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Cultivating Happiness:

Effects, Underlying Mechanisms, and Moderators of Positive-Psychological Interventions

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List of Included Papers

Heekerens, J. B., Eid, M., & Heinitz, K. (2019). Dealing with conflict: Reducing goal ambivalence using the best-possible-self intervention. *Journal of Positive Psychology*. Advance online publication. doi: 10.1080/17439760.2019.1610479

Heekerens, J. B., Eid, M., Heinitz, K., & Merkle, B. (submitted). Deconstructing positive-psychological interventions: Differential effects of optimistic, grateful, and self-compassionate writing on well-being. *Journal of Positive Psychology*.

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Chapter 1

Introduction

Since its early days, psychology has put tremendous effort into alleviating human suffering, which resulted in a range of powerful and widely used interventions (e.g., cognitive behavioral therapy; see Craske, 2017, for an introduction). The consequent improvement in mental health services is clearly remarkable. However, this achievement in part came at the cost of a narrow research focus that paid little attention to explicitly cultivating happiness beyond the absence of deficits (Diener, 1984). This shortcoming has only recently attracted serious research attention, specifically within the field of positive psychology (Seligman, 2019; Seligman & Csikszentmihalyi, 2000). Despite these efforts, current programs that aim to increase well-being among mentally healthy adults are still in their infancy and frequently effects fall short of expectations (e.g., Mongrain & Anselmo-Matthews, 2012; Tempel, 2016; Uliaszek, Rashid, Williams, & Gulamani, 2016). Oftentimes effects are small and do not last, which is particularly evident for positive-psychological interventions—relatively simple intentional activities that aim to lastingly increase well-being through cultivating positive thoughts, emotions, and behaviors (see Hone, Jarden, & Schofield, 2014; Parks & Biswas-Diener, 2013; Schueller, Kashdan, & Parks, 2014; Sin & Lyubomirsky, 2009, for a deeper discussion and formal definition). A current reanalysis of two independent meta-analyses on the well-being related effects of positive-psychological interventions highlights this limitation (White, Uttl, & Holder, 2019; see Bolier et al., 2013; Sin & Lyubomirsky, 2009, for the original meta-analyses). Considering the widespread application of positive-psychological interventions in schools, companies, digital formats, and increasingly clinics, the issue of small and unstable effects should be taken seriously (see Gilbert, Foulk, & Bono, 2018; Hone et al., 2014; Parks & Titova, 2014, for reviews on practical applications and dissemination). One promising way to increase and stabilize effects is to further develop existing positive-psychological interventions. Doing so in a structured manner requires comprehensive knowledge of the effects, mediators, and moderators of positive-psychological interventions (see Lyubomirsky & Layous, 2013, for a conceptual framework). In this context, three important questions currently remain wholly or partially unanswered: First, what are the effects of positive-psychological interventions (e.g., practical significance, additional effects, etc.)? Second, how do positive-psychological interventions work (i.e., what are relevant mediators)? Third, for whom and under which conditions do positive-psychological interventions work (i.e., what are relevant moderators)? Providing answers to these questions in order to inform the further development of positive-psychological interventions constitutes the overall aim of this thesis.

The following paragraphs discuss what happiness is, why increasing happiness is important in the first place, and why lastingly increasing happiness should be possible. The reason for this is to introduce the topic of this thesis and to provide a rationale for the undertaking of this thesis. If, for example, increasing happiness was undesirable, or if lasting changes in happiness were unlikely, further developing interventions that aim to increase well-being would be obsolete. Afterwards, it is explained how knowledge of mediators and moderators helps to further develop positive-psychological interventions. Finally, the overall aim of this thesis is stated.

1.1 What Happiness is

Happiness is a multifaceted and omnipresent term in our everyday vocabulary that usually refers to a certain state of mind or psychological condition (Haybron, 2013). Researchers mostly use the term "well-being" instead of "happiness". The terms are used interchangeably in this thesis. There are, however, different theories about what happiness (or well-being) is (see Lambert, Passmore, & Holder, 2015, for a comprehensive review). The three most important one's define happiness as a positive emotional condition (emotional state theory or hedonic approach), as being satisfied with one's life (life satisfaction theory), and for one's condition to be favorable on the whole (eudaimonic approach; Haybron, 2013). Drawing on the first two theories, some researchers combine affect and life satisfaction measures to assess well-being more comprehensively, thereby creating a construct that is referred to as subjective well-being (Diener, 1984). From this perspective, a happy person is one that experiences frequent positive affect, infrequent negative affect, and favorably evaluates his or her life as a whole and in specific areas (Diener, Suh, Lucas, & Smith, 1999). Although assessing happiness in terms of subjective well-being makes sense because emotional well-being ratings and judgements about one's life are typically positively correlated (see Schimmack, 2008, for a discussion), one downside of this approach is that two conceptually very different constructs are pooled together. For example, life satisfaction ratings require comparisons with own standards or any other, whereas affect ratings do not. To illustrate this, Haybron (2013) tells the story of Moreese Bickham, an Afro-American who, acting in self-defense, shot two policemen in 1958. He was imprisoned for 37 years before being released following revelations of an unjust conviction. When he walked out of the prison, he was asked how he felt about spending half of his life behind bars. He replied, "I don't have a minute's regret. It was a glorious experience." Although we do not know how Moreese Bickham felt during his years in prison, it was probably not a time in his life that was brimming with positive emotions. What he likely meant was

that he was glad to be alive and satisfied because this was more than he expected. By this standard he was happy. Thus, to allow for a more nuanced discussion and account for the fact that individuals can score high on one definition of happiness (e.g., life satisfaction) but low on another (e.g., affect), this thesis differentiates between cognitive and emotional definitions of well-being wherever applicable. Another distinction relates to the conceptualization of happiness as eudaimonic well-being. The eudaimonic tradition states that happiness constitutes a way of life, which is inherently virtuous and meaningful and wherein an individual realizes his or her potential, rather than a certain state of mind or psychological condition (Lambert et al., 2015). One prominent representative of this tradition is Ryff's (1989) model of psychological well-being, consisting of six dimensions that define happiness: (a) self-acceptance (i.e., holding positive attitudes towards oneself); (b) positive relations with others (i.e., achieving intimacy and generativity); (c) autonomy (i.e., evaluating oneself by own standards); (d) environmental mastery (i.e., choosing or creating environments suitable to one's needs); (e) purpose in life (i.e., feeling there is meaning to life); and (f) personal growth (i.e., developing one's potential; see Ryff, 2014, for a review of supporting evidence). Today, hedonic definitions of happiness are more common, although eudaimonic definitions, and particularly the psychological well-being construct, are also used (e.g., Bolier et al., 2013). Some researchers expressed concerns whether differentiating between hedonic and eudaimonic definitions of happiness is helpful (Kashdan, Biswas-Diener, & King, 2008, 2009). Specifically, subjective and psychological well-being have been proposed to form a more holistic well-being construct (thriving; Su, Tay, & Diener, 2014). Again, the argument for combining assessments is that indicators of different well-being constructs (affect, life satisfaction, self-acceptance, autonomy, etc.) are highly correlated (typically $r > .50$; Su et al., 2014). The correlations, however, also show that there are substantial differences between the variables. Distinguishing between subjective and psychological well-being brings the advantage of more nuanced investigations of the relationship between different definitions of happiness and the processes underlying happiness. For example, some theories state that aspects of psychological well-being constitute ways of achieving higher subjective well-being. Specifically, self-determination theory (Ryan & Deci, 2000) explains that having choice and control over one's life (i.e., autonomy) does not *define* happiness but *fosters* it. Because this thesis is concerned with the processes underlying happiness, a distinction is made between subjective and psychological well-being wherever appropriate. To sum up, happiness (or well-being) can be defined as feeling well (hedonic approach), evaluating one's life favorably (life satisfaction theory), and living virtuously (eudaimonic

approach). After having defined what happiness is, subsequent questions in the context of happiness research are: Which factors determine happiness? And how can happiness be increased? Before discussing these questions, the following paragraph explains why increasing happiness is desirable in the first place.

1.2 Why Increasing Happiness is Important

There are three reasons for why increasing happiness is important. First, many people around the world wish to live happy and fulfilled lives. For example, one study found that German college students reported that life satisfaction was "extraordinarily important and valuable" and happiness was "very important and valuable" in their lives, whereas money was only "somewhat important and valuable" (6.62 and 5.95 vs. 4.11 on a 7 points scale; Diener, 2000; also see King & Broyles, 1997). Similar findings emerged in other countries, including those with more collectivistic cultures such as Japan and India. Although not all individuals wish to become happier than they already are (Diener, 2008), researchers generally agree that the good life comprises more than the mere absence of misery (see Haybron, 2013, for a deeper discussion). Second, individuals themselves benefit from having higher levels of subjective well-being (see Diener, Oishi, & Lucas, 2009; Layous, Chancellor, & Lyubomirsky, 2014; Lyubomirsky, King, & Diener, 2005, for reviews). Specifically, a large body of longitudinal evidence shows that well-being predicts self-reported and actual physical health as well as longevity (e.g., the combined hazard ratio for the effect of positive affect and psychological well-being on mortality among healthy individuals several years later was 0.82, 95% CI [0.76, 0.89], 21 comparisons; Chida & Steptoe, 2008; see Pressman, Jenkins, & Moskowitz, 2019; Veenhoven, 2008, for additional reviews). This association is robust even after controlling for baseline health, health-related behaviors, and negative affect. One explanation for the possible causal mechanism underlying the well-being and health relationship is that experiencing positive emotions prompts individuals to enter and maintain adaptive physiological states that in turn benefit the immune and cardiovascular systems (e.g., through appropriate stress responses; see Pressman et al., 2019, for a theoretical framework). Thus, well-being might prove a vital component of a healthy life. Other evidence indicates that higher levels of positive affect and life satisfaction correlate with and predict indicators of career success (e.g., higher income; see Walsh, Boehm, & Lyubomirsky, 2018, for a recent review). Possible mechanisms explaining the longitudinal link between happiness and higher income include happier people getting more favorable evaluations from their supervisors, engaging less in withdrawal behaviors (e.g., absenteeism), and receiving more social support from coworkers. Third,

societies benefit from having individuals with higher levels of subjective well-being beyond cumulative

individual effects on health and career success (see Layard, 2011, for an introduction). For example,

recent evidence from the Gallup World Poll shows that happier individuals are more likely to vote in

elections, less likely to vote for populist candidates, and more likely to engage in politics (Ward, 2019).

Importantly, these associations were not explained by other variables included in the survey that relate to

both political engagement and well-being (e.g., inter-personal trust). In addition, evidence suggests that

happier individuals are more likely to show prosocial behaviors such as donating time and money,

although it remains largely unclear whether higher levels of well-being cause prosocial behaviors or the

other way round (see Aknin, Whillans, Norton, & Dunn, 2019, for a review). To sum up, increasing

happiness is important because many people desire to be happier and because high levels of well-being

have been shown to provide benefits both for individuals and for societies. This leaves us with the

questions which factors determine happiness and how happiness can be increased.

1.3 Why Lastingly Increasing Happiness Should be Possible

For many years, researchers have been skeptical about the possibility of lasting changes in well-

being. Classic theories explain that although changes in well-being do occur, these are mere fluctuations

that tend to occur around stable individual set-points, which are unlikely to change in the long run (e.g.,

hedonic adaptation model; Brickman & Campell, 1971; also see Frederick & Loewenstein, 2000; Headey

& Wearing, 1989; Lykken & Tellegen, 1996). Initial evidence in support of these theories comes from

cross-sectional studies that compared the happiness levels of individuals who experienced very positive

and very negative life events (see Sheldon & Lucas, 2016, for a comprehensive review and discussion).

For example, one popular interview-based study showed that recent lottery winners and accident victims

expected comparable levels of happiness in the future (4.20 vs. 4.32 on a 6 points scale), which led the

authors to conclude that these events have a minor impact on well-being (Brickman, Coates, & Janoff-

Bulman, 1978, study 1). Results, however, also show that present levels of happiness differed (4.00

among lottery winners vs. 2.96 among accident victims). One problem with this classic study is that self-

reports of anticipated well-being are inaccurate indicators of actual future well-being levels (see Wilson

& Gilbert, 2003, for a discussion). Additional evidence comes from cross-sectional studies showing that a

substantial share of interindividual differences in subjective well-being can be accounted for by genes and

personality (e.g., Steel, Schmidt, Schultz, 2008). The problem is that genes and personality are rather

stable. Thus, the argument goes that individual levels of well-being are unlikely to lastingly change (e.g.,

Brickman & Campell, 1971). In contrast, other researchers explained that differences in well-being are also determined by different life circumstances (e.g., income or education; Diener, Ng, Harter, & Arora, 2010; see Lyubomirsky, Sheldon, & Schkade, 2005; Sheldon & Lyubomirsky, 2019, for reviews). There is growing evidence in support of this notion. For example, cross-sectional evidence shows that objective indicators of life circumstances (e.g., gross domestic product) partly explain well-being differences across nations (see Diener, Lucas, & Scollon, 2006, for a review). In addition, longitudinal studies suggest that certain negative life events (e.g., death of a loved one) can undermine happiness over the course of several years (Lucas, Clark, Georgellis, & Diener, 2003). In addition, there is some evidence that positive events proceed sustained increases in well-being, although effects are typically smaller than for negative life events (see Luhmann, Hofmann, Eid, & Lucas, 2012; Mangelsdorf, Eid, & Luhmann, 2019, for reviews). For example, researchers who used data from the German socio-economic panel showed that individuals tend to experience increases in subjective well-being several years before they marry (Lucas, 2007; also see Lucas et al., 2003). The increases are initially sustained. Approximately 5 years later, however, individuals return to levels of subjective well-being that are comparable to 5 years before they married. Thus, according to these data, the well-being benefits of marriage are eventually lost, which is in line with predictions of set-point theories of well-being. One shortcoming of longitudinal studies on the effects of life events on well-being is that they do not compare changes in well-being against appropriate controls (e.g., individuals who did not experience a certain life event). One recent longitudinal study using data from the British household panel survey found that although individuals who married eventually returned to pre-marriage levels of life satisfaction years later, life satisfaction remained higher up to ten years later compared with individuals who never married (Yap, Anusic, Lucas, 2012). Thus, some of the happiness gains seem to have prevailed over the course of years. One explanation for enduring increases in well-being following certain life events is that these events encourage positive changes that result in recurring positive experiences (hedonic adaptation prevention model; Sheldon & Lyubomirsky, 2012). For example, moving together with one's spouse might continue to boost levels of well-being through having somebody to talk to over coffee most mornings, which ideally results in a continuous flow of positive emotions that in turn facilitates long-term well-being. If the positive experiences that are introduced by a positive life event are divers enough and if individuals do not heighten their expectations in response to their changed life circumstances, increases in well-being are proposed to prevail (see Lyubomirsky & Layous, 2019, for a recent discussion). Although most

individuals in Western societies can readily decide that they want to marry, it generally requires a great deal of targeted action to change other life circumstances (e.g., income or education) in order to influence well-being. Genes, personality, and life circumstances may, however, not be the sole determinants of interindividual differences in well-being. Specifically, researchers proposed that interindividual differences in individual patterns of thinking, behaving, and relating also explain differences in well-being (see Lyubomirsky, Sheldon, & Schkade, 2005; Sheldon & Lyubomirsky, 2019, for reviews). Although the amount of variance that can be explained by such variable factors is likely much smaller than the variance explained by more stable factors (see Brown & Rohner, 2019, for a discussion), cognitions and behaviors may nonetheless substantially influence individual levels of well-being (also see Lyubomirsky, 2007; Lyubomirsky & Layous, 2014; Sheldon & Lyubomirsky, 2019). Evidence in support of this notion primarily comes from randomized controlled intervention studies showing that training certain ways of thinking, behaving, and relating results in sustained increases in well-being. For example, one meta-analysis of positive-psychological interventions shows that engaging in activities that are designed to foster positive cognitions and behaviors can increase subjective well-being, Cohen's $d = 0.22$, 95% CI [0.05, 0.38], and psychological well-being, $d = 0.16$, 95% CI [0.02, 0.30], three to six months after the intervention (each effect size based on six studies; Bolier et al., 2013). Although effects are small, they may matter from a practical perspective because other than genes and life circumstances, individual patterns of thoughts, actions, and social interactions are relatively easy to change (see Schueller & Parks, 2012, for a deeper discussion). Another line of intervention research comes from the clinical psychology literature. Specifically, different forms of cognitive-behavioral therapy have been convincingly shown to alter cognitions and behaviors in order to regain and maintain mental health over the course of months and probably years (see Craske, 2017; Llewelyn & van Doorn, 2017; Wampold, 2010, for introductions). For example, one traditional meta-analysis of the effects of cognitive-behavioral therapy on mild to moderate depression in adults shows large decreases in depressive symptoms at the end of therapy compared with waiting list or placebo controls, Cohen's $d = 0.82$, 95% CI [0.81, 0.83] (based on 20 comparisons; Gloaguen, Cottraux, Cucherat, & Blackburn, 1998). In addition, cognitive-behavioral therapy was found to prevent individuals from returning to clinically significant levels of depressive symptoms up to 24 months later, indicating that some of the benefits of the treatment remained (Gloaguen et al., 1998). These findings are largely in line with more recent studies (see Butler, Chapman, Forman, & Beck, 2006; Hofmann, Asnaani, Vonk, Sawyer, & Fang, 2012, for reviews). Although the

skills required for achieving lasting happiness likely differ from those needed to stay mentally healthy, it should be principally possible to achieve happiness, given it is possible to effectively deal with distress (Seligman, 2019).

To sum up, researchers have traditionally been skeptical about the possibility of lasting changes in well-being. Recent evidence, however, shows that there are circumstances (e.g., marriage or cognitive-behavioral therapy) under which well-being increases over the course of months and possibly years. Thus, if researchers identify skills that are relevant to well-being and develop effective programs to teach them, intentionally increasing long-term happiness may be possible. In fact, this is what many researchers are currently working on (e.g., Seligman, 2019; Craske et al., 2019).

1.4 The Emergence of Positive-Psychological Interventions to Increase Happiness

Positive psychology is the scientific discipline that studies positive subjective experience, positive individual traits, and positive institutions (Seligman & Csikszentmihalyi, 2000). This thesis focuses on positive subjective experiences and is particularly concerned with the question how individual well-being can be raised. This question is not new. One frequently cited pioneer of happiness interventions is the American psychologist Michael Fordyce (1977), who developed and systematically evaluated a six weeks long self-study program to increase happiness among college students. The basic idea of the program was to encourage participants to adopt the behaviors and attitudes of very happy people. For example, participants were instructed to strengthen close relationships, develop optimistic thinking, and become involved with meaningful work. Each recommendation was accompanied by specific instructions on how to employ them. Results from three randomized controlled trials indicate that the program effectively increased self-reported happiness at the end of the intervention period relative to an active control condition (Fordyce, 1977). Fordyce's work was among the first to show that self-help interventions can be effective in increasing well-being. Modern happiness interventions, which predominantly come from the field of positive psychology (Spence & Green, 2013), have adopted the logic of Fordyc's early study in so far as they teach individuals skills of thinking, behaving, and relating that are frequently observed in very happy people. For example, evidence suggests that nearly all the happiest American undergraduates are highly social and have strong romantic and other close relationships (Diener & Seligman, 2002; also see Kaliterna-Lipovčan & Prizmić-Larsen, 2016; Oishi, Diener, & Lucas, 2007). Congruently, many current happiness interventions focus on improving social interactions (e.g., practicing kindness; Parks & Titova, 2014). Thus, one characteristic of current happiness interventions is that their development rests

on basic scientific findings (Parks & Biswas-Diener, 2013). Although Fordyce (1983) successfully replicated the promising results of his original paper, it took until after Martin Seligman's historical inaugural speech as president of the American Psychological Association in 1998, the "hour of birth" of positive psychology, that the empirical investigation of happiness interventions gained significant popularity (see Diener & Seligman, 2004; Linley, Joseph, Harrington, & Wood, 2006; Seligman, 2019; Seligman & Csikszentmihalyi, 2000, for an introduction to the history of positive psychology). Specifically, research into happiness interventions received fresh impetus after the publication of an influential paper by Seligman and colleagues (2005), in which the authors set out an agenda to further develop happiness interventions by rigorously testing stand-alone self-help exercises (e.g., writing a gratitude letter), rather than extensive programs (e.g., Fordyce's approach). The idea to investigate individual well-being strategies was eagerly absorbed by the developing field of positive psychology, new studies emerged, and a first meta-analysis was published that coined the term positive-psychological interventions (Sin & Lyubomirsky, 2009). The authors defined positive-psychological interventions as activities that "aim to cultivate positive feelings, behaviors, or cognitions" (p. 468). This definition was maintained in subsequent conceptual models (Lyubomirsky & Layous, 2013). There has, however, also been some controversy as to what constitutes as a positive-psychological intervention. Specifically, one subsequent meta-analysis required that positive-psychological interventions were explicitly developed within the theoretical tradition of positive psychology (Bolier et al., 2013), which was criticized as being an arbitrary boundary that unnecessary limits the scope of research into happiness interventions (Schueller, Kashdan, & Parks, 2014). Today, researchers generally agree that positive-psychological interventions focus on positive topics, operate by a positive mechanism or target a positive outcome, and are designed to promote well-being (see Parks & Biswas-Diener, 2013, for a discussion). One central criterion is that sustained positive results are achieved in the populations in which positive-psychological interventions are administered (Parks & Biswas-Diener, 2013). Ways to achieve this aim vary considerably. For example, positive psychologists also investigate interventions designed to deal with negative emotions (see Ivtzan, Lomas, Hefferon, & Worth, 2016, for an introduction). That being said, most positive-psychological interventions focus on encouraging positive experiences such as developing character strengths or talents (e.g., identifying, using, and developing one's strengths), cultivating gratitude (e.g., writing weekly gratitude journals) or optimism (e.g., visualizing one's best possible future), promoting forgiveness (e.g., writing about personal benefits that resulted from experienced

misconduct), and strengthening social connections (e.g., doing kind acts to others; see Parks & Biswas-Diener, 2013; Boehm, Ruberton, & Lyubomirsky, 2017, for reviews). After the first meta-analysis on the effects of positive-psychological interventions (Sin & Lyubomirsky, 2009), important conceptual frameworks were published that explain the boundary conditions for successfully implementing positive-psychological interventions and the mechanisms underlying their effectiveness. For example, the positive activity model (Lyubomirsky & Layous, 2013) makes predictions about the conditions under which various happiness activities are more (or less) effective. In addition, the process model of emotion regulation has been used to describe how positive-psychological interventions impact short- and long-term positive affect (Quoidbach, Mikolajczak, & Gross, 2015). Both models are discussed in more detail in Chapter 2. Importantly, the published meta-analyses (Sin & Lyubomirsky, 2009; Bolier et al., 2013) and the conceptual frameworks (Lyubomirsky & Layous, 2013; Quoidbach et al., 2015) further stipulated studies on the effects of positive-psychological interventions, which are as popular as ever (see Hendriks et al., 2019, for a recent review). Many fundamental questions regarding positive-psychological interventions, however, remain wholly or partially unanswered. For example, controversy exists as to how long effects of various positive-psychological interventions last (e.g., Bolier et al., 2013; White et al., 2019), which psychological mechanisms drive intervention effects (e.g., Heekerens & Heinitz, 2019; Mongrain & Anselmo-Matthew, 2012), and which groups of participants benefit most from positive-psychological interventions (e.g., Lyubomirsky, 2007; Wellenzohn, Proyer, & Ruch, 2018). A deeper discussion of knowledge gaps and controversies in the current literature is provided in Chapter 2. To sum up, positive-psychological intervention research originated from the pioneering work of Michael Fordyce and gained significance with the emergence and progress of the scientific discipline of positive psychology.

1.5 How Knowledge of Mediators and Moderators Helps to Further Develop Interventions

One ever-present question in intervention research is how the effects of existing interventions can be improved. Accordingly, one important aim of positive psychology is to further develop positive-psychological interventions (Seligman, 2019; Seligman & Csikszentmihalyi, 2000). Current theoretical models suggest three ways how positive-psychological interventions can be further developed in order to achieve greater effects on well-being (Lyubomirsky & Layous, 2013). First, researchers can identify active ingredients of positive-psychological interventions. Such knowledge is important because if we know what drives the success of a specific intervention, we can add more of what works in order to

increase the intervention's effectiveness. For example, modifying trauma related cognitions has been theoretically proposed and empirically confirmed to substantially mediate the effects of cognitive-behavioral therapy for post-traumatic stress disorder, which is why treatments continue to incorporate this technique in order to achieve best results (see Kazantzis et al., 2018, for a review). Second, researchers can obtain knowledge that helps to deliver interventions in an optimal manner (e.g., Lyubomirsky, 2019). For example, it is useful to know how long and how often an intervention needs to be applied in order to be effective. Is writing and delivering a letter of gratitude enough to lastingly increase happiness? Or are more intense interventions such as individual counseling needed? In addition, are online administrations as effective as in-person interventions? There is currently little research on the contextual moderators of the effects of positive-psychological interventions (see Fritz & Lyubomirsky, 2018; Lyubomirsky & Layous, 2014, for reviews) and consequently answers to these questions remain largely speculative. Third, researchers can examine who benefits most from any specific positive-psychological intervention and then target interventions to achieve larger effects (e.g., Schueller, 2014). For example, it has been proposed that extroverted individuals benefit more from interventions that require them to initiate social interactions (e.g., doing kind acts to others) because the social behavior activation component of such activities aligns well with the preferences of extroverts (Lyubomirsky, 2007; Schueller, 2011). A competing view is that introverted individuals, who would otherwise avoid approaching strangers, might particularly benefit from experiencing positive social interactions as a result of doing kind acts to others (Lyubomirsky, 2007). More research is needed to solve this and similar issues in order to better tailor positive-psychology interventions to the needs of participants. To sum up, knowledge of mediators and moderators helps to further develop positive-psychological interventions by extending active components, informing optimal delivery, and targeting interventions to those who benefit most.

1.7 Overall Aim and Scope of this Thesis

The overall aim of this thesis is to examine interventions designed to increase happiness. Specifically, the aim is to further develop positive-psychological interventions through investigating their effects, mediators, and moderators. Thus, the scope of this thesis is first limited to positive-psychological interventions. There are, however, dozens of positive-psychological interventions, each of them potentially with individual effect patterns, mediators, and moderators (see Parks & Titova, 2014, for an overview). It is impossible to investigate all of them in the context of this thesis. Thus, the scope of this

thesis is second limited to three positive-psychological interventions: the best-possible-self intervention (King, 2001), the gratitude letter exercise (Seligman, 2013; Seligman, Steen, Park, & Peterson, 2005), and self-compassionate writing (Shapira & Mongrain, 2010). These three interventions were chosen because they are popular among researchers, relatively easy to administer, and because they have been proposed to work through different psychological mechanisms, which makes it easier to derive related but distinct testable predictions. A special emphasis throughout this thesis is on the best-possible-self intervention because such an additional focus allows for even deeper examinations and more sophisticated considerations. The best-possible-self intervention was one of the first positive-psychological interventions and has been incorporated into numerous more extensive programs (e.g., positive psychotherapy; Rashid & Seligman, 2018). Detailed aims of the empirical studies presented in the main part of this thesis are provided in Chapter 2.

1.8 Chapter Summary and Preview

In this chapter the current thesis was situated in the positive psychology literature. It was stated that the aim of this thesis is to further develop positive-psychological interventions, which are designed to increase well-being. The case was made that increasing happiness is desirable because higher levels of well-being provide benefits for individuals and societies. Afterwards, it was explained that lasting changes in happiness should principally be possible, just as it is possible to effectively deal with distress. Then, it was stated that research on mediators and moderators of the effects of positive-psychological interventions is a promising way to increase and stabilize intervention effects. Specifically, knowledge of mediators helps to select active intervention components and omit components that provide no added value. Knowledge of moderators allows to target interventions to the need of specific groups of individuals. Finally, the scope of this thesis was limited to three popular positive-psychological interventions: the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing.

The following chapter provide an overview of well-being theories that explain the effects of positive-psychological interventions. Empirical findings related to the effects, mediators, and moderators of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing are discussed (Chapter 2). Afterwards, detailed accounts of three empirical studies are provided that aim to fill current knowledge gaps in order to stipulate the further development of positive-psychological

interventions (Chapters 3, 4, and 5). Finally, the theoretical and practical implications of the results are discussed. Limitations of the results and recommendations for future research are presented (Chapter 6).

