Savour Good Moments, Practice Mindfulness, and Reframe the Bad: A Randomized Controlled Trail (RCT) of the Mental Wellbeing Toolkit

by

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ABSTRACT

SAVOUR GOOD MOMENTS, PRACTICE MINDFULNESS, AND REFRAME THE BAD: A RANDOMIZED CONTROLLED TRAIL (RCT) OF THE MENTAL WELLBEING TOOLKIT

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This study examined whether an emotion regulation intervention promotes wellbeing beyond placebo effects. University students were randomly assigned to 10 days of self-practice in an intervention, placebo, or control condition. The intervention provided three 5-minute audio recordings of mindfulness, savouring, and positive reappraisal exercises. The placebo and control conditions provided “sham” exercises of breathing, distant memory recall, and recent memory recall. The intervention and placebo conditions were primed with expectancy beliefs, while the control was not. Multilevel growth modeling results showed all conditions declined in stress and negative affect and increased in positive affect and wellbeing up to 1 month later. Compared to placebo, the intervention led to a sharper positive growth in emotion regulation over time, with no other outcome differences. The placebo also showed a sharper positive growth in emotion regulation over time than the control. Suggestions are provided for future interventions to work with extant placebo effects.
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Savour Good Moments, Practice Mindfulness, and Reframe the Bad: Practicing Emotion Regulation with the Mental Wellbeing Toolkit

In today’s ambitious and fast-paced academic and working world, students and employees alike face high demands of busy schedules, telepressures, and balancing work and personal life. Such demanding environments can be triggers for stress, depression, anxiety, and burnout if not managed effectively (Carl, Fairholme, Gallagher, Thompson-Hollands, & Barlow, 2013; Demerouti, Bakker, Nachreiner, & Schaufeli, 2001; Barber & Santuzzi, 2014; Furr, Westefeld, McConnell, & Jenkins; 2001). One of the most well established ways to personally lessen emotional impact and increase positive functioning in the face of environmental demands is with effective emotion regulation, a process applying various cognitive, behavioural, and emotional strategies deliberately or implicitly to change the type, timing, and intensity of emotions (Gross, 1998; Gross; 2015). Recent intervention studies show that brief emotion regulation training requiring as few as one to two weeks can decrease stress, negative affect, and depressive symptomology (Cavanagh et al., 2013; Denny & Ochsner, 2014), as well as improve work performance and job satisfaction in diverse applied settings (Hülsheger, Alberts, Feinholdt, & Lang, 2013; Hülsheger, Lang, Schewe, & Zijlstra, 2014b).

Past emotion regulation interventions have heavily focused on training isolated techniques, predominantly positive reappraisal, despite the potential to practice and combine a number of emotion regulation techniques to increase (i.e., up-regulate) and decrease (i.e., down-regulate) any emotional experience with situation-specific approaches. In addition, many studies have been conducted in the lab, with limited implications for the feasibility and effectiveness of self-practice in daily life. To understand how to improve the viability of emotion regulation interventions for student health promotion, researchers can now explore the potential for multi-
component interventions that can be tailored to personal preferences and self-administered conveniently through technology. Combining well-established techniques can harness greater person-intervention fit, enjoyment, and utility for improved emotion regulation in more types of daily situations (Gross, 2015; Lyubomirsky & Layous, 2013). One promising path is through the exploration of emotion regulation processes in mindfulness-based and positive psychology interventions, which to date have mostly been studied in separation from the emotion regulation literature (Quoidbach, Mikolajczk, & Gross, 2015).

The current design will address these important gaps by testing a brief, self-administered multi-component emotion regulation intervention and rigorously exploring why changes take place. By integrating emotion regulation theory (Gross, 2015) and building upon past interventions, the current study will examine whether a toolkit of complementary emotion regulation strategies can promote mental wellbeing feasibly in an applied setting. The proposed intervention, “The Mental Wellbeing Toolkit,” will train participants in empirically established techniques combining traditional emotion regulation strategies with mindfulness and positive psychology practices. The current design will also tackle the need for psychological interventions to rigorously examine placebo effects (Boot, Simons, Stothart, & Stutts, 2013). Finally, a diary methodology will be utilized to examine gradual changes within-person and between-conditions for improved understanding of processes of change and intervention experiences. In all, the current study will advance the knowledge of processes behind emotion regulation interventions and provide richer understanding of emotion regulation trainability with a user-friendly, customizable intervention.

In the following sections, we begin by outlining emotion regulation theory and research as a lens for the proposed Mental Wellbeing Toolkit intervention. Secondly, we delineate how
the techniques of positive reappraisal, mindfulness, and savouring from the Mental Wellbeing Toolkit can form a complementary multicomponent intervention for training adaptive emotion regulation. Finally, the hypothesized outcomes of gradual mental wellbeing promotion via accumulation of effective emotion regulation practice will be explained.

**Emotion Regulation**

Emotions are conceptualized as specific positively or negatively valenced affective states (Barrett, Mesquita, Ochsner, & Gross, 2007; Gross, 2015). They elicit response tendencies and are often triggered by specific events. This definition sets emotions apart from moods, which are more generalized and enduring affective climates; and stress responses, which are specific affective reactions to environmental demands (Gross, 2015). Despite the differences, all three types of affective states correlate with one another and can be influenced by emotion regulation strategies (Gross, 2015). Whereas in the present document we will refer to emotion regulation for clarity, the idea of an overall affect regulation – comprised of general moods, specific emotions, and stress-related affective reactions – is implied.

According to the process model of emotion regulation (Gross, 2015; Quoidbach et al., 2015) an emotion regulation strategy is the deliberate or implicit altering of emotional impact (Gross 2015). The model explains that emotion regulation strategies span the entire cycle of the emotion trajectory, from selecting the situation that elicits emotion, to altering attention towards the situation, followed by appraisal of the situation, and finally changing the emotional response. While the utility of these varying emotion regulation stages can depend greatly on individual and situational needs, the process model of emotion regulation can guide interventions for selecting the most beneficial strategies for mental wellbeing promotion (Gross, 2015). Within applied settings, such as schools and organizations, individuals may not always have autonomy over
altering contextual factors. Thus, while actively selecting and modifying environments can be a fruitful avenue when feasible, there may be greater applicability for individual-level emotion regulation interventions to manage emotions in the face of situational demands.

Specifically, individual-level emotion regulation strategies that are antecedent-focused (i.e., taking place before the emotional response fully unfolds over time) have been empirically supported as most effective for sustained physical and psychological health benefits (Gross, 1998; Gross; 2015). Antecedent-focused strategies take place earlier in the emotion trajectory, and thus require less cognitive effort than regulation after negative emotional reactions transpire (Gross; 2015). In terms of practical value, proactive health promotion with antecedent-focused emotion regulation works as primary prevention, which provides greatest utility per dollar spent as compared to later stages of intervention (L’Abate, 2007). Given the high potential of antecedent-focused emotion regulation to promote mental wellbeing, the following sections will describe the development of a multicomponent intervention comprised of three emotion regulation techniques that have been found to relate to as well as promote mental wellbeing: positive reappraisal, mindfulness, and savouring.

These three strategies have been presented in the literature as antecedent-focused emotion regulation approaches for their buffering effects against stress appraisal in everyday life as well as distress after trauma (Garland, Gaylord, & Fredrickson, 2011; Larsen & Berenbaum, 2015). Longer-term practice of these strategies is associated with more automated emotion regulation brain activation (Chiesa, Serretti, & Jakobsen, 2013; Garland et al., 2010), which can proactively lead to the building of healthier habits and lifestyles (Fredrickson, Cohn, Coffey, Pek, & Finkel 2008). Three techniques were selected to provide users with a well-rounded toolkit to both up-regulate and down-regulate emotions readily in daily life, without providing excessive options
that could potentially overburden user experience (Schwartz, 2004; Schueller & Parks, 2012). The utility of combining these unique yet complementary techniques into a parsimonious and practical toolkit for daily life is explained in the following sections.

**Positive Reappraisal**

The Mental Wellbeing Toolkit teaches positive reappraisal, one of the most empirically supported antecedent-focused emotion regulation strategies for improving mental wellbeing (Gross, 2015; Denny & Ochsner, 2014). Also known as benefit finding, positive reappraisal involves a cognitive re-evaluation of the threatening aspects of a negative event to examine a situation as non-threatening or even beneficial for learning or personal growth (Gross, 2015; Folkman, 1997; 2008). Positive reappraisal is a type of meaning-focused coping, involving focus on one’s beliefs, values, and goals to motivate and sustain adaptive coping in the face of environmental stressors (Folkman, 2008). The generation of meaning fosters motivation to defy stressors, in turn promoting accrual of positive emotions and wellbeing (Folkman, 1997; Folkman, 2008).

Supporting the utility of meaning-focused coping, positive reappraisal has received wide empirical support as an adaptive emotion regulation strategy for promoting mental wellbeing (Gross & John, 2003). Laboratory evidence suggests that following positive reappraisal instructions is more effective for decreasing negative emotions than the strategy of emotional suppression (Mcrae et al., 2009; Kalokerinos, Greenaway, & Denson, 2015). For instance, in a within-persons design, completing reappraisal compared to distraction instructions in response to negative stimuli led to greater decreases in state negative affect and increased activation of brain regions associated with processing affective meaning (Mcrae et al., 2009). Similarly, in two large-scale studies with over 1,300 employees, while no differences were found for positive
emotions, the reappraisal condition reported significantly less sadness while viewing a sad film than the suppression and control conditions (Kalokerinos et al., 2015). Conversely, another study suggests that reappraisal training can in fact increase positive emotions, as well as smiling and forgiveness in interpersonal conflicts (vanOyen Witvliet, DeYoung, Hofelich, & DeYoung, 2011).

In regards to longer-term benefits, studies indicate that positive reappraisal is associated with lower distress, greater resilience, and post-traumatic growth in the face of stressful life events (Folkman, 1997; Larsen & Berenbaum, 2015). As well, an RCT found that four training sessions in distancing reappraisal (i.e., objective, third-person observation of stressors) led to decreased stress and negative affect in healthy adults, as compared to reinterpreting (i.e., altering the story behind stressors) or a control (Denny & Ochsner, 2014). Together, the literature presents positive reappraisal as a valuable and feasible strategy for buffering against stress and negative affect from difficult or unpleasant situations. However, as this is a specific strategy for regulating emotions in the face of negative events, other techniques can supplement positive reappraisal for more complete emotion regulation training.

**Mindfulness**

Mindfulness is a practice most widely conceptualized as present moment awareness with a curious, accepting, and non-judgmental approach (Bishop et al., 2004; Brown, Ryan, & Creswell, 2007; Kabat-Zinn, 1990) and may act as an important addition to positive reappraisal practice (Garland et al., 2011). The majority of mindfulness interventions are adapted from the well-established programs of Mindfulness Based Stress Reduction (MBSR; Kabat-Zinn, 1990) and Mindfulness Based Cognitive Therapy (MBCT; Segal, Williams, & Teasdale, 2013). MBSR and MBCT teach non-judgmental awareness through exercises such as yoga, body scan
relaxations, and guided meditations infused with self-compassion and acceptance themes. Support for mindfulness in reducing distress symptoms is vast and consistent (for a meta-analysis see Hofmann, Saywer, Witt, & Oh, 2010). Moreover, mindfulness training has been shown to improve positive functioning. Examples from non-clinical applied samples show that mindfulness interventions can foster psychological detachment from work (Michel, Bosch, & Rexroth, 2014), which can improve sleep quality (Hülsheger et al., 2014a), satisfaction with work-life balance (Michel et al., 2014), and positive affectivity (Fredrickson et al., 2008).

Empirical and theoretical research suggests that mindfulness benefits are mediated through antecedent-focused emotion regulation (Quoidbach et al., 2015; Gu et al., 2015; Hölzel et al., 2011; Cheisa, Serretti & Jakobsen, 2013). Specifically, the practice of mindfulness allows one to refrain from reactive thoughts and attribution habits to instil a more objective and accepting attentional focus (Quoidbach et al., 2015). Support from magnetic resonance imaging studies show that practicing guided mindfulness as compared to focusing on the breath alone (Zeidan, Martucci, Kraft, McHaffie, & Coghill, 2013), or participating in a stress management course (Hölzel et al., 2013), leads to activation and structural change in brain regions responsible for emotion regulation, such as the ventromedial prefrontal cortex.

Positive reappraisal is theorized to promote greater benefits when coupled with mindfulness, suggesting important combined utility of these two strategies for future emotion regulation interventions. According to Garland and colleagues’ (2011) mindful coping model, mindfulness allows the perceiver to distance themselves from their thoughts and feelings (i.e., decentering), creating an optimal foundation for the mind to broaden attention and subsequently reframe a seemingly negative situation with greater ease. Preliminary empirical evidence supports the mindful coping model and complementary utility between mindfulness and positive
Specifically, greater use of positive reappraisal was found to mediate the development of dispositional mindfulness in an eight-week mindfulness intervention (Garland et al., 2011). This relation is likely bi-directional, as it has also been shown that state mindfulness is mediated by improvements in positive reappraisal after a one-week intervention versus placebo (Garland, Hanley, Farb, & Froelier, 2013). While further research is necessary to directly compare the additive effects of multicomponent interventions, preliminary findings suggest that assignment to mindfulness and positive reappraisal practice combined, compared to positive reappraisal alone, leads to greater positive affect in university students (Craig, Pogrebtsova, Chris, & González-Morales, 2015). Thus, we expect to find similar utility in coupling the mindfulness technique with positive reappraisal in the proposed Mental Wellbeing Toolkit.

**Savouring**

Just as mindfulness and positive reappraisal can be applied as emotion regulation strategies to lessen negative emotions, both tools can also up-regulate positive emotions (Fredrickson et al., 2008; Folkman, 2008). However, because mindfulness and reappraisal do not fundamentally capitalize on positive events, there is need for another complementary technique with exclusively positive focus in a multicomponent emotion regulation intervention.

Incorporating a tool from the positive psychology literature can address this need. Positive psychology, known as the study of human strengths, flourishing, and positive institutions, is a rapidly emerging field that explores various low cost and self-administered interventions with a unique focus on increasing positive functioning (Seligman & Csikszentmihalyi, 2000; Seligman, Steen, Park, & Peterson, 2005). Positive psychology interventions utilize techniques that optimistic individuals naturally engage in regularly, such as appreciating enjoyable moments and practicing gratitude (Sin & Lyubomirsky, 2009). Two
recent positive psychology meta-analyses show that these techniques consistently increase positive affect and happiness with small to moderate effect sizes (Sin & Lyubomirsky, 2009; Bolier et al., 2013).

The Mental Wellbeing Toolkit will utilize the positive psychology technique of savouring, defined as intentionally generating, prolonging, and extending positive affect of pleasant moments (Bryant & Veroff, 2007). Savouring is theorized to promote wellbeing by triggering broad emotion regulation strategies of attention deployment (e.g., focusing on positive aspects), cognitive control (changing appraisal of the environment to perceive it as more positive), and response modulation (increasing the intensity of already positive emotions; Bryant & Veroff, 2007; Quoidbach et al., 2015). With similarity to mindfulness, savouring reduces habitual aesthetic judgments of daily events, and instead encourages appreciation of even the smallest and simplest everyday moments (Bryant & Veroff, 2007). Interventions utilizing savouring exercises and programs have shown support for promoting mental wellbeing across applied samples, particularly when savouring recent as compared to distant memories (Bryant & Veroff, 2007; Joormann, Siemer, & Gotlib, 2007; McMakin, Siegle, & Shirk, 2011; Bahník, Vranka, & Dlouhá, 2015). One exemplary three-week diary study by Bono, Glomb, Shen, Kim, and Kosh (2013) found that an end of the day positive reflection was related to health care employees’ increased physical health, work detachment, and decreased stress in the evenings after work. Thus, savouring acts as another important antecedent-focused emotion regulation strategy to personally promote positive functioning even in difficult or stressful environments.


**Emotion Regulation Skills Learning or Placebo Effect?**

Despite the vast experimental and intervention literature demonstrating wellbeing benefits from training these techniques of reappraisal, mindfulness, and savouring (e.g., Bono et al., 2013; Denny & Ochsner, 2014; Hofmann et al., 2010; Hülsheger et al., 2014b; Mcrae et al., 2009), it is less clear whether learning of emotion regulation skills actually occurs. It could very well be that hidden placebo effects are at play, even in studies with comparison to placebo conditions. A notable criticism of psychological intervention research is that despite placebo and weight-list control conditions, the degree to which placebo participants are in fact matched on expectancy beliefs for improvement with the experimental condition is largely under-investigated, or under-reported (Boot et al., 2013). When experimental conditions appear as more desirable, interesting, or beneficial than their comparison placebo conditions, it becomes inconclusive whether significant differences in post-study outcomes are due to the actual intervention materials, or extraneous placebo benefits (Boot et al., 2013; Temple & Ellenberg, 2000). Potential placebo effects encompass the full spectrum of “positive care effects”, known as any beneficial components extraneous to the active ingredients in the treatment itself (Blease, 2011). Likely positive care effects in psychology interventions include the “Hawthorne Effect” of improvement from awareness of being observed, study participation as a distraction from daily stressors, and social support from researchers and other participants, to name a few.

