Maris, 1998). In a chapter titled, “Myths About Longitudinal Research,” Rogosa (1988) argues that 50 years of psychometric literature has compounded the “myth” that, relative to residual change scores, difference scores are intrinsically unreliable and unfair. Difference scores are, indeed, reliable, when there are individual differences in true change (what we would expect in our studies), whereas residual change analyses attempt to assess correlates of change by ignoring individual growth. Difference scores are also fair, being unbiased estimates of true change, whereas residual change scores, according to Rogosa, are biased, not very precise, and no more reliable. Having said that, the results from analyses using difference scores and residual change scores were very similar for both studies. Finally, we have reduced measurement error at baseline by obtaining two assessments of well-being 1 week apart.

⁵ Interestingly, however, students who had self-selected into a study about happiness were more likely to continue engaging in their assigned activity at the 6-month follow-up than students who had not self-selected ($M_s = 3.17$ vs. 2.55), $t(212) = 2.51$, $p = .01$, $r = .17$).

Table 1

Means (SDs) for Changes in Well-Being by Experimental Condition (Top) and by Self-Selection Group (Bottom)

Contrast weight (λ)	Experimental condition					
	Optimism (+1)		Gratitude (+1)		Control (−2)	
	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>
Baseline to postintervention	−.02 (.56)	111	+.06 (.62)	107	−.07 (.57)	101
Baseline to 6-month follow-up	+.03 (.73)	66	+.17 (.77)	76	−.05 (.69)	72

Contrast weight (λ)	Self-selection group			
	Non-self-selected (−1)		Self-selected (+1)	
	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>
Baseline to postintervention	−.13 (.68)	94	+.04 (.54)	225
Baseline to 6-month follow-up	−.08 (.78)	58	+.10 (.71)	156

The Role of Effort

As noted above, we used hierarchical regression analyses to test the impact of coder-rated effort on well-being in the context of our happiness-enhancing activities. Step 1 predicted the change in postintervention well-being from baseline using a variable representing the treatment conditions versus the control condition and a centered effort variable. Step 2 included the interaction term between effort and condition. Supporting Hypothesis 4, Step 1 was significant ($R^2 = .02, p = .04$), with effort significantly predicting well-being ($\beta = .12, p = .03$), but condition not significantly predicting well-being ($\beta = .06, p = .29$). Supporting Hypothesis 5, in Step 2, the interaction term marginally significantly increased R^2 ($\Delta R^2 = .011, \beta = .16, p = .06$). As shown in Figure 1, when examining the simple slopes, the effect of effort was significant for the experimental conditions ($\beta = .19, p = .004$), but not for the control condition ($\beta = .006, p = .95$). This suggests that the amount of effort used to express optimism or gratitude affects subsequent gains in well-being, but does not have a significant effect when the task is neutral or less meaningful.

Summary

Individuals who selected themselves into a happiness intervention and completed one of the treatment conditions (i.e., expressing optimism or gratitude) reported the greatest boosts to their happiness both immediately postintervention and 6 months later. Further, the amount of effort that participants applied to the interven-

tion activities of conveying gratitude or imagining their best possible futures was directly related to improvements in their subsequent well-being, and, again, this effect occurred only in the treatment conditions, not the control condition.

Discussion

According to our model of well-being change (Lyubomirsky, Sheldon, et al., 2005; Sheldon & Lyubomirsky, 2004), sustainable increases in happiness are possible, but only if pursued under optimal conditions, such as when people are motivated to perform a positive activity, when they bring to bear effort and persistence, and when the activity is a legitimately efficacious one. Our study explored these arguments by testing several related hypotheses. First, we tested whether practicing optimism and gratitude would increase well-being both in the short-term (i.e., immediate postintervention) and over a longer period of time (i.e., 6 months later). More important, however, we wanted to know not just *whether* our activities were effective, but rather *how* a given activity might be working. To this end, we examined two metafactors thought to influence the success of any happiness-enhancing strategy—specifically, whether the individual engaging in an appropriate activity possesses the expectation and motivation to become happier and commits exerted effort into it.

Were the Positive Activities Effective?