1.9 References

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Chapter 2

Theory and Current State of the Evidence

The aim of this thesis is to further develop positive-psychological interventions. Research on the effects, mediators, and moderators of positive-psychological interventions is one promising way towards this aim. Such research requires guidance from relevant theories. The following chapter discusses prominent well-being theories that explain the mechanisms of positive-psychological interventions. Afterwards, current evidence regarding the effects, mediators, and moderators of the best-possible-self intervention (King, 2001), the gratitude letter exercise (Seligman, 2002; Seligman, Steen, Park, & Peterson, 2005), and self-compassionate writing (Shapira & Mongrain, 2010) is reviewed. Theoretical considerations that are specific to these interventions are discussed. As mentioned before, one reason for focusing on the three mentioned positive-psychological interventions is their current popularity (Parks & Biswas-Diener, 2013). The rationale of this chapter is to establish a context for the three empirical studies that are presented in Chapters 3, 4, and 5.

2.1 Well-Being Theories Explaining the Mechanisms of Positive-Psychological Interventions

As Table 2.1 shows, there are two main positive psychological frameworks that explain how positive-psychological interventions increase well-being: cognitive theories and evolutionary theories (Lambert, Passmore, & Holder, 2015).

2.1.1 Cognitive theories

First, cognitive theories propose that cognitive processes (e.g., attention or self-regulation) determine individual well-being. In doing so, they view well-being as resulting from individual patterns of thinking that shape experiences of the world in a specific manner. Just as cognitive theories of psychopathology assume that dysfunctional thinking undermines happiness (e.g., Ellis, 1962; Beck, 1976), cognitive theories of well-being state that functional thinking promotes happiness (e.g., Lyubomirsky & Dickerhoof, 2010). The difference is that the latter theories emphasize building adaptive cognitions whereas the former theories focus on changing maladaptive beliefs and appraisals.

Process model of emotion regulation. The process model of emotion regulation is one recent conceptual framework that can be categorized as a cognitive theory (Gross, 1998; Quoidbach, Mikolajczak, & Gross, 2015). The model states that short- and long-term increases in positive affect after positive-psychological interventions can be explained using five families of emotion regulation strategies

Table 2.1 Cognitive and Evolutionary Well-Being Theories Explaining the Effects of Positive-Psychological Interventions

Well-being theory	Classification	Best-possible-self intervention	Gratitude letter exercise	Self-compassionate writing
Process model of emotion regulation (Gross, 1989; Quoidbach et al., 2015)	Cognitive theory that focuses on specific factors	Writing about one's best possible future is proposed to increase short-term positive affect through encouraging favorable future expectations (i.e., changing cognitions for upcoming positive events).	Writing a letter of gratitude is proposed to increase short-term positive affect through encouraging participants to reappraise past experiences in a grateful manner (i.e., changing cognitions related to past positive events).	Engaging in self-compassionate writing is proposed to increase short-term positive affect through encouraging compassionate responses to own failures (i.e., modifying cognitive evaluations of unpleasant events).
Self-regulation theory (King, 2001)	Cognitive theory that focuses on specific factors	Writing about one's best possible future is proposed to promote short- and long-term well-being through promoting reflective processes about personal values and choice of life goals.	-	-
Positive self-representations hypothesis (Mongrain & Anselmo-Matthews, 2012)	Cognitive theory that focuses on one common factor	Positive-psychological interventions are collectively proposed to increase short- and long-term well-being through encouraging positive self-relevant thinking. Self-relevant thinking includes focusing on positive aspects of one's self and life as well as generally believing in positive change.		
Broaden-and-build theory (Fredrickson, 1998, 2001)	Evolutionary theory that focuses on one common factor	Optimal functioning is viewed as arising from cumulative experiences of positive emotions that help to build personal resources, which, in turn, increase long-term well-being. Thus, positive-psychological interventions are collectively proposed to operate through encouraging positive emotions. Specifically, the positive emotions involved in writing about one's best possible future are proposed to build optimism, the positive emotions involved in writing a gratitude letter help to cultivate a grateful disposition and develop strong social bonds, and the positive emotions involved in self-compassionate writing should drive the development of a self-compassionate attitude.		

Note. Cognitive theories focus on cognitive processes in explaining individual well-being, whereas evolutionary theories emphasize the evolutionary value of positive emotions. Specific factor approaches propose unique mechanisms for different positive-psychological interventions, whereas common factor approaches focus on one or few mechanisms that are proposed to explain the effects of a whole range of interventions.

(see Quoidbach et al., 2015, for a comprehensive review). Specifically, Quoidbach and colleagues explain that positive-psychological interventions may influence positive affect through (a) guiding the selection of situations in everyday life; (b) prompting individuals to actively modify situations; (c) helping individuals to deploy attention to positive aspects of a given situation; (d) supporting individuals in adaptively changing expectations, appraisals, and evaluations; and (e) influencing how individuals respond to situations. Importantly, each positive-psychological intervention is proposed to operate through one or a few specific mechanisms (Quoidbach et al., 2015). For example, doing acts of kindness is suggested to assist the selection of socially rewarding situations, which is why this specific intervention increases positive affect. On the other hand, journaling about things that one is grateful for in one's life operates on positive affect through encouraging favorable evaluations of past events and relationships. By strengthening emotion regulation, positive-psychological interventions may also support individuals in entering and maintaining emotional states that are instrumental to their long-term well-being (see Tamir & Gross, 2011, for a deeper discussion). In their review, Quoidbach and colleagues (2015) conclude that current evidence strongly suggests that some strategies (e.g., attentional deployment or changed cognitions) explain short- and long-term increases in positive affect following the administration of positive-psychological interventions. To support their claims, Quoidbach and colleagues first categorized positive-psychological interventions into one of the five families of emotion regulation. For example, the best-possible-self intervention was categorized as an emotion regulation strategy that increases positive affect through changing cognitions of upcoming events. Then the authors reviewed evidence from experimental studies showing that assigning participants to perform specific positive-psychological interventions increases positive affect. If there was an effect on positive affect, the related emotion regulation strategy was concluded to be effective (Quoidbach et al., 2015). For example, the review states that there is strong evidence that changing cognitions for upcoming positive events increase short-term positive affect because writing about one's best-possible future has been demonstrated to induce positive affect. This evidence, however, is insufficient to rule out alternative explanations for the effects of positive-psychological interventions (e.g., that the best-possible-self intervention increases positive affect because it encourages positive self-evaluations; Mongrain & Anselmo-Matthew, 2012). In addition, the evidence that Quoidbach and colleagues (2015) discuss largely provides indirect proof for the proposed mechanisms. Specifically, few studies have directly examined the link between altered cognitions following positive-psychological interventions and subsequent increases in positive affect. Because the

process model of emotion regulation postulates distinct mechanisms for different positive-psychological interventions it can be classified as a specific factors approach (Wampold & Imel, 2015; also see Table 2.1).

Self-regulation theory. Other popular cognitive theories emphasize the role of self-awareness and self-regulation. Self-awareness involves "the ability to represent oneself cognitively in abstract and symbolic ways" (Leary & Guadago, 2011, p. 135), which is central to thinking about and planning the future. Self-regulation refers to the conscious process of anticipating the future, setting goals, and mentally practicing future behaviors (Leary & Guadago, 2011). Researchers have suggested that there are certain ways of thinking about oneself and the future that promote well-being, whereas other ways compromise well-being (e.g., self-determination theory stresses the importance of choosing and pursuing intrinsically rewarding goals; Ryan & Deci, 2000). One way that the effects of positive-psychological interventions can be explained then is through encouraging adaptive information-processing (e.g., consciously reflecting on choices and values; self-regulation theory; King, 2001). Because self-regulation theory makes specific predictions regarding the processes of certain positive-psychological interventions it can be categorized as a specific factors approach (Wampold & Imel, 2015; also see Table 2.1).

Positive self-representations hypothesis. While some researchers propose specific self-regulatory mechanisms for certain positive-psychological interventions (e.g., the best-possible-self intervention; King, 2001) other researchers explained that positive-psychological interventions generally operate through encouraging positive self-relevant thinking (Mongrain & Anselmo-Matthews, 2012). Because the latter perspective suggests that there is just one cognitive process underlying all positive-psychological interventions it can be classified as a common factor approach (Wampold & Imel, 2015; also see Table 2.1). The general idea here is that "a common focus on positive aspects of one's self and one's life" (p. 383) and belief in positive change hold responsible for increased well-being after positive-psychological interventions (Mongrain & Anselmo-Matthews, 2012). Having such a focus involves high levels of self-awareness and ego-involvement (i.e., self-relevance and desirability of situations or thoughts induced by positive-psychological interventions). It is, however, controversial as to whether being involved in such egoic states contributes to or undermines well-being (see Leary & Guadago, 2011, for a discussion). In addition, from an empirical perspective, it remains unclear whether positive-psychological interventions elicit positive self-relevant thoughts in the first place. Thus, as discussed in

more detail in subsequent paragraphs, one aim of this thesis is to investigate the effect of different positive psychological interventions on positive self-relevant thinking.

2.1.2 Evolutionary theories

So far, cognitive theories that explain the effects of positive-psychological interventions based on cognitive processes have been discussed. Second, evolutionary theories stress the evolutionary value of positive emotions for optimal human functioning and well-being.

Broaden-and-build theory. One prominent representative of evolutionary theories is Fredrickson's (1998, 2001) broaden-and-build theory of positive emotions, which proposes that positive emotions enable optimal functioning through broadening individuals' momentary thought-action repertoires and building personal resources (see Fredrickson, 2004, 2013, for reviews of related evidence). Other than cognitive theories, which suggest that well-being results from individual patterns of thinking (e.g., adaptive cognitions), the broaden-and-build theory states that well-being arises from accumulating experiences of positive emotions (also see Sheldon & Lyubomirsky, 2011). The broaden-and-build theory can be thought of as a bottom-up approach to well-being because well-being is assumed to be determined by situational contexts and the degree to which these contexts elicit positive emotions. On the other hand, cognitive theories constitute top-down approaches because well-being is viewed as resulting from higher order cognitive processing (also see Lambert et al., 2015). Importantly, in contrast to cognitive theories, the broaden-and-build theory suggests that positive emotions do not result from but *proceed* changed cognitions (e.g., flexible and creative ways of thinking; Fredrickson, 2001). Thus, the positive emotions that are elicited by frequently participating in positive-psychological interventions, such as joy, interest, and content, are responsible for changed patterns of thinking (e.g., self-acceptance or positive future expectations), which, in turn, provide further benefits such as being more satisfied with one's life (see Fredrickson, Cohn, Coffey, Pek, & Finkel, 2008, for a deeper discussion).

To sum up, cognitive theories propose that positive-psychological interventions operate through influencing emotion regulation, encouraging conscious reflective processes, or promoting positive self-relevant thinking, whereas the broaden-and-build theory emphasizes the role of positive emotions in initiating sustained changes in well-being.

2.2 Effects, Mediators, and Moderators of Positive-Psychological Interventions

The previous paragraph reviewed well-being theories that explain possible mechanisms of positive-psychological interventions. Subsequently, these theories are further discussed in the context of research on the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing. Related evidence is summarized and gaps in the literature are identified. Finally, the specific aims of this thesis are stated.

Subsequent paragraphs organize current findings and debates against the background of the positive activity model (Sheldon & Lyubomirsky, 2019; Lyubomirsky & Layous, 2013). The positive activity model provides a conceptual framework for research on variables that affect the success of positive-psychological interventions, including underlying mechanisms. It is therefore well suited to provide an accessible structure for the following paragraphs. Specifically, the model posits that the effects of positive-psychological interventions on well-being are mediated by positive emotions, cognitions, and behaviors (see Lyubomirsky, 2007, for a deeper discussion), which is generally in line with the cognitive and evolutionary theories discussed prior. As mentioned before, an additional distinction can be made between mechanisms that specifically mediate the effects of single positive-psychological interventions (i.e., specific factor paradigm) and mechanisms that mediate the effects of multiple positive-psychological interventions (i.e., common factor paradigm; Wampold & Imel, 2015). Distinguishing between specific and common factor approaches helps to further differentiate between theoretical perspectives (see Table 2.1 for a summary). Finally, the positive activity model proposes that the effects of positive-psychological interventions are moderated by characteristics of the person (e.g., motivation) and features of the activity (e.g., dosage) as well as person-activity fit. Regarding the latter, Lyubomirsky and Layous (2013) explained that certain types of activities are better suited for certain types of people. Specifically, a good person-activity fit can be achieved through remedying sources of unhappiness (e.g., a pessimist may benefit from cultivating optimism), building on strengths and talents (e.g., a creative person may express gratitude through painting), or adapting activities to individual needs and lifestyles (e.g., a busy person may choose exercises that do not take extra time out of her day; Lyubomirsky, 2007). The current state of the evidence regarding the effects, mediators, and moderators of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing is summarized in Table 2.2 and subsequently discussed in more detail.

Table 2.2 State of Evidence Regarding the Best-Possible-Self Intervention, the Gratitude Letter Exercise, and Self-Compassionate Writing

	Outcomes	Mediators	Moderators
Best-possible-self intervention	One meta-analysis reported moderate between group differences on a combined measure of trait and state optimism at posttest (Hedge's $g = 0.64$; Malouff & Schutte, 2016). Another meta-analysis found small between group changes in optimism ($g = 0.33$) and moderate changes in positive affect ($g = 0.51$) but not negative affect at posttest (Carrillo et al., 2019). Results from single studies indicate fewer health center visits in the months after the intervention (King, 2001, no ES reported), increased life satisfaction four weeks later (Boehm et al., 2011, no ES reported), and depressive symptoms up to five weeks later (Cohen's $d = 0.41$; Shapira & Mongrain, 2010; Yogo & Fujihara, 2008, no ES reported).	One longitudinal study does not support increased importance placed on intrinsic relative to extrinsic goal pursuits as a mediator of increased well-being two weeks later (Heekerens & Heinitz, 2019). One longitudinal study using a multicomponent intervention generally supports state optimism as a mediator of decreased depressive symptoms six months later (Schotanus-Dijkstra et al., 2019). One longitudinal study does not support positive affect during the intervention as a mediator of improved self-care among diabetes patients one month later (Gibson et al., 2018). Another mediation study found that positive emotions mediate increased psychological well-being (Auyeung & Mo, 2018).	Several moderation studies support initial motivation to perform the exercise as a prerequisite for sustained effects (Lyubomirksy et al., 2005; Lyubomirksy et al., 2011; Sheldon & Lyubomirsky, 2006). One study found no moderation effect of intrinsic motivation (Meevissen et al., 2011). No evidence for trait optimism as a moderator (Meevissen et al., 2011; Peters et al., 2010; Peters et al., 2015; Waits, 2017). Low emotional processing was found to moderate effects on self-reported physical health (Austenfeld et al., 2006; Austenfeld & Stanton, 2008; Maddalena et al., 2014).
Gratitude letter exercise	One meta-analysis reported small between group differences on a well-being composite based on life satisfaction and depressive symptom ratings at posttest ($d = 0.14$; Davis et al., 2016). Another meta-analysis reported $d = 0.17$ for life satisfaction, $d = 0.13$ for depressive symptoms, $d = 0.31$ for grateful affect, $d = 0.18$ for positive affect, and $d = 0.22$ for state optimism (Dickens, 2017). There was no effect on negative affect. Small but significant follow-up effects (one week to six months) were found for positive affect ($d = 0.10$) and depressive symptoms ($d = 0.21$; Dickens, 2017).	One randomized controlled study found that the effect of listing things that one feels grateful for rather than daily hassles on positive affect was mediated by increased grateful affect at the same time of measurement (Emmons & McCullough, 2003).	Several moderation studies support motivation as a moderator (e.g., Lyubomirksy et al., 2005). Meta-analytic evidence shows that adults benefit more from gratitude interventions compared with college students and children (Dickens, 2017). Two studies indicate participants *higher* in trait gratitude benefit more (Rash, et al., 2011; Watkins et al., 2003), whereas one suggests those *lower* in trait gratitude gain more (Watkins et al., 2015). Other studies support extraversion (Schueller & Parks, 2012; Senf & Liau, 2013) and baseline positive affect (Froh et al., 2008) as moderators.
Self-compassionate writing	Follow-up studies indicate higher happiness and decreased depressive symptoms several months (Shapira & Mongrain, 2010, no ES reported) and two weeks later ($d = 0.49$; Johnson & O'Brien, 2013, study 2). Other studies found higher positive affect ($d = 0.48$) and body satisfaction ($d = 0.57$; Stern & Engeln, 2018, study 1), higher self-esteem (Imrie & Troop, 2012, no ES reported), and higher self-compassion ($d = 0.54$ and $d = 0.88$; Kelly & Wearing, 2018; Mosewich et al., 2013) at posttest. On study reported *higher* negative affect during the intervention ($d = 0.68$; Wong & Mak, 2016).	Two longitudinal mediation studies using a multicomponent intervention generally support self-compassion as a mediator of higher life satisfaction and happiness (Neff & Germer, 2013) and fewer depressive symptoms (Schotanus-Dijkstra et al., 2019) six months later. Another mediation study found links between increased self-compassion and reduced depressive symptoms (Johnson & O'Brien, 2013). One study reported no effects on self-compassion or emotion regulation (Wong & Mak, 2006).	One moderation study supports higher connectedness but not higher self-criticism as a moderator of effects on happiness (Shapira & Mongrain, 2010).

Note. The best-possible-self intervention asks participants to write about their best possible future for 20 minutes on three consecutive days; the gratitude letter requires participants write and deliver a letter to someone whom they are grateful to; self-compassionate writing instructs participants to think about an event that made them feel inadequate and then respond compassionately to this experience. ES = standardized effect size. Standardized effect sizes other than Hedge's g and Cohen's d were transformed according to the formulas provided by Cohen (1988).

2.3 The Best-Possible-Self Intervention

The original version of the best-possible-self intervention requires individuals to write about their

best possible future for 20 minutes on three consecutive days (King, 2001). Participants read the

following instruction:

Think about your life in the future. Image that everything has gone as well as it possibly could.

You have worked hard and succeeded at accomplishing all of your life goals. Think of this as the

realization of all of your life dreams. Now, write about what you imagined. (King, 2001, p. 801)

The rationale of the best-possible-self intervention is to increase well-being through cultivating optimism.

Optimism has been defined as the expectation of favorable outcomes in one's life (Scheier & Carver,

1985). Existing self-report assessments of optimism distinguish between optimism as a stable orientation

towards one's life (trait optimism; Life Orientation Test and Life Orientation Test Revised; Scheier &

Carver, 1985; Scheier, Carver, & Bridges, 1994) and current positive future expectations (state optimism;

Subjective Probability Task and Future Expectations Scale; MacLeod, 1996; Hanseen, Peters, Vlaeyen,

Meevissen, & Vancleef, 2012). Meta-analysts have demonstrated strong links between trait optimism and

indicators of well-being such as depressive symptoms (r = -.44, 95% CI [-.46, -.41], based on 129

samples), life satisfaction (r = .43, 95% CI [.39, .46], 50 samples), and psychological well-being (r = .41,

95% CI [.37, .44], 25 samples; Alarcon, Bowling, & Khazon, 2013; also see Andersson, 1996). Thus,

higher happiness may be achieved through increasing optimism. Examining this possibility was one

intention of King's (2001) landmark study. Another was to investigate the well-being enhancing potential

of the best-possible-self intervention as an alternative to the expressive writing paradigm. Expressive

writing tasks asks participants to write about their deepest thoughts and feelings related to a personal,

stressful event (Pennebaker & Seagal, 1999). Expressive writing been suggested to operate through

enabling new insights, as well as releasing pent-up emotions (Pennebaker & Seagal, 1999). Current meta-

analytic evidence suggests small intervention effects on a combined measure of positive psychological

functioning (e.g., including life satisfaction ratings; r = .03, 95% CI [.01, .08], 61 comparisons) and

distress (e.g., General Health Questionnaire; r = .06, 95% CI [.04, 0.16], 33 comparisons) but not

depressive symptoms (Frattaroli, 2006; see Kállay, 2015; Reinhold, Bürkner, & Holling, 2018; Rude &

Haner, 2018, for a deeper discussion). One downside of expressive writing is that participants tend to

experience negative affect during and after the writing sessions. Although initially, the release of negative

emotions in a safe setting has been thought of as a necessary change mechanism, more recent evidence

has shown that writing about positive aspects of one's life and related positive feelings may provide equal

benefits (King & Miner, 2000). Thus, the original aim of the best-possible-self intervention was to increase well-being without having to re-experiencing stressful memories. Thematic analyses of intervention texts confirm that the best-possible-self intervention effectively encourages positive topics as participants predominantly write about job success, family, travel, home ownership, leisure activities, and generally about desirable features of their future lives (Hill, Terrell, Arellano, Schuetz, & Nagoshi, 2015; King, 2001; Loveday, Lovell, & Jones, 2018).

2.3.1 Effects

Regarding intervention effects, randomized controlled trials using student and general public samples in predominantly Western cultures have repeatedly demonstrated that the best-possible-self intervention effectively increases optimism and positive affect (see Loveday, Lovell, & Jones, 2016, for a review). Results from two recent meta-analyses indicate that the posttest difference between intervention and active control conditions on a combined measure of state and trait optimism was Hedge's $g = 0.64$, 95% CI [0.42, 0.86] (based on 10 studies; Malouff & Schutte, 2016), whereas the difference in changes from pretest to posttest in a similar construct was $g = 0.33$, 95% CI [0.25, 0.42] (based on 13 studies; Carrillo et al., 2019). The difference in changes in positive affect from pretest to posttest was $g = 0.51$, 95% CI [0.26, 0.77] (based on 13 studies; Carrillo et al., 2019). There was no effect on negative affect ($g = 0.19$, 95% CI [-0.33, 0.71], 13 studies; Carillo et al., 2019). In addition, some studies indicate fewer health center visits in the months after the best-possible-self intervention (King, 2001), increased life satisfaction one month later (no standardized effect size reported; Boehm, Lyubomirsky, & Sheldon, 2011), fewer depressive symptoms after four weeks (Cohen's $d = 0.41$; Shapira & Mongrain, 2010) and five weeks (no standardized effect size reported; Yogo & Fujihara, 2008), lower self-reported pain during the cold pressor task ($d = 0.34$ after 20 seconds; Hanssen, Peters, Vlaeyen, Meevissen, & Vancleef, 2013; also see Boselie, Vancleef, & Peters, 2016, study 2), and increased attention towards positive stimuli immediately after the intervention (Peters, Vieler, & Lautenbacher, 2015). There is also evidence for beneficial effects among distressed individuals (e.g., $d = 0.74$ for pre to posttest change in state optimism; Huffman et al., 2014) and children ($d = 0.54$ for difference in changes in self-rated self-esteem relative to an active control; Owens & Patterson, 2013). Effects for participants from Eastern cultures remains controversial (Liau, Neihart, Teo, & Lo, 2016). To sum up, a strong empirical case can be made that the best-possible-self intervention increases positive affect and optimism (see Table 2.2 for a summary). However, one shortcoming of current effectiveness research, and specifically current meta-analyses of the

effects of the best-possible-self intervention (Carrillo et al., 2019; Malouff & Schutte, 2016), is that no detailed summary exists that displays effects at various times of measurement (e.g., immediately after the intervention vs. several days or weeks later) or accounts for the use of different conceptualizations of the same outcome (e.g., state vs. trait optimism). Such a summary is important because researchers rely on making accurate predictions about intervention effects when planning and carrying out studies that involve the best-possible-self intervention. Thus, one aim of this thesis is to comprehensively examine the effects of the best-possible-self intervention considering the time of outcome assessment and outcome conceptualization.

2.3.2 Mediators

As displayed in Table 2.1, the effects of the best-possible-self intervention can be explained using cognitive and evolutionary theories. From an empirical perspective, however, little is known about which theory makes more accurate predictions about underlying psychological processes.

Self-regulation theory. King (2001) explained that writing about one's best possible future might involve "bringing awareness and clarity to one's life goals, reorganizing priorities, [and] deciding on values" (p. 800), which in turn facilitates short- and long-term well-being. One way how reflective processes may unfold effects is through encouraging the choice of intrinsically rewarding goal pursuits, which have been proposed to promote long-term well-being (Ryan & Deci, 2000; see Ryan & Deci, 2019, for a current review). Evidence from one randomized controlled trial using a German student sample, however, suggests that participating in the best-possible-self intervention does not influence the relative importance that participants place on intrinsic compared with extrinsic goal pursuits (Heekerens & Heinitz, 2019). This speaks against the notion that writing about one's best possible future promotes reflective processes regarding life goals, at least if reflective processes are conceptualized as actively reorganizing priorities and deciding on values (King, 2001). It could still be that other conceptualizations of reflective processes explain the effects of the best-possible-self intervention. For example, being aware of one's meaningful life goals and feeling a sense of clarity regarding one's future life are important prerequisites for well-being from a motivational psychological perspective (see Kelly, Mansell, & Wood, 2015, for a theoretical framework). On the other hand, conflict between life goals and resulting ambivalence in the choice and pursuit of goals have been proposed to undermine well-being (Kelly et al., 2015). Thus, some benefits of the best-possible-self intervention may be accounted for by reflective processes that help to reduce goal ambivalence and foster clarity regarding one's life goals. This

hypothesis, however, has not yet been tested empirically. Accordingly, one aim of this thesis is to investigate reduced goal ambivalence as a mediator of the effects of the best-possible-self intervention on well-being.

Process model of emotion regulation. The process model of emotion regulation proposes that the short- and long-term affective benefits of the best-possible-self intervention result from increased positive future expectations (Quoidbach et al., 2015). The rationale here is that encouraging individuals to anticipate positive future situations induces positive affect, whereas reflective processes regarding personal values and life goals are largely disregarded. Importantly, the process model of emotion regulation argues that changed cognitions (e.g., altered future expectations) proceed emotional benefits, whereas the broaden-and-build theory argues that the development of adaptive cognitive skills (e.g., trait optimism) is driven by accumulative experiences of positive emotions. In line with predictions derived from the process model of emotion regulations, results from one longitudinal mediation study show that increased state optimism at the end of a three months long multicomponent positive-psychological intervention, which also included the best-possible-self intervention, mediated improvements on a combined measure of subjective and psychological well-being, as well as reduced anxiety and fewer depressive symptoms, six months later (Schotanus-Dijkstra, Pieterse, Drossaert, Walburg, & Bohlmeijer, 2018, 2019). Although these results provide initial evidence for optimism as a mediator, the study has two major limitations. First, effects were compared against a waiting list control and it cannot be ruled out that the mediation effect is explained by expectations of positive change that may have systematically influenced both optimism ratings at the end of the intervention period and the self-reports six months later. Second, results are insufficient to draw firm conclusions regarding the mechanisms of the best-possible-self intervention as a stand-alone exercise. Thus, one aim of this thesis is to investigate increased positive future expectations as a mediator of the effects of the best-possible-self intervention as a stand-alone intervention while using a rigorous control condition.

Broaden-and-build theory. Moreover, Fredrickson (1998, 2001) explained that positive emotions help to increase long-term well-being through broadening individuals' momentary thought-action repertoires and building personal resources. In line with this prediction, results from one randomized controlled mediation study indicate that the experience of positive emotions during an eight weeks long loving kindness meditation program predicted increases in optimism and psychological well-being at posttest, which, in turn, explained increases in life satisfaction two weeks later (Fredrickson, Cohn,

Coffey, Pek, & Finkel, 2008; see Gander, Proyer, Hentz, & Ruch, 2019; Meyers & van Woerkom, 2017, for similar evidence). Regarding the best-possible-self intervention, results from one randomized controlled trial suggest that positive emotions after the best-possible-self intervention mediate increases in psychological well-being (Auyeung & Mo, 2018). In line with the broaden-and-build theory, Auyeung and Mo conclude that positive emotions induced by the best-possible-self intervention drive effects of the best-possible-self intervention on increased personal resources (i.e., psychological well-being). The finding, however, should be considered cautiously because the researchers assessed positive emotions and psychological well-being at the same time of measurement. Drawing causal conclusions under such circumstances is very difficult. For example, the results also allow the conclusion that increases in psychological well-being result in the experience of more positive emotions or that a third variable (e.g., positive self-relevant thinking) explains the association. Proper mediation studies require randomized controlled longitudinal designs showing that the change in one variable predicts the change in another, while ruling out alternative explanations (see Heekerens & Heinitz, 2019; Schotanus-Dijkstra et al., 2019, for examples). Another randomized controlled best-possible-self trial, this time using a longitudinal mediation design, found that improved illness related self-care among diabetes patient four weeks after the best-possible-self intervention was unrelated to positive emotions during the intervention period (Gibson, Umeh, Newson, & Davies, 2018). This finding speaks against the broaden-and-build hypothesis because observed increases in personal resources were not accounted for by previous experiences of positive emotions. Thus, current evidence regarding the role of positive affect as a mediator of the effects of the best-possible-self intervention is inconclusive. To account for predictions derived from the broaden-and-build theory regarding the processes underlying the effects of the best-possible-self intervention this study also aims to examine the role of positive emotions as a mediator.