Importantly, there is theoretical and emerging empirical evidence to suggest that emotion regulation skills above and beyond placebo effects can in fact be trained through interventions. Notably, the broaden and build theory of positive emotions (Fredrickson, 2001) suggests that emotion regulation and positive emotions can accrue over time with practice. According to the “broaden” component of the theory, the experience of positive emotions triggers a sense of
safety in the environment, generating behavioural flexibility, defined by vulnerability, variety, and openness. Next, the “build” component suggests that this flexibility prompts building of physiological, social, or psychological resources over time. For instance, the experience of enjoyment, gratitude, and interest can trigger students’ perceptions that their campus and classroom are a safe and positive space for learning and engagement, influencing proactive actions such as creating new friendships and engaging in extracurricular activities. Over time with this increased proactiveness, students can continue the “building” of personal (e.g., knowledge, emotion regulation skills) and environmental (e.g., friendships) resources that perpetuate positive emotions. Thus, the broaden and build process promotes self-perpetuating and self-maximizing systems of bi-directional accrual of positive emotions and resources, known as upward spirals, to foster wellbeing (Fredrickson, 2001; Garland et al., 2010).

Field studies support the broaden and build theory, showing that interventions are not only a vehicle for generating positive affect, but can also build abundant resources over time. Frederickson and colleagues (2008) found that a loving kindness meditation intervention not only led to increased positive emotions, but that resource building (e.g., mindfulness, savouring, environmental mastery, self-acceptance, purpose in life, social support) fully mediated the relation between positive emotions and life satisfaction. Thus, a key to the sustainment of upward spirals is resource building, a process that can be targeted with the development of effective research-based interventions. In line with Frederickson’s (2001) theory and empirical evidence (Frederickson et al., 2008), it is expected that the proposed Mental Wellbeing Toolkit, which trains emotion regulation strategies in order to up-regulate positive emotions and down-regulate negative emotions, will promote wellbeing by broadening the positive emotional experience and building emotion regulation resources over time.
The Current Study

The current study will contribute to the health promotion intervention research by (a) testing the effectiveness of a brief, multicomponent intervention combining a host of current suggestions from interdisciplinary health promotion literature, (b) examining gradual emotion regulation processes of change and user intervention experiences through rich, diary methodology which provides more data points, and (c) rigorously testing potential placebo effects that have been underexplored in previous designs.

Firstly, this study will combine mindfulness, savouring, and positive reappraisal exercises into a customizable, self-administered toolkit for added effectiveness. These techniques have been examined in isolation as fruitful for wellbeing promotion (e.g., Mcrae et al., 2009; Zeidan et al., 2013) and several interventions have combined techniques such as mindfulness and positive reappraisal (e.g., “mindful-positive reappraisal”; Craig et al., 2015) and gratitude, savouring, and mindfulness (e.g., “loving-kindness meditation”; Fredrickson et al., 2008), suggesting complementary benefits of a multicomponent approach. However, to our knowledge, this will be the first examination of a program specifically targeted at empowering users to practice up-regulation of positive emotions coupled with down-regulation of negative emotions in their daily lives in a customizable and flexible way. Such a toolkit can provide users with added utility for promoting daily wellbeing by harnessing key intervention effectiveness moderators of person-intervention fit, autonomy, and intrinsic motivation (Lyubomirksy et al., 2011; Lyubomirsky & Layous, 2013; Peters & Calvo, 2014).

While there is increasing research on the utility of various self-administered health promotion interventions in general (for meta-analytic support see Cavanagh, Strauss, Forder, & Jones, 2014), further exploration of effectiveness over shorter periods of time outside of the
standard 6-8 week format is needed to help with feasibility in applied settings. The 10-day period for this program was chosen based on emerging findings from several self-administered, low-dose interventions suggesting that roughly two weeks of practice is sufficient to promote wellbeing (e.g., Cavanagh et al., 2013; Denny & Ochsner, 2014; Hülsheger et al., 2014b). Our daily measures will allow us to perform growth analysis of the pattern of daily incremental changes over the course of the 10 days, which is currently unclear from pre-test/post-test designs. Such diary studies have been vastly underused in emotion regulation literature; however, the need to examine within-person fluctuations in stress, emotions, and overall subjective wellbeing has been readily called upon for greater ecological validity (Lazarus, 2000; Kahneman & Krueger, 2006).

In addition to improving intervention design, there is a need to delineate the mechanisms behind emotion regulation interventions. To understand how they work and actually train important and sustainable skills above and beyond placebo effects, research should examine mediating mechanisms over the course of the intervention (Gross, 2015; Gu, Strauss, Bond, & Cavanagh, 2015). Specifically, research is needed to explore if benefits result from the sheer short-term induction of positive emotions or even placebo effects, or if these interventions actually train effective emotion regulation habits incrementally and sustainably in line with emotion regulation theory (Gross, 2015; Boot et al., 2013). To assess gradual skill development, emotion regulation will be self-reported daily for two weeks across conditions. We will test this proposed emotion regulation mechanism in the intervention against scores in the randomly assigned matched-expectancy placebo and a non-matched expectancy control. This design will allow for a more accurate placebo understanding by teasing apart the power of outcome expectancy from training and other contextually driven changes (Temple & Ellenberg, 2000).
To build upon existing literature, the current study combines the process model of emotion regulation (Gross, 2015) and empirical research on the techniques of mindfulness, positive reappraisal, and savouring (Bryant & Veroff, 2007; Garland et al., 2011; Gross & John, 2003). Applied to the broaden and build theory of positive emotions (Fredrickson, 2001) and its empirical support (Fredrickson et al., 2008), the resource of emotion regulation is expected to build up over time, perpetuated by the up-regulation of positive emotions. Thus, to examine building of emotion regulation resources over the course of the intervention, as well as sustainment of these changes post-intervention, we propose the following hypotheses in relation to emotion regulation improvements:

**Hypothesis 1:** Use of the Mental Wellbeing Toolkit will lead to a sharper positive growth curve in daily emotion regulation over 10 days of practice, as compared to both the placebo and active control conditions (1a) and from baseline to the 1-month follow up, as compared to the placebo and active control conditions (1b).

This accrual of emotion regulation resources should perpetuate upward spirals of positive emotions and protect against negative emotions (Garland et al., 2010). Similar to upward spirals, loss spirals are the perpetuating loss of resources and decrease in wellbeing over time. Empirical evidence suggests that the upward spirals generated by positive emotions can have a countering effect against loss spirals (Carl et al., 2013; Garland et al., 2010; Fredrickson et al., 2008). Diary research supports that greater up-regulation of positive emotions in daily life is related to lower anxiety and depressive symptomology (Carl et al., 2013). As participants readily practice down-regulation of negative emotions and up-regulation of positive emotions, changes in overall negative and positive affect is expected (Gross, 1998). These affective changes should build up gradually along with the growth of emotion regulation, supported by the broaden and build
theory of positive emotions (Fredrickson, 2001; Fredrickson et al., 2008) and will be tested with the following hypotheses:

**Hypothesis 2:** Use of the Mental Wellbeing Toolkit will lead to a sharper negative growth curve (decrease) in negative affect over 10 days of exercises, as compared to the placebo and active control conditions (2a) and a positive growth curve in positive affect over 10 days of exercises, as compared to the placebo and active control conditions (2b).

The Mental Wellbeing Toolkit should not only produce affective changes but should also buffer against stress perception. Stress literature suggests that up-regulation of positive emotions can promote resilience and proactive coping while preventing maladaptive coping (e.g., rumination, worry, and emotional suppression) in difficult times (Folkman, 1997; 2008; Gross, 2015; Watkins, 2008; Nolen-Hoeksema, Wisco, & Lyubomirsky, 2008) However, given that we do not know the natural occurring change of perceived stressors in the study population during the time of data collection, the specific form of the interaction between condition and time on stress is not predicted. Stress buffering effects can be due to a flat slope in the experimental condition (perceived stressors are unchanged) in comparison to positive slopes in the placebo and active control conditions (perceived stressors increase), or a negative slope in the experimental condition (perceived stressors decrease) in comparison to flat or positive slopes in the placebo (perceived stressors are unchanged). Thus, we will test the following exploratory hypotheses to fully examine the buffering potential of the Mental Wellbeing Toolkit against perceived stress:

**Hypothesis 3:** A significant interaction between experimental condition and time over 10 days of exercises (3a) and from baseline to the 1-month follow-up (3b) will predict daily perceived stress. The form of the interaction should indicate lower levels of perceived stress over
practice time among the participants using the Mental Wellbeing Toolkit, as compared to the placebo and active control conditions.

To test whether these wellbeing outcomes are generated via increased emotion regulation use over the course of the intervention, supporting the process model of emotion regulation (Gross, 2015), the following mediation is hypothesized (see Figure 1 for the proposed mediation process):

**Hypothesis 4:** Daily emotion regulation will mediate the relation between condition assignment and levels of daily perceived stress (4a), negative affect (4b), and positive affect (4c) over the 10 days of exercises. Specifically, the negative relation between use of the Mental Wellbeing Toolkit and daily perceived stress and negative affect, and the positive relation with positive affect will be mediated by increased levels of daily emotion regulation.

Finally, we address long term-outcomes in overall mental wellbeing expected based on the broaden and build theory of positive emotions (Fredrickson, 2001). In the long run, this net increase in positive affect and decrease in stress appraisal and negative affect should promote overall improved mental wellbeing (Refer to Figure 2 for the proposed theoretical process; Gross, 2015; Fredrickson; 2001; Garland et al., 2011). Because our intervention targets the specific resource of emotion regulation which has been widely supported to promote up-regulation of positive and down-regulation of negative emotions (Gross, 2015), we should see that positive emotions directly trigger resource building and in turn, self-perpetuating and self-maximizing upward spirals of wellbeing outcomes resistant to habituation. Thus, given the gradual spirals of positive affect promotion and decreases in stress and negative affect, we also expect sustained mental wellbeing measured holistically longer-term:
Hypothesis 5: Use of the Mental Wellbeing Toolkit will lead to a sharper positive growth curve in mental wellbeing from baseline to the 1-month follow up, as compared to the placebo and active control conditions.

Method

Participants

A total of 130 undergraduate students from a Canadian University were recruited from advertisements around campus and an online participant pool system from the Department of Psychology. Sample size was based on a one-way ANOVA a-priori power analysis, with desired power of .80, $\alpha = .05$, and expected effect size of .30. This conservative effect size estimate was based on past meta-analytic results from self-administered interventions targeting mental wellbeing (Cavanagh et al., 2014).

Procedure

Participants signed up for the study titled “The Life and Wellness Study” and selected a 1-hour training session on campus from fifteen time-slot options. Upon registration, participants completed a 30-minute baseline survey online. Each training cluster, consisting of 5-8 participants, was randomly assigned to a placebo, control, or experimental condition using a random number generator. Roughly one week upon sign up and baseline survey completion, participants attended their training session to motivate engagement with intervention materials and surveys. Participants were taught the techniques for their assigned conditions and how to easily complete daily surveys and exercises online and on smart phones. Instructions were also provided on how to use a voluntary “extra practice” survey to allow participants to access all of their assigned guided exercises and further information for each technique at their convenience. The sessions were highly interactive and included group practice with examples relevant to
university students’ lives and sample exercises to practice independently. The primary researcher and the same research assistant conducted all training sessions to ensure consistency.

To induce the same expectancy beliefs as the experimental condition, deception was used in the placebo condition. Specifically, all techniques were presented to participants as having grounded wellbeing benefits. In reality, research shows that the current study’s placebo exercises mainly act on expectancy beliefs and do not produce the same benefits as positive reappraisal, mindfulness, or savouring (Ng & Diener, 2009; Mongrain & Anselmo-Matthews, 2012; Zeidan et al., 2013). The active control condition received the same placebo exercises and instructions but did not learn about any benefits of the exercises. Instead, they were given instruction to complete one exercise of their choice each day from a “Cognitive Activity Toolkit.” No mention of mental wellbeing was made in the control, aside from the title of the study. Thus, the control condition was intended to act as an assessment of contextual factors independent of placebo effects arising from high expectancy beliefs for wellbeing improvement.

**Experience sampling methodology and follow-up.** Participants completed 10 days of intervention practice tracked with 11 days of self-report outcome surveys using event-sampling methodology. The outcome surveys required 5 minutes per day and assessed the daily DVs over the past 24 hours each day before 3:00 pm on a fixed-interval schedule. The 11-day data collection schedule took place from Monday to Friday for week 1 and from Monday to Saturday for week 2 (refer to Figure 4 for full survey schedule details). Upon completing each day’s outcome survey, participation in all conditions were asked to complete at least one technique of their choice from their assigned toolkit on an event-based interval schedule. Scheduling allowed for temporal separation of the predictor (daily exercise) and the outcome variable (outcome survey) to help reduce the risks of common-source variance (Ohly, Sonnentag, Niessen, & Zapf,
For added benefits, participants were encouraged to utilize techniques when they felt most in need or inclined to take part (e.g., perhaps while waiting for the bus and feeling exhausted after the school day), with guided audio making it possible to practice on the go using smartphones.

After the last day of exercises and one month later, participants received a link to a 30-minute online post-study survey and a 5-minute follow-up survey, respectively, for completion within a week. Upon survey completion, participants learned about the assignment of conditions through an online debrief form (See Appendix A for debrief form). Participants enrolled in undergraduate psychology courses received course credit for the pre and post surveys and training session and $5 per day of daily surveys and exercises. Participants not completing the study for course credit received $5 for the training and each survey completed.

“Mental Wellbeing Toolkit” experimental condition. Participants in the experimental condition were trained to use the Mental Wellbeing Toolkit in their daily lives. This Toolkit contained the evidence-based techniques of mindfulness, positive reappraisal, and savouring for self-practice in everyday situations (refer to Table 1 for technique summaries across conditions and Appendix B for full exercise scripts). The foundational tool, mindfulness, was taught based on MBCT materials (Segal et al., 2013) and Kabat-Zinn’s (1990) seminal work on mindfulness. Students learned the mindfulness principles of objective awareness of the external environment (e.g., noticing everyday surroundings with openness and curiosity, minimizing judgment) as well as internal environment (i.e., decentering from inner thoughts and emotions with acceptance and self-compassion). The savouring tool was grounded in Bryant and Veroff’s (2007) research on savouring in daily life and related positive psychology exercises such as “three good things” (Bono et al., 2013). The third tool, positive reappraisal, was established in research and theory on
positive emotions in stress (e.g., Folkman, 2008) as well as Gross and John’s (2003) research on adaptive emotion regulation. The foundational mindfulness principles of acceptance and non-judgmental awareness were integrated into the training of the positive reappraisal and savouring techniques for increased exercise benefits (Garland et al., 2011; Kabat-Zinn, 1990). As an example, in the case of unpleasant feelings triggered during class time, students learned to mindfully apply a positive reappraisal exercise to re-evaluate the situation to focus on potential benefits.

All exercises were provided with 5-minute guided audio embedded into Qualtrics mobile survey software. This feature was chosen based on past meta-analytic findings that self-administered interventions providing guided practice produce larger effect sizes than those with un-guided practice (Cavanagh et al., 2014). Participants learned the benefits of each technique to increase intrinsic motivation to engage in the intervention, which has been deemed a critical component to positive intervention effectiveness (Lyubomirksy et al., 2011). Past interventions have utilized similar motivation-inducing approaches with inspirational quotes and explanations (e.g., Hülsheger et al., 2013).

“Mental Wellbeing Toolkit” placebo and “Cognitive Activity Toolkit” control conditions. Participants in the placebo condition followed the exact same structure as the experimental group. The placebo condition only differed in the content of the tools that were provided in their identically titled “Mental Wellbeing Toolkit.” These tools were three carefully selected, research-based placebo techniques of focus on breathing, distant memory recall, and recent memory recall (refer to Table 1 for technique summaries across conditions and Appendix B for full exercise scripts). As described below, each of these tools resembled the intervention techniques on all extraneous factors (e.g., equal length and style of guided audio, daily practice,
benefits explained by researchers, etc.) but lacked the active ingredients proposed in past research to trigger effective emotion regulation strategies.

To mimic the mindfulness technique, Zeidan et al.’s (2013) breathing placebo was applied in which participants were instructed to “Relax by focusing on your breath,” as compared to the mindfulness meditation that involves explicit dialogue to distance oneself from thoughts and emotions and simply accept the present moment, without making relaxation a priority. To contrast the savouring technique in which participants were explicitly instructed to up-regulate positive emotions, we used a placebo exercise of examining distant memories, without any instruction to focus on positive aspects. This distant memory recall exercise is a common placebo in positive psychology interventions (e.g., Mongrain & Anselmo-Matthews, 2012; Proyer, Gander, Wellenz, & Ruch, 2015) and consistently shows lower wellbeing outcomes as compared to positively focused exercises. Finally, recall of a recent event was used instead of the positive reappraisal technique. Here, participants were instructed to merely think of a recent event from their day, without instruction to positively reappraise the situation, adapted from the standard placebo used in positive reappraisal laboratory experiments (e.g., Ng & Diener, 2009). This standard placebo involves “rumination” of negative stimuli without subsequent positive reappraisal, which has previously resulted in higher negative affect than reappraisal conditions (Ng & Diener, 2009). This placebo was adapted to a neutral recall of a recent event to provide for a more conservative comparison and reduce risk of detriments to the placebo group’s wellbeing over time.

Measures

**Manipulation and compliance checks.** Treatment credibility was measured as a manipulation check of indented induction of expectancy beliefs in wellbeing improvement from
using the experimental and placebo interventions. Compliance checks of intended intervention participation were assessed with self-reported daily adherence to instructions and frequency of tool use.