Previous research has shown that the effortful practice of visualizing one's best possible future selves and writing gratitude

Table 2

Means (SDs) for Changes in Well-Being in All Six Groups

Contrast weight (λ)	Optimism				Gratitude				Control			
	Non-self-selected (0)		Self-selected (+1)		Non-self-selected (0)		Self-selected (+1)		Non-self-selected (−1)		Self-selected (−1)	
	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>	<i>M</i> (<i>SD</i>)	<i>n</i>
Baseline to postintervention	−.12 (.71)	32	+.02 (.49)	79	−.13 (.79)	31	+.15 (.53)	76	−.15 (.54)	31	−.04 (.59)	70
Baseline to 6-month follow-up	−.04 (.76)	16	+.06 (.73)	50	+.10 (.79)	20	+.19 (.76)	56	−.27 (.79)	22	+.04 (.62)	50

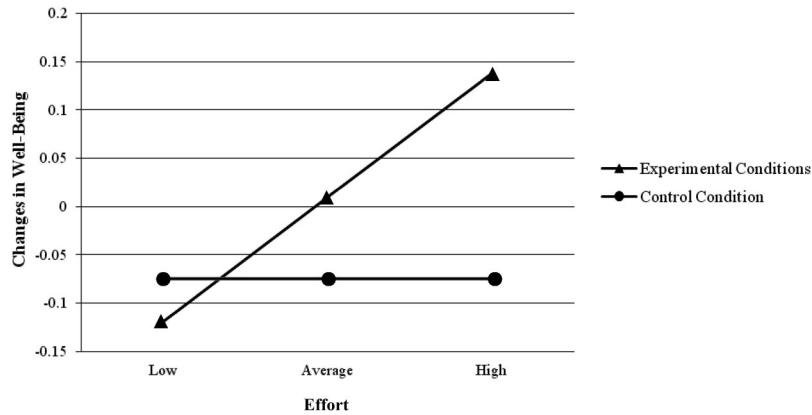


Figure 1. Effect of experimental versus control conditions on changes in well-being based on degree of effort.

letters leads to improved health and well-being (King, 2001; Sheldon & Lyubomirsky, 2006b; Seligman et al., 2005). At first glance, the results of our research fail to provide support for these earlier findings. After all, in the current study, which included both participants who selected themselves into a “happiness-boosting intervention” and those who pointedly did *not*, students who expressed optimism and gratitude did not report reliable increases in well-being relative to controls—both immediately after the intervention and at the 6-month follow-up.

Of course, these main effects come with a considerable qualification, as we would expect them to be modified by several critical boundary conditions. In other words, our participants needed both a “will” (i.e., appropriate expectations, motivation, or perhaps social/cultural support) and a “proper way” (i.e., an effective happiness-enhancing strategy). In our study, the “proper way” was specified by the treatment (or “active” experimental) conditions, relative to the inert control. The “will” was represented by the initial self-selection factor and by the sustained effort variable. The results for these factors, as well as their implications, are discussed below.

Becoming Happier Takes Both a Will and a Proper Way

Self-selection. Seligman et al.’s (2005) findings that several happiness-enhancing activities increase well-being for up to 6 months were obtained with a highly self-selected group of individuals—namely, those who sought out a Web site specifically focused on improving well-being and who then chose to participate in a study purported to make them happier. Thus, it is unclear whether these activities were generally effective for the average person or whether they “worked” only because the participants who practiced them were particularly driven and committed to laboring to become happier. Shedding new light on these findings, our study found a significant main effect of self-selection on improvements in well-being. Specifically, relative to their non-self-selected counterparts, participants who self-selected into a happiness study reported improved overall well-being at the end of the intervention (though the effect was not significant at the 6-month follow-up).

In addition to examining the main effect of participants’ choice to improve their happiness on well-being outcomes, we were especially interested in whether such a choice would influence the

effect of practicing optimism and gratitude in particular. Indeed, whether a person was motivated to work to become happier proved critical to the capacity of these two activities to improve well-being. That is, immediately at the end of our intervention, whether or not practicing happiness activities led to improvements in overall well-being relative to engaging in the control assignment depended on participants’ motivation to be happier. Moreover, this same pattern was still evident 6 months after the study was complete. That is, both right after the intervention and as long as 6 months subsequent to it, self-selected students who practiced one of our two happiness activities were still reporting the greatest gains in well-being relative to non-self-selected students who practiced one of these activities and relative to controls.

This last finding is especially revealing. If the effects of any activity are a mere artifact of people’s expectancies and motivation regarding that activity, then it should not matter what activity is practiced. By contrast, we found that one has to be engaged in doing the “right” thing, in addition to simply believing or hoping that it will be effective. This is an important piece of evidence for practitioners of positive psychology, as well as the growing number of health and wellness programs in health care facilities and workplaces.

In sum, our results suggest that the motivation or “will” to become happier is critical to the ability of a positive activity—in this case, the practice of writing about one’s best possible future or composing gratitude letters—to improve well-being. One potential explanation is that the expression of optimism and gratitude (or any happiness-enhancing strategy) simply is not as meaningful or as useful to people who are not motivated to practice these activities. That is, perhaps these individuals are already relatively well-adjusted and, therefore, do not have as much to gain by engaging in such activities. Providing support for this argument, Seligman et al. (2005) found that participants who were presumably very motivated to become happier were, on average, also slightly depressed when entering the study. Another study from the same laboratory revealed that positive psychological activities are particularly effective at alleviating depressive symptoms in severely depressed individuals (as compared to mild to moderately depressed ones), suggesting that these activities may work best for those who are already struggling with their mental health (Seligman et al., 2006). However, in the current sample, no differences were

observed between self-selected and non-self-selected study participants on any of our baseline measures of well-being.