Positive self-representations hypothesis. Finally, the positive self-representations hypothesis (Mongrain & Anselmo-Matthews, 2012) posits that positive-psychological interventions, including the best-possible-self intervention, increase well-being through providing an opportunity to focus on positive aspects of one's self and one's life (i.e., positive self-representations). In line with this notion, results from one mixed-method study suggest that participants who used more phrases that reflect personal improvements (e.g., "I will expose myself to what life brings to me") reported larger increases in positive affect immediately after the best-possible-self intervention (Carrillo, Martínez-Sanchis, Etchemendy, & Baños, 2019; see Hefferon, Ashfield, Waters, & Synard, 2017, for an introduction to qualitative

approaches in positive psychology). This evidence, however, is indirect at best. Studies that quantify the effect of positive-psychological interventions on positive self-relevant thoughts are needed to evaluate the credibility of the idea that such thoughts are responsible for observed increases in well-being. Accordingly, one aim of this study is to examine the effects of different positive-psychological interventions on positive self-relevant thinking.

2.3.3 Moderators

Some research has addressed questions regarding the circumstances under which the best-possible-self intervention shows optimal effects as well as for whom the best-possible-self intervention works best. Subsequently, evidence related to personal characteristics, activity features, and indicators of person-activity fit that have been proposed to influence the effectiveness of the best-possible-self intervention is reviewed (also see Loveday et al., 2016, for a review).

Personal characteristics. Participants' motivation to engage with an intervention has been identified as a relevant moderator at the level of the person (Lyubomirsky, Sheldon, & Schkade, 2005). Specifically, results from one randomized controlled best-possible-self trial that asked participants to either take part in a study involving "a happiness intervention" (high motivation) or "cognitive exercises" (low motivation) indicate larger effects for highly motivated, self-selected participants (Dickerhoof, 2007; Lyubomirsky, Dickerhoof, Boehm, & Sheldon, 2011). In addition, effects among more motivated participants lasted longer and the authors explained this finding with the observation that motivated participants were more likely to continue the intervention on their own (Lyubomirsky et al., 2011). In line with this result, subsequent studies found that higher baseline intrinsic motivation to perform the exercise increased the likelihood of continued performance after the intervention, which in turn bolstered effects at follow-up assessments (Seear & Vella-Brodrick, 2013; Sheldon & Lyubomirsky, 2006). Furthermore, providing participants with peer testimonials that advocated the benefits of the best-possible-self intervention resulted in larger effects in one study, presumably through increasing participants motivation to engage with the exercise (Layous, Nelson, & Lyubomirsky, 2013). On the contrary, one study did not find that intrinsic motivation to perform the best-possible-self intervention moderated effects on positive affect and positive future expectations two weeks later (Meevissen, Peters, & Alberts, 2011). All in all, however, current evidence supports the notion that successfully participating in the best-possible-self intervention requires "intentional buy-in by participants" (Sheldon & Lyubomirsky, 2019, p. 3), which is in line with the positive activity fit model (Lyubomirsky & Layous, 2013). Apart from motivation, culture

has been discussed as a relevant personal characteristic. Specifically, researchers explained that the best-possible-self interventions aligns with a cultural emphasis on self-improvement and personal agency, which is typically present in Western countries (Boehm et al., 2011). Supporting evidence comes from one cross-cultural study, indicating that effects are larger among participants from more individualistic societies (Boehm et al., 2011). This finding supports predictions derived from the positive activity model (Lyubomirsky & Layous, 2013).

Activity features. Different forms of the best-possible-self intervention and varying levels of intensity have been shown to produce significant effects on well-being. Most studies asked participants to *write* about their best possible future (e.g., King, 2001). Initial experimental evidence, however, indicates that *talking* about one's best possible future might be equally effective (Harrist, Carlozzi, McGovern, & Harrist, 2007). In addition, some studies instructed participants to *visualize* their best possible future before or after the writing session (e.g., Peters, Meevissen, & Hanssen, 2013). However, no study has directly tested whether writing or visualizing or a combination of both produces larger effects, although either version seems to provide beneficial effects. In addition, most researchers asked participants to write about their best possible future *in general* (e.g., King, 2001), whereas others instructed participants to focus on *specific life domains* such as health, career, or relationships (e.g., Meevissen et al., 2011). Again, it is currently unclear whether and how such differences influence the effects of the best-possible-self intervention. The best-possible-self intervention has also been successfully administered online (Shapira & Mongrain, 2010). Results from one experimental study show no difference between performing the intervention online and in-person (Layous et al., 2013), which highlights the intervention's potential for digital formats (see Diefenbach, 2018, for an introduction). Finally, regarding intensity, previous studies successfully delivered the best-possible-self intervention in a single session (e.g., Peters, Flink, Boersma, & Linton, 2010), or repeatedly over the course of several days (e.g., King, 2001) and weeks (e.g., Austenfeld, Paolo, & Stanton, 2006). However, it remains unclear how these variations affect outcomes. Thus, one aim of this thesis is to investigate whether and to which degree contextual moderators (e.g., length, delivery format) influence the effects of the best-possible-self intervention.

Person-activity fit. The person-activity fit hypothesis states that individuals particularly benefit from a positive-psychological intervention when the exercise either remedies person-specific sources of unhappiness or when it draws on existing personal strengths, thus creating a match between participants' preferred styles and exercise demands (Lyubomirsky, 2007; Schueller, 2010, 2011). For example,

pessimistic individuals have been proposed to benefit more from the best-possible-self intervention because the exercise might help them to cultivate optimism (see Lyubomirsky, 2007, for a deeper discussion). Another possibility is that the best-possible-self intervention is more effective among optimistic individuals because the exercise allows optimists to express their confidence about the future, thus building on an existing strength (e.g., Meevissen et al., 2011). Results from several studies from the USA, Sweden, and Germany support neither of the two hypotheses, indicating no moderation effect of trait optimism on the effects of the best-possible-self intervention (Meevissen et al., 2011; Peters et al., 2010; Peters et al., 2015; Waits, 2017). Thus, current evidence suggests that the best-possible-self intervention is equally suited for individuals low and high in trait optimism. Other researchers focused on different moderators. For example, one controlled study with students from Singapore found that individuals higher in baseline neuroticism but equal baseline happiness benefited more from the best-possible-self intervention (Ng, 2016). One explanation for this finding is that individuals who tend to worry about their future and are generally psychologically vulnerable particularly profit from deliberately adopting a more optimistic point of view (Ng, 2016; also see Lyubomirsky, 2007). All in all, however, the empirical basis for neuroticism as a moderator seems preliminary, especially because one earlier study that examined neuroticism as a moderator in a Western sample found no effect (Peters et al., 2010). More promising results come from studies that investigated the role of emotional approach coping (Austenfeld et al., 2006; Austenfeld & Stanton, 2008). Emotional approach coping encompasses active attempts to come to an understanding of own emotions (emotional processing) and attempts to communicate one's emotional experience (emotional expression; see Austenfeld & Stanton, 2004, for a review). Austenfeld and colleagues (2006) hypothesized that individuals who prefer not to approach unpleasant emotions (low emotional approach copers) should particularly benefit from the best-possible-self intervention because writing about one's best possible future might provide self-regulatory benefits without an exploration of negative emotions (also see King, 2001). In line with this prediction, results from two randomized controlled trials suggest that students lower in emotional processing (but not emotional expression) reported fewer depressive symptoms and lower hostility after participating in the best-possible-self compared with participants who wrote about a traumatic experience, whereas students high in emotional processing benefitted more from writing about trauma (Austenfeld et al., 2006; Austenfeld & Stanton, 2008). Further underpinning the moderating role of emotional approach coping, results from one subsequent study indicate that targeting the best-possible-self intervention to low emotional processors

resulted in increased self-reported physical health one month later relative to an active control condition (Maddalena, Reese, & Barnes, 2014). To date, however, it remains unclear, how participants' dispositional tendency to attend to own emotions influences the effects of the best-possible-self intervention on positive affect and positive future expectations. Such knowledge is important because positive affect and positive future expectations are the most consistently reported outcomes in the literature. Thus, one aim of this study is to investigate emotional self-awareness as a moderator of the effects of the best-possible-self intervention.

2.4 The Gratitude Letter Exercise

The gratitude letter exercise requires participants to write and deliver a letter to someone whom they are grateful to. The activity was first described by Martin Seligman in his popular scientific book *Authentic Happiness* (2002). The instruction was as follows:

> Select one important person from your past who has made a major positive difference in your life and to whom you have never fully expressed your thanks. Write a testimonial just long enough to cover one laminated page. Take your time composing this; […]. Invite that person to your home, or travel to that person's home. It is important you do this face to face, not just in writing or on the phone. Do not tell the person the purpose of the visit in advance; a simple "I just want to see you" will suffice. […] bring a laminated version of your testimonial with you as a gift. When all settles down, read your testimonial aloud slowly, with expression, and with eye contact. Then let the person react unhurriedly. Reminisce together about the concrete events that make this person so important to you. (Seligman, 2002, p. 74)

The rationale of the gratitude letter exercise is to increase well-being through cultivating gratitude. Gratitude has been defined as an emotion that is directed towards the appreciation of valuable aid received from others (grateful affect; Wood, Maltby, Stewart, Linley, & Joseph, 2008) and as a tendency to experience grateful affect frequently, intensely, and deeply (state and trait gratitude; Gratitude Questionnaire-6; McCullough, Emmons, & Tsang, 2002). Grateful affect simply refers to feeling thankful, whereas state and trait gratitude also include grateful appraisals (e.g., realizing that one has much in life to be thankful for; McCullough et al., 2002). Researchers have repeatedly demonstrated moderate to strong links between trait gratitude and indicators of well-being such as depressive symptoms ($r = -.34$, $r = -.54$, and $r = -.56$), life satisfaction ($r = .49$, $r = .50$, and $r = .62$), and positive affect ($r = .36$ and $r = .52$; all coefficients based on different general population samples; Watkins, Woodward, Stone, &

Kolts, 2003; see Wood, Froh, & Geraghty, 2010, for a review). Thus, higher happiness may be achieved through increasing gratitude. The gratitude letter exercise was first validated using a convenience sample of US American adults who logged into a website dedicated to happiness research that was created for Seligman's (2002) book and offered a whole range of happiness activities (Seligman et al., 2005). The aim of the study was to test which activities can make people lastingly happier. Results showed that participants who performed the gratitude letter exercise reported higher happiness and fewer depressive symptoms up to one month after the intervention compared with participants who wrote about early memories. The authors concluded that "participants in the gratitude visit condition showed the largest positive changes in the whole study" (Seligman et al., 2005, p. 417).

2.4.1 Effects

Since then, randomized controlled trials using diverse samples including students, adults, and distressed individuals repeatedly found that the gratitude letter exercise increases positive affect, happiness, state optimism, and life satisfaction as well as that it reduces depressive symptoms (Boehm et al., 2011; Dickerhoof, 2007; Froh, Kashdan, Ozimkowski, &Miller, 2009; Huffman et al., 2014; Lyubomirsky et al., 2011; Proyer, Gander, Wellenzohn, & Ruch, 2014; Schueller, 2011; Shin, Wong, Yancura, & Hsu, 2018; Toepfer, Cichy, & Peters, 2012; Toepfer & Walker, 2009; Wong & Mak, 2016). In addition, the gratitude letter has been shown to increase grateful affect (e.g., Froh et al., 2009) but not trait gratitude (e.g., Toepfer et al., 2012; see Wood, Froh, & Geraghty, 2010, for a review). One meta-analysis estimated that the standardized posttest effect of gratitude interventions compared with neutral control conditions was Cohen's $d = 0.14$, 95% CI [0.01, 0.27], for a well-being composite based on life satisfaction and depressive symptom ratings (based on 20 samples; Davis et al., 2016). Another meta-analysis reported $d = 0.17$ for life satisfaction (19 samples; Dickens, 2017) and $d = 0.13$ for depressive symptoms alone (9 samples; author reported effect sizes without confidence intervals; Dickens, 2017). Other effects include increased grateful affect ($d = 0.31$, 9 samples), positive affect ($d = 0.18$, 19 samples), and state optimism ($d = 0.22$, 5 samples; Dickens, 2017). Small but significant follow-up effects were found for positive affect ($d = 0.10$, 11 samples) and depressive symptoms ($d = 0.21$, 5 samples) but not for life satisfaction and optimism (follow-up assessments ranged from one week to six months after the intervention; Dickens, 2017). Compared with results from the original study (Seligman et al., 2005), meta-analytic effects (Dickens, 2017) are considerably smaller (e.g., $d = 1.37$ vs. $d = 0.21$ for follow-up effects on depressive symptoms). One explanation for the differing results is that Dickens

(2017) calculated average effects of various gratitude interventions, also including gratitude lists and journals, and thus the true effect of the gratitude letter exercise might be larger, although probably not as large as in the original study (see Mongrain & Anselmo-Matthews, 2012; Proyer et al., 2014, for replications of Seligman's original study). To sum up, there is robust evidence that the gratitude letter exercise effectively increases different indicators of well-being, albeit effects are generally small (see Table 2.2. for a summary).

2.4.2 Mediators

As Table 2.1 reveals, various theories propose different mechanisms underlying the gratitude letter exercise (see Alkozei, Smith, & Killgore, 2018; Lyubomirsky, 2007, for comprehensive reviews). As with the best-possible-self intervention, direct evidence regarding the validity of theoretically proposed mechanisms is sparse. Major findings and current gaps in the literature are discussed below (also see Table 2.2).

Process model of emotion regulation. Quoidbach and colleagues (2015) explained that writing a gratitude letter increases positive affect through encouraging individuals to adopt a grateful outlook on past events or relationships, which is a form of reappraisal that should result in elevated mood. In studies that ask participants to deliver the letter, effects may also be accounted for by emotional expression and social components rather than reappraisals (Quoidbach et al., 2015). The emotion regulation perspective corresponds to the amplification theory of gratitude (Watkins, 2014; Watkins, Cruz, Holben, & Kolts, 2008), which proposes that "gratitude enhances well-being because it amplifies the good in one's life" (Watkins, McLaughlin, & Parker, 2019, p. 25). Put simply, the idea is that writing a gratitude letter encourages individuals to notice and appreciate the gifts in life that they have received from others, whereas noticing and appreciating the good in one's life can be thought of as a cognitive skill relevant to high levels of happiness. In their review, Quoidbach and colleagues (2015) conclude that the empirical case for cognitive reappraisals as a mechanism of the gratitude letter exercise remains controversial because intervention studies that use gratitude lists or gratitude letters without delivery sometimes show no effects on positive affect (also see Wood et al., 2010). However, even though the latest evidence suggests that effects on positive affect are repeatedly observed after the gratitude letter exercise (see Davis et al., 2016; Dickens, 2017, for meta-analyses), this does not necessarily mean that the exercise increase positive affect *because* it increases grateful cognitions. Such conclusions require appropriate longitudinal mediation studies showing that higher well-being after the gratitude letter exercise results

from increased state gratitude during or immediately after the intervention. These studies are currently missing (however, see O'Connell, O'Shea, & Gallagher, 2018; Watkins, Uhder, & Pichinevskiy, 2015, for related evidence using gratitude lists). In addition, it remains unclear whether the gratitude letter exercise specifically affects gratitude or whether other positive-psychological interventions show similar effects. Such knowledge, however, is important to establish that benefits of the gratitude letter exercise are due to the initial induction of grateful cognitions. Thus, one aim of this study is to investigate the effects of the gratitude letter exercise in contrast to other positive-psychological interventions.

Broaden-and-build theory. The broaden-and-build theory (Fredrickson, 2004) holds that feeling grateful as a result of performing the gratitude letter exercise broadens one's thought-action repertoires, which helps to build social resources through a variety of prosocial responses towards one's benefactor (e.g., inviting him or her for diner), and ultimately increases well-being. Thus, from this perspective, the psychological mechanism of the gratitude letter exercise includes inducing grateful affect and enhancing one's relationships, which are indisputable an important foundation of well-being (see Algoe, 2012, for a deeper discussion). In addition, grateful affect closely relates to other positive emotions (e.g., joy; Watkins, Emmons, Greaves, & Bell, 2018) and gratitude interventions have been suggested to initiate an "upward spiral" of positive emotions that mutually reinforce each other and provide further benefits for the individual (Fredrickson, 1998; Watkins et al., 2019). Unlike the process model of emotion regulation, which emphasizes the role of cognitive changes (e.g., realizing that one has much in life to be thankful for) prior to the experience of positive affect, the broaden-and-build theory focuses on grateful affect (i.e., feeling thankful) as the initial change agent. In line with this reasoning, one randomized controlled study found that the effect of listing things that one feels grateful for rather than daily hassles on positive affect was mediated by increased grateful affect (Emmons & McCullough, 2003). This finding, however, should be treated cautiously as the authors based their conclusion on only one occasion of measurement and used a mediation approach that has been criticized and is no longer used today (Baron & Kenny, 1986; see Hayes, 2009, for a discussion). In addition, meta-analyses report robust effects of the gratitude letter exercise on grateful affect (Davis et al., 2016; Dickens, 2017). This generally supports the broader and build theory. As priori mentioned, however, the exact interplay of grateful affect, grateful cognitions, and other positive emotions following the gratitude letter exercise remains largely unknown.

Positive self-representations hypothesis. Finally, the positive self-representations hypothesis (Mongrain & Anselmo-Matthews, 2012) focuses on positive self-relevant thinking as an explanatory

factor. The idea is that writing a gratitude letter increases well-being because it gives participants an opportunity to see themselves and their lives in a good light. To support their notion, Mongrain and Anselmo-Matthews (2012) replicated Seligman and colleagues' (2005) original study. Results from the randomized controlled trial suggest that various positive-psychological interventions (e.g., repeatedly listing three good things that occurred during one's day) did not outperform writing about positive early memories in increasing happiness and reducing depressive symptoms several months after the intervention (Mongrain & Anselmo-Matthews, 2012). The authors concluded that because effects do not differ between conditions and because positive-psychological interventions and writing about positive early memories presumably activate positive self-relevant thoughts, salient positive self-representations must be responsible for the observed benefits of positive-psychological interventions. In the study, however, no attempt was made to directly assess the assumed increase in positive self-relevant thinking. This is an important shortcoming because based on available knowledge (Mongrain & Anselmo-Matthews, 2012) it is difficult to judge whether inducing positive self-representations accounts for the effects of both writing exercises used in the study. Another explanation for the observed effect pattern is that increases in happiness and decreases in depressive symptoms result from distinct specific mechanisms of positive-psychological interventions and writing about positive early memories (e.g., Seligman et al., 2005). Thus, as mentioned before, one aim of this study is to investigate the effect of different positive-psychological interventions, including the gratitude letter exercise, on positive self-relevant thoughts.

2.4.3 Moderators

As for other positive-psychological interventions, the effects of the gratitude letter exercise presumably vary across contexts and participants. Several variables have been discussed that might influence the effectiveness of the gratitude letter exercise. Currently, personal characteristics, activity features, and indicators of person-activity fit are discussed as moderators (Layous & Lyubomirsky, 2013).

Personal characteristics. Meta-analytic evidence shows that adults benefit more from gratitude interventions compared with college students and children (Dickens, 2017). The author explained that gratitude interventions may be too difficult for children and students may be less invested in the practice because students are likely receiving only course credit for participation. In addition, experimental evidence shows that participants who are motivated to perform the gratitude letter exercise typically gain more (Dickerhoof, 2007; Lyubomirsky et al., 2011; Sheldon & Lyubomirsky, 2006). As with the best-

possible-self intervention, motivation predicted continued exercise engagement, which, in turn, predicted

better outcomes. In line with this, Seligman and colleagues (2005) concluded that "participants who

continued the exercise were the happiest" (p. 419). This line of evidence supports predictions derived

from the positive activity model (Lyubomirsky & Layous, 2013). Finally, initial evidence indicates that

the gratitude letter exercise is effective among both participants from individualistic cultures such as the

USA and more collectivistic cultures such as India and China (Boehm et al., 2011; Titova, Wagstaff, &

Parks, 2017). However, there is an ongoing debate on the risks and benefits of applying the gratitude

letter exercise in collectivistic cultures and current studies may have overlooked side effects that could

result from culturally inappropriate applications (see Fritz & Lyubomirsky, 2018, for a discussion).

    Activity features. Some evidence indicates that the gratitude letter exercise is effective regardless

of whether the letter is delivered or not. Specifically, studies reported beneficial effects when participants

were asked not to deliver the letter (Lyubomirsky et al., 2011) and when the delivery was explicitly

optional (Dickerhoof, 2007). This finding speaks against the notion that emotional expression and social

components are the main drivers of the effects of the gratitude letter exercise and provides indirect

evidence that cognitive changes may play a more important role (Quoidbach et al., 2015). To date,

however, experimental evidence from studies comparing individuals who deliver the letter with

individuals who do not are missing. Such studies could also address the questions, how big the potential

benefit of delivering the letter is and whether there are side effects (Fritz & Lyubomirsky, 2018).

    Person-activity fit. Trait gratitude, extraversion, and positive affect have been investigated as

indicators of person-activity fit. Specifically, writing a gratitude letter has been suggested to be easier and

to feel more natural for inherently grateful individuals (Kaczmarek et al., 2015; Schueller, 2011). In line

with this, results from randomized controlled indicate that participants who reported higher trait gratitude

(Rash, Matsuba, & Prkachin, 2011; Watkins, Woodward, Stone, & Kolts, 2003, study 4) or a pronounced

character strength of gratitude (Dosset, 2011) benefitted more from gratitude interventions. In contrast,

descriptive results from one trial using gratitude lists showed that participants *low* in trait gratitude gained

more (Watkins et al., 2015). From a theoretical perspective, this also makes sense because participants

who seldomly experience grateful affect should particularly benefit from overcoming this potential source

of unhappiness (Lyubomirsky, 2007). Finally, extraversion and positive affect have been examined as

moderators. Results from two randomized controlled studies indicate that effects of the gratitude letter

exercise at 4- to 6-weeks follow-up on depressive symptoms (Schueller & Parks, 2012; Senf & Liau,

2013) and happiness (Senf & Liau, 2013) are larger among more extroverted individuals. One explanation for this is that extroverts are more likely to continue activities that bring them into regular contact with others (Lyubomirsky, 2007). Another randomized controlled study concluded that children who were higher in baseline positive affect reported greater grateful affect two months after listing grateful events compared with children lower in baseline positive affect (Froh et al., 2008). All in all, however, trait gratitude is the most promising moderator to date. One problem is that current evidence is inconclusive regarding the direction of the effect, while from a theoretical perspective both directions, namely elevated effects for participants *higher* or *lower* in trait gratitude, make sense. Thus, one aim of this thesis is to investigate trait gratitude as a moderator of the effects of the gratitude letter exercise.

2.5 Self-Compassionate Writing

Self-compassionate writing requires participants to think about an event that made them feel inadequate and then respond compassionately to this experience (Shapira & Mongrain, 2010). In the original paper participants received the intervention online and wrote for 15 minutes daily over the course of one week. After being asked to think about a distressing and upsetting event, participants read the following instructions:

> To start writing your own letter, try to feel that part of you that can be kind and understanding of others. Think about what you would say to a friend in your position, or what a friend would say to you in this situation. Try to have understanding for your distress (e.g., I am sad you feel distressed…) and realize your distress makes sense. Try and be good to yourself. We would like you to write whatever comes to you, but make sure this letter provides you with what you think you need to hear in order to feel nurtured and soothed about your stressful situation or event.
> (Shapira & Mongrain, 2010, p. 380)

The rationale of self-compassion interventions is to increase well-being through cultivating gratitude. Self-compassion has been defined as compassion directed towards oneself, which involves being kind and understanding towards oneself in instances of pain and failure (self-kindness), perceiving one's experiences as part of the larger human experience (common humanity), and holding painful thoughts and feelings in balanced awareness (mindfulness; Neff, 2003a; see Barnard & Curry, 2011; Strauss et al., 2016, for a deeper discussion). Self-compassion has been conceptualized as a fleeting experience (state self-compassion; Breines & Chen, 2012) and as a dispositional tendency (trait self-compassion; Neff, 2003b). Meta-analysts have demonstrated strong links between trait self-compassion and indicators of

well-being such as life satisfaction (r = .47, 95% CI [.45, .50], based on 48 samples), positive affect (r = .39, 95% CI [.34, .43], 33 samples), negative affect (r = -.47, 95% CI [-.50, -.43], 32 samples), and psychological well-being (r = .62, 95% CI [.56, .67], 12 samples; Zessin, Dickhäuser, & Garbade, 2015; see Bluth & Neff, 2018, for a recent review). Thus, higher happiness may be achieved through increasing self-compassion. In line with this, results from a recent meta-analysis show that intense self-compassion programs (e.g., compassion-focused therapy and compassion cultivation training) effectively build self-compassion (Cohen's d = 0.60, 95% CI [0.44, 0.76], 16 trials), increase subjective well-being (d = 0.48, 95% CI [0.28, 0.67], 9 trials), and reduce depressive symptoms (d = 0.62, 95% CI [0.44, 0.80], 10 trials; all compared against active control conditions at posttest; Kirby, Tellegen, & Steindl, 2017; see Kirby, 2017; Leaviss & Uttley, 2015, for reviews). The trials included in the meta-analysis typically administered treatments over the course of several weeks under the supervision of specifically trained psychologists. Such programs are resource intensive. The aim of the original self-compassionate writing study was to examine the effects of a brief self-help intervention to develop self-compassion (Shapira & Mongrain, 2010). Results from the randomized controlled trial suggest that participants report higher levels of happiness and fewer depressive symptoms up to three months after the intervention (Shapira & Mongrain, 2010). This finding, however, should be interpreted carefully, as almost eight in ten participants dropped out of the study and the authors reported systematic differences between participants who remained in the study and those who left.

2.5.1 Effects

More recently, results from a series of three randomized controlled studies indicate that self-compassionate writing increases body satisfaction (Cohen's d = 0.57) and positive affect (d = 0.48) among female college students at the end of a three weeks intervention period (Stern & Engeln, 2018, study 1). In addition, randomized controlled studies suggest increased self-compassion (d = 0.54) and decreased shame (d = 0.68) among non-treatment seeking females with anorexia nervosa after two weeks of daily writing assignments (Kelly & Waring, 2018), increased self-soothing and self-esteem among patients with life-limiting illnesses who received self-compassionate writing together with a stress relief intervention compared with stress-relief only (no standardized effect sizes reported; Imrie & Troop, 2012), and higher levels of self-compassion in female athletes who participated in the writing exercise after a brief psychoeducation session compared with a neutral control group (d = 0.88; Mosewich, Crocker, Kowalski, & DeLongis, 2013). Other researchers reported decreased depressive symptoms and

increased trait self-compassion among predominantly female, shame-prone students up to two weeks after administering three self-compassionate writing sessions within one week ($d = 0.49$; Johnson & O'Brien, 2013, study 2). Finally, one study reported *increased* negative affect among Chinese students during self-compassionate writing assignments on three consecutive days ($d = 0.68$; Wong & Mak, 2016). The study found no effects on depressive symptoms or trait self-compassion. To sum up, there is some evidence that self-compassionate writing increases well-being, especially among young women.

2.5.2 Mediators

As with the positive-psychological interventions discussed before, various researchers have proposed different mechanisms of self-compassionate writing (see Table 2.1 for a summary). In the following only the process model of emotion regulation is discussed. The broaden-and-build theory (Fredrickson, 2004) and the positive self-representations hypothesis (Mongrain & Anselmo-Matthews, 2012) are not reviewed again because the common factor explanations offered by these perspectives are naturally similar when applied to different positive-psychological interventions. In addition, to my best knowledge, no study has directly tested common factors of self-compassionate writing. Thus, there is little evidence to discuss. That being said, one important question that remains unanswered is whether and to which degree self-compassionate writing affects positive self-relevant thinking (Mongrain & Anselmo-Matthews, 2012). Accordingly, one aim of this thesis is to investigate the effects of self-compassionate writing on positive self-relevant thoughts.