_Treatment credibility manipulation check (post-training baseline and post-intervention)._ To assess differences in improvement expectancy beliefs across conditions, three questions from the Borkovec and Nau (1972) treatment credibility scale were used (refer to Appendix C). A sample item adapted for this intervention is: “How confident would you be in recommending this ‘Toolkit’ to a friend who was facing difficulties with mental health?” measured on a 7-point likert scale ranging from “Not at all” to “Extremely confident.” Establishing matched expectancy at the onset of the intervention in the experimental and placebo conditions allows for a more accurate measure of potential placebo effects (Temple & Ellenberg, 2000). Expectancy was measured on the first day of intervention exercises upon completing the training session and at post-intervention to examine stability of expectancy beliefs. In this sample, the treatment credibility scale showed an internal consistency of $a = .79$ on the first intervention day and $a = .88$ in the post-intervention survey.

_Self-reported instruction adherence and frequency of tool use (daily)._ A daily three-item scale assessed participants’ frequency of each assigned tool use with the prompt: “Please indicate how many times you have practiced the following tools over the past 24 hours. This includes all practice with guided exercises and on your own in your everyday activities.” Practice was measured on a scale from “0” to “Over 6 times that day” (refer to Appendix D). This is based on similar intervention compliance measures used in past studies (e.g., Fredrickson et al., 2008; Senf & Liau, 2013). Daily compliance and engagement with training was also evaluated using time stamps on the online survey software.
Positive and negative affect (baseline, post, follow-up, and daily). Positive and negative affect was measured using the Scale of Positive and Negative Experiences (SPANE; Diener et al., 2010), with two 6-item scales to measure the frequency of positive and negative affect separately over the past four weeks (refer to Appendix E). The SPANE measures both general feelings (e.g., good) and specific emotions (e.g., angry) to capture a full range of affective experiences, showing good convergence with other measures of wellbeing. The SPANE also examines a range of emotional activation (e.g., from contented to joyful), improving upon previous affect scales that focus solely on high arousal emotions (e.g., PANAS; Watson, Clark, & Tellegen, 1988). The SPANE was adapted from a 5-point frequency scale to 7-points, ranging from “Never” to “Always” to reduce scale coarseness (Aguinis, Pierce, & Culpepper, 2009).

Baseline, post intervention, and 1-month follow-up changes in SPANE scores were examined by adapting the prompt to assess experiences over the past two weeks. To measure daily changes, the full scales were adapted to assess general affect experienced over the past 24 hours as done in previous diary research (Craig et al., 2015). Baseline, post-intervention, and 1-month internal consistencies for the negative affect scale were $a = .91, .89, .89$ and $a = .90, .93, .95$ for the positive affect scale, respectively. Daily internal consistencies ranged from $a = .88 - .93$ for negative affect and $a = .92 - .95$ for positive affect.

Emotion regulation (baseline, post, follow-up, daily). Emotional regulation was assessed using Gross and John’s (2003) 6-item reappraisal subscale of the Emotion Regulation Questionnaire (ERQ; refer to Appendix F). The reappraisal subscale comprises down-regulation of negative emotions, such as: “When I want to feel less negative emotion, I change the way I am thinking about the situation,” and up-regulation of positive emotions: “When I want to feel more positive emotion, I change the way I am thinking about the situation.”
Baseline, post intervention, and 1-month follow up changes were assessed with the full 6-item emotion regulation subscale, ranging from “Strongly agree” to “Strongly disagree” to measure more enduring emotion regulation. Daily changes in emotion regulation use over the intervention were measured with the subscale adapted to assess experiences over the past 24 hours with 4 items previously shown to have the highest factor loadings (Gross & John, 2003) using a frequency scale ranging from “Never” to “Always.” Baseline, post-intervention and 1-month follow-up internal consistencies for the reappraisal facet of the emotion regulation scale were $a = .88, .89$ and .95, respectively. Daily internal consistencies ranged from $a = .90 - .95$.

Perceived general stress (baseline, post, follow-up, daily). The Stress in General Scale (SIG; Stanton, Balzer, Smith, Parra, & Ironson, 2001) made up of a pressure and threat subscale was used to examine how much stress participants appraise in their lives (refer to Appendix G). Responses were adapted to a 7-point agreement scale ranging from “Not at all” to “Extremely.” In the interest of capturing a more negative stress appraisal, we analyzed stress using only the 7-item threat subscale. This subscale showed greater convergence with other measures of wellbeing as compared to the pressure subscale. The SIG uses short, quick-response items to capture a general negatively valenced stress appraisal. Sample items for the threat subscale include: “Irritating” and “Hassled.”

Baseline, post-intervention and 1-month follow-up change in stress appraisal was examined with a general prompt: “Describe the past two weeks.” Daily fluctuations in perceived stress were assessed with a prompt adapted to measure experiences over the past 24 hours with a shortened 4-item scale consisting of items previously shown to have the highest factor loadings (Stanton et al., 2001). Baseline, post-intervention and 1-month follow-up internal consistencies
for the threat facet of the SIG were $a = .88, .91$ and .89, respectively. Daily internal consistencies ranged from $a = .82 - .97$.

**Psychological wellbeing (baseline, post, follow-up).** Mental wellbeing was assessed at baseline, post-intervention, and the 1-month follow-up using the Warwick-Edinburgh Mental Wellbeing Scale (WEMWBS; Tennant et al., 2007; refer to Appendix H). The WEMWBS consists of 14 items pertaining to feelings and thoughts related to various aspects of positive mental health (i.e., psychological, affective, cognitive functioning) combined to capture overall mental wellbeing. The measure was adapted to 7-points ranging from “None of the time” to “Always,” prompted to measure wellbeing over the past two weeks. The WEMWBS shows good convergent validity with overall health, positive affectivity, and life satisfaction, while showing lower convergence with social desirability than past measures (e.g., Satisfaction with Life Scale, WHO-5; Tennant et al., 2007). This scale has been selected in previous intervention studies for its sensitivity to show change over time (e.g., Seear & Vella-Brodrick, 2013). The internal consistencies found in this sample at baseline, post-intervention, and 1-month follow-up for the WEMWBS scale were $a = .92, .94$ and .94, respectively.

**Time/day.** Time was measured as days 1-11 (Monday - Friday of week 1 and Monday - Saturday of week 2) of participation in the assigned intervention, denoting a linear slope for time. Day 1 represented the post-training baseline and Day 11 represented the final outcome survey after completing the last intervention exercise on Day 10. Time was also measured over 3 points roughly three weeks apart (i.e., pre-training baseline, post-intervention, 1-month follow-up) to assess longer-term patterns, again representing a linear slope for time.

**Perceived benefits and intervention experience.** A “process evaluation” outside of the main hypotheses was conducted with several user-experience measures to help guide future
health promotion intervention development (O'Shea, O'Connell, & Gallagher, 2016). In the supplemental analyses, all user-experience variables were examined as moderators to intervention effectiveness to better explain wellbeing changes over time across all conditions. Upon daily exercise completion, participants rated their change in mood, ease of completion, and pleasantness on 7-point scales in relation to their completed exercise. Participants also checked yes/no to a range of wellbeing benefits (e.g., reduced frustration, increased gratitude) and provided qualitative comments on exercise experiences (refer Appendix I). In the post-intervention survey, participants again reported perceived intervention ease and pleasantness, but in relation to their assigned Toolkit as a whole. As well, the six-item “autonomous program participation” scale of the Treatment Self-Regulation Questionnaire (TSRQ; Ryan & Connell, 1989; Levesque et al., 2007) was used to measure participants’ intrinsic motivation for intervention participation rated on a 7-point scale from “Not at all true” to “Very true” (e.g., “It [assigned intervention] helps me accomplish my goals”; refer to Appendix J). The TSRQ showed good internal consistency in this sample of $a = .80$. The rest of the variables above were assessed as single items designed for this study. In addition to the self-reports, the survey software tracked user-experience data of number of completed exercises and type of exercise selected.

**Data Analysis**

Descriptive statistics were calculated with IBM SPSS version 23, random coefficient (multilevel) growth modeling was performed with R (R Core Team, 2014) using RStudio (RStudio Team, 2015), and multilevel structural equation modeling (MSEM) was conducted using Mplus (Muthén & Muthén, 1998-2011).

To test hypotheses 1-3 and 5, the multilevel growth modeling sequence based on recommendations by Bliese and Ployhart (2002) was applied for each daily outcome variable.
Multilevel growth modeling is a recommended approach for analysis of nested longitudinal data at the person-level with at least three time points. This analysis allows for improved power to analyze datasets with large missing data, correct for autocorrelation often seen with within-person data, and test for non-linear growth patterns (Ployhart & Vandenberg, 2010). Repeated measures over 11 days of outcome surveys over the course of the two-week intervention were nested within participants, resulting in a two-level model with \( n = 1133 \) days at Level 1 (within-persons) and \( n = 119 \) participants at Level 2 (between-persons) after exclusions. Repeated measures over the three time points (baseline, post-intervention, 1-month follow-up) were also nested within participants, resulting in a two-level model with \( n = 250 \) days at Level 1 (within-persons) and \( n = 113 \) participants at Level 2 (between-persons) after exclusions.

To test hypothesis 4, MSEM was performed. MSEM improves upon common biases and limited predictive capabilities of previous versions of multilevel mediation modeling (e.g., bias in indirect effects with nested data; Zhang, Zyphur, & Preacher, 2009) and allows for a multivariate analysis. We used syntax for a Level 2-1-1 multivariate MSEM model, which assessed if daily emotional regulation (Level 1) mediated the relation between condition (Level 2) and multivariate daily DVs (Level 1) in the second week of the intervention, after participants had already practiced the full first week of the intervention. Thus, only the second week of daily surveys were analyzed, resulting in a model with \( n = 449 \) days at Level 1 (within-persons) and \( n = 116 \) participants at Level 2 (between-persons).

With our theory-driven a-priori hypotheses, it was appropriate to compare between the three conditions using planned orthogonal contrasts. We followed Temple and Ellenberg’s (2000) recommendations for RCT placebo effect analysis. Contrast 1 compared the experimental condition against the placebo and Contrast 2 compared the placebo against the active control.
Contrast 1 provided a conservative measure of intervention effectiveness over and above placebo effects, while Contrast 2 delineated any potential placebo effect against an internal standard.

**Results**

**Preliminary Analysis**

**Participant exclusion.** Analyses were conducted on a conservative intent-to-treat basis with all participants who began the intervention materials included in the final sample (Sagarin et al., 2014). Surveys were systematically reviewed and excluded for Insufficient Effort Responding (IER), recommended for online survey data to reduce Type I and Type II error (Huang, Liu, & Bowling, 2015). All 130 participants completed the baseline survey. We excluded 11 participants for not completing any of the daily intervention components (8.5% dropout) and 6 participants for failing control questions as a measure of IER (e.g., “Please select never for this control question”), resulting in a total of 113 baseline surveys analyzed. A total of 1169 daily surveys were collected during the two-week intervention. Blank or extra entries ($n_{days} = 19$), unmatched identification codes ($n_{days} = 3$), surveys 2 standard deviations below average response time as a measure of IER ($n_{days} = 11$), and surveys of individuals who did not complete any corresponding intervention exercises ($n_{days} = 4$) were removed from the final daily sample. This resulted in the analysis of 1133 daily surveys from 119 participants (39 intervention, 41 placebo, 39 control). For the post-intervention survey, responding effort dropped with 109 recorded entries and 20 surveys removed for failing the IER control questions, resulting in 89 analyzed surveys. All 48 surveys from the 1-month follow-up survey were analyzed (refer to Figure 3 for full CONSORT diagram; Schulz et al., 2010).

No significant baseline differences between the final and dropout/excluded participants were found using t-test and chi-square analyses, indicating that the data was missing at random
(Ployhart & Vandenbergh, 2010; refer to Table 2 for comparisons). The final sample ranged from 17-25 years \(M_{age} = 18.36, SD = .98\) and eighty-nine percent of the sample was female. Means, between and within-person standard deviations, and correlations are presented in Table 3.

**Test assumptions.** To ensure appropriate testing of hypotheses, psychometric assumptions were explored and met. Participants did not statistically differ at baseline on a breadth of demographic and wellbeing measures across the three conditions (see Table 2 for condition comparisons). Variance in daily outcomes at the within and between-person levels was assessed by calculating the interclass correlation coefficient (ICC1; Bliese & Ployhart, 2002). Daily negative affect, positive affect, perceived stress, and emotion regulation showed ICC1 values of .58, .62, .56, and .72 respectively, representing the amount of variance explained by between-person differences. Similarly, as calculated by 1− ICC1, daily negative affect, positive affect, perceived stress, and emotion regulation showed .42, .38, .44 and .28 variance explained within-person, respectively (see Table 4 for parameter estimates). ICC1 values examining dispositional emotion regulation, perceived stress, and mental wellbeing across baseline, post-intervention, and 1-month follow-up measures also showed large amounts of variance explained by between-person differences (.65, .39, .69, respectively) and within-person differences (.35, .61, .31, respectively). Given the substantial percentage of variance explained by between and within-person differences across outcome variables, it was appropriate to proceed with a multilevel analysis with time points nested within participants (Bliese & Ployhart, 2002).

**Manipulation and compliance checks.** As a manipulation check of inducing expectancy beliefs for wellbeing improvement, high expectancy beliefs were found with no significant differences across the Mental Wellbeing Toolkit intervention \(M = 5.06, SD = 1.32\), placebo \(M = 5.28, SD = 0.96\), or control condition \(M = 5.05, SD = 1.13\) measured on the first day of the
intervention, $F(1, 117) = .53, p = .59, n^2_p = .01$. Similarly, there were no significant differences across the Mental Wellbeing Toolkit intervention ($M = 5.43, SD = 1.22$), placebo ($M = 5.67, SD = 1.08$), or control condition ($M = 5.47, SD = 1.11$) in expectancy beliefs measured again at post-intervention, $F(1, 86) = .32, p = .73, n^2_p = .01$. Thus, although expectancy beliefs were similarly high in the intervention and placebo condition as intended, we failed to evoke the expectancy belief decrease in the control relative to the placebo. Positive user experiences across all conditions are a likely cause for the equal scores (described below in “Process Evaluation”: User Intervention Experience”).

Intervention compliance was assessed through time stamps and self-reports. Participants completed an average of 7.87 days of their assigned exercises, with no significant differences between the intervention ($M = 8.18, SD = 2.40$), placebo ($M = 7.54, SD = 2.58$), or control conditions ($M = 7.90, SD = 2.71$), $F(1, 118) = .63, p = .53, n^2_p = .01$. On average, participants spent an adequate 9.69 minutes on the daily intervention exercise with questionnaires, with no significant differences between the intervention ($M = 10.63, SD = 3.81$), placebo ($M = 8.91, SD = 4.18$), or control conditions ($M = 9.58, SD = 3.66$), $F(1, 116) = 1.94, p = .15, n^2_p = .03$. Of the 934 online exercises completed, 78% of the durations were at least as long as the required five-minute audio component.

Every day, participants self-reported how frequently they practiced their assigned Toolkit exercises over the past 24 hours (using the guided audio provided as well as informally on their own) and how closely they followed exercise instructions. One-way ANOVA results show significant variance across conditions in number of reported exercises practiced daily in the intervention ($M = 8.36, SD = 3.76$), placebo ($M = 10.17, SD = 5.68$), and in the control ($M = 8.76, SD = 4.00$), $F(1, 1011) = 14.65, p < .001, n^2_p = .03$. Tukey’s post hoc tests showed that
participants in the placebo condition reported using their toolkit exercises more often than the control, \( p < .001 \), and the intervention participants, \( p < .001 \). There were no significant differences between the control and intervention condition. On average, participants endorsed following the exercise instructions “Closely,” with no significant differences across conditions, \( F(1, 1013) = 2.50, p = .08, n^2_p = .01 \).

**“Process Evaluation”: User Intervention Experience**

A process evaluation of participants’ subjective experiences with the Mental Wellbeing Toolkit and placebo exercises allowed us to gain a richer understanding of the utility, challenges, and strengths of implementation in students’ daily life (Nielsen et al., 2007). There were 485 exercises completed in the first week compared to 449 in the second week, with highest completion from Monday to Wednesday in both weeks. Participants in the Mental Wellbeing Toolkit completed a total of 315 exercises, made up of a majority of mindfulness exercises (52%), followed by savouring (26%), and positive reappraisal (22%). Participants in the placebo condition completed 310 exercises, with the breathing exercise chosen most frequently (56%), followed by recent memory recall (25%), and distant memory recall (19%). Finally, control participants completed 309 exercises, again with the breathing exercise chosen most frequently (57%), followed by distant memory recall (24%), and recent memory recall (19%).