If our non-self-selected participants were not any less generally adjusted than their self-selected counterparts, what explains their apparent inability to benefit from positive activities? We speculate that a relative lack of intrinsic interest in working to be happier—and, perhaps, even skepticism about the pursuit of ever-greater well-being—may be driving this difference. Indeed, it is possible that non-self-selected participants felt “forced” to participate in the happiness-enhancing study, because they had opted against registering for such a study when given the chance. Self-determination theory (Deci & Ryan, 1985, 2000) suggests that such “controlled” motivation can adversely affect individuals’ abilities to find meaning and purpose in an activity, which, in turn, can ultimately prevent them from gaining any benefit from the activity.

Effort. Positive expectancies and initial motivation are vital to any positive activity’s success, but so is the labor required to carry it out. Accordingly, we also examined the impact of effort on gains in well-being. We conceptualized effort, it should be noted, not as an indicator of compliance (i.e., whether one is doing the assignments each week) or as an indicator of skill (i.e., whether one’s labors lead to superior accomplishment of the assignments), but rather as an indicator of how seriously students appeared to be practicing these assignments. Indeed, judges’ ratings of the essays revealed that, across all conditions, students wrote equally lengthy essays each week. However, we sought to determine whether the effort evident in these written assignments would play a role in the success of the positive exercises in bolstering well-being. As predicted, we found evidence to suggest that participants who exerted relatively more effort when practicing their assigned activity (i.e., those who were taking the activity seriously) were more likely to report gains in well-being. Even more important, as Figure 1 depicts, the effect of effort was only apparent in the treatment conditions, suggesting that, analogous to the results for positive expectations or motivation, effort matters, but only when one is doing the right thing.

Our findings regarding the importance of effort are empirically supported by work on goal pursuit and attainment (Brunstein, 1993). That is, the effect of goal attainment on well-being has been found to depend on how committed (i.e., determined and willing) one is to achieve a given goal. Although commitment may be, at best, only a proxy for our assessment of effort, we believe that the two constructs are closely related. Presumably, if you use effort, are committed, and take seriously the positive activity you are practicing, the activity should be more effective. Indeed, we found this to be the case. Our findings are also corroborated by Sheldon and Lyubomirsky (2006b) and Seligman et al. (2005), both of whom found that continued practice of happiness activities led to enhanced well-being. Presumably, people who exert effort into performing an activity do so because they believe that the activity is worthwhile, meaningful, and/or enjoyable.

Caveats and Limitations

Several potential limitations to this research, which primarily stem from data collection procedures and design, need to be addressed. First, all the dependent measures in our experiment were self-reported and thus raise the possibility of social desirability and response biases. Then again, most of our measures—for

example, mood, satisfaction with life, and happiness—are, by definition, tapping subjective constructs. Indeed, it could be argued that when it comes to understanding happiness, people’s subjective appraisals are the gold standard of measurement.

Another limitation concerns our use of a convenience sample—undergraduate university students. Although our sample was particularly ethnically and socioeconomically diverse, only 17% of whom were Caucasian, future research should be conducted with a more representative set of participants to permit generalization of our results to individuals from a range of age levels, educational attainment, and occupations. A particularly important question concerns whether *cultural* membership would moderate the effects of any happiness-increasing intervention. The pursuit of happiness is a staple ideology in Western culture, and non-Western or collectivist cultures tend to downplay the significance of happiness (Diener & Suh, 1999; Triandis, 1995), possibly because such cultures emphasize group and family harmony over individual goals and personal agency. Hence, testing whether our intervention activities can deliver benefits in participants from non-Western cultures—the aim of one of our recent studies (Boehm, Lyubomirsky, & Sheldon, 2010)—should be a priority for future researchers.

Further, as previously mentioned, we chose to create a self-selection factor by allowing participants to select themselves into appropriate categories. As a result, definitive causal claims cannot be made about the effect of self-selection (viz., motivation) on the benefits of practicing happiness activities. Thus, the possibility exists that self-selected and non-self-selected participants (much like those who buy self-help books and those who do not) may differ in other ways (aside from motivation or positive expectancies), which might confound our findings. Fortunately, we found no baseline group differences in well-being between self-selected and non-self-selected study participants. However, it is possible that those who sign up for a “happiness study” and those who actively chose *not* to sign up for such a study differ in some other unknown personality characteristic, such as openness to experience (e.g., perhaps non-self-selected people are particularly skeptical about a study labeled as a “happiness intervention”). Further research would benefit from exploration of this issue.