Process model of emotion regulation. Regarding specific factors underlying the effects of self-compassionate writing, researchers explained that the intervention builds well-being through supporting individuals in cultivating a specific kind of mindful awareness that allows them to overcome negative thoughts and feelings involved in personal suffering (i.e., the exercise builds self-compassion; Neff, 2011; Shapira & Mongrain, 2010). This perspective corresponds to the process model of emotion regulation, which proposes that modifying cognitive evaluations of unpleasant events helps to upregulate positive and downregulate negative emotions (e.g., meeting failure with kindness; Gross, 1998). Supporting this notion, results from one longitudinal mediation study suggest that increased self-compassion during an eight week long self-compassion program, which included writing a letter to oneself from the perspective of an ideally compassionate friend, was associated with subsequent gains in life satisfaction and happiness six months later (Neff & Germer, 2013). In addition, one study found that self-compassionate writing increased self-compassion among shame-prone individuals and linked this change to other

benefits of the intervention (Johnson & O'Brien, 2003, study 2). Results from another trial indicate that reduced depressive symptoms and lower anxiety six months after a multicomponent positive-psychological intervention, which also comprised self-compassion exercises, were partially explained by increased self-compassion at the end of the three months intervention period (Schotanus-Dijkstra et al., 2019). It, however, remains unclear whether the self-compassion exercises or other components of the program hold responsible for the observed effects on self-compassion. In contrast to the perspective offered by the process model of emotion regulation, one randomized controlled trial found that self-compassionate writing neither affected participants' ability to reflect upon and manage their emotions (i.e., emotion regulation) nor trait self-compassion (Wong & Mak, 2006). Thus, it currently remains controversial whether self-compassionate writing affects self-compassion and to which degree. In addition, very few studies have addressed the question whether effects of self-compassion interventions on self-compassion are specific or whether other positive-psychological interventions provide similar benefits (also see Seligman, et al., 2005). Establishing that self-compassion interventions specifically affect self-compassion, however, is important to gain confidence in self-compassion as an underlying mechanism. Accordingly, one aim of this thesis is to investigate the effect of self-compassionate writing on self-compassion in contrast to the effects of other positive-psychological interventions.

2.5.3 Moderators

Regarding moderators of self-compassionate writing, current research has focused on discussing personal characteristics that might explain who benefits most from such an intervention. Shapira and Mongrain (2010) explained that connected individuals, who can establish reciprocal bonds and nurture others, benefit more from self-compassionate writing because connected individuals likely have bonding capacities that they are able to extent to themselves. In addition, the authors hypothesized that self-critics, who tend to hold perfectionist dysfunctional beliefs and have difficulties being kind towards themselves, gain more because self-compassionate writing enables them to realize an adaptive skill they lack (also see Lyubomirsky, 2007). Results from moderator analyses generally support the first hypothesis (connectedness) but not the second (self-criticism; Shapira & Mongrain, 2010; also see Kelly, Zuroff, Foa, & Gilbert, 2010). Another proposal is that, following the notion that positive-psychological interventions are more successful if they fit with a person's strengths (Lyubomirsky, 2007), self-compassionate writing should be more effective among emotionally self-aware individuals who generally pay attention to own emotions. The reason for this is that emotionally self-aware individuals more readily

explore negative emotions and more often alleviate them using emotion-focused strategies (Austenfeld & Stanton, 2004). Self-compassionate writing then offers a way to approach negative emotions after a perceived failure without ruminating about them (also see Mor & Winquist, 2002). However, this hypothesis has never been tested empirically. Accordingly, one aim of this thesis is to examine the role of emotional self-awareness as a moderator of the effects of self-compassionate writing.

2.6 Chapter Summary

This chapter has described current theories and evidence regarding the effects, mediators, and moderators of three prominent positive-psychological interventions. Findings strongly suggest that the best-possible-self intervention and the gratitude letter exercise effectively increase various indicators of well-being, including theoretically expected effects on state optimism and state gratitude. The size of the effects is typically small. Currently, however, a detailed summary of the effects of the best-possible-self intervention that accounts for different times of outcome assessment (e.g., follow-up effects) and different conceptualizations of the same outcome (e.g., state and trait optimism) is missing. Thus, it remains unclear how long intervention effects last and whether trait variables are also affected. This complicates the planning and execution of best-possible-self intervention trials. Self-compassionate writing has attracted less research attention and evidence regarding the effectiveness of this intervention is tentative, yet promising. Currently, more studies are needed to confirm that the exercise effectively builds self-compassion. Regarding mediators of different positive-psychological interventions, relevant theories either emphasize specific cognitive factors based on emotional regulation strategies (process model of emotion regulation) and reflective processes (self-regulation theory) or common factors such as positive emotions (broaden-and-build theory) and positive self-relevant thoughts (positive self-representations hypothesis). Regarding specific effects, the best-possible-self intervention has been proposed to operate through increasing positive future expectations (process model of emotion regulation) and through reducing experienced goal ambivalence (self-regulation theory). Current evidence is insufficient to clearly favor any of the two perspectives. Thus, it remains unclear which variables explain the effects of the best-possible-self intervention. In addition, the process model of emotion regulation states that the gratitude letter exercise works through increasing state gratitude and self-compassionate writing works through building self-compassion. To date, however, it remains controversial as to whether the effects of the gratitude letter exercise on state gratitude and the proposed effect of self-compassionate writing on self-compassion are specific to these interventions or whether other positive-psychological interventions

provide similar benefits. Specificity of effects, however, is an important assumption of the process model of emotion regulation. No study has investigated the effect of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing on positive self-relevant thoughts, although the positive self-representations hypothesis proposes that the activation of such thoughts explains the effects of different positive-psychological interventions. Finally, researchers agree that contextual and person-specific characteristics influence the effectiveness of positive-psychological interventions. However, apart from motivation, which clearly plays a role, we currently know little about for whom certain interventions are more effective. Specifically, it remains controversial whether naturally more grateful individuals benefit more from the gratitude letter exercise or whether effects are more pronounced among individuals lower in trait gratitude. Another open question is whether interindividual differences in emotional self-awareness explain who benefits more from the best-possible-self intervention and self-compassionate writing. In addition, knowledge is lacking regarding how the best-possible-self intervention should be applied to achieve optimal results.

2.7 Specific Aims of This Thesis and Preview

As stated in Chapter 1, the overall aim of this thesis is to contribute to the further development of positive-psychological interventions through investigating their effects, mediators, and moderators. The specific aims of this thesis are (1) to investigate goal ambivalence and positive future expectations as mediators of the effects of the best-possible-self intervention on positive affect; (2) to examine unique and shared effects of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing as well as to investigate emotional self-awareness and trait gratitude as moderators; and (3) to comprehensively examine the effects of the best-possible-self intervention considering the time of outcome assessment and outcome conceptualization as well as to investigate contextual moderators (e.g., delivery format).

To achieve the first aim, we conducted a longitudinal randomized controlled intervention trial with baseline, immediate posttest, and 1-week follow-up measures of positive affect, goal ambivalence, and positive future expectations (Chapter 3). Mediation hypotheses were tested using two latent cross-lagged panel design models. Cross-lagged panel models provide the advantage of simultaneously testing two possible mechanisms of the best-possible-self intervention: First, the intervention effect on positive affect at follow-up may be mediated by increased positive future expectations (or decreased goal ambivalence) at posttest. Second, effects on positive future expectations (or goal ambivalence) at follow-up may be

mediated by induced positive affect at posttest. This way, the design allows to concurrently investigate predictions derived from the process model of emotion regulation, self-regulation theory, and the broaden-and-build theory. To achieve the second aim, we used a four groups online randomized controlled intervention trial with baseline assessments of emotional self-awareness and trait gratitude as well as immediate posttest measures of positive affect, state optimism, state gratitude, state self-compassion, and current thoughts (Chapter 4). By directly comparing different positive-psychological interventions against one control condition, the design allows to examine which effects are specific to certain interventions and which effects are common across interventions. For example, we expected that the gratitude letter exercise increases state gratitude but not state optimism, whereas the best-possible-self intervention should increase state optimism but not state gratitude. Moderation hypotheses were tested using latent multiple group analyses. Multiple group analyses allow to simultaneously investigate the influence of one moderator on various outcomes. For example, we expected trait gratitude to moderate effects on both positive affect and state gratitude following the gratitude letter exercise. To achieve the third aim, we performed a systematic literature search that resulted in a total of 34 randomized controlled best-possible-self intervention trials that were combined in various meta-analyses (Chapter 5). Moderator analyses were, inter alia, performed based on coding of the time of outcome assessment (e.g., positive affect at immediate posttest or several days later) and how outcomes were conceptualized (e.g., state or trait optimism). This way, a detailed summary of the effects of the best-possible-self intervention can be provided that, for example, allows to draw conclusions regarding the duration of different intervention effects.

2.7 References

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Chapter 3

Dealing with Conflict: Reducing Goal Ambivalence Using the Best-Possible-Self Intervention

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[1] Due to copyright issues, the authors' personal copy of the article rather than the version of record is included in this thesis.

Abstract

One of the flagship exercises in positive psychology is the best-possible-self (BPS) intervention, which has been repeatedly shown to increase positive affect. Yet little is known about the intervention's underlying psychological mechanisms. We propose that goal ambivalence and positive future expectations should operate as mediators because both variables might be affected by the BPS intervention and have been shown to promote positive affect. To investigate this issue, we randomized clusters of 188 psychology undergraduates to write about either their best possible future or their previous day. Participants reported goal ambivalence, positive future expectations, and positive affect before, immediately after, and 1 week after the intervention. Path analysis results indicated that the BPS intervention increased positive affect and decreased goal ambivalence up to 1 week later. Neither goal ambivalence nor positive future expectations mediated the effect of the BPS intervention on positive affect in the week after its implementation. Future studies should investigate how repeated administrations of the BPS intervention affect goal ambivalence over time and whether resulting lower levels of distress might explain the intervention's effect on depressive symptoms.

Keywords: positive psychology intervention; best-possible self; well-being; positive affect; positive future expectations; goal ambivalence; process model of emotion regulation; self-regulation

The last decade has witnessed an explosion of research on positive interventions—relatively simple intentional activities aimed at cultivating positive feelings, behavior, and cognition to increase well-being (see Quoidbach, Mikolajczak, & Gross, 2015, for a recent review). The development of such interventions is important because happiness has been shown not only to be an important goal in itself but also to be correlated with positive outcomes including productivity at work, prosocial behavior, engagement in social activities, high immune functioning, and the ability to effectively cope with distress (see Layous, Chancellor, & Lyubomirsky, 2014; Lyubomirsky, King, & Diener, 2005, for reviews). Whereas many positive interventions have been demonstrated to be effective (see Bolier et al., 2013; Sin & Lyubomirsky, 2009, for two independent meta-analyses), little is known about their underlying mechanisms. In particular, research has yet to clearly identify what drives the success of one of the most popular positive interventions, the best-possible-self (BPS) intervention (see Loveday, Lovell, & Jones, 2016, for a recent review). Understanding the mechanism behind the BPS intervention is important because it may help increase its effectiveness (Lyubomirsky & Layous, 2013). We propose that the BPS intervention increases positive affect by reducing goal ambivalence (based on King, 2001) and by building positive future expectations (based on Quoidbach et al., 2015).

3.1 Goal Ambivalence and Positive Future Expectations as Mediators of Change in Positive Affect

A large body of evidence obtained from diverse samples including students, adults, and distressed individuals, has shown that the BPS intervention increases positive affect (e.g., Harrist, Carlozzi, McGovern, & Harrist, 2007; Huffman et al., 2014; King, 2001; Sheldon & Lyubomirsky, 2006) and positive future expectations (see Malouff & Schutte, 2016, for a meta-analysis). However, all in all, the magnitude of the effects has been small to medium and has been found to decrease over time, which may be resolved by identifying change mechanisms and developing the intervention further (Bolier et al., 2013). Previous studies have named positive emotions, positive thoughts, positive behaviors, and need satisfaction as potential mediators of positive psychology interventions (Lyubomirsky & Layous, 2013). Regarding the BPS intervention, past research has focused on cognitive effect mechanisms (see Loveday et al., 2016, for a discussion), but various theoretical perspectives have suggested different ones, and the current empirical evidence has been inconclusive.

Based on King's (2001) ideas, we propose that the BPS intervention increases positive affect by reducing goal ambivalence. Goal ambivalence has been conceptualized as an approach-avoidance conflict

in which a person simultaneously hopes for and fears the attainment of a personal goal (Emmons, 1986). Struggling toward a goal that is both desirable and undesirable generally hampers positive emotions, whereas a reduction in such struggles should undo this effect (see Harreveld, Pligt, & Liver, 2009, for a discussion). For example, your hopes of becoming a math professor may conflict with your dream of having a career as a clinical psychologist (Cross & Hayel, 1991). But after you decide to study mathematics, you may feel less ambivalent about your vocational future. Indeed, cross-sectional and prospective studies using student samples have indicated that lower goal ambivalence is related to and predicts positive affect (see Kelly, Mansell, & Wood, 2015, for a review).

In this study, we focused on the feelings of distress that arise from the experience of goal ambivalence, and we define goal ambivalence as the coexistence of inconsistent affective, cognitive, and conative reactions to personal goals that lead to the experience of conflict (Priester & Petty, 1996). The BPS intervention may reduce goal ambivalence by encouraging decisions regarding which personal goals to pursue, and this should subsequently facilitate adaptive goal-directed self-regulation (Dunkel, Kelts, & Coon, 2006). Specifically, in the BPS intervention, participants are instructed to write about their best possible selves, which are defined as representations of a person's aspirations for the future (Hazel & Nurius, 1986). During the exercise, participants develop a coherent, meaningful, and positive narrative of their future life in which they have achieved all their goals, and all their dreams have come true (King, 2001). This narrative can be thought of as a higher level goal in which current conflicts are resolved because one has made choices about which personal goals to pursue (Kelly et al., 2015). For example, when you imagine your best possible future, if you picture yourself giving an inspiring lecture on some hot topic in mathematics, this may encourage you to pursue that dream and study mathematics, although this means reducing the likelihood that you will become a clinical psychologist. Congruently, psychotherapy research has shown that considering higher level goals is an effective strategy for reducing ambivalence and for increasing well-being (Carey, Mansell, & Tai, 2015). In addition, one study showed that British students who wrote about their deepest thoughts and feelings relating to their ambivalence were less concerned about ambivalence 3 weeks later (Kelly, Wood, Shearman, Phillips, & Mansell, 2012). Although previous research has suggested that writing about self-regulatory topics helps to reduce goal ambivalence, to our knowledge, this is the first study to examine the effect of the BPS intervention on goal ambivalence.

Based on Quoidbach et al. (2015), we propose that the BPS intervention increases positive affect through the building of positive future expectations, which are referred to as state optimism (e.g., Peters, Vieler, & Lautenbacher, 2015). Positive future expectations generally promote positive affect, whereas negative future expectations have the opposite effect (see Lazarus, 1991; Roseman & Evdokas, 2004, for a discussion and experimental evidence that expectations cause emotions). Specifically, a large body of research has linked positive future expectations to positive affect (see Carver, Scheier, & Segerstrom, 2010, for a review). Building on this, researchers have used the process model of emotion regulation to hypothesize that having positive future expectations is a potent strategy for upregulating positive emotions (Quoidbach et al., 2015). The BPS intervention has been shown to increase positive future expectations, a finding that reflects the perspective that participants engage with abstract representations of their current hopes (Hazel & Nurius, 1986). Specifically, participants are instructed to write about a positive personal target state that should raise positive future expectations (Malouff & Schutte, 2016). It is like mimicking an optimist's mindset and seeing the best in things for a while. According to Quoidbach (2015), building positive future expectations explains why the BPS intervention encourages positive emotions. However, there are very few studies that have directly tested this idea.

3.2 Exploring Additional Outcomes of the Best-Possible-Self Intervention

In addition to effects of the BPS intervention on positive affect and optimism, researchers have reported effects on other positive outcomes. Specifically, results from one study indicated increased life satisfaction (Boehm, Lyubomirsky, & Sheldon, 2011). Gratitude and hope have also been discussed as outcome variables (Loveday et al., 2016). Loveday et al. (2016) explained that the BPS intervention affects gratitude because the focus of the exercise on positive aspects of life may facilitate an appreciation of the things that one has already received. Likewise, it affects hope because the documentation of one's future life plans potentially boosts one's confidence in initiating actions and generating outcomes in order to achieve goals. We included life satisfaction, gratitude, and hope as outcome variables in order to verify the role they play in understanding how the BPS intervention operates. Finally, we included a measure of goal clarity in order to explore whether the BPS intervention affects participants' degree of certainty about their life goals.

3.3 Aims of the Present Study

The overall aim of the present study was to investigate the underlying mechanisms of the BPS intervention. Specifically, we hypothesized:

(1)   (a) Participants in the BPS condition will report a greater decrease in goal ambivalence immediately after the intervention (posttest) compared with participants in the control condition. (b) The intervention effect on positive affect in the week after the intervention (follow-up) will be mediated by the greater decrease in goal ambivalence.

(2)   (a) Participants in the BPS condition will report a greater increase in positive future expectations immediately after the intervention (posttest) compared with participants in the control condition. (b) The intervention effect on positive affect in the week after the intervention (follow-up) will be mediated by the greater increase in positive future expectations.

3.4 Methods

3.4.1 Participants

Participants were recruited from psychology lectures as well as through posts in students' social network groups, offering them course credit for their participation. To determine the size of the sample, we computed an a priori power analysis in which we assumed that the BPS intervention would have a total effect of $d = 0.34$ (Bolier et al., 2013) and that the intervention effect would be 50% mediated, revealing that 150 participants would enable us to detect the expected indirect effect with a power of .80, applying a bias-corrected bootstrap confidence interval (see Fritz & MacKinnon, 2007; Zhang, 2013; Zhang & Wang, 2013, for introductions). We included students who were at least 18 years old. The final sample consisted of 188 undergraduate psychology students, 92 in the BPS condition and 96 in the control condition. Twelve participants of whom 10 were assigned to the control condition left the study before the intervention for unknown reasons. Another five participants, three of them in the control condition, did not provide follow-up measures but were included in the hypothesis tests by applying full information maximum likelihood. Regarding the demographics of the final sample, the mean age of participants was 22.35 ($SD = 5.04$, Range = 18 to 54), 78.72% were women, and 2.13% indicated a gender other than male or female. On average, participants had studied for 2.96 semesters ($SD = 1.42$, Range = 1 to 6). Data were collected in April and May 2017.

3.4.2 Interventions

Two interventions were administered in 17 different groups in the lab by a trained psychologist (average group size = 11, *SD* = 6.25). Participants in the BPS condition were instructed to write about their ideal future for 20 minutes (based on King, 2001), after which they were asked to briefly imagine their ideal future (based on Sheldon & Lyubomirsky, 2006). As a homework assignment, participants were told to write three diary entries about their ideal future focusing on the topics "study and work," "love and partnership," and "leisure and hobbies" (e.g., as used by Boehm et al., 2011; Meevissen, Peters, & Alberts, 2011). Participants in the control condition were instructed to write about their previous day for 20 minutes, after which they were asked to briefly imagine their previous day (e.g., as used by Lyubomirsky & Layous, 2013; Odou & Vella-Brodrick, 2013). As a homework assignment, participants were told to write three diary entries about their previous day. We chose this control condition because the format is comparable to the BPS condition, but the content was past instead of future-oriented and was thus expected to be emotionally neutral on average.

All participants were informed that they would not be asked to share their notes with anyone and that their diary entries would not be read. Regarding the homework assignment, participants were asked to spend 20 minutes on each diary entry and to write no more than one diary entry per day. All instructions were provided in German. See the Appendix for the complete instructions and a translation.

2.4.3 Procedure

Prior to the intervention, participants were given one of 17 possible dates for their intervention session. Afterwards, they completed an online questionnaire that focused on goal ambivalence, future expectations, subjective well-being as well as their age and gender in that order (pretest). Participants were informed that the purpose of the study was to examine the effects of an intervention for addressing the topic of life goals. They were also informed about the voluntary nature of their participation and data protection. Depending on the date they selected for the intervention session, clusters of participants were randomly assigned to either the BPS intervention or the control group. After implementation, participants were informed about their homework assignment and completed a paper-pencil questionnaire on goal ambivalence, future expectations, and subjective well-being (posttest). Three days later, we sent participants an email reminding them to complete their assignments. Another 4 days after that, they received an email inviting them to take another online questionnaire that contained questions about goal ambivalence, future expectations, and subjective well-being. In addition, participants were asked whether

they liked the intervention, whether they believed they benefitted from the intervention, how easily and vivid they could imagine the situations they wrote about, and how often they completed their homework. The ethics committee of the Department of Education and Psychology of the Freie Universität Berlin approved the study (No 145/2017).

3.4.4 Measures

Affect. We assessed affect using the German version of the Positive and Negative Affect Schedule (PANAS; Krohne, Egloff, Kohlmann, & Tausch, 1996; Watson & Clark, 1988). The scale includes 10 items referring to positive affect (e.g., "interested") and 10 items referring to negative affect (e.g., "distressed"). Participants were asked how they felt "in general" at pretest, how they felt "at the moment" at posttest, and how they felt "during the last week" at follow-up. Items ranged from 1 (*not at all*) to 5 (*extremely*). McDonald's omegas (computed with the R package MBESS; Kelley, 2007; McDonald, 1991) for positive affect were .86 (95% CI [0.82, 0.90]) at pretest, .89 (95% CI [0.86, 0.91]) at posttest, and .89 (95% CI [0.86, 0.91]) at follow-up. For negative affect, the omega values were .85 (95% CI [0.81, 0.88]), .83 (95% CI [0.78, 0.87]), and .88 (95% CI [0.84, 0.92]), respectively.

Life satisfaction. We assessed life satisfaction using the German version of the Satisfaction with Life Scale (SWLS; Diener, Emmons, Larsen, & Griffin, 1985; Glaesmer, Grande, Braehler, & Roth, 2011). The scale includes five items referring to general life satisfaction (e.g., "I am satisfied with my life") with a response scale ranging from 1 (*strongly disagree*) to 5 (*strongly agree*). Because the original English scale ranges from 1 to 7, we transformed the scale prior to our analyses to match the original scaling and to ensure comparability with other studies. McDonald's omegas for life satisfaction were .80 (95% CI [0.74, 0.85]) at pretest, .78 (95% CI [0.73, 0.83]) at posttest, and .81 (95% CI [0.77, 0.86]) at follow-up.

Future expectations. We assessed future expectations using the Future Expectations Scale (FEX; Peters et al., 2015), which was based on the Subjective Probability Task (SPT; MacLeod, 1996). The scale includes 10 items referring to positive future expectations (e.g. "You will get a lot of satisfaction out of life") and 10 items referring to negative future expectations (e.g., "You will have health problems"). It ranges from 1 (*not likely at all*) to 7 (*extremely likely*). McDonald's omegas for positive future expectations were .84 (95% CI [0.79, 0.89]) at pretest, .87 (95% CI [0.82, 0.91]) at posttest, and .87 (95% CI [0.83, 0.90]) at follow-up. For negative future expectations, the omega values were .84 (95% CI [0.80, 0.88]), .81 (95% CI [0.77, 0.86]), and .85 (95% CI [0.81, 0.89]), respectively.

Goal ambivalence. We assessed goal ambivalence using the Goal Ambivalence Scale (Koletzko, Herrmann, & Brandstatter, 2015). The scale includes eight items referring to conflicting affective (e.g., "When I think about my life goals, I have mixed feelings"), cognitive ("… my thoughts are both positive and negative"), and conative ("… I am torn") reactions to life goals as well as affective-cognitive inconsistency ("… my feelings contrast with my convictions"). It ranges from 1 (*not at all*) to 7 (*completely*). McDonald's omegas for goal ambivalence were .89 (95% CI [0.87, 0.92]) at pretest, .89 (95% CI [0.87, 0.92]) at posttest, and .92 (95% CI [0.90, 0.93]) at follow-up.

Goal clarity. We assessed goal clarity using an adapted subscale borrowed from the Landgauer Working Style Questionnaire (LFA; Braun, 2000). Participants were asked to list five current life goals and answer eight items referring to the degree of clarity regarding their life goals (e.g., "With regard to my life, I know exactly what I want"). Because listing one's life goals would be likely to impact the other outcomes used in this study (e.g., goal ambivalence), we assessed goal clarity only at follow-up, and the scale was presented at the end of the questionnaire. The scale ranges from 1 (*not at all*) to 5 (*completely*). McDonald's omega for goal clarity was .85 (95% CI [0.81, 0.88]).

Gratitude. We assessed gratitude using the Gratitude Questionnaire-6 (GQ-6; McCullough, Emmons, & Tsang, 2002), which includes six items referring to individual differences in grateful affect (e.g., "I am grateful to a wide variety of people"). The German version was derived through a translation and back translation process (Guillemin, Bombardier, & Beaton, 1993). The scale ranges from 1 (*strongly disagree*) to 7 (*strongly agree*). McDonald's omegas for gratitude were .70 (95% CI [0.63, 0.77]) at pretest, .66 (95% CI [0.58, 0.75]) at posttest, and .67 (95% CI [0.58, 0.75]) at follow-up.

Hope. We assessed hope using the State Hope Scale (SHS; Snyder et al., 1996). The German version was derived through a translation and back translation process (Guillemin et al., 1993). The scale includes three items referring to agentic thinking (or the capacity to initiate and sustain actions; e.g., "I can think of many ways to reach my current goals") and three items referring to pathway thinking (or the capacity to generate outcomes; e.g., "Right now I see myself as being pretty successful"). It ranges from 1 (*strongly disagree*) to 7 (*strongly agree*). McDonald's omegas for agency were .81 (95% CI [0.76, 0.87]) at pretest, .87 (95% CI [0.78, 0.87]) at posttest, and .84 (95% CI [0.80, 0.88]) at follow-up. For pathway thinking, the omega values were .83 (95% CI [0.80, 0.88]), .81 (95% CI [0.75, 0.86]), and .87 (95% CI [0.82, 0.91]), respectively.

Intervention check. Participants were asked whether they liked the exercise, whether they benefitted from the exercise, whether it was easy for them to imagine the situation they wrote about, and whether the situation they wrote about was vivid using a dichotomous response format (based on Blackwell et al., 2013; Proyer, Wellenzohn, Gander, & Ruch, 2015). In addition, participants reported how many of their three homework assignments they completed.

3.4.5 Statistical Analysis

We tested the main hypotheses with two latent cross-lagged panel models because this approach allows researchers to study directional influences between variables over time while controlling for correlations within time-points and autoregressive effects (see Kearney, 2017; Selig & Preacher, 2009, for a deeper discussion). It can also be applied to separate true systematic change from unsystematic change due to measurement error in order to avoid estimated bias in the regression parameters and indirect effects caused by measurement error. In addition, the models allowed us to test both the expected direction of effects (i.e., that goal ambivalence and positive future expectations influence positive affect) and the obvious alternative (i.e., that positive affect influences goal ambivalence and positive future expectations). We used robust maximum likelihood estimators because the variables were not normally distributed. Full information maximum likelihood was applied to account for missing responses. Indirect effects were tested using bias-corrected bootstrap confidence intervals (MacKinnon, Lockwood, & Williams, 2004). For the main analysis, we used MPlus version 8 (Muthén & Muthén, 1998-2017). Preliminary and additional analyses were computed with R version 3.4.0 (R Core Team, 2017).

3.5 Results

3.5.1 Preliminary Analysis

Prior to the main analysis, we conducted a MANOVA to test whether participants in the BPS and daily activities control conditions differed in their baseline scores on goal ambivalence, positive and negative future expectations, positive and negative affect, life satisfaction, gratitude, hope, and age. Results indicated no difference between conditions, Pillai's Trace = 0.06, $F(10,177) = 1.18$, $p = .306$, η^2_{adj} = 0.06 (Grissom & Kim, 2012; Serlin, Carr, & Marascuilo, 1982). However, Pearson's Chi-square test indicated that relatively more men than women participated in the BPS intervention compared with the control condition (BPS: 66 women vs. 23 men; control: 82 women vs. 13 men), $\chi^2(1, N = 184) = 4.32$, p

= .038, ω = 0.15, 95% CI [0.03, 0.27] (Kelley, 2007). Four participants who indicated a gender other than male or female were excluded prior to this analysis because of their small number. Taken together, there were only small differences between the experimental groups.