Interestingly, even though the mindfulness and breathing exercises were selected most frequently on all days, they did not necessarily lead to the greatest subjective benefits. In the Mental Wellbeing Toolkit intervention, participants reported significant variance in pleasantness across the mindfulness (\( M = 4.72, SD = 1.14 \)), positive reappraisal (\( M = 4.69, SD = 1.02 \)), and savouring exercise (\( M = 5.12, SD = 1.12 \)), \( F(1, 307) = 4.23, p < .05, n^2_p = .03 \). Tuckey’s post hoc results indicate that savouring was associated with higher pleasantness scores than both the
mindfulness and positive reappraisal exercise, \( p \)'s < .05, with non-significant differences between mindfulness and positive reappraisal. Similarly, there was significant variance in perceived mood improvement across the mindfulness \((M = 4.92, SD = 1.09)\), positive reappraisal \((M = 5.06, SD = 1.09)\), and savouring exercise \((M = 5.29, SD = 1.05)\), \(F(1, 307) = 3.27, p < .05\), \(n^2_p = .02\). Savouring was also related to higher perceived mood improvement scores compared to mindfulness \((p < .05)\), with non-significant differences between mindfulness and positive reappraisal and positive reappraisal and savouring. In the placebo and control exercises combined, participants reported significant variance in pleasantness across the breathing \((M = 4.95, SD = 1.15)\), distant memory \((M = 5.17, SD = 1.23)\), and recent memory exercise \((M = 4.78, SD = 1.10)\), \(F(1, 615) = 3.86, p < .05\), \(n^2_p = .01\). Tuckey’s post hoc results show that distant memory recall was associated with higher pleasantness than the recent memory recall exercise \((p < .05)\) with no significant differences between distant memory and breathing and recent memory and breathing. There were no significant mood improvement differences across the breathing \((M = 5.15, SD = 1.05)\), distant memory \((M = 5.22, SD = 1.27)\), and recent memory exercise \((M = 4.93, SD = 1.06)\), \(F(1, 616) = 2.63, p = .07\), \(n^2_p = .01\).

Overall, evaluations were equally favorable across all conditions. Multilevel analyses on the daily intervention data showed no differences between conditions on daily exercise pleasantness, immediate mood improvement post-exercise, or number of immediate perceived wellbeing benefits participants endorsed (e.g., reduced stress, increased gratitude). Similarly, upon completing the intervention from the post-evaluation, one-way ANOVAs showed no significant differences across conditions on intervention enjoyment, usefulness, ease, or intended future use. The majority of participants intended to continue using the techniques taught at least “once a week” (60%) and described their assigned intervention as “extremely enjoyable” to
“enjoyable” (58%), “useful” to “extremely useful” (72%), and “easy” to “extremely easy” (83%). A small minority reported unfavourable evaluations, never intending to use the techniques again (1%) and describing their intervention as “not enjoyable” (7%), “not useful” (6%) and “not easy to use” (2%). Qualitative comments helped to clarify intervention experiences. Some unpleasant experiences in the Mental Wellbeing Toolkit intervention were: “Pressure to do it everyday,” and “The videos were very repetitive into the second week.” Positive experiences ranged from: “I liked that once I have completed the mental wellbeing toolkit, I understood why I was feeling the way I was a bit better,” and “Making yourself feel good about a situation or how your day went,” to detailed accounts about specific tools such as the positive reappraisal tool:

I was really able to sit down and think about what was bothering me. Sometimes with so much anxiety you only think of the worse possible scenario. With a relaxed state of mind, you are able to pinpoint what is causing your distress, and being able to accept that makes it seem okay, and once its accepted that you cannot change what has happened, and you have found a way to improve for next time, its encouraging because you know what not to do anymore.

Similarly, positive intervention experiences were described in the placebo condition, such as for the recent memory recall exercise: “Brings back good moments, clears heads, de-stresses since I'm remembering good times.” It may be that when participants utilized the placebo exercises to actively remember positive moments or with the intention of reducing stress, they crafted the exercise into an emotion regulation practice similar to the intervention.

Finally, a content analysis of the qualitative responses further denoted high positive experiences and perceived benefits consistently across conditions, with patterns of only small increased benefits seen in the intervention condition. Participants’ qualitative descriptions of
experiences after each exercise were categorized into neutral, positive, and negative valences in regards to wellbeing benefits. Participants in the experimental condition reported 6.6% negatively coded experiences (e.g., “tiring”) as compared to 12.4% in the placebo and 9.0% in the control. Conversely, participants in the experimental condition reported 24.8% neutral or uncategorized experiences (e.g., “deep”), as compared to 23.2% in the placebo and 33.0% in the control. Positively coded experiences (e.g., “helpful” and “calming stressful thoughts”) were unanimously the most frequent, with 68.6% coded in the experimental condition, followed by 64.7% in the placebo and 58.0% in the control.

Growth Analyses: Daily Data Over Time

First, the most parsimonious and best fitting null model for each outcome was established to test the independent variables of condition and time. It was appropriate to begin with a random intercept model given the high within-person nesting of data indicated by the ICC1 values (Bliese & Ployhart, 2002). The addition of time (Day 1-11) to the null model revealed a significant linear relation between time and negative affect, $b = -0.06$, $t(1013) = -8.17$, $p < .001$, 95% CI $[-0.07, -0.05]$, positive affect, $b = 0.01$, $t(1013) = 2.24$, $p < .05$, 95% CI $[0.00, 0.03]$, perceived stress, $b = -0.06$, $t(1013) = -7.58$, $p < .001$, 95% CI $[-0.07, -0.05]$, and emotion regulation, $b = -0.02$, $t(1013) = -3.20$, $p < .01$, 95% CI $[-0.03, -0.01]$. Thus, there was a significant decrease in negative affect, perceived stress, and emotion regulation, paired with an increase in positive affect over the two weeks of the intervention across all conditions. A quadratic relation over time was also significant for negative affect, $b = 0.01$, $t(1012) = 3.38$, $p < .001$, 95% CI $[0.00, 0.01]$, perceived stress, $b = 0.01$, $t(1012) = 2.60$, $p < .01$, 95% CI $[0.00, 0.01]$, and emotion regulation, $b = 0.01$, $t(1012) = 2.75$, $p < .01$, 95% CI $[0.00, 0.01]$, but non-significant for positive affect, $b = 0.00$, $t(1012) = 1.28$, $p = .20$, 95% CI $[0.00, 0.00]$. Given the smaller effect sizes with confidence
intervals overlapping with zero in the quadratic models, we chose to explore the most parsimonious models of linear growth.\(^2\)

Log likelihood tests were used to compare the goodness of fit across increasingly complex models to ensure analyses were conducted with greatest power and parsimony (Bliese & Ployhart, 2002). Based on net increases in log likelihood values indicating greater model fit, it was determined that models with random slopes fit the data significantly better than models with fixed slopes across participants for daily negative affect (\(\Delta -2 \text{ log likelihood} = 14.01, df = 2, p < .0001\)), positive affect (\(\Delta -2 \text{ log likelihood} = 18.13, df = 2, p < .0001\)), perceived stress (\(\Delta -2 \text{ log likelihood} = 29.90, df = 2, p < .0001\)), and emotion regulation (\(\Delta -2 \text{ log likelihood} = 32.04, df = 2, p < .0001\)). Similarly, models that corrected for autocorrelation fit the data better than without the correction for daily negative affect (\(\Delta -2 \text{ log likelihood} = 38.14, df = 2, p < .0001\)), positive affect (\(\Delta -2 \text{ log likelihood} = 13.79, df = 2, p < .0001\)), perceived stress (\(\Delta -2 \text{ log likelihood} = 28.00, df = 2, p < .0001\)), and emotion regulation (\(\Delta -2 \text{ log likelihood} = 9.53, df = 2, p < .0001\)).

Thus, linear growth hypotheses were analyzed with random slopes models corrected for autocorrelation for all daily outcomes.

**Does the Mental Wellbeing Toolkit produce changes over two weeks?** To examine hypotheses 1-3, condition (Mental Wellbeing Toolkit intervention, placebo, control) was added as a predictor in Model 1, followed by the interaction term between time (Day 1-11) and condition in Model 2. This assessed whether the participants differed across condition in slope of affect, perceived stress, and emotion regulation over the two weeks. A significant time by condition interaction showed that participants in the intervention compared to the placebo condition showed a more pronounced increase in daily emotion regulation reports over the two weeks, \(b = .02, t(1011) = 2.04, p < .05, 95\% \text{ CI } [.00, .05]\) supporting hypothesis 1a (see Table 5
for results and Figure 5 for growth plot). The slope difference between the placebo and control condition was also significant showing greater emotion regulation use over the two weeks in the placebo, $b = .03$, $t(1011) = 2.56, p < .05$, 95% CI [.01, .05]. However, the slope by condition interaction was not significant between the intervention and placebo groups for daily negative affect, $b = .01$, $t(1011) = 0.59, p = .56$, 95% CI [.02, .04] (see Table 7 for results and Figure 6 for growth plot), or daily positive affect, $b = .01$, $t(1011) = 0.77, p = .44$, 95% CI [-.01, .03] (see Table 8 for results and Figure 6 for growth plot), failing to support hypothesis 2a and 2b, respectively. There was also no significant placebo effect when comparing the slopes between placebo and control conditions over time for daily negative affect, $b = .00$, $t(1011) = -0.06, p = .96$, 95% CI [-.03, .03], and daily positive affect, $b = .01$, $t(1133) = 0.60, p = .55$, 95% CI [-.02, .03]. Similarly, the slope by condition interaction was not significant for daily perceived stress between the intervention and placebo groups, $b = -.01$, $t(1011) = -0.32, p = .75$, 95% CI [-.03, .04], or between the placebo and control, $b = -.02$, $t(1011) = -1.29, p = .20$, 95% CI [-.01, .05], failing to support hypothesis 3a (see Table 9 for results and Figure 7 for growth plot).

Effect sizes for multilevel analyses were calculated as the net increase in variance explained within-person, between-person, and slope variance over the previous model. The addition of the interaction in multilevel Model 2 explained an additional 1% of the between-person variance and 37% of the slope variance in daily emotion regulation, but no additional within-person variance. No additional variance was explained in Model 2 for daily negative affect, positive affect, or perceived stress.

**Does emotion regulation mediate the effect of condition on changes in the second week?** For the MSEM analyses, first the path between condition and emotion regulation was examined (path $a$), followed by the multivariate paths between emotion regulation and the three
dependent variables (path $b$). Finally, the product of path $a$ with each path $b$ was examined as the indirect mediation effect (path $ab$; see Table 12 for MSEM results). Path $a$ was not significant, as condition (intervention vs. placebo) did not predict week-two daily emotion regulation, $\beta = -.10$, $p = .64$, 95% CI $[-.57, .35]$. Conversely, all multivariate $b$ paths were significant, with week-two emotion regulation predicting week-two daily negative affect, $\beta = -.26, p < .01$, 95% CI $[-.47, -.06]$, positive affect, $\beta = .37, p < .001$, 95% CI $[.21, .54]$, and perceived stress, $\beta = -.19, p < .05$, 95% CI $[-.38, -.01]$ across both conditions. However, the indirect mediation paths were not significant for negative affect, $\beta = .04, p = .64$, 95% CI $[-.14, .23]$, positive affect, $\beta = -.05, p = .63$, 95% CI $[-.26, .16]$, or perceived stress, $\beta = .04, p = .64$, 95% CI $[-.11, .18]$. Similarly, assignment to the placebo versus control condition did not lead to any week-two changes mediated by emotion regulation, with non-significant indirect paths for negative affect, $\beta = -.12, p = .29$, 95% CI $[-.33, .10]$, positive affect, $\beta = .14, p = .23$, 95% CI $[-.09, .37]$, and perceived stress, $\beta = .11, p = .64$, 95% CI $[-.11, .18]$. Thus, hypothesis 4 was fully unsupported. This mediation model failed to explain any percentage of variance in week-two daily outcomes, indicating that alternative mechanisms may be at play in explaining intervention benefits.

**Growth Analyses: Baseline, Post-Intervention, 1-Month Follow-Up Data**

To test the long-term growth hypotheses over three time points (baseline, post-intervention, 1-month follow up) the same multilevel modeling procedure as above was applied to determine the best-fitting and most parsimonious models for each outcome variable. The addition of time (1-3) to the random-intercepts null model revealed a significant linear relation between time and perceived stress, $b = -.70, t(133) = -11.51, p < .001$, 95% CI $[-.82, -.58]$, and mental wellbeing, $b = .23, t(135) = 5.43, p < .001$, 95% CI $[.15, .31]$, but not emotion regulation, $b = .04, t(135) = 0.75, p = .45$, 95% CI $[-.07, .15]$. Thus, there was a significant decrease in
perceived stress and increase in mental wellbeing, but no significant changes in emotion regulation from baseline to the 1-month follow-up across all conditions. A quadratic relation over time was not significant for emotion regulation, $b = .04, t(134) = 0.40, p = .69$, 95% CI $[-.14, .21]$, perceived stress, $b = -.05, t(132) = -0.48, p = .63$, 95% CI $[-.24, .15]$, or mental wellbeing, $b = -.07, t(134) = -0.97, p = .32$, 95% CI $[-.20, .07]$. Given no improvement of fit with the quadratic models, we chose to explore the most parsimonious model of linear growth.

Based on net increases in log likelihood values, it was determined that models with random slopes did not fit the data significantly better than models with fixed slopes for emotion regulation ($\Delta -2 \log \text{likelihood} = 0$, $df = 2$, $p < 1.00$), perceived stress ($\Delta -2 \log \text{likelihood} = 0.01$, $df = 2$, $p = .99$), or mental wellbeing ($\Delta -2 \log \text{likelihood} = 0.31$, $df = 2$, $p = .74$). However, because numerically the log likelihood values improved or at least stayed the same for all variables, we chose to analyze with random slopes models to test for the cross-level time by condition interaction. Models that corrected for autocorrelation did not fit the data better than without the correction for emotion regulation ($\Delta -2 \log \text{likelihood} = 1.13$, $df = 2$, $p = .13$), perceived stress ($\Delta -2 \log \text{likelihood} = 0.03$, $df = 2$, $p = .82$), or mental wellbeing ($\Delta -2 \log \text{likelihood} = 0.18$, $df = 2$, $p = .55$). Thus, linear growth hypotheses were analyzed with random intercept and random slopes models, uncorrected for autocorrelation over the three time points.

**Does the Mental Wellbeing Toolkit produce changes from baseline to the 1-month follow-up?** To test the long-term growth hypotheses, condition (Mental Wellbeing Toolkit intervention, placebo, control) was added as a predictor in Model 1, followed by the interaction term between time (baseline, post-intervention, 1-month follow-up) and condition in Model 2. A significant time by condition interaction showed that participants in the intervention compared to the placebo showed a more pronounced increase in emotion regulation use over the three time
points, $b = .19$, $t(133) = 2.31$, $p < .05$, 95% CI [.03, .35] supporting hypothesis 1b (see Table 6 for results and Figure 5 for growth plot). There was a marginally significant placebo effect found with the placebo also showing a more pronounced increase in emotion regulation over the three time points compared to the control, $b = .14$, $t(133) = 1.91$, $p = .06$, 95% CI [−.01, .29]. The slope by condition interaction was marginally significant in showing a more pronounced decrease in perceived stress over time in the intervention versus placebo, $b = −.15$, $t(131) = −1.68$, $p = .09$, 95% CI [−.33, .03], marginally supporting hypothesis 3b (see Table 10 for results and Figure 7 for growth plot). As well, the slope by condition interaction was marginally significant in showing a more pronounced increase in mental wellbeing over time in the intervention versus placebo, $b = .11$, $t(133) = 1.67$, $p = .09$, 95% CI [−.02, .23], marginally supporting hypothesis 5 (see Table 11 for results and Figure 8 for growth plot). There was no significant placebo effect found when comparing the slopes between placebo and control conditions over time for perceived stress, $b = .02$, $t(131) = 0.26$, $p = .79$, 95% CI [−.11, .16], or mental wellbeing, $b = .07$, $t(133) = 1.24$, $p = .22$, 95% CI [−.04, .18]. No additional between-person variance was explained with the interaction terms. However, the interaction did explain an additional 4% of the within-person variance and 48% of the slope variance in emotion regulation, 2% of the within-person variance and 7% of the slope variance in perceived stress, and 1% of the variance within-person in mental wellbeing, above and beyond Model 1.

**Supplemental Analyses**

Given the mixed results and large effect size of time in wellbeing improvements across all conditions, several supplemental analyses were conducted. First, all daily data analyses were run a second time, controlling for baseline scores corresponding to each outcome variable to determine if the slight yet non-significant baseline differences across conditions (despite random
sampling) were confounding results. All patterns of results and significance remained the same when controlling for baseline scores, indicating unlikely confounding from baseline differences.

**Did the type of exercise selected moderate changes over time?** While the type of exercise participants selected led to significant differences in immediate mood change and pleasantness (see “Process Evaluation: User Intervention Experience” section above), exercise selection did not predict wellbeing changes over time. The same multilevel growth modeling procedure from the main hypotheses was applied to test time interactions. In the intervention condition, daily exercise selection (i.e., mindfulness, savouring, or positive reappraisal) did not lead to significant changes in daily emotion regulation, $b = -0.02$, $t(238) = -1.69$, $p = .09$, 95% CI $[-.05, .02]$, positive affect, $b = 0.00$, $t(238) = 0.10$, $p = .92$, 95% CI $[-.03, .03]$, negative affect, $b = .01$, $t(238) = 0.61$, $p = .55$, 95% CI $[-.02, .04]$, or stress over the two-week intervention, $b = .01$, $t(238) = 0.46$, $p = .64$, 95% CI $[-.03, .05]$. Similarly, in the placebo and control conditions combined, daily exercise selection (i.e., breathing, distant memory recall, or recent memory recall) did not lead to significant changes in emotion regulation, $b = 0.01$, $t(467) = 1.23$, $p = .22$, 95% CI $[-.01, .03]$, positive affect, $b = 0.00$, $t(467) = 0.08$, $p = .93$, 95% CI $[-.02, .02]$, negative affect, $b = 0.01$, $t(467) = 0.39$, $p = .70$, 95% CI $[-.02, .04]$, or stress over the two-week intervention, $b = 0.00$, $t(467) = 0.15$, $p = .88$, 95% CI $[-.03, .03]$.  