Finally, it is important to note that although some of the effect sizes found in these studies are considered to be small by conventional standards (Cohen, 1988; our *rs* spanned the low to mid .10s), such small effects are comparable or even larger than those found for critical or life-saving treatments, such as tamoxifen for breast cancer ($r = .04$), aspirin for preventing heart attacks ($r = .03$), and the Salk vaccine for polio ($r = .01$) (Rosenthal & Rosnow, 2008).⁶ To be sure, the present findings—stemming from a single positive behavioral intervention—do not belong to the same category as such clinical treatments with respect to their weight, consequence, and import. Nevertheless, others have pointed out that even a very small statistical effect can be important when the independent variable is relatively weak (here, a cost-effective [i.e., “free”] activity, which is unlikely to produce harm and takes a mere 15 min per week to perform), when the dependent variable may be

⁶ Rosenthal and Rosnow (2008) presents a long, remarkable list of studies that have produced findings that, despite having small statistical effect sizes, are justifiably interpreted as enormously useful and important (see pp. 325–326).

particularly resistant to change (here, probable for outcomes such as satisfaction with life and happiness; Prentice & Miller, 1992), and when the population is nonclinical (thus, perhaps evidencing ceiling effects). Indeed, that self-selected individuals maintain gains in well-being up to 6 months subsequent to practicing gratitude or optimism for just 15 min a week for an 8-week period may be viewed as a rather remarkable result.

Conclusions and Future Questions

Research in the field of positive psychology is finally beginning to systematically test happiness interventions to investigate the impact of activities such as expressing optimism and gratitude, committing acts of kindness, and using personal strengths—a goal that Fordyce (1977, 1983) set forth several decades ago, but that was not taken up until quite recently. Whereas previous interventions to improve well-being have focused on the overall effectiveness of particular happiness activities, the present research sought to advance beyond this question by examining the factors predicted by our model of well-being change that might moderate the effect of such interventions on improvements in well-being.

First, and most important, we found that to become happier, people need both a will and a proper way. The will can come from motivation, expectations, and diligence. The proper way comes from performing the “right” activity, not merely a placebo. Accordingly, we found that motivation and investment in becoming a happier person matters. That is, expressing gratitude and optimism did not generally increase well-being *unless* a person was truly cognizant of the exercises’ purpose and motivated to improve his or her happiness. Second, effortful pursuit of happiness activities was found to be important to improving and maintaining well-being.

The above-mentioned findings address the “how” of pursuing happiness, but what about the “why”? In addition to replicating and extending our initial examination of the optimal conditions for happiness-enhancing interventions, future studies should develop an account of why such interventions work. Possible mediators underlying gains in well-being include positive events that occur over the course of the intervention, as well as the satisfaction of significant human needs. For example, does considering one’s best possible future selves or writing gratitude letters foster greater long-term happiness because it jump-starts an upward spiral of positive and need-satisfying experiences (cf. Fredrickson, Cohn, Coffey, Pek, & Finkel, 2008)?

Research from our laboratory has begun to explore such questions (e.g., Lyubomirsky & Boehm, 2010), but much remains unknown about the role of vital factors in the success of happiness-bolstering activities. What function do optimal timing, frequency, and variety of a particular positive activity have in inhibiting hedonic adaptation and thereby increasing its well-being benefits (cf. Boehm & Lyubomirsky, 2009; Lyubomirsky, Sheldon, et al., 2005; Sheldon et al., in press)? How might positive reinforcement from friends and family—as well as one’s larger culture—moderate one’s success at happiness activities (cf. Boehm et al., 2010)? Might combining two or more activities produce additive or interactive effects? And, what is the optimal level of practicing activities, such as optimism and gratitude, such that one does not become overly optimistic or “too grateful” (cf. Oishi, Diener, & Lucas, 2007)? To the extent that we can understand why, how, and

when these activities work to improve happiness, we can optimize the conditions under which such activities are ultimately practiced in real-world settings.

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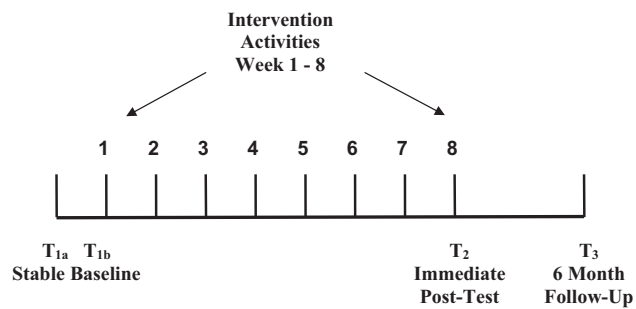
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(Appendix follows)

Appendix

Study Timeline



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