3.5.2 Main Analysis

The models for the hypothesis test are depicted in Figure 3.1 (Hypothesis 1) and Figure 3.2 (Hypothesis 2). In order to separate unsystematic measurement error from true systematic change, latent state variables were defined for each construct at each measurement occasion. In the two models, there were three observed indicator variables that loaded on a common latent state variable at each measurement occasion. In addition, there was an indicator-specific factor for the second and third indicators to account for indicator-specific effects over time. Because the first indicator served as a reference indicator, only two indicator-specific factors were necessary (see Eid, 2000; Eid, Geiser, Koch, & Heene, 2017; Eid, Lischetzke, Nussbeck, & Trierweiler, 2003, for a deeper discussion). In all models, strong measurement invariance over time was assumed. Each observed indicator variable reflected a parcel that was formed by aggregating three randomly allocated items (as recommended by Matsunaga, 2008). Within each model, two indirect effects were of special interest. One reflected the respective hypothesis and was either the product of the regression weight from predicting goal ambivalence from ambivalence at posttest and the regression weight from predicting positive affect at follow-up from goal ambivalence at posttest (a1*b1; Hypothesis 1b; Figure 3.1) or the product of the corresponding regressions in which goal ambivalence was replaced with positive future expectations (a3*b3; Hypothesis 2b; Figure 3.2). The other reflected the alternative explanation mentioned above (i.e., that positive affect would mediate the effects of the intervention on goal ambivalence and positive future expectations) and was either the product of the regression weight from predicting positive affect at posttest from condition and the regression weight from predicting goal ambivalence at follow-up from positive affect at posttest (a2*b2; Figure 3.1) or the product of the corresponding regressions in which goal ambivalence was replaced with positive future expectations (a4*b4; Figure 3.2). Model fit results indicated an appropriate fit for the model displayed in Figure 3.1, $\chi^2(132, N = 188) = 182.31$, $p = .003$, CFI = .98, RMSEA =
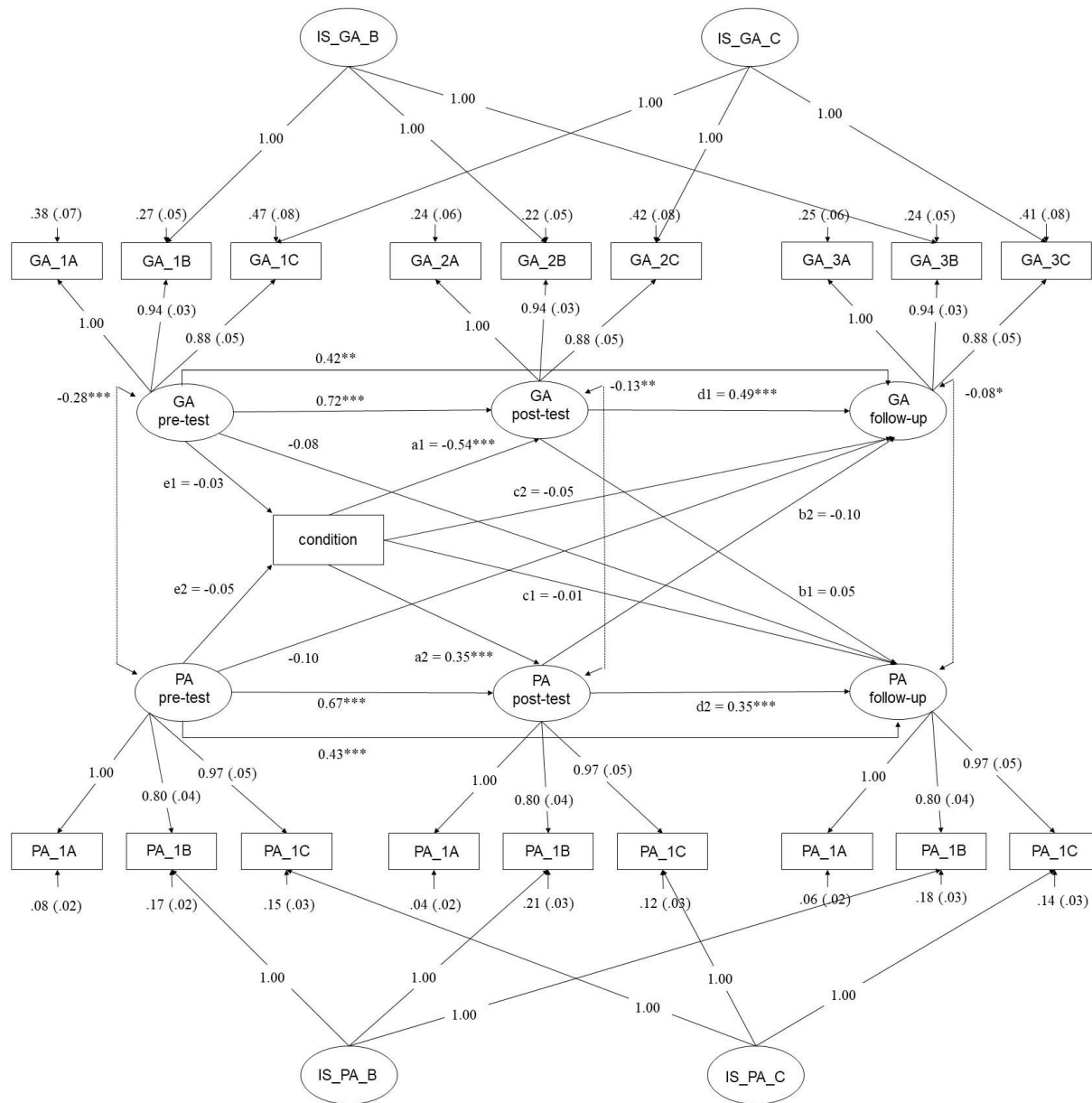
Figure 3.1 Latent cross-lagged panel design model of GA and PA across pretest, immediate posttest, and 1-week follow-up. Unstandardized maximum likelihood parameter estimates. We displayed the corresponding standard errors and confidence intervals in Table 3.2. Condition = best-possible-self intervention versus daily activities control. GA_tA, GA_tB, GA_tC = observed variables (parcels) for goal ambivalence for three occasions of measurement (t = 1,2,3); PA_tA, PA_tB, PA_tC = observed variables (parcels) for positive affect for three occasions of measurement (t = 1,2,3); GA_pretest, GA_posttest, GA_follow-up = common latent state variables for goal ambivalence for three occasions of measurement; PA_pretest, PA_posttest, PA_follow-up = common latent state variables for goal ambivalence for three occasions of measurement; IS_GA_B, IS_GA_C = indicator-specific factors for goal ambivalence; IS_PA_B, IS_PA_C = indicator-specific factors for positive affect.

*p < .05. **p < .01. ***p < .001.
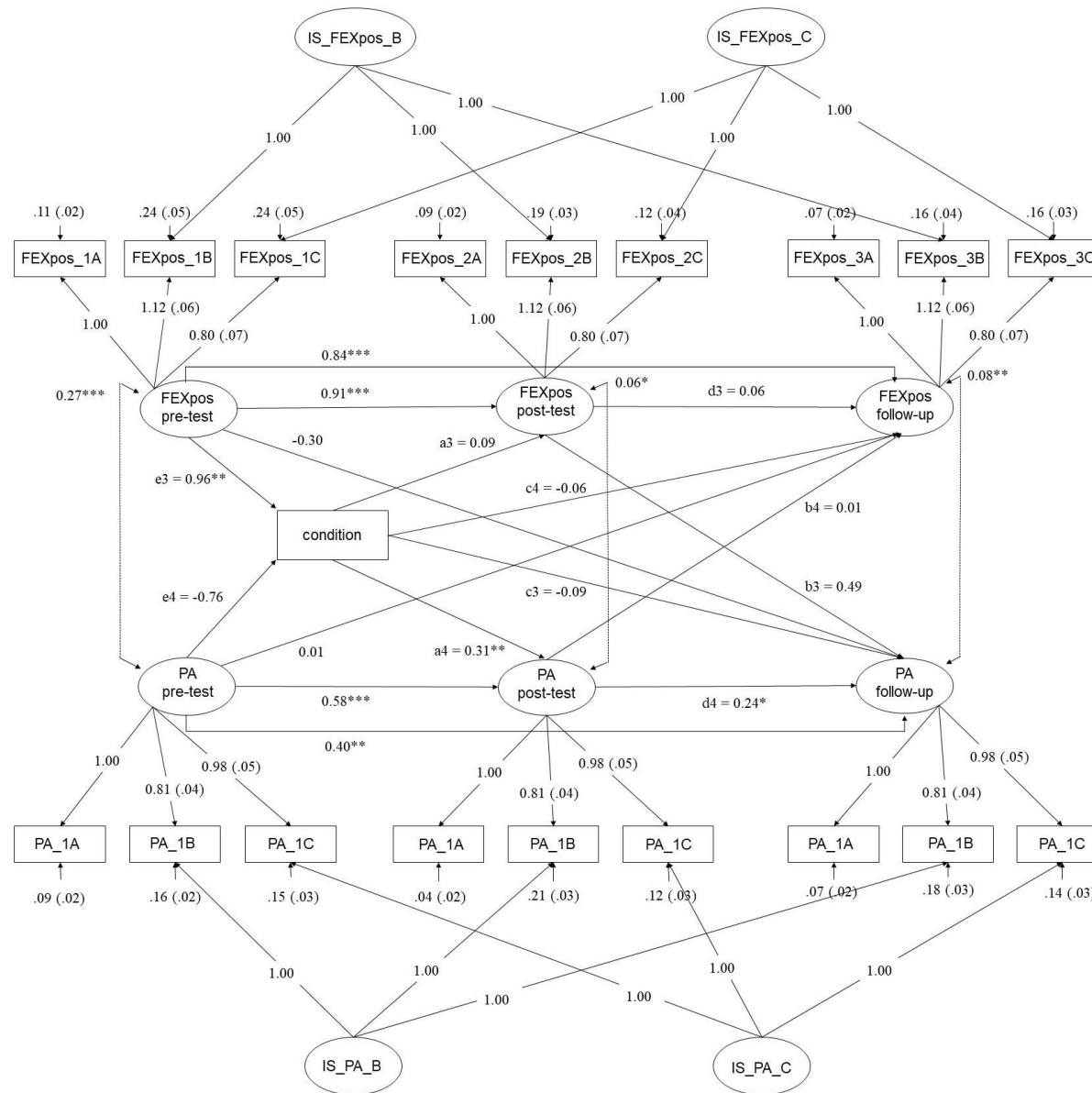
Figure 3.2 Latent cross-lagged panel design model of FEXpos and PA across pretest, immediate posttest, and 1-week follow-up. Unstandardized maximum likelihood parameter estimates. We displayed the corresponding standard errors and confidence intervals in Table 3.2. Condition = best-possible-self intervention versus daily activities control. FEXpos_tA, FEXpos_tB, FEXpos_tC = observed variables (parcels) for positive future expectations for three occasions of measurement (t = 1,2,3); PA_tA, PA_tB, PA_tC = observed variables (parcels) for positive affect for three occasions of measurement (t = 1,2,3); FEXpos_pretest, FEXpos_posttest, FEXpos_follow-up = common latent state variables for positive future expectations for three occasions of measurement; PA_pretest, PA_posttest, PA_follow-up = common latent state variables for goal ambivalence for three occasions of measurement; IS_FEXpos_B, IS_FEXpos_C = indicator-specific factors for positive future expectations; IS_PA_B, IS_PA_C = indicator-specific factors for positive affect.

*p < .05. **p < .01. ***p < .001.

0.05, 95% CI [0.03, 0.06], SRMR = .06, as well as for the model displayed in Figure 3.2, $\chi^2(132, N = 188) = 185.08$, $p = .006$, CFI = .98, RMSEA = 0.05, 95% CI [0.03, 0.06], SRMR = .06 (Hooper, Coughlan, & Mullen, 2008; Hu & Bentler, 1999). As mentioned above, the interventions were administered in 17 different groups. We did not expect effects on the group level because the interventions were implemented individually, and the grouping was for practical purposes only. Congruently, intraclass correlations for positive future expectations, goal ambivalence, and positive affect at posttest and follow-up ranged from 0.01 to 0.04 and could be considered small by conventional standards (Hox, 2002).

3.5.3 Tests of Hypotheses

According to our first hypothesis, we expected that participating in the BPS intervention would reduce goal ambivalence (Hypothesis 1a) and that this reduction would translate into positive affect in the following week (Hypothesis 1b). The results displayed in Table 3.1 and Figure 3.1 supported Hypothesis 1a but not Hypothesis 1b. Regarding Hypothesis 1a, Table 3.1 reveals that average goal ambivalence in the BPS condition fell from 3.37 before the intervention to 3.03 immediately after the intervention and was 3.11 one week later on a scale ranging from 1 to 5. In the control group, the means remained relatively stable at 3.39, 3.51, and 3.43, respectively. In order to evaluate the size of the effect, we calculated the standardized difference in mean changes from pretest to posttest (Becker, 1988; Morris, 2007; Viechtbauer, 2010), which was $d_{tk} = -0.36$, 95% CI [-0.57, -0.15]. Consistently, Figure 3.1 and Table 3.2 show that condition predicted a decrease in goal ambivalence at posttest, B = a1 = -0.54, 95% CI [-0.80, -0.31] and this decrease was still evident at follow-up, B = a1*d1 = (-0.54)*(-0.49) = -0.26, 95% CI [-0.50, -0.10]. As expected and as evident in Table 3.2, positive affect at posttest did not mediate the effect of condition on goal ambivalence at follow-up, B = a2*b2 = (0.35)*(-0.10) = -0.04, 95% CI [-0.14, 0.07]. Regarding Hypothesis 1b, Table 3.1 reveals that average momentary positive affect immediately after the intervention was 3.39 on a scale ranging from 1 to 5 in the BPS condition and 3.07 in the control condition, Cohen's $d = 0.44$, 95% CI [0.14, 0.74], whereas habitual positive affect before the intervention did not differ between conditions, 3.40 versus 3.41, $d = 0.01$, 95% CI [-0.28, 0.30]. Consistently, Figure 3.1 and Table 3.2 show that condition predicted higher positive affect at posttest, B = a1 = .35, 95% CI [0.16, 0.55], and this difference was still evident at follow-up, B = a2*d2 = (0.35)*(0.35) = 0.12, 95% CI [0.04, 0.21]. Further, the results showed that decreased goal ambivalence

Table 3.1 Means, Standard Deviations, and Intercorrelations for PA, NA, LS, GA, Grat, Agency, Path, FEXpos, and FEXneg in the BPS ($n = 87$) and Control ($n = 84$) Conditions at Pretest, Posttest, and Follow-up, as well as for GC at Follow-up

Measure	PA	NA	LS	GA	GC	Grat	Agency	Path	FEXpos	FEXneg	M	SD
					pre-test							
1. PA	-	-.17	.28**	-.46***	-	.31**	.53***	.50***	.56***	-.34***	3.40	0.59
2. NA	-.21*	-	-.47***	.27**	-	-.26*	-.23*	-.26*	-.37***	.47***	1.82	0.58
3. LS	.49***	-.40***	-	-.40***	-	.39***	.44***	.34***	.36***	-.42***	4.98	1.06
4. GA	-.30**	.38***	-.32**	-	-	-.39***	-.47***	-.40***	-.51***	.54***	3.37	1.24
5. GC	-	-	-	-	-	-	-	-	-	-	-	-
6. Grat	.28**	-.27**	.51***	-.13	-	-	.24*	.23*	.31**	-.32**	5.78	0.69
7. Agency	.57***	-.36***	.57***	-.39***	-	.27**	-	.46***	.63***	-.51***	4.30	0.91
8. Path	.46***	-.33**	.42***	-.26*	-	.24*	.52***	-	.47***	-.29**	4.01	0.86
9. FEXpos	.58***	-.39***	.58***	-.41***	-	.30**	.57***	.56***	-	-.60***	5.26	0.70
10. FEXneg	-.48***	.45***	-.64***	.43***	-	-.27**	-.48***	-.50***	-.68***	-	3.02	0.94
M	3.41	1.88	4.94	3.39	-	5.89	4.17	3.94	5.03	3.22		
SD	0.63	0.57	1.12	1.23	-	0.75	0.95	0.89	0.80	0.97		
					post-test							
1. PA	-	-.28**	.38***	-.42***	-	.28**	.41***	.48***	.51***	-.34***	3.39	0.71
2. NA	-.38*	-	-.19	.39***	-	-.20*	-.25*	-.43***	-.37***	.48***	1.40	0.40
3. LS	.32**	-.32**	-	-.35***	-	.44***	.58***	.52***	.46***	-.51***	5.14	0.98
4. GA	-.33**	.42***	-.46***	-	-	-.24*	-.38***	-.38***	-.43***	.46***	3.03	1.15
5. GC	-	-	-	-	-	-	-	-	-	-	-	-
6. Grat	.14	-.22*	.56***	-	-	-	.31**	.29**	.32**	-.36***	5.85	0.72
7. Agency	.45***	-.33**	.60***	-.44***	-	.28*	-	.61***	.64***	-.53***	4.45	0.92
8. Path	.38***	-.37***	.48***	-.45***	-	.34**	.46***	-	.58***	-.53***	4.14	0.91
9. FEXpos	.41***	-.30**	.53***	-.50***	-	.32**	.59***	.56***	-	-.67***	5.40	0.72
10. FEXneg	-.30**	.42***	-.47***	.47***	-	-.22*	-.49***	-.45***	-.67***	-	2.91	0.86
M	3.07	1.57	5.11	3.51	-	5.91	4.29	4.09	5.09	3.19		
SD	0.73	0.56	1.12	1.19	-	0.70	0.94	0.85	0.80	0.92		
					follow-up							
1. PA	-	-.32**	.30**	-.45***	.40***	.23*	.52***	.43***	.56***	-.28**	3.38	0.69
2. NA	-.36***	-	-.34***	.39***	-.46***	-.16	-.33**	-.32**	-.40***	.46***	1.76	0.68
3. LS	.45***	-.33**	-	-.45***	.45***	.23*	.59***	.42***	.42***	-.44***	5.23	1.00
4. GA	-.33***	.35**	-.45***	-	-.67***	-.18	-.40***	-.48***	-.56***	.48***	3.11	1.20
5. GC	.36***	-.36***	.51***	-.69***	-	.24*	.54***	.58***	.64***	-.48***	3.67	0.77
6. Grat	.34***	-.16	.48***	-.16	.15	-	.27**	.24*	.39***	-.33***	5.76	0.82
7. Agency	.52***	-.40***	.53***	-.55***	.54***	.32**	-	.59***	.70***	-.55***	4.44	0.94
8. Path	.45***	-.33***	.40***	-.29**	.23*	.53***	.49***	-	.60***	-.50***	4.14	0.96
9. FEXpos	.53***	-.35***	.44***	-.44***	.37***	.41***	.57***	.63***	-	-.56***	5.38	0.72
10. FEXneg	-.45***	.43***	-.50***	.57***	-.47***	-.31**	-.59***	-.53***	-.62***	-	2.92	0.91
M	3.32	1.96	5.24	3.43	3.58	5.84	4.24	4.03	5.24	3.19		
SD	0.66	0.64	1.09	1.25	0.72	0.82	0.90	0.94	0.80	0.99		

Note. We present Pearson correlations for participants in the BPS condition above the diagonals and Pearson correlations for participants in the control condition below the diagonals. Means and standard deviations for participants in the BPS condition are presented in the vertical columns; means and standard deviations for participants in the control condition are presented in the horizontal rows. PA = positive affect; NA = negative affect; LS = life satisfaction; GA = goal ambivalence; GC = goal clarity; Grat = gratitude; Agency = agentic thinking; Path = pathway thinking; FEXpos = positive future expectations; FEXneg = negative future expectations.

*p < .05. **p < .01. ***p < .001.

Table 3.2 Unstandardized Coefficients, Standard Errors, and Confidence Intervals for the Regressions in the Models Depicted in Figures 3.1 and 3.2

Regression	indication	B	SE B	p	95% CI
Model depicted in Figure 1					
GA t2 on condition	a1	-0.54	0.13	.00	[-0.80, -0.31][a]
PA t2 on condition	a2	0.35	0.10	.00	[0.16, 0.55][a]
PA t3 on GA t2	b1	0.05	0.09	.62	[-0.16, 0.21][a]
GA t3 on PA t2	b2	-0.10	0.14	.46	[-0.36, 0.18][a]
PA t3 on condition	c1	-0.01	0.10	.90	[-0.20, 0.19][a]
GA t3 on condition	c2	-0.05	0.13	.72	[-0.30, 0.20][a]
GA t3 on GA t2	d1	0.49	0.19	.01	[0.18, 0.89][a]
PA t3 on PA t2	d2	0.35	0.09	.00	[0.18, 0.52][a]
condition on GA t1	e1	-0.03	0.15	.83	[-0.33, 0.25][a]
condition on PA t1	e2	-0.05	0.29	.86	[-0.63, 0.51][a]
PA t3 on condition through GA t2	a1*b1	-0.03	0.05	-	[-0.14, 0.07][b]
GA t3 on condition through PA t2	a2*b2	-0.04	0.05	-	[-0.14, 0.07][b]
GA t3 on condition through GA t2	a1*d1	-0.26	0.10	-	[-0.50, -0.10][b]
PA t3 on condition through PA t2	a2*d2	0.12	0.05	-	[0.04, 0.21][b]
Model depicted in Figure 2					
FEXpos t2 on condition	a3	0.09	0.07	.16	[-0.04, 0.22][a]
PA t2 on condition	a4	0.31	0.10	.00	[0.11, 0.50][a]
PA t3 on FEXpos t2	b3	0.49	0.41	.23	[-0.03, 1.41][a]
FEXpos t3 on PA t2	b4	0.01	0.09	.91	[-0.15, 0.18][a]
PA t3 on condition	c3	-0.09	0.09	.33	[-0.28, 0.09][a]
FEXpos t3 on condition	c4	-0.06	0.08	.48	[-0.22, 0.12][a]
FEXpos t3 on FEXpos t2	d3	0.06	0.45	.90	[-0.67, 0.67][a]
PA t3 on PA t2	d4	0.24	0.10	.02	[0.02, 0.43][a]
condition on FEXpos t1	e3	0.96	0.37	.01	[0.34, 1.84][a]
condition on PA t1	e4	-0.76	0.45	.09	[-1.89, -0.02][a]
PA t3 on condition through FEXpos t2	a3*b3	0.05	0.05	-	[-0.03, 0.18][b]
FEXpos t3 on condition through PA t2	a4*b4	0.00	0.03	-	[-0.05, 0.06][b]
FEXpos t3 on condition through FEXpos t2	a3*d3	0.01	0.05	-	[-0.09, 0.10][b]
PA t3 on condition through PA t2	a4*d4	0.07	0.04	-	[0.00, 0.16][b]

Note. Maximum likelihood parameter estimates. B = unstandardized coefficient; FEXpos = positive future expectations; PA = positive affect; GA = goal ambivalence; t1 = pretest; t2 = immediate posttest; t3 = 1-week follow-up; a = maximum likelihood confidence intervals; b = bias-corrected bootstrap confidence intervals.

immediately after the BPS intervention did not mediate the sustained difference in positive affect during the following week. Specifically, the indirect effect of condition on positive affect at follow-up through goal ambivalence at posttest was B = a1*b1 = (0.54)*(0.05) = -0.25, 95% CI [-0.14, 0.07] and, therefore, it was not significantly different from 0.

According to our second hypothesis, we expected that participating in the BPS intervention would increase positive future expectations (Hypothesis 2a) and that this increase would translate into positive affect in the following week (Hypothesis 2b). The results displayed in Table 3.1 and Figure 3.1 did not support Hypothesis 2a or Hypothesis 2b. Regarding Hypothesis 2a, Table 3.1 shows that average positive future expectations in the BPS condition tended to increase from 5.26 before the intervention to 5.40 immediately after the intervention and were 5.38 one week later on a scale ranging from 1 to 7. In the control group, the means were 5.03, 5.09, and 5.24, respectively. The standardized difference in mean changes from pretest to posttest was not statistically significant, d_{tk} = 0.12, 95% CI [-0.03, 0.28]. Consistently, Table 3.2 and Figure 3.2 reveal that condition did not predict increases in positive future expectations at posttest, B = a3 = .094, 95% CI [-0.04, 0.22]. In addition, Table 3.2 shows that positive future expectations at pretest predicted condition assignment, B = e3 = .963, 95% CI [0.34, 1.84], showing that participants with higher baseline scores in positive future expectations were more likely to end up in the BPS condition. Regarding Hypothesis 2b, Figure 3.2 and Table 3.2 reveal that there was no mediation through positive future expectations. Specifically, the indirect effect of condition on positive affect at follow-up through positive future expectations at posttest was B = a3*b3 = (.09)*(.49) = .05, 95% CI [-0.03, 0.18].

3.5.4 Additional Analyses and Moderation Analyses

In accordance with previous research (Boehm et al., 2011), we explored whether the BPS intervention would increase life satisfaction. Table 3.1 shows no effects at posttest, d_{tk} = -0.00, 95% CI [-0.16, 0.15], or follow-up, d_{tk} = 0.05, 95% CI [-0.16, 0.27]. In addition, we found no effects on gratitude, d_{tk} = 0.08, 95% CI [-0.11, 0.27] and d_{tk} = -0.04, 95% CI [-0.20, 0.13], or hope as indicated by pathway thinking, d_{tk} = -0.02, 95% CI [-0.19, 0.16] and d_{tk} = 0.05, 95% CI [-0.16, 0.25], and agentic thinking, d_{tk} = 0.03, 95% CI [-0.13, 0.19] and d_{tk} = 0.07, 95% CI [-0.13, 0.28] (Loveday et al., 2016). However, Table 3.1 reveals a small effect of the BPS intervention on goal clarity at follow-up that did not reach statistical significance, d = 0.12, 95% CI [-0.18, 0.42]. Finally, we investigated factors that potentially influenced the effectiveness of the BPS intervention (e.g., Proyer et al., 2015). First, Pearson's Chi-square

test indicated that there was no significant difference in the number of participants who reported liking the exercise in the BPS condition compared with the control condition, 71 out of 87 versus 63 out of 87, $\chi^2(1, N = 174) = 2.07$, $p = .150$, $\omega = 0.11$, 95% CI [0.00, 0.23]. However, participants in the BPS condition more often reported that they benefitted from the exercise, 64 out of 85 versus 51 out of 87, $\chi^2(1, N = 172) = 5.39$, $p = .020$, $\omega = 0.18$, 95% CI [0.05, 0.30]. Second, participants in the BPS condition did not differ from the control condition regarding how often they reported that it was easy to imagine the content of their writings, 79 out of 88 versus 83 out of 87, $\chi^2(1, N = 175) = 2.02$, $p = .156$, $\omega = 0.11$, 95% CI [0.00, 0.23], and that the imagined content was vivid, 71 out of 85 versus 71 out of 86, $\chi^2(1, N = 171) = 0.03$, $p = .866$, $\omega = 0.01$, 95% CI [0.00, 0.11]. Third, the results indicated a difference in the number of completed homework assignments between conditions, $\chi^2(3, N = 175) = 9.58$, $p = .023$, $\omega = 0.23$, 95% CI [0.06, 0.34]. Specifically, participants in the BPS condition more often reported that they completed none of the assignments compared with participants in the control condition, 15 out of 88 versus 3 out of 87, $\chi^2(1, N = 175) = 8.77$, $p = .003$, $\omega = 0.22$, 95% CI [0.10, 0.35].

3.6 Discussion

The aim of this study was to test two cognitive effect mechanisms that have been proposed to explain the effectiveness of the BPS intervention because such knowledge is indispensable to the further development of the exercise (Lyubomirsky & Layous, 2013).

3.6.1 Theoretical and Practical Implications

As hypothesized, results from this study provide initial evidence that participating in the BPS intervention provides self-regulatory benefits by reducing ambivalence about life goals. Specifically, the BPS intervention may help people make decisions about which goals to pursue (Kelly et al., 2015). This finding supports the notion that writing about one's best possible future "might involve bringing awareness and clarity to one's life goals, [and] might also serve to reduce goal conflict" (King, 2001, p. 800). Congruently, participants in earlier studies reported that they had gained new insights as a result of the exercise (Heekerens & Heinitz, 2019). It seems that writing about one's best possible future self provides new information or at least a novel perspective on aspects of the self, which helps people make decisions about values and commit to some life goals at the expense of pursuing others (see Dunkel et al., 2006, for a discussion). Besides adding to our understanding of how the BPS intervention operates, reducing goal ambivalence seems desirable in and of itself because high levels of ambivalence are by

definition associated with significant feelings of distress (Priester & Petty, 1996) and potentially undermine well-being over time (Kelly et al., 2015). In addition, this study replicates earlier research that showed that the BPS intervention increased positive affect (Bolier et al., 2013). Other than hypothesized, changes in goal ambivalence did not mediate changes in positive affect. This result contradicts earlier studies that found a longitudinal link between reductions in ambivalence and increases in well-being in students (Koletzko et al., 2015). The deviating results may be explained by the different follow-up period. Specifically, Koletzko et al. (2015) examined effects of goal ambivalence on well-being 1 semester later and showed that goal progress played a key role in the mediation effect. This again raises the question of which other factors explain the short-term effectiveness of the BPS intervention (Lyubomirsky & Layous, 2013). In this study, we examined positive future expectations as a second promising mediating variable (Quoidbach et al., 2015). Other than expected, the results did not replicate earlier studies that demonstrated increases in positive future expectations after performing the BPS intervention (e.g., Boselie, Vancleef, & Peters, 2016; Peters et al., 2015). According to our results, positive future expectations do not serve as a mediator of the effect of the BPS intervention on well-being, challenging the prediction that changing cognitions about upcoming events in a positive way is an effective strategy for upregulating positive emotions (Quoidbach et al., 2015). Note that BPS participants reported higher positive future expectations before the intervention, and this could have been an obstacle to finding further increases in comparison with the control condition (see Lyubomirsky & Layous, 2014, for a discussion on how baseline levels of outcome variables may affect the success of positive interventions). Therefore, this result should be interpreted cautiously until more studies are available. Contrary to more recent considerations, the BPS intervention showed no effects on dispositional gratitude or hope (Loveday et al., 2016), and we found no effect on life satisfaction (Boehm et al., 2011).