**Did positive user experience explain improvements over time?** Internal analyses with all conditions combined were conducted to examine if positive user experiences were driving the significant wellbeing changes found over time across all conditions. The multilevel growth modeling analysis from the main hypotheses was applied, now examining the interaction between time and various intervention evaluations in predicting wellbeing outcomes. It appears that participants’ intervention compliance did not have a significant effect on any changes over
time, as measured by the total number of exercises completed, daily self-reported instruction compliance, and total intervention efforts reported at the end of the two weeks. Results also suggest that certain positive intervention evaluations, specifically exercise enjoyment, ease of exercise use, and immediate mood improvement, did not have any significant effect on changes in any of the wellbeing outcomes over time.

However, more complex intervention experiences of intrinsic motivation and number of perceived benefits did in fact predict several improvements over time. While intrinsic motivation to participate in the intervention did not have a significant effect on stress, it predicted a significant increase in emotion regulation, \( b = .21, t(132) = 3.11, p < .01, 95\% \text{ CI} [.08, .34] \), and mental wellbeing, \( b = .10, t(132) = 2.10, p < .05, 95\% \text{ CI} [.01, .20] \) from baseline to the 1-month follow-up. Intrinsic motivation explained 4\% of the slope variance over time for emotion regulation, and a large 97\% of the slope variance in students’ mental wellbeing over time. As well, the number of benefits students reported each day upon exercise completion predicted significant increases in positive affect, \( b = .01, t(711) = 2.19, p < .05, 95\% \text{ CI} [.00, .02] \), and marginally significant increases in emotion regulation, \( b = .01, t(711) = 1.81, p = .07, 95\% \text{ CI} [.00, .01] \), over the two weeks of exercises. No significant interaction with daily stress or negative affect was found. The number of perceived benefits explained 4\% of the slope variance in daily positive affect and 3\% of the slope variance in daily emotion regulation over time. It appears that these positive intervention experiences played a slight, albeit inconsistent role on the large wellbeing improvements found across conditions over time.

Discussion

This RCT examined the viability of the “Mental Wellbeing Toolkit” – a two-week self-administered intervention of complementary, well-established emotion regulation exercises. The
three techniques of mindfulness, positive reappraisal, and savouring provided participants with a well-rounded toolkit to both up-regulate and down-regulate emotions in daily life. Results were contrasted against a toolkit of sham exercises presented as beneficial for wellbeing in a matched-expectancy placebo condition. This placebo was contrasted to a third control group, which also received the sham toolkit but lacked any explanation of wellbeing benefits.

Growth modeling results showed significantly more pronounced increases in daily emotion regulation over two weeks in the intervention compared to the placebo condition, as well as at the 1-month follow-up. A placebo effect was also found as the placebo condition showed significantly more pronounced emotion regulation increases over two weeks compared to the control. However, there may be other mechanisms at play other than daily emotion regulation in explaining intervention benefits over and above placebo effects, as all mediation hypotheses were unsupported. Although paths between daily emotion regulation during the second week of the intervention were significantly related to lower daily negative affect, perceived stress, and higher daily positive affect, there was no significant indirect effect of emotion regulation mediating condition assignment on these daily outcomes.

Interestingly, all three conditions showed significant linear decreases in perceived stress and negative affect and increases in positive affect over two weeks of assigned intervention practice. Rich qualitative and quantitative results tracking daily intervention experiences suggest similarly high perceived benefits, exercise enjoyment, and mood improvements across all conditions that likely accounted for some of these changes. Of course, in terms of possible history effects, it is notable that students participated in the intervention at the end of the semester, when course load is typically the heaviest in most programs. Conversely, the 1-month follow-up was assessed at the start of the second semester, when students likely did not have as
many course demands. However, supplemental analyses show that while history effects are likely accounting for many of these temporal changes across conditions, intervention experiences also played an important role. Specifically, intrinsic motivation and perceived intervention benefits predicted as much as 97% of the slope variance in wellbeing over time.

Unlike hypothesized, growth modeling results showed no significant differences across conditions over two weeks in positive affect, negative affect, or perceived stress. As well, there were no significant differences between the placebo and control in any wellbeing outcomes (with the exception of emotion regulation) over the two-week intervention or at the 1-month follow-up. However, longer-term growth results from baseline to the 1-month follow-up showed marginally significant increases in the intervention compared to placebo in mental wellbeing, and marginally significant decreases in perceived stress, with no significant differences between placebo and the control. In line with the emotion regulation literature (viz., Gross & John, 2003) and the broaden and build theory of positive emotions (Fredrickson et al., 2001; 2008), it was expected that greater positive reappraisal use (i.e., up-regulation of positive emotions and down-regulation of negative emotions in the face of daily stressors) should foster upward spirals of positive emotions that counteract negative spirals and stress over time. It could be that full intervention effects above and beyond placebo improvements take longer than two weeks to proliferate, as seen with the marginally significant improvements in wellbeing and perceived stress in the intervention from baseline to the 1-month follow-up.

These results should also be interpreted with caution due to overall small-medium effect sizes (i.e., from .01 – .48) and large daily fluctuations that were in part accounted for by placebo effects across all conditions (e.g., high intrinsic motivation to participate in intervention materials). Some marginally significant findings and confidence intervals approaching zero raise
the need for further exploration and replication. Of course, this is not uncommon for low-dose positive interventions in healthy populations, as found in previous meta-analyses (see Cavanaugh et al., 2014). It is also important to note that results were analyzed conservatively to provide a more practical representation of what could be expected when implementing this intervention in the field. Specifically, no controls or covariates were used in data cleaning. Furthermore, analysis was conducted on an intent-to-treat basis in which all participants who completed at least one of ten days of exercises were included. While intent-to-treat presents more generalizable outcomes, it can also underestimate effects as compared to a per-protocol analysis in which effects may be larger with a more compliant sub-group.

Comparison of the intervention to a placebo toolkit that produced equally high expectancy beliefs for improvement also provided a conservative test, isolating for the “active” emotion regulation ingredients above and beyond a number of extraneous factors in the intervention (e.g., social support, daily practice, choice of enjoyable guided audio exercises, etc.). Coupling this conservative approach with the low-intensity, short-term, and self-administered health promotion intervention design, small effect sizes were expected. Thus, considering the feasibility of this intervention in an applied setting (i.e., minimal resources, cost, and time requirements), the small benefits that were found are worth noting to encourage and guide the development and testing of similar health promotion interventions in research and in the field. Additionally, given this rigorous placebo testing and the general improvements across time in all conditions, this study sheds light on the value of promoting positive user experience. Supplemental analyses show that positive intervention experiences appear to account for as much variance in improvements over time as the specific techniques themselves.
Limitations and Future Directions

As with all field studies, the execution of the current intervention carried several limitations. An important assumption of the design was to induce equally high expectancy beliefs in the placebo and intervention condition, with lower expectancy beliefs in the control condition to act as an internal standard (Temple & Ellenberg, 2000). Although expectancy beliefs were generally high (i.e., mean of 5 on a 7-point scale), which is important for intervention effectiveness and compliance, we did not establish substantial differences between the placebo and control condition in these beliefs. It appears that providing a thorough explanation of intervention wellbeing benefits via an in-person training session and labeling the sham intervention as a “Mental Wellbeing Toolkit” versus a “Cognitive Activity Toolkit” did not have a visible effect on wellbeing improvement expectations.

This was further exemplified in the findings that the placebo and control group did not significantly differ in any wellbeing outcomes over the two-week intervention or at the 1-month follow-up, with only significantly greater emotion regulation use found in the placebo versus the control. One explanation for this is that the sham intervention materials used (i.e., breathing exercise, distant memory recall, recent memory recall) were exercises that students intuitively perceive as helpful for their wellbeing, with or without researchers priming these benefits. However, the placebo group reported using all exercises significantly more than the control, which may help explain why they reported greater emotion regulation than the control.

Future research is recommended to devise a stronger manipulation between a matched-expectancy placebo condition and active control (e.g., apply more neutral control exercises unrelated to common wellbeing beliefs, such as math equations) to act as a better internal standard to compare placebo effects against. Administration of inactive, “do nothing” control
conditions is another option, although this would make it harder to determine if improvements in the placebo group arise due to expectancy beliefs or other participant experiences. Despite the limitations, the current design was useful for uncovering that while explanation and priming of wellbeing benefits may help increase frequency of self-practice, it is no more effective at establishing higher expectancy beliefs than sheer assignment to these commonly-used sham exercises.

In addition, missing data was prominent and increased over the course of the intervention, most apparent at the 1-month follow-up. Based on recommendations by Armijo-Olivo, Warren, and Magee (2009), no additional corrections were applied for handling the missing data because of upwards of 20% missing data in our sample. Many participants reported the survey questions to be “too long” and “tedious” to complete everyday, which can hinder effortful responding and retention. One study found that participant dropout probability increased by 6% when testing a 30-item survey as compared to a 1-item survey (Sitzmann & Wang, 2015). On top of increasing attrition, research suggests that overburdening participants with repeated self-reports can also have an adverse effect on data quality (Sitzmann & Wang, 2015; Sagarin et al., 2014). A few participants alluded that the daily surveys counteracted exercise benefits, with comments such as: “Having realized I missed the time parameter for the first survey distracted me a lot from this exercise today!” This is a common difficulty in longitudinal studies, as researchers are expected to navigate in a grey area with no set standards on how to balance quantity and quality of measures (Ployhart & Vandenberg, 2010). Given the high internal consistencies we found in using the shorter daily questionnaires (e.g., $a = .90-.95$ for the 4-item daily ERQ), which were no different from full length scales used (e.g., $a = .88-.95$ for the full 6-
item ERQ), researchers are encouraged to use condensed scales made up of items with highest factor loadings in diary research.

Despite dropout over time, it was determined that this sample’s data was Missing at Random, indicated by non-significant differences in baseline outcome measures and demographics between the final and dropout/excluded samples (Ployhart & Vandenberg, 2010). It is suggested that data Missing at Random, although it may reduce power to find effects, should not be viewed as detrimental to prediction or implications (Ployhart & Vandenberg, 2010). Attrition is typical in all longitudinal research, with commonly seen dropout rates upwards of 50% between the first and final measures, as was found in this study between the baseline survey and 1-month follow-up (Sagarin et al., 2014). However, at least during the two-week intervention period, attrition was lower than average as compared to other online field studies, likely aided by the incremental $5 payments for each day of participation. Future research should continue to provide intrinsic (e.g., in-person explanation of study benefits) and extrinsic motivation in the form of piece-rate payments when possible for each completed survey to maintain data that is Missing at Random and avoid systematic error from attrition. However, the results should still be interpreted with caution given the increased likelihood of Type I and Type II error, predominantly at the 1-month follow up results, due to large dropout.

Conversely, self-report questionnaires have also been shown to provide benefits. Specifically, self-regulatory questions focusing on participants’ internal experiences, such as the experiential questions administered to all conditions in this study (e.g., “How do you feel after completing this exercise?”) are suggested to benefit learning via self-reflection, feedback, and awareness (Sitzmann & Wang, 2015). In fact, awareness of inner states and emotions is a pivotal teaching in mindfulness practice (Segal et al., 2013). Indeed, participants in this study alluded to
these extraneous survey benefits with comments such as: “The questionnaires make you think about your emotional state more deeply as well as objectively.” It is beneficial for researchers to provide the same self-regulatory questions across all conditions to isolate for true intervention benefits as conducted with this design. However, a downside to this approach is that it becomes more difficult to pinpoint causes for improvements when all conditions show change, such as the improvements in daily mood and stress found in this study across all conditions.

Future research is needed to better understand the extent to which intervention improvements are due to the actual materials as opposed to repeated use of introspective surveys. Although self-reports are an essential part of understanding subjective wellbeing, future researchers can rely more on new software becoming readily available for portable, real-time physiological measures (e.g., mobile apps and lightweight accessories measuring heart rate, cortisol, sleep, etc.) to supplement introspective self-reports. Alternatively, given the inevitable mindfulness component to emotional self-reflection questions, perhaps such surveys can be capitalized on as a core component of the intervention materials themselves, with more surveys reserved for the experimental conditions only.

**Strengths and Implications**

The emphasis of this intervention was on promoting subjective wellbeing with practical implications for applied work and academic contexts. Empirical evidence suggests that heightened positive emotions, compared to negative emotions, promote pro-social activity and work engagement, which are valuable outcomes for both school and organizational success (Fredrickson, 2001; Garland et al., 2010; Seligman & Csikszentmihalyi, 2000). Meta-analytic findings also support a causal effect of positive affect on future success and health (Lyubomirsky, King, & Diener, 2005). Of course, negative and neutral emotions are an
inseparable and necessary component of daily life. The intention of the intervention was to help students handle demanding environments in more adaptive and personally valuable ways. For example, instead of ruminating about disappointing details after a low grade on a test, or suppressing negative reactions (i.e., both emotion regulation strategies have been found to be generally maladaptive and linked with insomnia, depression, and learned helplessness, Gross & John, 2003; Watkins, 2008), students can instead positively *reframe*, or mindfully *accept* this event in order to better help them sleep, recover, and feel motivated to continue studying the next day. During the training session we emphasized that emotion regulation techniques were at students’ disposal to apply in ways that were appropriate and desired for their given situations. Thus, although emotion regulation can be applied to increase all types of emotions under appropriate circumstances (e.g., anger to motivate social change, sadness to empathize with a friends’ loss), for the goal of promoting mental wellbeing, the cognitive strategies to increase positive emotions and decrease negative emotions were strategically targeted with a toolkit of mindfulness, positive reappraisal, and savouring exercises. This balanced view of *accepting emotions*, while empowering users to regulate emotions how and when they choose without making “happiness” the goal, incorporates the current recommendations and best practices in health promotion and positive psychology literature (Catalino, Algoe, & Fredrickson, 2014; Mauss, Tamir, Anderson, & Savino, 2011; Seligman et al., 2005).

The current study found support for the benefits of emotion regulation in a field setting with a positive association between daily emotion regulation and lowered daily stress, negative affect, and greater positive affect. Results also suggests that emotion regulation is trainable above and beyond increases seen in a matched-expectancy placebo after only 10 days of self-practice and sustained until at least 1-month later. Given the increases found in emotion
regulation and favourable qualitative intervention evaluations, researchers and practitioners are encouraged to utilize and explore these promising techniques in combination. If possible, future designs should evaluate the benefits of brief emotion regulation training with even longer-term follow-ups for exploration of extended effects.

This randomized, matched-expectancy controlled design was critical for shedding light on a relatively underrepresented and inconclusive area of placebo research in health promotion interventions. Trials uncovering placebo benefits have increased in the fields of medicine, leading to the re-conceptualizing of placebo from an inert substance to a beneficial, active “positive care effect” (Blease, 2014). For example, studies have suggested that non-specific effects of treatment do in fact produce clinically significantly outcomes, with the patient-practitioner relationship being the most robust predictor (Kaptchuk et al., 2008). Moreover, pain symptoms are shown to decrease more when patients are told that they are receiving painkiller injections, in comparison to when the same injections are provided but they are not described as a pain killer (Peterson et al., 2014). Similarly, a meta-analysis compiling 35 antidepressant RCTs found non-significant differences in improvements in placebo pill versus antidepressant medication assignment, except for patients with very severe depression (Kirsh et al., 20018). Due to these known placebo benefits, scholars have expressed the need to better compare psychological interventions against matched-expectancy placebo conditions. However, these comparisons are seldom conducted due to feasibility and ethical constraints (viz., Boot et al., 2013). In addition, more evaluation of potential placebo effects against internal standard control conditions is encouraged to further deduce which intervention components contribute to placebo effects (Temple & Ellenberg, 2000).
The current study tackled both recommendations to provide valuable insight for future health promotion interventions. Firstly, our compliance check indicated that the placebo and intervention condition held equally high expectancy for wellbeing improvement, which was a critical assumption to allow us to directly attribute intervention effects to the content itself. Given mostly non-significant differences between the intervention and placebo toolkits, it appears that both toolkits promoted subjective wellbeing and perceived benefits. Implications for practice may be to focus less on the actual techniques for health promotion programs, and instead emphasize the development of a positive user experience aligned with participants’ values, interests, and schedules to have the greatest sustained effects. Research from clinical psychology suggests that actual intervention techniques only contribute 15-30% to patients’ improvements, with the 70-85% remainder attributable to a host of other common factors (Thomas, 2006). Potential common factors across conditions in the current study included social support from daily emails, interactive, in-person group training, autonomy and enjoyment from using the flexible, online Toolkits, and the mere act of distracting oneself from daily activities to engage in the intervention. These extraneous factors were readily seen in comments across the placebo and control condition such as: “This exercise relaxed me and gave me a breather from all the stress of school.”

Aside from extraneous factors, it is likely that the actual content of the placebo exercises in this study provided benefits to participants. While some studies show differential brain activation and null effects with the placebo exercises adapted for this study (e.g., Proyer et al., 2015; Zeidan et al., 2013), other meta-analytic research warns that placebo exercises can activate brain regions associated with top-down emotion regulation, similar to those active with mindfulness (Chiesa et al., 2010). In particular, Garland et al. (2013) found that a one-week
mindfulness intervention produced similar increases in emotion regulation as a sham suppression and mind wandering condition, controlled for extraneous factors.

It becomes more apparent that the construct of a definitive placebo condition may not actually exist, as its effects inextricably depend on how exercises are utilized and perceived. While some participants used the placebo exercises in more neutral ways to recall memories (e.g., “Helped me recall memories that I wasn't aware of before.”) other participants explicitly tapped into mindfulness, positive reappraisal, and savouring practices on their own in the placebo exercises to regulate their emotions (e.g., “This helps me reflect on the positives on my day and is a great way to end my day.”). It appears that when participants actively engage in exercises with the intention to improve their wellbeing, emotion regulation brain areas can become activated and affect can subsequently improve, even long-term.

This act of positively engaging with placebo exercises may be dependent on participants’ disposition, and/or situational factors such as the intervention experience and previously held beliefs about the exercises. For instance, previous research suggests that participants lower in dispositional mindfulness may benefit more from mindfulness interventions (Seear & Vella-Brodrick, 2013). Thus, dispositions associated with lower baseline mental health may find more value and consequently feel more motivated to engage positively in any given intervention. Further, certain types of dispositions, such as those susceptible to hypnosis, are more likely to experience heightened placebo effects (Lynn et al., 1991). Future studies can further explore the moderating role of disposition in health promotion interventions. Secondly, interventions that facilitate a positive user experience involving exercises (whether real or sham) that are intuitively believable as beneficial or even produce mild benefits (e.g., exploring past memories
and breathing versus a more neutral number sorting task) may further promote benefits across conditions.

Not only did this matched-expectancy design allow for rigorous placebo testing, it also helped with participant motivation and intervention engagement. To induce matched-expectancy of their wellbeing improvement, participants learned the benefits of each technique from their assigned toolkit from an engaging, in-person training session. As a result, participants in the placebo condition primed with expectancy beliefs reported practicing their assigned exercises significantly more than those in the control condition, despite assignment to identical materials. Participant motivation in self-administered wellbeing interventions has been established as a critical component to benefits (Lyubomirksy et al., 2011) and past interventions have utilized similar motivation-inducing approaches with inspirational quotes and explanations (e.g., Hülsheger et al., 2013). Future researchers and practitioners are encouraged to continue to prime participants with expectancy to enhance intervention compliance and engagement, optimally with comparison to an equally high expectancy placebo group (Boot et al., 2013).

However, an important consideration for priming expectancy beliefs in health promotion interventions is to avoid dissatisfaction from unrealistic and unmet expectations. A review of expectancy beliefs suggests that overall, higher expectancy beliefs bolster satisfaction with both already positive experiences and experiences that fail to meet expectations, as a means of reducing cognitive dissonance (Brown, Venkatesh, Kuruzovich, & Massey, 2008). This beneficial effect of high expectancy beliefs has been empirically supported. For instance, an RCT by Szabo and Kocsis (2016) found larger wellbeing improvements after an expectancy-primed intervention, in comparison to an un-primed deep breathing intervention. Conversely, there is evidence to suggest that high expectations paired with negative experiences (e.g., in
relation to an organizational training program) can actually lead to dissatisfaction and demotivation (viz., Tannenbaum, Mathieu, Salas, & Cannon-Bowers, 1991; Taris, Feij, & Capel, 2006). To explore this effect, the current study measured expectancy beliefs at both the onset and at the conclusion of the intervention, finding similarly high expectancy beliefs at both time points. While we did not directly measure the extent to which participants’ expectations for the intervention were met, this finding of high expectancy beliefs, along with the positive qualitative experiences participants reported across all conditions, suggest that users likely saw the intended value in their materials.

Another strength of the Mental Wellbeing Toolkit was the provision of three complementary techniques practiced conveniently via online audio when, where, and combined how participants desired. Ryan and Deci’s (2000) self-determination theory (SDT) suggests that fulfillment of humans’ basic psychological needs of autonomy, competence, and relatedness is fundamental to wellbeing. Need fulfillment is self-perpetuating, driving intrinsic motivation to continue to engage in activities that fulfill basic needs, in turn sustaining wellbeing outcomes (Ryan & Deci, 2000). While rigid interventions may thwart need fulfillment, providing choices to participants can fulfill the need of autonomy, or the ownership or volition over one’s behaviours (Ryan & Deci, 2000). Subsequently, more autonomy can lead to greater engagement and mastery of materials, fulfilling a second need of competence, or the perceived capability to overcome challenges and attain desired outcomes. An intervention that fulfills these needs should not only directly promote wellbeing, but also indirectly through increased intrinsic motivation to engage in the intervention (Ryan & Deci, 2000).

The current design equalized choice and autonomy across all conditions with the administration of customizable toolkits and flexible self-practice via technology. This enabled
isolation of the direct effects of the intervention materials and manipulations themselves, beyond positive user experience. Given the improvements seen across all conditions, enhanced by participants’ intrinsic motivation to participate in the intervention, future interventions can potentially harness more autonomy, choice, and positive user experiences as a component of the experimental and matched-expectancy placebo groups’ materials only. Using less engaging or inactive control conditions can allow future designs to discard history effects.

Scholars designing future interventions are encouraged to further bridge the gap between technology and psychology to create convenient and enriching user experiences with software to better promote mental wellbeing through SDT need fulfillment (e.g., mobile apps monitoring physiological stress responses wherever users go, on-demand guided exercises, personalized wellbeing profiles and suggestions, etc.). This fruitful area of research is increasing in the education literature, with a meta-analysis depicting that providing students with autonomous learning via technological instruction (e.g., podcasts, websites, e-books) has an overall small but beneficial effect on behavioural outcomes such as motivation and self-efficacy (Karich, Burns, & Maki, 2014). Exploring how technology can aid in learning and practice of health promoting behaviours has high potential to provide the autonomy, mastery experiences with real-time data monitoring, convenience, person-intervention fit, and cost-effectiveness needed for wellbeing interventions in the general population (Peters & Calvo, 2014; Lyubomirsky & Layous, 2013).

In all, exploring the Mental Wellbeing Toolkit with University students has vast practical implications for students, research, and society at large. Similar multicomponent interventions can help empower organizations, academic institutions, and individuals in the general population to personally learn and form new habits of engaging in emotion regulation exercises in daily life. University students face a host of unique academic, social, and developmental challenges as they
embark on new experiences often for the first time independently in their lives. The high rates of mental illness and sleep problems in this population, in addition to the link between mental health and academic performance (Furr et al., 2001; Keyes et al., 2012), present a critical case for further development of interventions for the university population. As well, this student sample of computer-literate young adults embarking on new academic challenges and stressors can resemble the workforce of Millennial employees. This younger workforce may be particularly inclined to benefit from electronic, flexible, and self-administered wellbeing interventions that have potential for application in organizations.

**Conclusion**

This two-week intervention field study suggests that self-practice of the Mental Wellbeing Toolkit combining mindfulness, positive reappraisal, and savouring exercises may be particularly effective at increasing both daily and dispositional adaptive emotion regulation over and above placebo effects. Interestingly, placebo effects were strong with the matched-expectancy sham interventions as seen with all of the wellbeing outcomes improving over time across all conditions, even up to the 1-month follow-up. Longer-term measures could potentially uncover more improvements in the intervention group over time as placebo effects wear off. However, given the present quantitative and qualitative participant experience data, it appears that both introspective sham exercises and the proposed emotion regulation toolkit can help students cope with stress and promote mental wellbeing in daily life. Specifically, promoting a positive participant experience (e.g., support, enjoyment, motivation, and expectancy) may facilitate benefits seen with brief, self-administered health promotion interventions.
References


Endnotes

1 To conduct the best fitting and most parsimonious multilevel analyses, the recommended growth modeling sequence by Bliese and Ployhart (2002) was followed: (1) Calculate ICC values for all dependent variables using the Null Models to determine if multilevel analysis is appropriate based on sufficient between and within person variance for each outcome variable, (2) Determine the best representation of time (e.g., linear, quadratic) based on significance tests in random intercepts/fixed slopes models (i.e., participants allowed to vary at baseline from one another but not over time) for each outcome variable, (3) Determine if random intercepts/random slopes models (i.e., participants allowed to vary at baseline and over time) fit the data better than random intercepts only models for each outcome variable, based on increased log likelihood values, (4) Determine if data requires corrections (e.g., need for autocorrelation correction based on increases in log likelihood ratios in models with the correction versus models without the correction), (5) Once the best Model 1 is constructed based on steps 1-4, build up Model 2 by adding variables in order of theoretically-driven importance, (6) estimate effect sizes by comparing the amount of variance (i.e., within-person, between-person, and slope) in outcome variables explained by each added predictor in Model 2 as compared to the previous model without the predictor.

2 Given the significant quadratic relation over time found for daily negative affect, daily perceived stress, and daily emotion regulation, multilevel modeling results were re-run with quadratic growth models in addition to linear growth models. No hypotheses were significant for quadratic growth models, suggesting that the significant condition differences that were present (i.e., sharper growth of emotion regulation in the intervention compared to placebo) were specifically in relation to a sharper linear slope in the intervention.
Figure 1. Proposed multivariate, multilevel 2-1-1 mediation model of daily emotion regulation mediating changes in the second week of the intervention in daily negative affect, positive affect, and perceived stress as a result of condition assignment. Arrows with circles indicate random effects and arrows without circle indicate fixed effects. The combined $ab$ path represents the indirect mediation effect.
Figure 2. Proposed theory for the incremental changes expected from consistent practice of the Mental Wellbeing Toolkit. The broaden and build process (Fredrickson, 2001) triggers upward spirals of positive emotions and emotion regulation resource building that counteract negative affect and stress, in combination promoting sustained mental wellbeing in the long-term.
Figure 3. CONSORT Diagram of participant flow and analysis (Schulz et al., 2010).
Figure 4. Process of daily data collection and exercise completion instructions. Each daily outcome survey links to the previous day’s daily exercise (with the exception of the outcome survey of week 1 measuring the post-training baseline). Thus, each outcome survey represents the incremental fluctuations in DVs (daily negative affect, positive affect, and perceived stress) after the previous day’s exercise completion.
**Figure 5.** Multilevel growth changes in emotion regulation over 10 days of exercise practice measured over 11 days of morning outcome surveys (Day 1 = Post-training baseline and first exercise day, Day 10 = Final exercise day, Day 11 = Final outcome survey) and over 3 time points (pre-training baseline, post-intervention, 1-month follow-up). Time*Condition differences between Intervention and Placebo were significant over the daily measures, supporting H1a, and significant between Intervention and Placebo over the three time points, supporting H1b.
Figure 6. Multilevel growth changes in daily negative and positive affect over 10 days of exercise practice measured over 11 days of morning outcome surveys (Day 1 = Post-training baseline and first exercise day, Day 10 = Final exercise day, Day 11 = Final outcome survey). Time*Condition differences across conditions were non-significant over the daily measures for either negative or positive affect, not supporting H2a or H2b, respectively.
Figure 7. Multilevel growth changes in perceived stress over 10 days of exercise practice measured over 11 days of morning outcome surveys (Day 1 = Post-training baseline and first exercise day, Day 10 = Final exercise day, Day 11 = Final outcome survey) and over 3 time points (pre-training baseline, post-intervention, 1-month follow-up). Time*Condition differences across conditions were non-significant over the daily measures, not supporting H3a, and marginally significant between Intervention and Placebo over the three time points, marginally supporting H3b.
Figure 8. Multilevel growth changes in mental wellbeing scores over 3 time points (pre-training baseline, post-intervention, 1-month follow-up). Time*Condition differences between the Intervention and Placebo group were marginally significant, marginally supporting H5.
### Table 1

*Summary of Toolkit techniques used in the experimental, placebo and control conditions.*

<table>
<thead>
<tr>
<th>Technique</th>
<th>Mindfulness / Breathing Exercise</th>
<th>Positive Reappraisal/ Recent Memory Recall</th>
<th>Savouring/ Distant Memory Recall</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Experimental</strong></td>
<td>(A) Close eyes and sit comfortably. (B) Observe thoughts, bodily sensations, and emotions in a non-judgemental and curious manner. (C) Gently let go of these internal experiences as easily as they come to the mind. Training and online Toolkit instructions explain research-based support for mindfulness.</td>
<td>(A) Close eyes and sit comfortably. (B) Select a negative event currently on the mind. (C) Replay event from a third person, objective perspective. (D) Explore any possible good that can come from this event (e.g., ways to grow or learn from hardship). Training and online Toolkit instructions explain research-based support for positive reappraisal.</td>
<td>Training and online Toolkit instructions explain research-based support for savouring.</td>
</tr>
<tr>
<td><strong>“Mental Wellbeing Toolkit”</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Placebo</strong></td>
<td>(A) Close eyes and sit comfortably. (B) No instruction to breathe deep or slow, just to listen to breathing. Training and online Toolkit instructions explain “sham research” support for focus on breathing.</td>
<td>(A) Close eyes and sit comfortably. (B) Replay any event from today. (C) Think about why this event came to mind. Training and online Toolkit instructions explain “sham research” support for recent memory recall.</td>
<td>Training and online Toolkit instructions explain “sham research” support for distant memory recall.</td>
</tr>
<tr>
<td><strong>“Mental Wellbeing Toolkit”</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Control</strong></td>
<td>Same exercise as placebo. No explanation of any “sham research”. Told to complete exercises purely for exploratory research purposes.</td>
<td>Same exercise as placebo. No explanation of any “sham research”. Told to complete exercises purely for exploratory research purposes.</td>
<td>Same exercise as placebo. No explanation of any “sham research”. Told to complete exercises purely for exploratory research purposes.</td>
</tr>
<tr>
<td><strong>“Cognitive Activity Toolkit”</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 2

Means, standard deviations and statistical tests comparing baseline measures, exercise length and intervention days completed across condition and sample retention.

<table>
<thead>
<tr>
<th>Baseline Measure</th>
<th>Control M(SD)</th>
<th>Placebo M(SD)</th>
<th>Intervention M(SD)</th>
<th>Test Statistic</th>
<th>p</th>
<th>Analyzed M(SD)</th>
<th>Dropout/Excluded M(SD)</th>
<th>Test Statistic</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gender (% male)</td>
<td>10.00%</td>
<td>9.76%</td>
<td>13.16%</td>
<td>F = 0.11</td>
<td>ns</td>
<td>10.92%</td>
<td>9.10%</td>
<td>( \chi^2 = 3.26 )</td>
<td>ns</td>
</tr>
<tr>
<td>Negative Affect</td>
<td>3.67 (1.32)</td>
<td>3.93 (1.19)</td>
<td>3.91 (1.32)</td>
<td>F = 0.48</td>
<td>ns</td>
<td>3.84 (1.27)</td>
<td>3.49 (1.04)</td>
<td>t = −1.07</td>
<td>ns</td>
</tr>
<tr>
<td>Positive Affect</td>
<td>4.48 (.69)</td>
<td>4.40 (0.98)</td>
<td>4.27 (0.96)</td>
<td>F = 0.59</td>
<td>ns</td>
<td>4.39 (0.88)</td>
<td>4.20 (1.01)</td>
<td>t = −0.80</td>
<td>ns</td>
</tr>
<tr>
<td>Perceived Stress (Threat)</td>
<td>4.75 (1.09)</td>
<td>4.84 (0.92)</td>
<td>5.12 (0.85)</td>
<td>F = 1.05</td>
<td>ns</td>
<td>4.90 (0.96)</td>
<td>4.60 (1.02)</td>
<td>t = −1.17</td>
<td>ns</td>
</tr>
<tr>
<td>Mental Wellbeing</td>
<td>4.20 (0.71)</td>
<td>4.00 (0.98)</td>
<td>3.98 (0.88)</td>
<td>F = 0.77</td>
<td>ns</td>
<td>4.06 (0.86)</td>
<td>4.00 (0.75)</td>
<td>t = −0.29</td>
<td>ns</td>
</tr>
<tr>
<td>Emotion Regulation (Reappraisal)</td>
<td>4.72 (0.97)</td>
<td>4.58 (1.19)</td>
<td>4.43 (1.15)</td>
<td>F = 0.64</td>
<td>ns</td>
<td>4.58 (1.11)</td>
<td>4.49 (1.50)</td>
<td>t = −0.27</td>
<td>ns</td>
</tr>
<tr>
<td>Exercise Length (Mean Minutes/ Day)</td>
<td>9.58 (3.66)</td>
<td>8.91 (4.18)</td>
<td>10.63 (3.81)</td>
<td>F = 1.94</td>
<td>ns</td>
<td>9.69 (3.93)</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Assigned Daily Exercises Completed</td>
<td>7.90 (2.71)</td>
<td>7.54 (2.58)</td>
<td>8.18 (2.40)</td>
<td>F = 0.63</td>
<td>ns</td>
<td>7.87 (2.56)</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

Note. SD = standard deviation, ns = not significant. Comparisons between the three conditions were analyzed with one-way ANOVA analyses. Post hoc tests between groups were not conducted due to non-significant main effects of condition. Comparisons between the analyzed versus dropout/excluded participants were conducted with t-test and chi-square analyses. All continuous outcome variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement.
### Table 3