On a practical level, the BPS intervention could be helpful for resolving the distress that arises from conflicting goals by encouraging value-based decisions about which goals to pursue (e.g., in career and life counseling; Zikic & Franklin, 2010). The exercise should be particularly effective when administered at a time when clients can imagine various possible futures for themselves that may be difficult to reconcile. Previous studies showed that during times in which individuals contemplate changing their behavior, the number of possible future selves they can envision increases, and this is when clients typically seek counseling (Dunkel et al., 2006). Once clients decide which values to pursue, the number of possible future selves and the corresponding ambivalence should decline. We suggest that

counselors who apply the BPS intervention discuss the content of the writing with their clients, support the formulation of attainable goals that relate to the clients' best possible future, and discuss what it takes to make progress toward these goals. Furthermore, the role of a counselor in such situations is often to assist clients in deferring some hoped-for goals for the good of achieving other valued outcomes (Prochaska, DiClemente, & Norcross, 1992). One general advantage of the BPS intervention over existing approaches that are designed to reduce ambivalence (e.g., expressive writing; Kelly et al., 2012) is that the BPS intervention encourages positive emotions, which have been shown to accelerate development (Fredrickson, 2004).

3.6.2 Limitations and Future Research

Whereas our study provides initial evidence that the BPS intervention reduces goal ambivalence, several limitations and perspectives for future research should be mentioned. First, participants with higher pretest scores on positive future expectations had a greater probability of being selected into the BPS condition. Because evidence suggests that positive psychology interventions are generally more effective for individuals with lower pretest scores on outcome variables, our analysis may have underestimated the effect of the intervention on positive future expectations (Bolier et al., 2013). In addition, there were relatively more men in the BPS condition. Because some evidence has shown that men are less motivated than women to engage in positive interventions, the results should be treated with care (Thompson, Peura, & Gayton, 2014). Second, 17 participants, of whom 12 were assigned to the control condition, left the study for unknown reasons. Although no specific pattern could be discerned as to why participants left, we cannot rule out the possibility that drop-out was selective, and the estimates of the intervention effects could be biased (Bell, Kenward, Fairclough, & Horton, 2013). Specifically, it could be the case that relatively more men left the control condition. Third, our self-selected sample comprised German, predominantly female psychology students. Because evidence indicates that the effectiveness of positive psychology interventions varies between populations, the results cannot be generalized to other populations (e.g., adolescents; Bolier et al., 2013). Fourth, we exclusively used self-report measures that rely on conscious assessments. Research has shown that, for example, emotions can be genuinely unconscious, and therefore, our results are limited to conscious aspects of the constructs at hand (see Winkielman & Berridge, 2004, for a review). Finally, the reliability of our gratitude measure was poor, and the corresponding results should be interpreted with care.

Future research should investigate how repeated administrations of the BPS intervention affect goal ambivalence over time. Specifically, it would be interesting to determine whether sustained decreases in goal ambivalence and the resulting lower levels of distress might explain the intervention's effect on depressive symptoms (Shapira & Mongrain, 2010). In addition, researchers have argued that the activation of positive self-relevant information is what actually explains why positive psychology interventions work (Mongrain & Anselmo-Matthews, 2012), and this idea might be put to the test in future studies. All in all, learning about the effect mechanisms underlying the BPS intervention is an important step toward increasing the effectiveness of the intervention, which seems desirable considering its widespread practical application (see Hone, Jarden, & Schofield, 2014, for a review).

3.7 References

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3.8 Appendix

A. Original German Instructions for the Experimental Conditions

<u>Best-possible-self intervention group</u>

Verbal instructions:

Herzlich Willkommen zu meiner Studie zur Selbstreflexion von Lebenszielen und vielen Dank für deine Teilnahme. Nach der Begrüßung werde ich dich bitten, an einer 30 minütigen Übung teilzunehmen und für 20 Minuten einen Fragebogen auszufüllen. Die Übung besteht aus einem schriftlichen Teil für ca. 20 Minuten und einer angeleiteten Visualisierung. Insgesamt werden wir 50 Minuten benötigen. Im Anschluss an den heutigen Termin werde ich dich bitten, in der nächsten Woche eine Hausaufgabe zu bearbeiten. Nach einer Woche schließt die Studie mit einem Onlinefragebogen ab. Ich informiere dich rechtzeitig darüber, wie du deine Versuchspersonenstunden einlösen kannst.

Die Teilnahme an dieser Studie ist freiwillig. Du kannst die Studie zu jedem Zeitpunkt und ohne die Nennung von Gründen abbrechen. Verlasse in diesem Fall bitte leise den Raum. Bitte unterscheibe vor der Übung eine Einverständniserklärung zur Studienteilnahme. Grund hierfür ist eine Auflage der Ethikkommission.

Wir starten nun mit der Übung. Ich möchte dich einladen, über dein bestmöglichstes Zukunfts-Ich zu schreiben. Bitte richte deinen Blick auf das Papier vor dir und schreibe einen Tagebucheintrag aus der Perspektive deines bestmöglichen Zukunfts-Ichs in zehn Jahren. Stelle dir dazu vor, dass ab dem jetzigen Moment alles so gut wie überhaupt möglich verläuft. Du hast hart gearbeitet und alle deine Ziele erreicht. Stelle dir vor, dass alle deine Träume Wirklichkeit geworden sind und versuche, alle Gedanken und Gefühle im Zusammenhang mit dieser Vorstellung zu beschreiben. Du kannst deinen Tagebucheintrag am Ende der Studie mit nach Hause nehmen. Das heißt, niemand außer dir wird deinen Tagebucheintrag lesen, wenn du es nicht anders möchtest. Vor dir auf dem Tisch findest du ein Blatt mit einer Wiederholung der Anleitung. Beginne nun mit deinem Tagebucheintrag und nimm dir dafür 20 Minuten Zeit.

Bitte höre nun auf zu schreiben, unabhängig davon, ob du mit deinem Tagebucheintrag fertig geworden bist. Komm mit deiner Aufmerksamkeit bewusst zurück ins Hier und Jetzt und suche dir mit deinen Augen einen ruhigen Punkt im Raum. Du kannst deine Augen auch schließen, wenn du möchtest. Bitte denke nun an dein bestmögliches Zukunfts-Ich in zehn Jahren und stelle dir deinen bestmöglichen Tag in zehn Jahren für 60 Sekunden bildlich vor.

Komm nun mit deiner Aufmerksamkeit erneut bewusst zurück ins Hier und Jetzt. Falls du deine Augen geschlossen hast, kannst du sie nun wieder öffnen. Vielleicht hast du bisher noch nicht auf diese Art und Weise über dich nachgedacht. Gleichzeitig möchte ich dich einladen, die Übung zu genießen und als Bereicherung zu verstehen. Falls du noch einen Gedanken zu deinem Tagebucheintrag hast, hast du nun kurz Zeit, diesen aufzuschreiben.

Vielen Dank für deine Studienteilnahme bis zu diesem Punkt. Im Anschluss möchte ich dich bitten für 20 Minuten den ausgeteilten Fragebogen auszufüllen.

Um zu untersuchen, welche Effekte diese Übung auf deine Stimmung und deine Lebenszufriedenheit haben, möchte ich dich einladen, deinen Tagebucheintrag in der kommenden Woche zu ergänzen. Schreibe dazu drei Tagebucheinträge zu deinem bestmöglichen Zukunfts-Ich und konzentriere dich jeweils auf die Lebensbereiche Studium und Karriere, Hobbys und Freunde oder Liebe und Partnerschaft. Nutze die Übung dazu, Gedanken aus deinem Tagebucheintrag von heute weiterzuentwickeln oder Neues zu erkunden. Schreibe zu jedem Lebensbereich nur einen vertiefenden Tagebucheintrag. Bitte schreibe pro Tag nur einen vertiefenden Tagebucheintrag. Darüber hinaus entscheidest du, wann du deine Tagebucheinträge schreiben möchtest.

Zum Abschluss der Studie erhältst du ein Blatt mit der Anleitung für deine Hausaufgabe in der kommenden Woche. Die Blätter liegen umgedreht vor euch. Außerdem erhältst du nach einer Woche eine E-Mail zum abschließenden Onlinefragebogen der Studie.

Vielen Dank für deine Teilnahme.

Written instructions:

Schreibe einen Tagebucheintrag aus der Perspektive deines bestmöglichen Zukunfts-Ichs in zehn Jahren. Stelle dir dazu vor, dass ab dem jetzigen Moment alles so gut wie überhaupt möglich verläuft. Du hast hart gearbeitet und alle deine Ziele erreicht. Stelle dir vor, dass alle deine Träume Wirklichkeit geworden sind und versuche, alle Gedanken und Gefühle im Zusammenhang mit dieser Vorstellung zu beschreiben.

Homework assignment:

Bitte schreibe in der nächsten Woche drei weitere Tagebucheinträge. Nutze die Übung dazu, Gedanken aus deinem Tagebucheintrag unserer gemeinsamen Sitzung weiterzuentwickeln oder neue Gedanken aufzunehmen. Bitte schreibe nur einen Tagebucheintrag pro Tag.

Übung 1: Studium und Karriere

Nimm dir mindestens 20 Minuten Zeit und schreibe einen Tagebucheintrag aus der Perspektive deines bestmöglichen Zukunfts-Ichs in zehn Jahren. Stelle dir dazu vor, dass ab dem jetzigen Moment alles so gut wie überhaupt möglich verläuft. Du hast hart gearbeitet und alle deine Ziele erreicht. Stelle dir vor, dass alle deine Träume Wirklichkeit geworden sind und versuche, alle Gedanken und Gefühle im Zusammenhang mit dieser Vorstellung zu beschreiben. Konzentriere dich dabei auf den folgenden Bereich deines Lebens: Studium und Karriere.

Übung 2: Hobbys und Freizeit

Nimm dir mindestens 20 Minuten Zeit und schreibe einen Tagebucheintrag aus der Perspektive deines bestmöglichen Zukunfts-Ichs in zehn Jahren. Stelle dir dazu vor, dass ab dem jetzigen Moment alles so gut wie überhaupt möglich verläuft. Du hast hart gearbeitet und alle deine Ziele erreicht. Stelle dir vor, dass alle deine Träume Wirklichkeit geworden sind und versuche, alle Gedanken und Gefühle im Zusammenhang mit dieser Vorstellung zu beschreiben. Konzentriere dich dabei auf den folgenden Bereich deines Lebens: Hobbys und Freizeit.

Übung 3: Liebe und Partnerschaft

Nimm dir mindestens 20 Minuten Zeit und schreibe einen Tagebucheintrag aus der Perspektive deines bestmöglichen Zukunfts-Ichs in zehn Jahren. Stelle dir dazu vor, dass ab dem jetzigen Moment alles so gut wie überhaupt möglich verläuft. Du hast hart gearbeitet und alle deine Ziele erreicht. Stelle dir vor, dass alle deine Träume Wirklichkeit geworden sind und versuche, alle Gedanken und Gefühle im Zusammenhang mit dieser Vorstellung zu beschreiben. Konzentriere dich dabei auf den folgenden Bereich deines Lebens: Liebe und Partnerschaft.

Daily activities control group

Verbal instructions:

Herzlich Willkommen zu meiner Studie zur Selbstreflexion von Lebenszielen und vielen Dank für deine Teilnahme. Nach der Begrüßung werde ich dich bitten, an einer 30 minütigen Übung teilzunehmen und für 20 Minuten einen Fragebogen auszufüllen. Die Übung besteht aus einem schriftlichen Teil für ca. 20 Minuten und einer angeleiteten Visualisierung. Insgesamt werden wir 50 Minuten benötigen. Im Anschluss an den heutigen Termin werde ich dich bitten, in der nächsten Woche eine Hausaufgabe zu

bearbeiten. Nach einer Woche schließt die Studie mit einem Onlinefragebogen ab. Ich informiere dich rechtzeitig darüber, wie du deine Versuchspersonenstunden einlösen kannst.

Die Teilnahme an dieser Studie ist freiwillig. Du kannst die Studie zu jedem Zeitpunkt und ohne die Nennung von Gründen abbrechen. Verlasse in diesem Fall bitte leise den Raum. Bitte unterscheibe vor der Übung eine Einverständniserklärung zur Studienteilnahme. Grund hierfür ist eine Auflage der Ethikkommission.

Wir starten nun mit der Übung. Ich möchte dich einladen, über deinen gestrigen Tag zu schreiben. Bitte richte deinen Blick auf das Papier vor dir und schreibe einen Tagebucheintrag über deinen gestrigen Tag. Beginne beispielsweise, indem du beschreibst, wie du gestern aufgewacht bist, was du getan hast und welchen Menschen du begegnet bist. Du kannst deinen Tagebucheintrag am Ende der Studie mit nach Hause nehmen. Das heißt, niemand außer dir wird deinen Tagebucheintrag lesen, wenn du es nicht anders möchtest. Vor dir auf dem Tisch findest du ein Blatt mit einer Wiederholung der Anleitung. Beginne nun mit deinem Tagebucheintrag und nimm dir dafür 20 Minuten Zeit.

Bitte höre nun auf zu schreiben, unabhängig davon, ob du mit deinem Tagebucheintrag fertig geworden bist. Komm mit deiner Aufmerksamkeit bewusst zurück ins Hier und Jetzt und suche dir mit deinen Augen einen ruhigen Punkt im Raum. Du kannst deine Augen auch schließen, wenn du möchtest. Bitte denke nun an deinen gestrigen Tag und stelle dir deinen gestrigen Tag in zehn Jahren für 60 Sekunden bildlich vor.

Komm nun mit deiner Aufmerksamkeit erneut bewusst zurück ins Hier und Jetzt. Falls du deine Augen geschlossen hast, kannst du sie nun wieder öffnen. Vielleicht hast du bisher noch nicht auf diese Art und Weise über dich nachgedacht. Gleichzeitig möchte ich dich einladen, die Übung zu genießen und als Bereicherung zu verstehen. Falls du noch einen Gedanken zu deinem Tagebucheintrag hast, hast du nun kurz Zeit, diesen aufzuschreiben.

Vielen Dank für deine Studienteilnahme bis zu diesem Punkt. Im Anschluss möchte ich dich bitten für 20 Minuten den ausgeteilten Fragebogen auszufüllen.

Um zu untersuchen, welche Effekte diese Übung auf deine Stimmung und deine Lebenszufriedenheit haben, möchte ich dich einladen, in der kommenden Woche drei Tagebucheinträge zu deinem gestrigen Tag zu schreiben. Bitte schreibe pro Tag nur einen Tagebucheintrag. Darüber hinaus entscheidest du, wann du deine Tagebucheinträge schreiben möchtest.

Zum Abschluss der Studie erhältst du ein Blatt mit der Anleitung für deine Hausaufgabe in der kommenden Woche. Die Blätter liegen umgedreht vor euch. Außerdem erhältst du nach einer Woche eine E-Mail zum abschließenden Onlinefragebogen der Studie.

Vielen Dank für deine Teilnahme.

Written instructions:

Schreibe einen Tagebucheintrag zu deinem gestrigen Tag. Beschreibe beispielsweise, wie du gestern aufgewacht bist, was du getan hast und welchen Menschen du begegnet bist.

Homework assignment:

Bitte schreibe in der nächsten Woche drei weitere Tagebucheinträge. Nutze die Übung dazu, Gedanken aus deinem Tagebucheintrag unserer gemeinsamen Sitzung weiterzuentwickeln oder neue Gedanken aufzunehmen. Bitte schreibe nur einen Tagebucheintrag pro Tag.

Übung 1: Erster Tagebucheintrag

Nimm dir mindestens 20 Minuten Zeit und schreibe einen Tagebucheintrag zu deinem gestrigen Tag. Beschreibe beispielsweise, wie du gestern aufgewacht bist, was du getan hast und welchen Menschen du begegnet bist.

Übung 2: Zweiter Tagebucheintrag

Nimm dir mindestens 20 Minuten Zeit und schreibe einen Tagebucheintrag zu deinem gestrigen Tag. Beschreibe beispielsweise, wie du gestern aufgewacht bist, was du getan hast und welchen Menschen du begegnet bist.

Übung 3: Dritter Tagebucheintrag

Nimm dir mindestens 20 Minuten Zeit und schreibe einen Tagebucheintrag zu deinem gestrigen Tag. Beschreibe beispielsweise, wie du gestern aufgewacht bist, was du getan hast und welchen Menschen du begegnet bist.

B. Translated English Instructions for the Best-Possible-Self Intervention Condition

Best-possible-self intervention group

Verbal instructions:

Welcome to the self-reflection of life goals workshop and thank you for attending. In the following, you will be asked to take part in a 50-minute session, which is comprised of 20 minutes of writing, a guided visualization, and answering a questionnaire for 20 minutes. After today's session, you will be asked to do a small homework assignment over the course of the next week. Afterwards, the study

ends with an online questionnaire. I am going to inform you, in a timely manner, about how to receive course credit for participation.

Participation is voluntary. You may discontinue participation at any time and without giving reasons for doing so. If you decide to discontinue, please leave the room quietly. Before we begin, please sign the informed consent sheet in front of you, which is a requirement of the ethics committee.

The session starts now. Please write about your best possible future self. Direct your attention towards the blank sheet of paper in front of you and write a diary entry from the perspective of your best possible self in ten years. Imagine that everything has gone as well as it possibly could. You have worked hard and succeeded at accomplishing all of your life goals. Think of this as the realization of all your life dreams. Try to write down all thoughts and feelings related to this picture. For example, start your diary entry by describing how you wake up on your best possible day in ten years, what you do throughout the day, and who do you meet. You may take your diary entry home at the end of the session and no one except you is going to read it if you do not want them to. Now take 20 minutes to complete your diary entry.

Please stop writing now, no matter whether you have completed your diary entry or not. Actively bring your attention back to here and now and let your eyes rest on a still spot in the room. If you want to, you may close your eyes. Please think about your best possible future self and picture your best possible day in ten years for 60 seconds.

Now again actively bring your attention back to here and now. If you have closed your eyes, you may reopen them. Maybe you have never thought about yourself this way. At the same time, I encourage you to enjoy the practice and see it as an enrichment. If you want to write down something, you may take a moment to do so now.

Thank you for your participation until now. Please fill out the questionnaire in front of you, which should take you about 20 minutes.

During the next week, please engage with your best possible future self by writing three more diary entries, which focus on the topics "study and work", "fun and friendship", and "love and partnership". Use this exercise to further develop thoughts that you may have had during the workshop today. Please only write one diary entry per topic. This helps us to understand how the practice affects your mood and life satisfaction. You are free to decide the time and date of your practice.

Finally, there is a sheet summarizing the homework assignment in front of you. Additionally, I am going to send you a reminder to do your assignment via e-mail in one week. After two weeks you will receive an e-mail with a link to the final online questionnaire of this study.

Thank you for attending.

Written instructions:

Write a diary entry from the perspective of your best possible self in ten years. Imagine that everything has gone as well as it possibly could. You have worked hard and succeeded at accomplishing all of your life goals. Think of this as the realization of all your life dreams. Try to write down all thoughts and feelings related to this picture.

Homework assignment:

Please write another three diary entries in the following week. Use this exercise to further develop thoughts that you may have had during the workshop today. Please only write one diary entry per topic.

Assignment 1: study and work

Take at least 20 minutes time and write a diary entry from the perspective of your best possible self in ten years. Imagine that everything has gone as well as it possibly could. You have worked hard and succeeded at accomplishing all of your life goals. Think of this as the realization of all your life dreams. Try to write down all thoughts and feelings related to this picture. Focus on the following topic: study and work.

Assignment 2: fun and friendship

Take at least 20 minutes time and write a diary entry from the perspective of your best possible self in ten years. Imagine that everything has gone as well as it possibly could. You have worked hard and succeeded at accomplishing all of your life goals. Think of this as the realization of all your life dreams. Try to write down all thoughts and feelings related to this picture. Focus on the following topic: fun and friendship.

Assignment 3: love and partnership

Take at least 20 minutes time and write a diary entry from the perspective of your best possible self in ten years. Imagine that everything has gone as well as it possibly could. You have worked hard and succeeded at accomplishing all of your life goals. Think of this as the realization of all your life dreams. Try to write down all thoughts and feelings related to this picture. Focus on the following topic: love and partnership.

Daily activities control group

Verbal instructions:

Welcome to the self-reflection of life goals workshop and thank you for attending. In the following, you will be asked to take part in a 50-minute session, which is comprised of 20 minutes of writing, a guided visualization, and answering a questionnaire for 20 minutes. After today's session, you will be asked to do a small homework assignment over the course of the next week. Afterwards, the study ends with an online questionnaire. I am going to inform you, in a timely manner, about how to receive course credit for participation.

Participation is voluntary. You may discontinue participation at any time and without giving reasons for doing so. If you decide to discontinue, please leave the room quietly. Before we begin, please sign the informed consent sheet in front of you, which is a requirement of the ethics committee.

The session starts now. Please write about your past day. Direct your attention towards the blank sheet of paper in front of you and write a diary about your past day. For example, begin by describing how you woke up yesterday, what you did throughout the day, and who you met. You may take your diary entry home at the end of the session and no one except you is going to read it if you do not want them to. Now take 20 minutes to complete your diary entry.

Please stop writing now, no matter whether you have completed your diary entry or not. Actively bring your attention back to here and now and let your eyes rest on a still spot in the room. If you want to, you may close your eyes. Please think about your past day and picture your past day for 60 seconds.

Now again actively bring your attention back to here and now. If you have closed your eyes, you may reopen them. Maybe you have never thought about yourself this way. At the same time, I encourage you to enjoy the practice and see it as an enrichment. If you want to write down something, you may take a moment to do so now.

Thank you for your participation until now. Please fill out the questionnaire in front of you, which should take you about 20 minutes.

During the next week, please write three more diary entries about your past day. Please only write one diary entry per day. This helps us to understand how the practice affects your mood and life satisfaction. You are free to decide the time and date of your practice.

Finally, there is a sheet summarizing the homework assignment in front of you. Additionally, I am going to send you a reminder to do your assignment via e-mail in one week. After two weeks you will receive an e-mail with a link to the final online questionnaire of this study.

Thank you for attending.

Written instructions:

Schreibe einen Tagebucheintrag zu deinem gestrigen Tag. For example, describe how you woke up yesterday, what you did throughout the day, and who you met.

Homework assignment:

Please write another three diary entries in the following week. Use this exercise to further develop thoughts that you may have had during the workshop today. Please only write one diary entry per topic.

Assignment 1: first diary entry

Take at least 20 minutes time and write a diary entry about your past day. For example, describe how you woke up yesterday, what you did throughout the day, and who you met.

Assignment 2: second diary entry

Take at least 20 minutes time and write a diary entry about your past day. For example, describe how you woke up yesterday, what you did throughout the day, and who you met.

Assignment 3: third diary entry

Take at least 20 minutes time and write a diary entry about your past day. For example, describe how you woke up yesterday, what you did throughout the day, and who you met.

Chapter 4

Deconstructing Positive-Psychological Interventions: Differential Effects of Optimistic, Grateful, and Self-Compassionate Writing on Well-Being

Heekerens, J. B., Eid, M., Heinitz, K., & Merkle, B. (submitted). Deconstructing positive-psychological interventions: Differential effects of optimistic, grateful, and self-compassionate writing on well-being. *Journal of Positive Psychology*.

Abstract

Growing evidence suggests that positive-psychological interventions increase well-being. Little is known, however, about the interventions' unique and shared effects as well as for which group of people optimal results are achieved. We propose that positive-psychological interventions simultaneously affect specific and common outcomes, and that effects depend on person-activity fit. To investigate this issue, we randomized 432 German adults to perform either optimism, gratitude, self-compassion, or control writing interventions in an online setting. Participants reported emotional self-awareness and trait gratitude before, and positive affect, optimism, gratitude, self-compassion, and current thoughts immediately after the intervention. Results indicate higher momentary optimism after the best-possible-self intervention and higher momentary gratitude after the gratitude letter exercise even after controlling for positive affect. Both interventions increased the number of positive self-relevant thoughts. The self-compassion condition showed no effects. Neither emotional self-awareness nor trait gratitude moderated the intervention effects. Future studies should investigate the role of positive self-relevant thinking as a common mechanism in positive-psychological interventions.

Keywords: positive psychology intervention, well-being, optimism, gratitude, self-compassion, positive thinking, mechanism of action, person-activity fit

Growing evidence suggests that positive-psychological interventions, which are intentional activities designed to cultivate positive feelings, behavior, and cognition (Sin & Lyubomirsky, 2009), effectively increasing well-being (see Bolier et al., 2013; Sin & Lyubomirsky, 2009, for two independent meta-analyses). Little is known, however, about the unique and shared effects of such interventions and for whom they work best. Filling this knowledge gap is important because it may help to develop more potent positive-psychological interventions (Lyubomirsky & Layous, 2013).

In this study, we focus on three popular positive-psychological interventions: The best-possible-self (BPS) intervention (King, 2001), which has been shown to increase positive affect and optimism (see Loveday, Lovell, & Jones, 2016; Malouff & Schutte, 2016, for a review and meta-analysis), the gratitude letter exercise (Seligman, Steen, Park, & Peterson, 2005), which has been shown to increase positive affect and gratitude (see Davis et al., 2016, for a meta-analysis), and self-compassionate writing (Shapira & Mongrain, 2010), which has been shown to increase positive affect and self-compassion (see Kirby, 2017; Kirby, Tellegen, & Steindl, 2017, for a review and meta-analysis). Some studies show that the effects of the BPS intervention, the gratitude letter exercise, and self-compassionate writing can last up to one month and longer (Boehm, Lyubomirsky, & Sheldon, 2011; Seligman et al., 2005; Shapira & Mongrain, 2010). However, all in all the magnitude of the effects of positive-psychological interventions is small to medium and decreases over time, which may be resolved by further developing the interventions (Bolier et al., 2013).

4.1 Common and Specific Intervention Effects

Prominent representatives of the field of positive psychology suggested that positive-psychological interventions operate through "powerful specific ingredients in the exercises" (Seligman et al., 2005, p. 418). The basic idea here is that positive-psychological interventions activate targeted positive emotions, behaviors, and thoughts, which in turn increase well-being (Lyubomirsky & Layous, 2013). Specifically, it has been proposed that the BPS intervention builds positive future expectations, the gratitude letter allows to adopt a grateful outlook, and self-compassionate writing induces a mindful awareness that allows to overcome negative thoughts and feelings involved in personal suffering (see Gross, 1998; Quoidbach, Mikolajczak, & Gross, 2015, for detailed conceptual frameworks). As prior discussed, there is some evidence in support of this notion (Davis et al., 2016; Kirby et al., 2017; Malouff & Schutte, 2016). However, two studies show that gratitude and self-compassion interventions not only promote gratitude and self-compassion, but also optimism (Huffman et al., 2014; Smeets, Neff, Alberts,

& Peters, 2014). Thus, the specificity of the effects of positive-psychological interventions remains controversial. In addition, some researchers have emphasized common over specific effect mechanisms. Specifically, the activation of positive self-relevant thinking has been proposed as a mechanism of positive-psychological interventions (Mongrain & Anselmo-Matthews, 2012). This idea, however, has attracted little research attention, probably because major theoretical frameworks on the effects of positive-psychological interventions currently do not include positive self-relevant thoughts as a common mechanism (Lyubomirsky & Layous, 2013). As a result, the size of the effect of positive-psychological interventions on positive self-relevant thinking has not yet been quantified, and it remains controversial as to whether having such thoughts is beneficial or not (see Killam & Kim, 2014, for a discussion). Despite the question whether raising positive self-relevant thinking is desirable, it seems valuable to estimate the effect of positive-psychological interventions on such thinking in order to inform further development of theories and interventions.

4.2 Differential Intervention Effects

Another line of research has addressed the question for which groups of people positive-psychological interventions show optimal effects (see Fritz & Lyubomirsky, 2018, for a recent review). Specifically, current conceptual frameworks suggest that the effectiveness of positive-psychological interventions depends on the degree of fit between features of the activity and characteristics of the participants (Schueller, 2011; Lyubomirsky & Layous, 2013). The basic idea is that a good fit intensifies intervention effects on well-being, whereas a poor fit may lead to no or detrimental effects. In this study, we focus on emotional self-awareness and trait gratitude as moderators because current theory and preliminary evidence indicate that these variables may play a role in differentiating intervention effects between participants.