**Means, standard deviations, and zero-order correlations among study variables and demographics**

<table>
<thead>
<tr>
<th>Variables</th>
<th>M/%</th>
<th>Within-Person SD</th>
<th>Between-Person SD</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Mental Wellbeing Toolkit vs. Placebo¹</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>2. Placebo vs. Control²</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>3. Day</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>4. Daily Negative Affect</td>
<td>2.98</td>
<td>0.79</td>
<td>0.92</td>
<td>-0.01</td>
<td>0.02</td>
<td>–0.15**</td>
<td>(0.88–0.93)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Daily Positive Affect</td>
<td>4.52</td>
<td>0.65</td>
<td>0.83</td>
<td>-0.04</td>
<td>0.00</td>
<td>0.04</td>
<td>–0.73**</td>
<td>(0.92–0.95)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6. Daily Perceived Stress (Threat)</td>
<td>3.60</td>
<td>0.91</td>
<td>0.73</td>
<td>-0.05</td>
<td>0.03</td>
<td>–0.15**</td>
<td>0.76**</td>
<td>–0.58**</td>
<td>(0.82–0.87)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>7. Daily Emotion Regulation</td>
<td>4.20</td>
<td>0.63</td>
<td>0.83</td>
<td>-0.07</td>
<td>0.08*</td>
<td>–0.05</td>
<td>–0.31**</td>
<td>0.44**</td>
<td>–0.20**</td>
<td>(0.90–0.95)</td>
<td></td>
</tr>
<tr>
<td>8. Previous Mindfulness Practice³</td>
<td>5.30%Yes</td>
<td>-</td>
<td>-</td>
<td>0.02</td>
<td>–0.01</td>
<td>–0.02</td>
<td>0.00</td>
<td>0.02</td>
<td>–0.01</td>
<td>0.08*</td>
<td></td>
</tr>
<tr>
<td>9. Gender⁴</td>
<td>89.08%Female</td>
<td>-</td>
<td>-</td>
<td>-0.01</td>
<td>–0.02</td>
<td>0.04</td>
<td>–0.04</td>
<td>0.07*</td>
<td>–0.04</td>
<td>–0.01</td>
<td>0.04</td>
</tr>
</tbody>
</table>

*Note. SD = standard deviation. Correlations at the day level are displayed (n=1133) for daily measures (i.e., negative affect, positive affect, perceived stress, emotion regulation use). ¹ 1 = Mental Wellbeing Toolkit Intervention, 0= Placebo; ² 1 = Placebo, 0 = Control. ³ 1 = Yes, 0 = No. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. ⁴ 1 = Female, 0 = Male. Values on the diagonal represent internal consistencies of scales, with daily alphas calculated for each day and presented as a range. *p < .05. **p < .01. ***p < .001
### Table 4

*Parameter Estimates and Variance Components of Null Models for Pre, Post, and 1-Month Follow-up Outcome Ratings and Daily Measures.*

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Intercept ($\gamma_{00}$)</th>
<th>Between-individual variance ($\tau_{00}$)</th>
<th>Within-individual variance ($\rho_{00}$)</th>
<th>% Variability within-individual</th>
</tr>
</thead>
<tbody>
<tr>
<td>Daily Emotion Regulation</td>
<td>4.20</td>
<td>0.77</td>
<td>0.31</td>
<td>28.40</td>
</tr>
<tr>
<td>Daily Negative Affect</td>
<td>3.00</td>
<td>0.85</td>
<td>0.62</td>
<td>42.08</td>
</tr>
<tr>
<td>Daily Positive Affect</td>
<td>4.52</td>
<td>0.69</td>
<td>0.42</td>
<td>37.68</td>
</tr>
<tr>
<td>Daily Perceived Stress (Threat)</td>
<td>3.61</td>
<td>0.94</td>
<td>0.74</td>
<td>44.22</td>
</tr>
<tr>
<td>Pre, Post, 1-M Perceived Stress (Threat)</td>
<td>4.43</td>
<td>0.53</td>
<td>0.85</td>
<td>61.29</td>
</tr>
<tr>
<td>Pre, Post, 1-M Mental Wellbeing</td>
<td>4.22</td>
<td>0.58</td>
<td>0.26</td>
<td>31.21</td>
</tr>
</tbody>
</table>

Note. $\gamma_{00} =$ pooled intercept representing the average level of dependent variable across individuals; $\tau_{00} =$ between-individual variance in the dependent variable; $\rho_{00} =$ within-individual variance in the dependent variable. Percentage of variability within-individual ($1 – ICC$) was computed as $\rho_{00}/(\rho_{00} + \tau_{00})$. All variables are measured on 7-point likert scales with larger numbers indicating higher construct endorsement.
Table 5

*Hypothesis 1a: Goodness of fit across multi-level growth models predicting daily emotion regulation over 10 days of exercises.*

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Model 1</th>
<th></th>
<th></th>
<th>Model 2</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimate</td>
<td>SE</td>
<td>df</td>
<td>t</td>
<td>Estimate</td>
<td>SE</td>
<td>df</td>
</tr>
<tr>
<td><strong>Between-Persons</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>4.29</td>
<td>0.09</td>
<td>1013</td>
<td>49.02***</td>
<td>4.29</td>
<td>0.09</td>
<td>1011</td>
</tr>
<tr>
<td>Mental Wellbeing Toolkit vs. Placebo¹</td>
<td>-0.07</td>
<td>0.12</td>
<td>116</td>
<td>-0.86</td>
<td>-0.15</td>
<td>0.12</td>
<td>116</td>
</tr>
<tr>
<td>Placebo vs. Control²</td>
<td>0.02</td>
<td>0.12</td>
<td>116</td>
<td>-0.16</td>
<td>-0.08</td>
<td>0.12</td>
<td>116</td>
</tr>
<tr>
<td>Residual Variance</td>
<td>0.77</td>
<td>0.88</td>
<td></td>
<td></td>
<td>0.76</td>
<td>0.87</td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>-0.01</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Within-Persons</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Time</td>
<td>-0.02</td>
<td>0.01</td>
<td>1013</td>
<td>-2.03*</td>
<td>-0.02</td>
<td>0.01</td>
<td>1011</td>
</tr>
<tr>
<td>Time* Mental Wellbeing Toolkit vs. Placebo²</td>
<td>0.02</td>
<td>0.01</td>
<td>1011</td>
<td>2.04*</td>
<td>0.03</td>
<td>0.01</td>
<td>1011</td>
</tr>
<tr>
<td>Residual Variance</td>
<td>0.28</td>
<td>0.53</td>
<td></td>
<td></td>
<td>0.28</td>
<td>0.53</td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>0.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note. SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. ¹1 = Mental Wellbeing Toolkit Intervention, 0 = Placebo; ²1 = Placebo, 0 = Control. Average number of exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size (R²) was calculated as the within and between person variance explained by the predictors, 1 – (variance of model – variance of previous model). †p < .10 *p < .05 **p < .01 ***p < .001
Table 6

*Hypothesis 1b: Goodness of fit across multi-level growth models predicting emotion regulation over pre, post and 1-month follow-up measures.*

<table>
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*Note. SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. $^1$ = Mental Wellbeing Toolkit Intervention, 0 = Placebo; $^2$ = Placebo, 0 = Control. Average number of intervention exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size ($R^2$) was calculated as the within and between person variance explained by the predictors, $1 – (\text{variance of model} – \text{variance of previous model})$. †$p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$
Table 7
Hypothesis 2a: Goodness of fit across multi-level growth models predicting daily negative affect over 10 days of exercises.

<table>
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<td>1011</td>
<td>33.54***</td>
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<td>0.14</td>
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<td>0.01</td>
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<td>-6.48***</td>
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<td>Time* Placebo vs. Control</td>
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</table>

Note. SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. \(^1\) = Mental Wellbeing Toolkit Intervention, 0 = Placebo; \(^2\) = Placebo, 0 = Control. Average number of intervention exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size (R\(^2\)) was calculated as the within and between person variance explained by the predictors, 1 – (variance of model – variance of previous model). \(^\dagger\) p < .10 *p < .05 **p < .01 ***p < .001
Table 8

*Hypothesis 2a: Goodness of fit across multi-level growth models predicting daily positive affect over 10 days of exercises.*

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<th>Parameter</th>
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<td>116</td>
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<tr>
<td>Residual Variance</td>
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</table>

*Note.* SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. $^1$ = Mental Wellbeing Toolkit Intervention, 0 = Placebo; $^2$ = Placebo, 0 = Control. Average number of intervention exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size ($R^2$) was calculated as the within and between person variance explained by the predictors, $1 – (\text{variance of model} – \text{variance of previous model})$. $^\dagger p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$
### Table 9

**Hypothesis 3a: Goodness of fit across multi-level growth models predicting daily perceived stress over 10 days of exercises.**

<table>
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<th>Parameter</th>
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<th>SE</th>
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<td>1013</td>
<td>37.54***</td>
<td>3.93</td>
<td>0.10</td>
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<td>37.54***</td>
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</tr>
<tr>
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<td>0.01</td>
<td>1013</td>
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<td>0.01</td>
<td>1011</td>
<td>5.48***</td>
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<td>(R^2)</td>
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</table>

**Note.** SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. \(^1\) = Mental Wellbeing Toolkit Intervention, 0 = Placebo; \(^2\) = Placebo, 0 = Control. Average number of intervention exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size \(R^2\) was calculated as the within and between person variance explained by the predictors, \(1 – \text{(variance of model – variance of previous model)}\). \(^1\) \(p < .10\) \(^2\) \(p < .05\) \(^*\) \(p < .01\) \(^**\) \(p < .001\)
Table 10

Hypothesis 3b: Goodness of fit across multi-level growth models predicting perceived stress over pre, post and 1-month follow-up measures.

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<td>Time* Placebo vs. Control</td>
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Note. SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. ¹¹ = Mental Wellbeing Toolkit Intervention, 0 = Placebo; ²² = Placebo, 0 = Control. Average number of intervention exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size (R²) was calculated as the within and between person variance explained by the predictors, 1 – (variance of model – variance of previous model). †p < .10 *p < .05 **p < .01 ***p < .001
Table 11

_Hypothesis 5: Goodness of fit across multi-level growth models predicting mental wellbeing over pre, post and 1-month follow-up measures._

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</table>

Note. SE = standard error. All models are random intercept models. Model 2 includes the Time*Condition cross-level interaction term to assess growth. ¹¹ Mental Wellbeing Toolkit Intervention, 0 = Placebo; ²² Placebo, 0 = Control. Average number of intervention exercises completed per person = 7.87. All continuous variables were measured on a 7-point likert scale with larger numbers indicating higher construct endorsement. Effect size (R²) was calculated as the within and between person variance explained by the predictors, 1 – (variance of model – variance of previous model). † p < .10 * p < .05 ** p < .01 *** p < .001
Table 12

_Hypothesis 4: Path results of a 2-1-1 MSEM analysis examining daily emotion regulation mediating the effects of condition on daily wellbeing._

<table>
<thead>
<tr>
<th>Daily Variables (Level 1)</th>
<th>Path a</th>
<th>Path b</th>
<th>Path ab</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>β</td>
<td>SE</td>
<td>p</td>
</tr>
<tr>
<td>Mental Wellbeing Toolkit vs. Placebo</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Emotion Regulation Use (M)</td>
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<td>0.22</td>
<td>0.64</td>
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<tr>
<td>Negative Affect</td>
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<td></td>
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<tr>
<td>Positive Affect</td>
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<td>0.08</td>
<td>0.00***</td>
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<tr>
<td>Perceived Stress</td>
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<td>0.04*</td>
</tr>
<tr>
<td>Control vs. Placebo</td>
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<tr>
<td>Emotion Regulation Use (M)</td>
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<td>0.22</td>
<td>0.17</td>
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<tr>
<td>Negative Affect</td>
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<tr>
<td>Positive Affect</td>
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<td>0.00***</td>
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<tr>
<td>Perceived Stress</td>
<td>-0.21</td>
<td>0.10</td>
<td>0.03*</td>
</tr>
</tbody>
</table>

_R² represents the variance explained by the total mediation model (effect size) †p < .10 *p < .05 **p < .01 ***p < .001_

_Note. SE = standard error. All models include the independent variable of condition at level 2, the mediator of daily emotion regulation over the second week of intervention (Monday – Saturday) at level 1 and three multivariate dependent outcome measures from the second week of the intervention (Monday – Saturday) at level 1. Path a represents the effect of condition on emotion regulation. Path b represents the effect of emotion regulation on all multivariate outcome measures. Path ab represents the indirect mediation effect for all dependent daily variables._
DEBRIEF FORM AND POST-DEBRIEF CONSENT

The main goal of this study was to assess the effectiveness of training participants in evidence-based positive psychology techniques for improving their well-being (e.g., emotion regulation skills, engagement, life satisfaction, reduced stress). In order to conduct a true experiment and control for any confounding variables, such as the common placebo effect found when participants improve simply due to receiving help they believe is effective, we have created three conditions and randomly assigned participants in this study to: the control group, the placebo group, and the experimental group. In order to control for any confounding variables and truly assess the benefits in training well-being techniques, it was necessary to use deception and not inform participants which condition they were assigned to. Thus, if you received the “Cognitive Activity Toolkit” you were assigned to the control condition, if you were assigned to the “Mental Well-being Toolkit” consisting of the tools of the breathing exercise, a recent memory self-reflection exercise, and a distant memory self-reflection exercise, you were assigned to the placebo condition, while if you received the “Mental Well-being Toolkit” with the tools of savouring, mindfulness, and positive reappraisal, you were assigned to the experimental condition.

If you were in the placebo condition training, you were led to believe that the exercises you were trained in should improve your well-being. In actuality, these exercises have not been established in the research to have any effect on the well-being measures that we examined, although some of you may have noticed changes depending on a variety of individual differences. Similarly, if you were in the “Cognitive Activity Toolkit”, you were trained in these same placebo exercises but did not receive explanation of any well-being benefits; you were simply told to practice the tools to see what happens. Use of this deception was needed to examine if any placebo effects arise due to participants expecting that their training and daily exercises should improve their well-being. Now that the study is complete, you are welcome to receive the same Well-being Toolkit materials as the experimental group if you wish.

The research-based intervention of the “Well-being Toolkit” taught participants the techniques of savouring (i.e., appreciating and reflecting on positive moments that happen each day), positive reappraisal (i.e., re-framing negative events and feelings to change our emotional responding to be more beneficial for us) and mindfulness (i.e., present moment awareness that goes beyond just listening to your breathing as with the placebo activity, but allowing your judgments and thoughts to gently pass).

If you were in the “Mental Well-being Toolkit” training, you did in fact learn and use these techniques shown in past research to improve various aspects of your well-being over consistent practice. However, you were not informed that there were also two other conditions in which participants received training in other “placebo” exercises that should not lead to the same type and degree of well-being benefits or other noticeable improvements.

The experimenter and all researchers involved would like to apologize for deceiving you and hope you understand why the deception was necessary to accurately understand the effectiveness of the well-being exercises.
If, after reading about the deception involved in this study, you are willing to allow the researchers to use your results in the analyses, please complete the information below.

Thank you for your participation!

This study has been reviewed and received ethics clearance through the University of Guelph Research Ethics Board. If you have questions regarding your rights as a research participant, contact:

Sandy Auld, Director, Research Ethics University of Guelph
437 University Centre Guelph, ON N1G 2W1
Telephone: (519) 824-4120, ext. 56606
E-mail: sauld@uoguelph.ca
Fax: (519) 821-5236

*Have you read the information provided for the study “The Life and Wellness Study” as described herein and still agree to have the researchers analyze the data you have provided?*

**YES/NO**
Appendix B

Experimental Exercise Scripts

**Mindfulness.**
Try to let go of any preconceived notions and simply see where this awareness practice takes you. Take a deep and steady breath in and slowly exhale. Start to notice the physical sensations in your body. Where do you feel any pressures or tensions? With a deep inhale, begin to feel yourself loosening these pressures, beginning to let go, becoming more accepting of your bodily sensations. Begin by thinking about how you are feeling right now. What emotions and moods come to mind? What thoughts do you hear? Try to stay curious about what your mind and body are telling you, while letting attachment to these thoughts gently slip away just as they come in.

10-second pause for reflection
Now start to gently bring the focus of your awareness down the right arm, into the right elbow, and slowly feel the energy trickling down into the right hand and fingers. Try to bring a gentle curiosity to explore the sensations that you find. You may become aware of the air on your hand, the touch of your fingers, the surfaces that your hand rests on. Any sensations are perfectly natural. Breathe in deeply, and gently release this breath, holding the attention on your right arm. Remember, any and all sensations are perfectly normal. It is important to just simply become aware of the sensations, without casting any judgment on the experience.

When you’re ready, take a deep inhale, feel the lower belly expanding, and then slowly contracting as you exhale. Now inhale, and try imagining the breath entering deep into your lungs. Then, on your exhale, feel the air passing down through the right arm, through the right elbow, and through the right hand, fingers, and slowly out of the fingertips. If your mind begins to wander, remember that this is okay and completely natural. Simply acknowledge this thought, and then gently let go of that thought. Start to gently bring the focus of your awareness down the left arm, into the left elbow, and slowly feel the energy trickling down into your left hand and fingers. Start to notice the sensations though each of your left fingers. Try to bring a gentle curiosity to explore the sensations that you find. Breathe in deeply, and gently release this breath, holding your attention on the sensations that you feel in your left hand. Now take a deep inhale with your lower belly expanding, and slowly contract it as you exhale. Inhale and feel the breath entering deep into your lungs. On your exhale, feel the air passing down through the left arm, the left elbow, and out the left hand, fingers, and fingertips. Thank yourselves for taking five minutes now to practice a mindful state with yourself and your surroundings. Take the time right now to continue practicing this mindful state and deep breathing on your own if you wish.