Emotional self-awareness describes how frequently individuals generally pay attention to their own emotions (see Eid, Lischetzke, Nussbeck, & Trierweiler, 2003; Lischetzke, Eid, & Diener, 2012; Swinkels & Giuliano, 1995, for a deeper discussion). The self-compassion intervention should yield better results for individuals high in emotional self-awareness because the intervention offers an opportunity to explore and alleviate negative emotions, which reconciles with the needs of highly emotionally self-aware individuals (Austenfeld & Stanton, 2004). On the contrary, the BPS intervention should be particularly helpful for individuals low in emotional self-awareness because they prefer not to approach strong emotions and writing about goals provides self-regulatory benefits without an

exploration of unpleasant emotions (Heekerens, Eid, Heinitz, 2019; King, 2001). Congruently, results from two randomized controlled studies show that students who reported more active attempts to acknowledge their emotions reported larger reductions in depressive symptoms and hostility after writing about negative emotions, compared with writing about their best possible future (Austenfeld, Paolo, & Stanton, 2006; Austenfeld & Stanton, 2008). The moderation effect, however, has not yet been investigated using outcomes that are more characteristic of the BPS intervention and more closely relate to well-being such as positive affect.

Trait gratitude describes how frequently, intensely, and deeply individuals generally experience grateful affect (McCullough, Emmons, & Tsang, 2002). The gratitude intervention should be more effective for individuals high in trait gratitude because expressing gratitude should feel natural to them. In line with this, initial evidence suggests that individuals high in trait gratitude expected gratitude interventions to be easier, more socially accepted, and more effective (Kaczmarek et al., 2015) and that they reported higher increases in positive affect after writing about someone to whom they felt grateful (Watkins, Woodward, Stone, & Kolts, 2003, study 4). However, one study did not find a moderation effect of trait gratitude on happiness and life satisfaction for the gratitude letter exercise (Toepfer, Cichy, & Peters, 2012). Thus, current evidence is mixed and evidence to resolve this issue seems feasible.

4.3 Aims of the Present Study

The aims of the present study were to investigate specific (i.e., unique) and common (i.e., shared) effects of positive-psychological interventions as well as to explore differential effect patterns. Specifically, we hypothesize:

(1) Participants in all positive-psychological intervention conditions report higher positive affect compared with participants in the control condition. Additionally, participants in the optimism condition report higher optimism, in the gratitude condition higher gratitude, and in the self-compassion condition higher self-compassion.

(2) The intervention effects are moderated such that participants low in emotional self-awareness report stronger effects in the optimism condition, participants high in trait gratitude report stronger effects in the gratitude condition, and participants high in emotional self-awareness report stronger effects in the self-compassion condition.

4.4 Method

4.4.1 Participants

Participants were recruited online through the German platform respondi, offering them 5€ for their participation. We included German natives who were at least 18 years old and who passed all our quality checks, including assessments of whether participants read the instructions and questions carefully (see Merkle & Kaczmirek, 2016, for an introduction). In determining the size of the sample, an a priori power analysis was used, assuming that the intervention effects are Cohen's $d = 0.34$ (Bolier et al., 2013). Results reveal that a total of 108 participants per group (432 in total) is required to reach a power of .80 at an alpha level of .05 using independent t-tests (using G*Power version 3.1.9.2; Faul, Erdfelder, Buchner, & Lang, 2009). The final sample comprised 425 adults of whom 106 were assigned to the control condition, 110 to the optimism condition, 105 to the gratitude condition, and 104 to the self-compassion condition. We excluded ten participants, three in the control condition, one in the optimism condition, and five in the self-compassion condition, due to insufficient text quality as indicated by meaningless or defiant input. Text quality was assessed by two independent raters with full agreement. The mean age of participants was 43.26 years ($SD = 12.67$, Range = 18 to 75) and 57.2% were female. The sample comprised 7.8% students and individuals undergoing vocational training, 73.4% employees and freelancers, and 3.5% jobseekers, and 15.3% others, including retirees and housewives. Data were collected in March 2018.

4.4.2 Interventions

Participants were randomly assigned to one of four online interventions that required participants to write a text for 15 minutes. The randomization sequence was computer generated and experimenters had access to the sequence at any time of the study. All participants were informed that their input remains anonymous and were asked not to worry about grammar and spelling. As in previous studies, participants in the optimism condition were instructed to write about their ideal future (based on King, 2001), participants in the gratitude condition wrote a letter about experiences for which they feel grateful towards the person who did the kind act for them (based on Lyubomirsky, Dickerhoof, Boehm, & Sheldon, 2011; Seligman et al., 2005) and participant in the self-compassion condition reflected upon their shortcomings from the perspective of a compassionate other (based on Shapira & Mongrain, 2010). Participants in the control condition were instructed to write about their previous week (based on Layous,

Nelson, & Lyubomirsky, 2013; Odou & Vella-Brodrick, 2013). We chose the control condition because the format is comparable to the positive-psychological interventions, however the content should have been emotionally neutral on average. All instructions were provided in German. See the Appendix for the complete instructions and a translation.

4.4.3 Procedure

Participants accessed our study through a link. On the first page they were informed that the purpose of the study was to examine effects of writing on emotions as well as to the voluntary nature of participation and data protection. On the second page, participants answered questions regarding emotional self-awareness and trait gratitude. Afterwards, participants were randomly assigned to either perform one of the positive-psychological interventions or the control task. We designed the survey such that participants had to spend at least 15 minutes on the writing exercises. After implementation, participants listed 10 current thoughts and rated the self-reference and valence of each thought. Next, participants reported momentary optimism, gratitude, self-compassion, and affective state. They indicated how much they liked the intervention, how much they have benefitted from the intervention, and how difficult the intervention was for them. Finally, participants answered socio-demographic questions, indicated their level of experience with self-help techniques, and whether they have been undergoing or currently undergo psychotherapeutic treatment. The ethics committee of the department of education and psychology at Freie Universität Berlin approved the study (No 177/2018).

4.4.4 Measures

Affect. We assessed affect using the short version A of the Multidimensional Mood Questionnaire (MDBF; Hinz, Daig, Petrowski, & Braehler, 2012; Steyer, Schwenkmezger, Notz, & Eid, 1994). The scale includes each four items referring to positive-negative mood (e.g., "happy"), alert-tired mood (e.g., "rested"), and calm-agitated mood (e.g., "restless"). Participants were asked how they feel "at the moment". The scale is anchored at 1 (*not at all*) and 5 (*very*). McDonald's omega (using the R package MBESS; Kelley, 2007; McDonald, 1991) for positive affect was .93, 95% CI [.92, .94].

Optimism. We assessed optimism using the German version of the Life Orientation Test Revised (LOT-R; Carver, Scheier, & Segerstrom, 2010; Glaesmer, Grande, Braehler, & Roth, 2011). We only included the three items capturing optimism. Participants were asked to respond with regard to the present

moment (e.g., "At the moment, I'm optimistic about my future"). The scale is anchored at 1 (*strongly disagree*) and 5 (*strongly agree*). McDonald's omega for optimism was .88, 95% CI [.85, .91].

Gratitude. We assessed gratitude using three items borrowed from the Gratitude Questionnaire-6 (GQ-6; McCullough et al., 2002). We only used items that make sense when assessing gratitude in the present moment (e.g., "At the moment, I have something in life to be thankful for"). The German item versions were derived by a translation and back translation process (Proyer, 2007). The scale is anchored at 1 (*strongly disagree*) and 7 (*strongly agree*). McDonald's omega for gratitude was .91, 95% CI [.89, .93].

Self-compassion. We assessed self-compassion using 10 items borrowed from the German short version of the Self Compassion Scale (SCS; Hupfeld & Ruffieux, 2011; Raes, Pommier, Neff, & van Gucht, 2011). We reworded the items to reflect current self-compassion and participants were asked to respond with regard to the present moment (e.g., "At the moment, I give myself the caring and tenderness I need"; Breines & Chen, 2012). Prior to the analysis, we excluded the item "I can imagine that feelings of inadequacy are shared by most people" from all analyses because the item was negatively correlated with all other items in the scale, demonstrating that the German translation of the item was ambiguous (see Wieland, Durach, Kembro, & Treiblmaier, 2017, for a discussion). The scale is anchored at 1 (*strongly disagree*) and 4 (*strongly agree*). McDonald's omega for self-compassion was .81, 95% CI [.77, .84].

Positive self-relevant thinking. We assessed positive self-relevant thinking using the thought listing technique (Cacioppo, Glass, & Merluzzi, 1979). Participants were asked to list 10 current thoughts and afterwards they were to indicate whether each thought was self-relevant or not and whether each thought was positive, neutral, or negative. The average number of thoughts falling into each category (e.g., positive thoughts) was assessed by adding them up and dividing them by the total number of reported thoughts. Previous studies have established that the data obtained using the thought listing technique meet common psychometric standards (see Cacioppo, Hippel, & Ernst, 1997; Glass & Arnkoff, 1994, for reviews). For example, the number of negative thoughts has been shown to relate to lower self-evaluations, providing evidence for criterion-related validity (Cacioppo, Glass, & Merluzzi, 1979). Another study found that the responses of participants who rated how comfortable they would feel in a hypothetical situation, were similar whether or not participants completed the measure, indicating that the technique is not reactive (Fichten, Amsel, & Robillard, 1988). However, clinical intervention studies

demonstrated that the number of positive and negative thoughts can be changed through targeted action (Heimberg, 1994).

Emotional self-awareness. We assessed emotional self-awareness using the Attention to Feelings Scale (Lischetzke, Eid, Wittig, & Trierweiler, 2001). The scale includes six items referring to individual differences in attention to one's own feelings (e.g., "I think about my feelings.") and has been originally develop in German. It is anchored at 1 (*almost never*) and 4 (*almost always*). McDonald's omega for emotional self-awareness was .94, 95% CI [.93, .95].

Trait gratitude. We assessed trait gratitude using the Gratitude Questionnaire-6 (GQ-6; McCullough et al., 2002). The scale includes six items referring to individual differences in grateful affect (e.g., "I am grateful to a wide variety of people"). The German version was derived by a translation and back translation process (Proyer, Wellenzohn, Gander, & Ruch, 2015). The scale is anchored at 1 (*strongly disagree*) and 7 (*strongly agree*). McDonald's omega for trait gratitude was .79, 95% CI [.76, .83].

Preference. We asked participants how much they liked the exercise, how much they benefited from the exercise and how difficult the exercise was for them (based on Schueller, 2011). Items were assessed separately to allow for more nuanced interpretations of the results. The scale is anchored at 1 (*strongly disagree*) and 7 (*strongly agree*).

Demographics and quality check items. Participants were asked to indicate gender, age, and current job status. Additionally, we asked how often they use self-help techniques such as books or mobile application on the topic of happiness offering the answer options "never", "sometimes (once or twice a year)", "regularly (once or twice a month)", and "often (once or twice a week)". Afterwards participants were asked whether they currently receive or have been receiving psychotherapeutic treatment. Furthermore, we used two quality check items to make sure that participants have read the instructions carefully (as recommended by Merkle & Kaczmirek, 2016). First, we included an instructional manipulation check and asked participants to respond "blue" to the question "Which color matches your text?" as part of the intervention description. The question and the instructed response "blue" were displayed after the intervention, together with the answer options "red", "green", "yellow", and "purple". Second, we displayed a five-points rating scale anchored at 1 (*not at all*) and 5 (*very*) together with the item "sad" and asked participants "Please choose the answer option 'very' to show that you have read the instructions".

4.4.5 Statistical Analysis

We tested our hypotheses with three multiple group structural equation models (SEM). For the main analysis we used MPLUS version 8.1 (Muthén & Muthén, 1998-2017). We used robust maximum likelihood estimators because variables were not normally distributed. Preliminary and additional analyses were performed using R version 3.4.3 (R Core Team, 2017). For computerized text analysis we used the German version of the Linguistic Inquiry and Word Count (LIWC) program version 2015 (see Hirsh & Peterson, 2009; Pennebaker, 2011; Tausczik & Pennebaker, 2010; Wolf et al., 2008, for a deeper discussion).

4.5 Results

4.5.1 Preliminary Analysis

Prior to the main analysis, we conducted a MANOVA to test whether participants in the positive intervention conditions and the control condition differ regarding emotional self-awareness, trait gratitude, and age. Results indicate no difference between conditions, Pillai's Trace = 0.02, $F(3,421) = 0.76$, $p = .656$, $\eta^2_{adj} = 0.01$ (using Serlin's correction as recommended by Grissom & Kim, 2012; Serlin, Carr, & Marascuilo, 1982). In addition, Pearson's Chi-squared test results suggest no difference regarding gender, $\chi^2(3, N = 425) = 2.63$, $p = .452$, $\omega = 0.08$, 95% CI [0.00, 0.13], experience with self-help, $\chi^2(9, N = 425) = 8.02$, $p = .532$, $\omega = 0.14$, 95% CI [0.00, 0.15], and therapy status, $\chi^2(6, N = 425) = 3.87$, $p = .694$, $\omega = 0.10$, 95% CI [0.00, 0.12] (using the R package MBESS to calculate confidence intervals; Kelley, 2007). Taken together, results indicate no group differences before the interventions.

4.5.2 Main Analysis

We used multiple group analyses to test our hypotheses because this approach allowed us to test the expected group differences in latent means (Hypothesis 1) and differential effects (Hypothesis 2) within one statistical framework. Data were analyzed in two steps. In the first step, we defined three
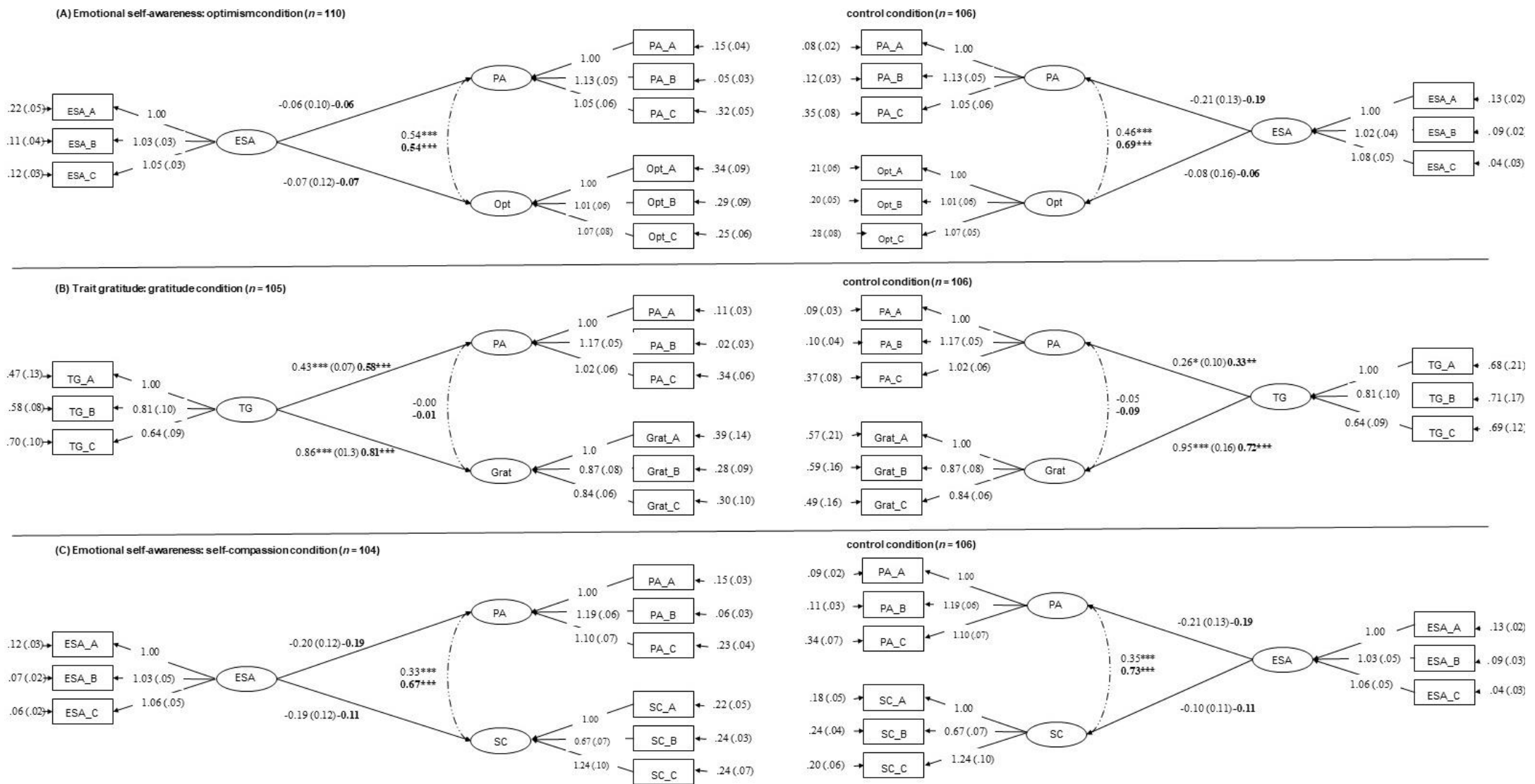
Figure 4.1 Multiple group standard equation models comparing participants in the optimism, gratitude, and self-compassion condition with participants in the control condition. We displayed the models that freely estimate the regression coefficients to provide additional information although none of the differences between intervention and control groups reach statistical significance. Unstandardized parameter estimates with standard errors in brackets and standardized solutions for the latent regression part in bold. PA_A, PA_B, PA_C = observed variables (parcels) for positive affect; Opt_A, Opt_B, Opt_C = observed variables (items) for optimism; Grat_A, Grat_B, Grat_C = observed variables (items) for gratitude; SC_A, SC_B, SC_C = observed variables (parcels) for self-compassion; ESA_A, ESA_B, ESA_C = observed variables (parcels) for emotional self-awareness; TG_A, TG_B, TG_C = observed variables (parcels) for trait gratitude; PA = common latent state variable for positive affect; Grat = common latent state variable for gratitude; Opt = common latent state variable for optimism; SC = common latent state variable for self-compassion; ESA = common latent state variable for emotional self-awareness; TG = common latent state variable for trait gratitude.

*p < .05. **p < .01. ***p < .001.

models with correlated factors to assess model fits either assuming different or equal factor loadings and intercepts across the intervention and control groups. Figure 4.1 displays the measurement invariant solutions and shows that each model contained three observed indicator variables loading on a common latent variable for each construct under investigation. The observed indicator variables for positive affect and self-compassion reflect parcels that were formed by aggregating randomly allocated items (as recommended by Matsunaga, 2008), whereas the indicator variables for optimism and gratitude reflect single items. Results indicate appropriate fit indices for the optimism intervention model, $\chi^2(60, N = 216)$ = 68.24, p = .218, CFI = .99, RMSEA = 0.04, 95% CI [0.00, 0.07], SRMR = .06, the gratitude intervention model, $\chi^2(60, N = 211) = 89.49$, p = .008, CFI = .97, RMSEA = 0.07, 95% CI [0.04, 0.10], SRMR = .08, and the self-compassion intervention model, $\chi^2(60, N = 210) = 67.15$, p = .246, CFI = .99, RMSEA = 0.03, 95% CI [0.00, 0.07], SRMR = .06 (as indicated by CFI > .97, RMSEA < .05, and SRMR < .08; Hooper, Coughlan, & Mullen, 2008; Hu & Bentler, 1999). Importantly, scaled χ^2 difference tests (Satorra & Bentler, 2010) showed that assuming measurement invariance did not significantly worsen model fit for the three models, $\chi^2{}_{diff} = 19.30$, , $df_{diff} = 12$, p = .082, , $\chi^2{}_{diff} = 15.67$, $df_{diff} = 12$, p = .207, and , $\chi^2{}_{diff} = 19.44$, $df_{diff} = 12$, p = .079, respectively (see Cheung & Rensvold, 2002, for a discussion). Second, we regressed our outcome variables on the proposed moderators and freely estimated the regression coefficients (see Figure 4.1). In order to test the proposed multivariate moderation effects (Hypothesis 2) we compared the resulting models with models assuming equal regression coefficients across conditions. The variance-covariance matrices for our main analyses are shown in the Appendix.

4.5.3 Tests of Hypotheses

According to our first hypothesis, we expected that participating in the optimism, gratitude, and self-compassion interventions generally increases positive affect and specifically increases optimism after the optimism intervention, gratitude after the gratitude intervention, and self-compassion after the self-compassion intervention. Regarding the assumed general effect, Table 4.1 reveals that average positive affect measured on a scale ranging from 1 to 5 was 3.48 in the optimism, 3.43 in the gratitude, and 3.30 in the self-compassion condition compared with 3.29 in the control condition. In order to evaluate the effect size, we calculated Cohen's standardized mean differences (using the R package MBESS; Cohen, 1988; Kelley, 2007) that were $d = 0.22$, 95% CI [-0.05, 0.48], $d = 0.16$, 95% CI [-0.12, 0.43], and $d = 0.01$, 95% CI [-0.26, 0.28], respectively. Accordingly, Table 4.2 shows that the latent mean differences between the optimism, gratitude, and self-compassion conditions and the control condition were 0.19,

Table 4.1 Means, Standard Deviations, and Intercorrelations for PA, Opt, Grat, SC, ESA, TG, and Text Analysis Variables in the Control (*n* = 106), Optimism (*n* = 110), Gratitude (*n* = 105), and Self-compassion (*n* = 104) Conditions

Measure	PA	Opt	Grat	SC	ESA	TG	PST	TxPE	TxNE	TxIn	TxCa	M	SD
					Control and optimism condition								
1. PA	-	.50***	.32***	.57***	-.06	.30**	.31**	.13	-.06	.02	.10	3.48	0.88
2. Opt	.64***	-	.42***	.58***	-.07	.31***	.31**	.02	-.19*	-.10	.04	3.92	0.93
3. Grat	.17	.48***	-	.39***	.00	.68***	.08	.09	-.15	-.07	.19*	5.91	1.16
4. SC	.67***	.61***	.23*	-	.01	.32***	.38**	.06	-.07	-.03	.17	3.60	0.70
5. ESA	-.19	-.06	.10	-.11	-	.18	.06	-.07	.03	-.05	.00	2.86	0.72
6. TG	.27**	.48***	.57***	.28**	.31**	-	.12	-.00	-.12	-.18	.23*	5.26	1.04
7. PST	.34**	.38**	.04	.31**	.01	.16*	-	.09	-.03	.04	-.19	3.96	3.04
8. TxPE	.01	.00	.12	-.02	-.00	.12	.09	-	-.07	.27**	-.14	5.53	2.70
9. TxNE	-.28*	-.18	.03	-.24*	-.09	.12	-.04	.32***	-	-.05	.07	1.09	1.09
10. TxIn	.14	.04	.04	.00	.00	.15	.15	.41***	.18	-	-.18	3.07	1.38
11. TxCa	-.15	-.20*	-.27*	.03	-.08	-.19*	-.12	-.02	.06	.03	-	1.21	0.98
M	3.29	3.55	5.58	3.43	2.76	5.08	2.32	2.77	1.01	1.82	1.12		
SD	0.85	0.97	1.23	0.70	0.70	0.93	2.16	1.38	0.95	1.12	0.77		
					Gratitude and self-compassion condition								
1. PA	-	.60***	.28**	.62***	-.18	.21*	.34**	.04	-.22*	.11	.01	3.30	0.84
2. Opt	.63***	-	.51***	.67***	-.13	.52***	.27**	.06	-.18	-.12	.13	3.64	1.07
3. Grat	.39***	.59***	-	.39***	.24*	.52***	.18	.03	-.10	-.04	.07	5.68	1.22
4. SC	.65***	.47***	.32***	-	-.18	.36***	.27**	-.08	-.24*	.07	-.09	3.39	0.72
5. ESA	-.08	.05	.08	-.06	-	.17	-.08	-.22*	.09	-.13	-.08	2.87	0.72
6. TG	.46***	.60***	.69***	.38***	.21*	-	.17	-.08	.02	-.08	-.05	5.34	0.91
7. PST	.16	.24*	.25*	.13	.08	.14	-	.12	-.03	.18	.00	2.20	2.03
8. TxPE	.26**	-.02	.00	.18	.12	.06	.09	-	.22*	.12	-.13	6.31	2.46
9. TxNE	-.11	-.03	.09	-.09	.09	.06	-.07	-.01	-	-.05	-.16	1.65	1.07
10. TxIn	.08	-.13	-.01	.11	-.05	-.16	-.04	.27**	-.14	-	.02	2.45	1.37
11. TxCa	.06	.02	.01	.01	.03	-.05	.08	-.07	.03	.12	-	1.64	1.06
M	3.43	3.71	6.04	3.42	2.76	5.14	3.73	3.13	3.60	3.04	2.08		
SD	0.87	0.93	1.07	0.70	0.77	0.98	2.91	1.86	1.08	1.66	1.31		

Note. The first section shows Pearson correlations for the control condition below the diagonal and for the optimism condition above the diagonal. Means and standard deviations for the control condition are presented in the rows and for the optimism condition in the columns. The second section shows Pearson correlations for the gratitude condition below the diagonal and for the self-compassion condition above the diagonal. Means and standard deviations for the gratitude condition are presented in the rows and for the self-compassion condition in the columns. PA = positive affect; Opt = optimism; Grat = gratitude; SC = self-compassion; PST = positive self-relevant thoughts; ESA = emotional self-awareness; TG = trait gratitude; TxPE = positive emotion words in text; TxNE = negative emotion words in text; TxIn = insight related words in text; TxCa = causal words in text.

*p < .05. **p < .01. ***p < .001.

Table 4.2 Latent Mean Differences, Standard Deviations, and Latent Associations for PA, Opt, Grat, and SC in the Control ($n = 106$), Optimism ($n = 110$), Gratitude ($n = 105$), and Self-compassion ($n = 104$) Conditions

Measure	1. PA	2. Opt, Grat, or SC	3. ESA or TG	M	SD
(A) optimism condition					
1. PA	-	0.36*** (0.08) **0.55***	-0.03 (0.05) **-0.06**	0.19	0.88
				[-0.03, 0.40]	
2. Opt	0.47*** (0.09) **0.69***	-	-0.04 (0.07) **-0.07**	0.36	0.93
				[0.11, 0.61]	
3. ESA	-0.09 (0.06) **0.19**	-0.04 (0.07) **0.06**	-	0.10	0.09
				[-0.08, 0.29]	
(B) gratitude condition					
1. PA	-	0.43*** (0.12) **0.47***	0.50*** (0.14) **0.58***	0.14	0.11
				[-0.07, 0.36]	
2. Grat	0.17 (0.12) **0.18**	-	1.00*** (0.24) **0.82***	0.51	0.19
				[0.14, 0.87]	
3. TG	0.24* (0.10) **0.33**	0.87*** (0.20) **0.72***	-	0.10	0.16
				[-0.22, 0.42]	
(C) compassion condition					
1. PA	-	0.34*** (0.08) **0.68***	-0.09 (0.06) **-0.18**	0.01	0.11
				[-0.20, 0.21]	
2. SC	0.36*** (0.07) **0.73***	-	-0.09 (0.06) **-0.19**	-0.05	0.10
				[-0.24, 0.15]	
3. ESA	-0.09 (0.06) **-0.19**	-0.05 (0.05) **-0.11**	-	0.12	0.10
				[-0.07, 0.30]	

Note. Within each section we displayed the latent covariances and correlations for the control condition below the diagonal and for the intervention condition above the diagonal. Parameter estimates with standard errors in brackets and standardized solutions in bold. The latent means in the control condition were set to zero. Latent mean differences and standard deviations in the intervention condition are presented in the columns along with the corresponding confidence intervals. All models assume measurement invariance across conditions. PA = positive affect; Opt = optimism; Grat = gratitude; SC = self-compassion; ESA = emotional self-awareness; TG = trait gratitude.

*p < .05. **p < .01. ***p < .001.

95% CI [-0.03, 0.40], 0.14, 95% CI [-0.07, 0.36], and 0.01, 95% CI [-0.20, 0.21], respectively. None of the mean differences was significantly different from 0. Regarding the assumed specific effects, Table 4.1 reveals that average optimism was significantly higher in the optimism compared with the control condition, 3.92 vs. 3.55 on a scale ranging from 1 to 5, $d = 0.38$, 95% CI [0.11, 0.65] and average gratitude was significantly higher in the gratitude condition, 6.04 vs. 5.58 on a scale ranging from 1 to 7, $d = 0.40$, 95% CI [0.12, 0.67]. There was no significant difference in average self-compassion in the self-compassion condition, 3.39 vs. 3.43, $d = -0.05$, 95% CI [-0.32, 0.22]. Accordingly, Table 4.2 shows that the latent mean differences between the optimism, gratitude, and self-compassion conditions and the control condition were 0.36, 95% CI [0.11, 0.61], 0.51, 95% CI [0.14, 0.87], and -0.05, 95% CI [-0.24, 0.15], respectively.