**Positive Reappraisal.**
Let's begin by taking notice of your starting sensations, thoughts, and feelings. Try to lower any expectations you may have to block or suppress your internal states right now. Open your mind to becoming aware and accepting of your inner experiences. Start to gently close your eyes, breathe in deeply, and slowly exhale. Notice if you feel any tension in your body. Feel free to make any necessary movements. You may find it harder to let go of these tensions after a negative event, and this is completely normal. Take a deep and steady breath in, and slowly release it. Try to maintain this deep and steady breathing as you start to think about the negative event that may be worrying, saddening, or bothering you in any way today. Start to reply this event in your mind, taking yourself away from the moment, and simply watching it, as an observer would, play in back in your mind.

10-second pause for reflection
Try to simply observe what objectively took place in this event.

10-second pause for reflection

If you start to sense any intruding thoughts or judgments, simply become aware of them, and just watch, as an observer, as they pass through your mind.

10-second pause for reflection

We will now explore into the future. Although there are many ways this event may be affecting you negatively at this moment, try to explore the ways that you can grow from this experience for the future. What lessons, new knowledge, or change in perspective has this event given you that can help you from this point forward?

10-second pause for reflection

Think about how experiencing this event now can help you feel stronger or more capable of facing something similar, or even facing new challenges in the future?

10-second pause for reflection

How can you learn to relate to, empathize with, or help others overcome similar challenges in the future?

10-second pause for reflection

Now try to open up your mind and think of all other potential benefits, no matter how small, which may come from experiencing this event.

10-second pause for reflection

Begin to think about the feelings related to all the possible good from this event.

10-second pause for reflection

Take a deep and final breath in, and slowly exhale. Thank yourself for taking the time to gain strength and learning from difficult moments.

Savouring.

Start to gently close your eyes, and breathe deeply. Allow yourself to get a little bit more comfortable, wherever you are right now. Breathe in nice and deeply, and then gently release the air with an extended exhale. Take this deep breathing with you as we now begin to savour a positive event that you chose. Replay this event slowly and carefully in your mind, as if you were right in this very place, right here, right now.

10-second pause for reflection

Really capture the positive moment and your appreciation and gratitude.

10-second pause for reflection

Now we will start to take your savouring experience even deeper and try to focus on the smaller details of this moment. Really take them in and appreciate these small positive parts.

10-second pause for reflection

If you start to notice any lingering thoughts, worries, or noise, remember that this is completely natural. Just simply notice these thoughts, and refocus your mind on the positive moments.

10-second pause for reflection

Remember to try and maintain your slow and steady breathing, as you continue to explore and bask in this moment on your own for a few more deep breaths.

10-second pause for reflection

Now let's begin to fully draw your attention to the feelings that you are experiencing as you explore this positive moment with awareness and curiosity. Let these feelings from the positive moment wash over you. How are you feeling right now?

10-second pause for reflection

Let's focus now on why this positive moment happened to you. Simply try to come up with something that feels right for you. Take note of even the smallest role that you played in creating this moment today.
10-second pause for reflection

Try to remind yourself of these small moments throughout your day today. Observe how thinking about such a small moment can make you feel. Stay with these feelings and thank yourself for taking the time to practice savouring.

**Placebo/Control Exercise Scripts**

**Breathing Exercise.**

Begin by getting comfortable, wherever you are. Close your eyes, if you wish. Start to focus on your breathing. What are the types of sounds that you hear in your breathing, right now? Do you notice yourself breathing fast, or slow, or maybe just average pace? Start to notice any changing sensations in your breathing. Simply listen to your breathing, right now.

10-second pause for reflection

Notice if there are any other sounds that you hear around you, right now. What are these sounds? What do you hear?

10-second pause for reflection

Are the sounds around you very loud, or very soft, or maybe somewhere in the middle? What kinds of things do these sounds make you think about? Continue to listen to your own breathing.

10-second pause for reflection

Start to also notice the rhythm of your inhales and your exhales. Are both the same length and rhythm, or different?

10-second pause for reflection

What other thoughts are coming to mind right now?

10-second pause for reflection

Try to integrate the sounds that you hear all around you with your breath. What is it like for you to try to listen to your breathing, overtop of all of the different sounds around you right now? Is this this difficult, easy, or normal for you to experience? Are you noticing any unusual sounds in your environment right now? Continue to listen to all of these sounds around you.

10-second pause for reflection

What about your breathing? Do you notice anything unusual about your breath right now? Continue to integrate the sounds that you hear with your breathing. What are all the thoughts that are coming to mind?

10-second pause for reflection

Thank you for practicing the breathing exercise. Continue to listen to your breathing right here and now if you wish.

**Distant Memory Recall.**

Let's begin by sitting or standing comfortably, wherever we are, right now. Now start to think about the distant memory that you chose. What is this distant memory? Where were you when this happened, who were you with, and what happened in this moment? Take a look back at this moment and now fully replay the details in your mind, right here and now.

10-second pause for reflection

What parts of the memory are immediately coming to mind?

10-second pause for reflection

What parts are you having difficulty remembering?

10-second pause for reflection

What parts of the memory are unusual?
10-second pause for reflection
Continue to simply observe this memory in your mind and watch it play back in your head.

10-second pause for reflection
Think about the parts that you are having difficulty remembering. Start to think about why you are having difficulty remembering these moments. How are these gaps different from the parts that are most clear in your mind?

10-second pause for reflection
Try to think about the thoughts that you were having when this moment took place. What were you thinking at the time?

10-second pause for reflection
What are your thoughts about the memory now?

10-second pause for reflection
Continue to really replay this whole memory in your mind for a few more moments.

10-second pause for reflection
Do you notice any other parts becoming more noticeable as you observe this memory further?

10-second pause for reflection
Are the parts that are most clear or important to you now the same as the parts that were most clear at the start of this memory reflection?

10-second pause for reflection
Do you notice any new thoughts coming to mind about this memory?

10-second pause for reflection
Thank you for practicing the distant memory recall. You may continue thinking about this distant memory on your own if you wish.

Recent Memory Recall.
Begin by taking a seat, or standing comfortably wherever you are. Maybe close your eyes if this helps you with your reflection process. Start to go back to this memory that you came up with from today. Remember that this can be any memory or event, big or small, from today. Start to replay this memory in your mind, just as it happened. What parts are really coming to mind?

10-second pause for reflection
What parts are less noticeable in your mind?

10-second pause for reflection
What were you thinking when this moment was happening? Start to go back to your thought process and what was going on in your mind.

10-second pause for reflection
What are your thoughts about your memory now? Examine these thoughts.

10-second pause for reflection
Why do you think this memory came to your mind from today?

10-second pause for reflection
Is this memory starting to bring back other memories or events from today, or from this past week?

10-second pause for reflection
Are you noticing anything in particular from this event coming to your mind right now?

10-second pause for reflection
Now start to think about why this moment happened to you today. This may be hard to come up with at first, but just notice any explanation that you can come up with.

10-second pause for reflection
Take a few more moments to really replay this memory in your mind, just as it happened.
10-second pause for reflection
Bring your mind, your body, and your awareness, back to this memory right here and now.

10-second pause for reflection
Conclude this practice by noticing any lingering thoughts or questions that you have about this moment today. What are you thinking about?

10-second pause for reflection
Thank you for practicing the recent memory recall today. Feel free to continue playing back recent memories in your mind if you wish.
Appendix C

Borkovec and Nau (1972) Treatment Credibility Scale

Post-training, first day of daily measures only

Please provide your honest opinion regarding the Toolkit for which you received training. You will only be asked these questions on the first day of your daily surveys.

1) How confident are you that this Toolkit will be successful in helping with your mental wellbeing?

1=Not At All, 2=Barely, 3=Slightly, 4=Moderately, 5=Confident, 6=Very Confident, 7=Extremely Confident

2) How confident would you be in recommending this Toolkit to a friend who was facing mental wellbeing challenges?

1=Not At All, 2=Barely, 3=Slightly, 4=Moderately, 5=Confident, 6=Very Confident, 7=Extremely Confident

3) If you were facing mental wellbeing challenges, how willing would you be to use this Toolkit?

1=Not At All, 2=Barely, 3=Slightly, 4=Moderately, 5=Willing, 6=Very Wiling, 7=Extremely Willing
Appendix D

Frequency of Tool Use

Daily

Please indicate how many times you have practiced the following tools over the past 24 hours. This includes all practice with guided exercises and on your own in your everyday activities.

1) **Mindfulness (Experimental) / Breathing Relaxation (Placebo and Control)**
   
   0, 1, 2, 3, 4, 5, 6+

2) **Positive Reappraisal (Experimental) / Recent Memory Recall (Placebo and Control)**
   
   0, 1, 2, 3, 4, 5, 6+

3) **Savouring (Experimental) / Distant Memory Recall (Placebo and Control)**
   
   0, 1, 2, 3, 4, 5, 6+
Appendix E

Schedule of Positive and Negative Experiences (SPANE; Diener et al., 2010).

Baseline, Post-Intervention, 1-Month Follow-up

Please think about what you have been doing and experiencing during the past two weeks. Then report how much you experienced each of the following feelings, using the scale below. Don’t try to count the number of times you felt a particular way, but rather indicate what seems like a reasonable estimate.

1=Never, 2=Almost Never, 3=Rarely, 4=Sometimes, 5=Often, 6=Very Often, 7=Always

1) Positive
2) Negative
3) Good
4) Bad
5) Pleasant
6) Unpleasant
7) Happy
8) Sad
9) Afraid
10) Joyful
11) Angry
12) Contented

Daily

Please think about what you have been doing and experiencing during the past 24 hours. Then report how much you experienced each of the following feelings, using the scale below. Don’t try to count the number of times you felt a particular way, but rather indicate what seems like a reasonable estimate.

(Full scale used as above)
Appendix F

Emotion Regulation Questionnaire – Reappraisal Subscale (ERQ; Gross & John, 2003).

Baseline, Post- Intervention, 1-Month Follow-up

Please select the response that fits you best using the following scale.

1=Strongly Disagree, 2=Disagree, 3=Slightly Disagree, 4=Neutral, 5=Slightly Agree, 6=Agree, 7=Strongly disagree

1) When I want to feel more positive emotion (such as joy or amusement), I change what I am thinking about.
2) When I want to feel less negative emotion (such as sadness or anger), I change what I am thinking about.
3) When I am faced with a stressful situation, I make myself think about it in a way that helps me stay calm.
4) When I want to feel more positive emotion, I change the way I am thinking about the situation.
5) I control my emotions by changing the way I think about the situation I am in.
6) When I want to feel less negative emotion, I change the way I am thinking about the situation.

Daily

Please think about how you felt over the last 24 hours. Then report how much you experienced the following, using the scale below. Don’t try to count the number of times you felt a particular way, but rather indicate what seems like a reasonable estimate. Over the last 24 hours…

1=Never, 2=Almost Never, 3=Rarely, 4=Sometimes, 5=Often, 6=Very Often, 7=Always

1) When I wanted to feel more positive emotion (such as joy or amusement), I changed what I was thinking about.
2) When I wanted to feel less negative emotion (such as sadness or anger), I changed what I was thinking about.
3) When I wanted to feel more positive emotion, I changed the way I was thinking about the situation.
4) When I wanted to feel less negative emotion, I changed the way I was thinking about the situation.
Appendix G

Stress in General Scale – Threat Subscale (SIG; Stanton et al., 2001)

Baseline, Post-Intervention, 1-Month Follow-up

How would describe the last two weeks for you?

1=Not at All 2=Barely, 3=Not Much, 4=Moderately, 5=Slightly, 6=Very Much, 7=Extremely

1) Irritating
2) Under control (R)
3) Nerve-wracking
4) Hassled
5) Comfortable (R)
6) More stressful than I’d like
7) Smooth running (R)
8) Overwhelming

Daily

How would you describe the last 24 hours for you?

1=Not at All, 2=Barely, 3=Not Much, 4=Moderately, 5=Slightly, 6=Very Much, 7=Extremely

1) Irritating
2) Nerve-wracking
3) Hassled
4) More stressful than I’d like
Appendix H

The Warwick-Edinburgh Mental Wellbeing Scale (WEMWBS; Tennant et al., 2007)

**Baseline, Post-Intervention, 1-Month Follow-up**

Below are some statements about feelings and thoughts. Please select the response that best describes your experience of each over the last two weeks…

1= None of the Time, 2=Very Rarely, 3=Rarely, 4=Some of the Time, 5=Often, 6=Very Often, 7=Always

1) I’ve been feeling optimistic about the future
2) I’ve been feeling useful
3) I’ve been feeling relaxed
4) I’ve been feeling interested in other people
5) I’ve had energy to spare
6) I’ve been dealing with problems well
7) I’ve been thinking clearly
8) I’ve been feeling good about myself
9) I’ve been feeling close to other people
10) I’ve been feeling confident
11) I’ve been feeling loved
12) I’ve been able to make up my own mind about things
13) I’ve been interested in new things
14) I’ve been feeling cheerful
Appendix I

Perceived benefits and intervention experience items created for this study

*Post-Intervention*

The following questions refer to your experience with the Toolkit over the past two weeks. Please answer openly and honestly to help us improve this study. Remember, there are no right or wrong answers.

1) How useful did you find the Toolkit overall?
   1=Not at All 2=Barely, 3=Not Much, 4=Moderately, 5=Useful, 6=Very Useful, 7=Extremely Useful

2) How easy was it for you to use the Toolkit exercises overall?
   1=Not at All 2=Barely, 3=Not Much, 4=Moderately, 5=Easy, 6=Very Easy, 7=Extremely Easy

3) How enjoyable was it for you to use the Toolkit exercises overall?
   1=Not at All 2=Barely, 3=Not Much, 4=Moderately, 5=Enjoyable, 6=Very Enjoyable, 7=Extremely Enjoyable

4) How much effort did you put into using the Toolkit exercises overall?
   1=None, 2=Barely, 3=Not a Lot, 4=Somewhat, 5=A Little, 6=Quite a Bit, 7=A Lot

5) How often will you continue to use the techniques you learned in this training?
   1=Never, 2=A Few Times a Year or Less, 3=Once a Month or Less, 4=Once a Week, 5=A Few Times a Week, 6=Everyday, 7=Multiple Times a Day

6) What did you like or think was effective or helpful about the Toolkit? 

7) What did you dislike or think was ineffective or unhelpful about the Toolkit?

*Daily*

1) Please write a keyword or phrase to describe your exercise practice today. This can be anything that is meaningful to you. 

2) How pleasant did you find this exercise?
   1=Not At All Pleasant, 2=Barely Pleasant, 3=Slightly Pleasant, 4=Moderately Pleasant, 5=Pleasant, 6=Very Pleasant, 7=Extremely Pleasant
3) **How do you feel after completing this exercise?**

1=A Lot Worse, 2=Worse, 3=Slightly Worse, 4=No Different, 5=Slightly Better, 6=Better, 7=A Lot Better

4) **In which ways do you feel that you have benefited from this exercise?** Please select as many that apply, if any.

- Reduced frustration and/or stress ☐
- Increased clarity of the situation ☐
- Improved mood ☐
- Better prepared to encounter a similar situation in the future ☐
- Improved listening skills ☐
- Learned to avoid a similar situation in the future ☐
- Increased confidence to revisit and deal with difficulties in my life ☐
- Better understanding of self (e.g., knowing how you react to certain problems) ☐
- Better understanding of others ☐
- Improved ability to look at situations objectively ☐
- Increased gratitude ☐
- Increased sense of acceptance ☐
- Increased ability to remember details ☐
- Increased speed of recalling memories ☐
- Increased ability to visualize events in my mind ☐
- Other (please explain): ___________ ☐

5) **Please share any other comments on this exercise experience that you may have:**

__________
Appendix J

The Treatment Self-Regulation Questionnaire (TSRQ; Ryan & Connell, 1989; Levesque et al., 2007)

Post-Intervention

The following questions relate to your reasons for continuing to participate in the program. Different people have different reasons for continuing in such a program, and we want to know how true each of these reasons is for you. There are two groups of questions. The questions in each group pertain to the sentence that begins that group.

Please indicate how true each reason is for you, using the following scale:

1=Not True At All 2=Barely, 3=Not Much, 4=Moderately, 5=Slightly, 6=Very Much, 7=Extremely True

I have continued to use the Mental Well-being Toolkit/Cognitive Activities Toolkit because:

1) I would have felt bad about myself if I didn’t (Extrinsic/Controlled)
2) I would have felt like a failure if I didn’t (Extrinsic/Controlled)
3) I feel like it’s a good way to help myself (Intrinsic/Autonomous)
4) People would think negatively of me if I didn’t (Extrinsic/Controlled)
5) Because I have chosen not to quit (Intrinsic/Autonomous)
6) Because it helps me accomplish my goals (Intrinsic/Autonomous)
7) Because I have invested so much time into this Toolkit (Extrinsic/Controlled)

I have been following the procedures of this Toolkit because:

1) I believe they help me solve my problems (Intrinsic/Autonomous)
2) I have been worried that I would face consequences if I didn’t follow all of the guidelines (Extrinsic/Controlled)
3) It is important to me that my efforts succeed (Intrinsic/Autonomous)
4) I feel guilty if I don’t comply with all the procedures (Extrinsic/Controlled)