According to our second hypothesis, we expected that the effects of the optimism and self-compassion interventions depend on pretest emotional self-awareness as well as that the effects of the gratitude condition depend on pretest trait gratitude. To test our hypothesis, we compared two multiple group models against each other. The first model is depicted in Figure 4.1. It freely estimates the regressions between the proposed moderators and the outcome variables in the intervention and the control conditions. According to our hypothesis, the size of the negative regressions in the optimism condition should be larger than in the control condition because we expected individuals low in emotional self-awareness to benefit more. Model A in Figure 4.1 and Table 4.2 reveal that the size of the negative regression for positive affect was *smaller* in the optimism compared with the control condition, B = -0.06 vs. B = -0.21. For optimism, the coefficients were B = -0.07 vs. B = -0.08. To test whether these differences were statistically significant, we calculated a second model under the assumption of equal regression coefficients across groups. If the second, more restrictive model does not yield a significantly worse model fit, we would assume no moderation effect. Indeed, a scaled χ^2 difference test showed that the fit of the second model was not significantly worse than the fit of the first model, $\chi^2{}_{diff} = 1.00$, $df_{diff} = 2, p = .606$. We repeated the steps to test the expected moderation effects in the gratitude and self-compassion conditions. According to our hypothesis, the size of the positive regressions in the gratitude condition should be larger than in the control condition because we expected individuals high in trait gratitude to benefit more. Model B in Figure 4.1and Table 4.2 reveal that the coefficients for positive affect were B = 0.43 vs. B = 0.26 and for gratitude B = 0.89 vs. 0.95. The differences were not statistically significant, $\chi^2{}_{diff} = 2.59$, $df_{diff} = 2, p = .274$. Finally, we assumed that the size of the

negative regressions in the self-compassion condition should be smaller than in the control condition because we expected individuals high in emotional self-awareness to benefit more. Model C in Figure 4.1 and Table 4.2 reveal that the coefficients for positive affect were B = -0.20 vs. B = -0.21 and for gratitude B = -0.19 vs. -0.10. Again, the differences were not statistically significant, χ^2_{diff} = 3.07, df_{diff} = 2, p = .215.

4.5.4 Additional and Text Analyses

Following previous studies, we looked at variables that potentially influence the effectiveness of positive-psychological interventions (Proyer, Wellenzohn, Gander, & Ruch, 2015; Schueller, 2011)[2]. Results from additional analysis first show that participants liked the positive interventions better than the daily activities control. Specifically, the average score for liking on a scale ranging from 1 to 7 was 4.91 in the control condition compared with 5.36 in the optimism condition, d = 0.31, 95% CI [0.04, 0.58], 5.47 in the gratitude condition, d = 0.38, 95% CI [0.11, 0.65], and 5.40 in the self-compassion condition, d = 0.36, 95% CI [0.09, 0.63]. Second, participants in the optimism and gratitude conditions reported that they had benefited more than participants in the control condition, 4.70 vs. 4.25, d = 0.28, 95% CI [0.02, 0.55] and 5.42 vs. 4.25, d = 0.77, 95% CI [0.49, 1.04], respectively. There was no significant difference between the self-compassion and the control condition, 4.58 vs. 4.25, d = 0.22, 95% CI [-0.05, 0.49]. Third, participants perceived the self-compassion intervention as more difficult than the control intervention, 2.83 vs. 2.28, d = 0.34, 95% CI [0.06, 0.61], whereas there were no significant differences between the optimism and the control conditions, 2.53 vs. 2.28, d = 0.15, 95% CI [-0.12, 0.42] or the gratitude and the control conditions, 2.65 vs. 2.28, d = 0.23, 95% CI [-0.04, 0.50]. Finally, participants on average wrote 341 words in the control condition, which exceeded the average word counts in the optimism condition, 245, d = 0.51, 95% CI [0.24, 0.78], the gratitude condition, 247, d = 0.56, 95% CI [0.29, 0.84], and the self-compassion condition, 196, d = 0.96, 95% CI [0.68, 1.25]. The data contained 4 extreme values (optimism condition: 2, gratitude condition: 1, control condition: 1) identified as values

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[2] For reasons of simplicity, we reported standardized mean difference and confidence intervals here because we assumed normal approximations due to our large sample. Sensitivity analyses were conducted for all tests. Specifically, group differences for rating scales were tested using probit regressions, for thought ratings using negative binomial regressions, and for text analyses (percentages) using zero inflated beta regressions. Statements about the significance of group differences do not differ between the results of the more advanced methods and the normal approximations.

that are beyond 3 standard deviations above the third quartile, which corresponds to participants who wrote more than 931 words (mean for all: 257.69, median for all: 224). Statements about the significance of group differences do not differ between the results obtained from the complete data and results obtained after removing outliers, which were 332 vs. 226 words, $d = 0.73$, 95% CI [0.45, 1.00], in the optimism condition, 332 vs. 240 words, $d = 0.63$, 95% CI [0.35, 0.90], in the gratitude condition, and 332 vs. 196 words, $d = 0.98$, 95% CI [0.70, 1.27], in the self-compassion condition (Aguinis, Beaty, Boik, & Pierce, 2005; Dickerhoof, 2007).

In addition, we used text analysis to further investigate emotional and cognitive processing during the writing process (Guastella & Dadds, 2006). Specifically, we looked at participants' texts and determined the percentage of positive emotion words (e.g., love), negative emotion words (e.g., sad), insight words (e.g., notice), and causal words (e.g., because) participants used. Results from text analysis show that participants in the optimism and gratitude conditions used more positive emotion words than participants in the control condition. As Table 4.1 reveals, the average amount of positive emotion words was 2.77% in the control condition compared with 5.53% in the optimism condition, $d = 1.28$, 95% CI [0.99, 1.57] and 6.31% in the gratitude condition, $d = 1.78$, 95% CI [1.46, 2.10]. Participants in the self-compassion condition did not use significantly more positive emotion words than control participants, 3.13% vs. 2.77%, $d = 0.22$, 95% CI [-0.06, 0.49]. In addition, participants in the gratitude and self-compassion conditions used more negative emotion words compared with the control condition, 1.65% vs. 1.01%, $d = 0.63$, 95% CI [0.36, 0.91] and 3.60% vs. 1.01%, $d = 1.80$, 95% CI [1.48, 2.13], respectively. Participants in the optimism condition did not use more negative emotion words, 1.09% vs. 1.01%, $d = 0.08$, 95% CI [-0.18, 0.35]. Notably, as shown in Table 4.1, the use of negative emotions words was negatively associated with positive affect in the control condition, $r = -.28$, $p = .004$, but not in the optimism condition, $r = -.06$, $p = .513$, which suggests that writing about negative emotional states has different implications depending on the received instructions. Finally, participants in the optimism, gratitude, and self-compassion conditions used more insight words compared with control participants, 3.07% vs. 1.82%, $d = 0.99$, 95% CI [0.71, 1.28], 2.45% vs. 1.82%, $d = 0.50$, 95% CI [0.23, 0.78], and 3.04% vs. 1.82%, $d = 0.87$, 95% CI [0.58, 1.15], respectively. Participants in the gratitude and self-compassion conditions also used more causal words, 1.64% vs. 1.12%, $d = 0.56$, 95% CI [0.29, 0.84] and 2.08% vs. 1.12%, $d = 0.90$, 95% CI [0.61, 1.18], respectively. There was no significant difference between the optimism and the control conditions, 1.21% vs. 1.12%, $d = 0.10$, 95% CI [-0.17, 0.37].

Finally, we examined intervention effects on self-reported thoughts after the intervention (as suggested by Mongrain & Anselmo-Matthews, 2012). Results displayed in Table 4.1 reveal that participants in the optimism condition self-rated 3.95% of their thoughts as both positive and self-relevant compared with 2.32% in the control condition $d = 0.62$, 95% CI [0.34, 0.89]. The same pattern occurred in the gratitude condition, 3.73% vs. 2.32%, $d = 0.55$, 95% CI [0.28, 0.83], but not the self-compassion condition, 2.20% vs. 2.32%, $d = -0.06$, 95% CI [-0.33, 0.21]. In line with this, text analysis results show that participants in the optimism and gratitude conditions used more positive emotion words to describe their thoughts compared with control participants, 11.68% vs. 8.41%, $d = 0.62$, 95% CI [0.34, 0.89] and 16.62% vs. 8.41%, $d = 0.55$, 95% CI [0.28, 0.83], respectively. There was no significant difference in the self-compassion condition, 7.81% vs. 8.41%, $d = -0.06$, 95% CI [-0.33, 0.21].

4.6 Discussion

The aim of the present study was to investigate the effects of positive-psychological interventions on specific and common outcomes and to explore differential effect patterns. Such knowledge is fundamental to the effective use and further development of positive-psychological interventions, which seems desirable considering their widespread application in organizations, schools, clinics, and digital formats (see Diefenbach, 2018; Hone, Jarden, & Schofield, 2014, for reviews).

4.6.1 Specific, Common, and Moderation Effects

As expected in our first hypothesis, the BPS intervention increased optimism and the gratitude letter exercise increased gratitude immediately after the activity and even when controlling for positive affect (Davis et al., 2016; Malouff & Schutte, 2016). This finding supports the postulate that positive-psychological interventions specifically impact targeted outcomes (Lyubomirsky & Layous, 2013; Quoidbach et al., 2015; Seligman et al., 2005). Contrary to earlier reports, there was no effect of the gratitude letter on optimism (Huffman et al., 2014). Other than expected, self-compassionate writing showed no beneficial effects, which questions the merit of brief self-compassion interventions (Mantelou & Karakasidou, 2017; Odou & Vella-Brodrick, 2013; Shapira & Mongrain, 2010). One reason for the absent effects may be that it was difficult for participants to properly implement the intervention without the opportunity for instructive feedback. Maybe self-compassion interventions require multiple administrations and guidance by a trained counselor to be effective (see Kirby et al., 2017; Kirby, 2017, for a deeper discussion). In addition, descriptive results show trends towards higher positive affect

following the BPS intervention and the gratitude letter exercise. Effects were as expected, however, they remained below the threshold of statistical significance despite our large sample, which is not uncommon in positive-psychological intervention trials (Bolier et al., 2013). Researchers explained that one reason for small or absent intervention effects can be that individuals respond differently to psychological treatments (Cronbach, 1957; Lyubomirsky & Layous, 2013). Results from this study show that, other than expected in our second hypothesis, participants higher in trait gratitude did not report larger increases in positive affect after writing a gratitude letter, although the descriptive results were in the expected direction, which is in line with earlier studies that administered gratitude interventions in a single session (Kaczmarek et al., 2015; Toepfer et al., 2012; Watkins et al., 2003, study 4). In contrast to this finding, two studies that asked participants to write about things they were particularly grateful for over the course of several weeks found that *lower* trait gratitude reported larger gains in life satisfaction (Rash, Matsuba, & Prkachin, 2011) and positive affect (Harbaugh & Vasey, 2014). It could be that prolonged gratitude interventions counteract the negative link between low trait gratitude and well-being (see Harbaugh & Vasey, 2014; Wood, Froh, & Geraghty, 2010, for a deeper discussion). The possible moderating effect of trait gratitude on the effects of gratitude interventions might then be reverse for brief compared with prolonged interventions, such that individuals high in trait gratitude benefit more from single session interventions, whereas individuals low in trait gratitude benefit more from multiple session interventions. Furthermore, results from the current study indicate that different levels of emotional self-awareness did not affect responses to the BPS intervention. This finding may help to differentiate results from earlies studies showing that individuals low in emotional processing particularly benefitted from the BPS intervention (Austenfeld et al., 2006; Austenfeld & Stanton, 2008). One explanation for the different results is that Austenfeld and colleagues (2006, 2008) used repeated administrations of the BPS intervention and assessed benefits after several weeks. Maybe individuals who generally pay little attention to their own emotions require longer interventions to derive an additional self-regulatory advantage or this effect builds up over time.

4.6.2 Change Mechanisms

We examined participants' writing and asked them to list 10 current thoughts after the completion of the activities with the aim of exploring underlying change mechanisms of positive-psychological interventions. As expected, participants in the BPS intervention and gratitude letter exercise conditions used more positive emotion and insight words than participants in the control condition (Heekerens &

Heinitz, 2019; Owens & Patterson, 2013). It might be that these interventions allow participants to draw connections between their present life and future dreams or reflect their relationships with meaningful others, which may facilitate self-exploration and understanding (King, 2002). Evidence from the expressive writing paradigm suggests that participants who used more positive emotion and insight words while writing about traumatic experiences gained most from the writing sessions (see Pennebaker, 2011, for a review). Building on this, results from the current study suggest that positive-psychological interventions may accomplish the same, providing a vital alternative to reactivating negative experiences to increase psychological health (King, 2001). In addition, participants in the gratitude condition used more negative emotion words in their writings compared with control participants and although participants in the BPS intervention did not show an increased use of negative emotion words, the use of such words was unrelated to positive affect, whereas in the control condition a negative relationship was observed. This finding suggests that some positive-psychological interventions do require participants to confront unpleasant emotions and that positive-psychological interventions might help to facilitate an adaptive integration of negative emotional states, probably through simultaneously experiencing positive emotions (Killam & Kim, 2014). Finally, participants in the BPS intervention and the gratitude letter condition reported more positive self-relevant thoughts immediately after the intervention. This finding supports the notion that positive-psychological interventions generally activate positive self-relevant thinking, which has been suggested as a common effect mechanism (Mongrain & Anselmo-Matthews, 2012).

4.6.3 Limitations and Future Research

Several limitations and proposals for future research should be mentioned. There was only one occasion of measurement after the intervention and our design did not permit conclusions regarding follow-up effects. For example, it remains unclear whether increases in positive self-relevant thinking are maintained throughout the days and weeks after the intervention and how they relate to other well-being related outcomes. Future studies should apply longitudinal designs and test how positive-psychological interventions differentially affect various outcomes over time (Maxwell & Cole, 2007). In addition, the self-reports we used limit our results to conscious aspects of the constructs under investigation. Although there is compelling evidence that the subjective indicators that we used are meaningful (e.g., Oswald & Wu, 2010), future studies should also evaluate positive-psychological interventions based on more objective metrics (e.g., real-time measures; Alexandrova, 2005; Kahneman, 2000). Another issue that

limits the generalizability of our results is that participants' motivation to complete the interventions might primarily stem from the payment they received. Research shows that motivation influences the effectiveness of positive-psychological interventions and effect sizes are likely larger in samples of individuals who actively seek to become happier (Lyubomirsky et al., 2011; Parks, Della Porta, Pierce, Zilca, & Lyubomirsky, 2012). Finally, although results partially support the notion that positive-psychological interventions operate through distinct effect mechanisms, longitudinal mediation studies are needed to allow for robust conclusions (see Fredrickson et al., 2008; Heekerens & Heinitz, 2019; Heekerens, Heinitz, & Eid, 2019, for examples). Such studies should also investigate the role of positive self-relevant thinking (Mongrain & Anselmo-Matthews, 2012) and include text analysis to improve our knowledge of emotional and cognitive processing during the interventions (Pennebaker, 2011).

4.7 References

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4.8 Appendix

A. Original German Instructions for the Intervention and Control Conditions

<u>For all conditions:</u>

Nun möchten wir Sie bitten, für 15 Minuten einen Text zu schreiben. Nach Ablauf der 15 Minuten erscheint die Schaltfläche "Weiter" unter dem Eingabefeld. Machen Sie sich bitte keine Gedanken um Grammatik oder Rechtschreibung. Ihre Eingabe wird in anonymisierter Form verarbeitet und lässt keinen eindeutigen Schluss auf Ihre Person zu. Vorab interessiert uns, ob Sie sich wirklich die Zeit nehmen, die Instruktion aufmerksam zu lesen, weil das an dieser Stelle wichtig ist. Bitte zeigen Sie uns, dass Sie die Instruktion lesen, indem Sie auf die Frage "Welche Farbe passt zu Ihrem Text?" mit "grün" antworten. Vielen Dank für Ihr Verständnis.

<u>Specific for optimism condition:</u>

Denken Sie an Ihr Leben in zehn Jahren. Stellen Sie sich vor, wie Sie in Zukunft leben werden, wenn alles so gut wie überhaupt möglich verlaufen ist. Sie haben hart gearbeitet und alle Ihre Ziele erreicht. Stellen Sie sich vor, dass Sie Ihre Lebensträume verwirklicht und Ihr Potential voll ausgeschöpft haben.

Nehmen Sie sich einen Moment Zeit, darüber nachzudenken, wie Ihr Leben in der Zukunft aussehen würde. Schreiben Sie nun auf, was Sie sich vorstellen. Beschreiben Sie Ihr Leben in der Zukunft in so vielen Details wie möglich. Beginnen Sie Ihren Text mit "In der Zukunft werde ich ...".

<u>Specific for gratitude condition</u>**:**

Erinnern Sie sich zunächst an eine Person, die in der Vergangenheit für Sie bedeutsam war, der Sie Wichtiges zu verdanken haben, und der Sie noch nicht ausreichend gedankt haben (Eltern, Kinder, Lebenspartner/in, Freunde, Lehrer/in, Mentor/in und so weiter). Sie können jede Person wählen, die einen positiven Einfluss auf Ihr Leben genommen hat, und der Sie nie (oder selten) Ihre Dankbarkeit ausgedrückt haben. Vielleicht gibt es einen Freund in Ihrem Leben, der Ihnen in einer schwierigen Lebensphase Halt gegeben hat, oder Ihnen eine neue Perspektive auf Dinge ermöglicht hat, als Sie aufgebracht waren. Nehmen Sie sich einen Moment Zeit, darüber nachzudenken, was diese Person für Sie getan hat, was in Ihnen ein Gefühl von Dankbarkeit auslöst. Schreiben Sie dieser Person nun einen Brief, in dem Sie Ihre Dankbarkeit zum Ausdruck bringen. Beschreiben Sie die Dinge, die diese Person für Sie getan hat, in so vielen Details wie möglich. Stellen Sie heraus, in welcher Weise diese Dinge Ihr Leben beeinflusst haben, was Sie heute tun und wie Sie sich in Dankbarkeit daran erinnern, was diese Person für Sie geleistet hat.

- Beginnen Sie Ihren Brief mit „Liebe/r …". Um Ihre Anonymität zu wahren, können Sie den Namen der Person, die Sie gewählt haben, verschweigen und zum Beispiel „Liebe Freundin" oder „Lieber Professor" schreiben.

- Sprechen Sie die Person in Ihrem Brief direkt an („Du hast ..." bzw. „Sie haben …").

<u>Specific for self-compassion condition</u>**:**

Jeder erlebt Momente, in denen etwas passiert, bei dem er sich unsicher fühlt, sich schämt oder denkt er genüge nicht. Vielleicht haben Sie sich beim Mittagessen über einen Kellner geärgert, der ewig brauchte, um Ihnen die Rechnung zu bringen. Sie haben ihm ein paar unfreundliche Worte gesagt und sind dann aus dem Restaurant gestürmt, ohne ein Trinkgeld zu hinterlassen. Anschließend war Ihnen die Sache peinlich und Sie haben sich dafür geschämt.

Versuchen Sie, an etwas zu denken, was Ihnen unangenehm war, wofür Sie sich verurteilt haben, oder was Ihnen Schmerz bereitet hat.

Schritt 1: Beschreiben Sie kurz neutral die von Ihnen gewählte Situation. Zum Beispiel: Ich saß beim Mittagessen und habe auf die Rechnung gewartet.

Schritt 2: Schreiben Sie nun darüber, wie Sie sich gefühlt haben. Versuchen Sie, die Erfahrung, die Sie in dieser Situation gemacht haben, beim Schreiben zu akzeptieren und nicht zu verurteilen, nicht zu verharmlosen und auch nicht übermäßig zu dramatisieren. Zum Beispiel: Ich war frustriert, weil der Kellner so langsam war. Ich war wütend, habe überreagiert und bin mir anschießend lächerlich vorgekommen. Weitere Gefühle, die in solchen Situationen auftreten können, sind Irritation, Stress, Angst, Trauer, Scham und so weiter.

Schritt 3: Stellen Sie sich jetzt einen liebevollen Freund (oder eine Freundin) vor, der Sie akzeptiert, freundlich und mitfühlend ist. Sie können einen Freund wählen, den es tatsächlich gibt, oder sich einen solchen Freund vorstellen. Schreiben Sie auf, wie dieser Freund das tiefe Mitgefühl ausdrücken würde, das er für Sie empfindet, vor allem im Hinblick auf Ihr Unbehagen in dieser Situation. Zum Beispiel: Ich verstehe, dass du frustriert warst und die Nerven verloren hast. Ich weiß, wie wichtig es dir ist, andere Menschen freundlich zu behandeln und wie schwierig die Situation im Moment für dich ist. Bitte formulieren Sie Ihre Zeilen so, dass Sie Freundlichkeit, Ruhe und Güte für Sie ausstrahlen. Dabei können Sie nichts „richtig" oder „falsch" machen, schreiben Sie einfach was Ihnen in den Sinn kommt.

Schritt 4: Schreiben Sie auf, was dieser Freund sagen würde, um Sie daran zu erinnern, dass Sie „auch nur ein Mensch" sind und dass jeder solche schmerzlichen Erfahrungen macht. Zum Beispiel: Jeder kann mal überreagieren, das ist nur menschlich. Der Freund könnte Sie auch auf verschiedene Ursachen und Umstände hinweisen, die diesem schmerzlichen Ereignis vorausgegangen waren. Zum Beispiel: Dein Frust hat sich dadurch verstärkt, dass du einen dringenden Termin hattest und schon spät dran warst an diesem Tag. Unter anderen Umständen hättest du anders reagiert. Auch hier können Sie nichts „richtig" oder „falsch" machen, schreiben Sie einfach was Ihnen in den Sinn kommt.

<u>Specific for daily activities control condition</u>**:**

Beschreiben Sie, was Sie in der vergangenen Woche getan haben. Es ist wichtig, dass Sie Ihre Aktivitäten in so vielen Details wie möglich beschreiben. Schreiben Sie über möglichst viele unterschiedliche Dinge, die Sie getan haben. Bitte strukturieren Sie Ihren Text anhand der Tage Ihrer vergangenen Woche und beschreiben Sie, was Sie an den entsprechenden Tagen getan haben. Beginnen Sie Ihren Text beispielsweise mit "Am Montag habe ich ...".

B. Translated English Instructions for the Intervention and Control Conditions

<u>For all conditions:</u>

Now we would like you to write a text for the next 15min. After 15min, the button "Continue" appears below the entry field. Don't worry about grammar or spelling. Anything you write will be processed in an anonymous way and doesn't allow any conclusions being drawn to your person. Before, we are interested whether you actually take the time to read the instructions carefully because this is important at this point. Please show us that you read the instructions by answering "green" to the question "Which color fits this text?" Thank you for your understanding.

<u>Specific for optimism condition:</u>

Think about your life in 10 years. Imagine how you will live in the future after everything has gone as well as it possibly could. You have worked hard and accomplished all your goals. Imagine that you realized all your life dreams and your own best potential. Take a moment to think about how your life in the future could look like. Now, write about what you imagined. Describe your life in the future in as much details as possible. Begin your text with "In the future I will…".

<u>Specific for gratitude condition</u>**:**

First, remember a person who has been significant for you in the past, whom you owe important things and whom you haven't properly thanked yet (parents, children, partner, friends, teacher, mentor and so on). You can choose any person who positively affected your life and to whom you never/rarely have expressed your gratitude. Maybe there is a friend in your life who supported you in a difficult life phase or helped give you a new perspective on things when you were upset. Take a moment to think about what the person has done for you that causes a feeling of gratitude in you. Write a letter to this person in which you express your gratitude. Describe the things that person did for you as detailed as possible. Emphasize in what way those things affected your life, your current actions, and how you gratefully remember the efforts of the person.

- Begin your letter with "Dear…". To maintain anonymity, you can keep the name of the person you chose secret and write e.g. "Dear friend" or "Dear professor"

- Address the person directly in the letter ("You have…")

<u>Specific for self-compassion condition</u>**:**

Everyone experiences moments when something happens that makes them feel unsure, ashamed or not good enough. Maybe you were annoyed by a waiter at lunch who took ages to bring the bill. You said

some unfriendly words and rushed out of the restaurant without leaving a tip. Afterwards you felt embarrassed and ashamed. Try to think about something that was unpleasant, for which you judged yourself or that hurt you.

Step 1: Shortly describe the situation you chose in a neutral way. Example: I sat at lunch and waited for the bill.

Step 2: Now describe how you felt. While writing, try to accept and do not judge the experience you made in this situation, and neither play it down nor dramatize it. Example: I was frustrated because the waiter was so slow. I was angry, overreacted and afterwards felt ridiculous. Other feelings you can experience in such situations are irritation, stress, fear, sadness, shame, amongst others.

Step 3: Now imagine a loving, kind and compassionate friend who accepts you. You can choose an existing friend, or you can imagine such a friend. Write down how this friend would express the deep compassion that he/she holds towards you, especially regarding your discomfort in this situation. Example: I understand that you were frustrated und lost your temper. I know how important it is to you to treat other people kindly and how difficult this situation must be for you now. Please phrase your writing in a way that expresses kindness, calmness and mercy. There's nothing you can do "right" or "wrong", just write down what comes to your mind.

Step 4: Write down what this friend would say to remind you that you're just a human and that everyone makes such painful experiences. Example: Everyone can overreact from time to time, that's human. The friend could point out different causes and circumstances to you that had preceded the painful event. Example: Your frustration was amplified because you had an urgent appointment and you were already late that day. Under different circumstances you'd have reacted differently. Here again, there's nothing you can do "right" or "wrong", just write what comes to your mind.

Specific for daily activities control condition:

Describe what you did last week. It's important that you describe your activities in as much detail as possible. Write about as many of your activities as possible. Please structure your text chronologically, starting with the first day of the week and describe what you did on the respective days. Begin your text for example with "On Monday I…".

C. Variance-Covariance Matrices

Table 4.A1 Variances and Covariances for the Observed Variables in the Models depicted in Figure 4.1

Measure	1. PA A	2. PA B	3. PA C	4. SO A	5. SO B	6. SO C	7. Mod A	8. Mod B	9. Mod C
(A) optimism condition									
1. PA_A	0.64 / 0.76	0.73	0.65	0.47	0.34	0.35	-0.08	-0.02	-0.07
2. PA_B	0.64	0.86 / 0.86	0.73	0.49	0.40	0.39	-0.05	-0.01	-0.06
3. PA_C	0.60	0.72	1.03 / 0.98	0.46	0.34	0.37	-0.35	0.04	-0.02
4. Opt_A	0.48	0.54	0.57	0.98 / 1.19	0.79	0.78	-0.09	-0.03	-0.04
5. Opt_B	0.50	0.52	0.57	0.79	1.04 / 1.00	0.70	-0.05	0.01	-0.05
6. Opt_C	0.48	0.50	0.52	0.87	0.93	1.24 / 0.97	-0.11	-0.02	-0.07
7. ESA_A	-0.09	-0.11	-0.15	-0.05	-0.10	-0.11	0.56 / 0.60	0.49	0.48
8. ESA_B	-0.08	-0.10	-0.11	0.02	-0.00	0.00	0.43	0.52 / 0.56	0.50
9. ESA_C	-0.11	-0.10	-0.15	-0.01	-0.07	-0.07	0.48	0.48	0.57 / 0.57
(B) gratitude condition									
1. PA_A	0.64 / 0.76	0.75	0.62	0.33	0.39	0.44	0.65	0.37	0.27
2. PA_B	0.64	0.86 / 0.90	0.73	0.39	0.46	0.52	0.68	0.37	0.30
3. PA_C	0.60	0.72	1.03 / 0.94	0.14	0.22	0.38	0.48	0.27	0.16
4. Grat_A	0.17	0.07	0.17	2.25 / 1.53	1.06	1.05	0.99	0.73	0.58
5. Grat_B	0.16	0.11	0.13	1.46	1.75 / 1.26	0.99	0.92	0.57	0.57
6. Grat_C	0.27	0.21	0.30	1.32	1.13	1.57 / 1.27	0.92	0.68	0.56
7. TG_A	0.26	0.21	0.19	0.83	0.41	0.69	1.43 / 1.74	0.90	0.75
8. TG_B	0.28	0.22	0.37	0.88	0.55	0.85	0.75	1.43 / 1.22	0.59
9. TG_C	0.14	0.16	0.09	0.62	0.47	0.53	0.55	0.56	1.08 / 1.15
(C) compassion condition									
1. PA_A	0.64 / 0.70	0.67	0.58	0.31	0.24	0.46	-0.14	-0.07	-0.08
2. PA_B	0.64	0.86 / 0.86	0.0.72	0.38	0.27	0.52	-0.16	-0.09	-0.10
3. PA_C	0.60	0.72	1.03 / 0.88	0.38	0.24	0.54	-0.18	-0.08	-0.07
4. SC_A	0.38	0.42	0.47	0.65 / 0.64	0.28	0.57	-0.16	-0.11	-0.06
5. SC_B	0.25	0.29	0.34	0.35	0.44 / 0.44	0.41	-0.08	-0.02	-0.01
6. SC_C	0.43	0.47	0.51	0.54	0.35	0.82 / 1.00	-0.22	-0.12	-0.08
7. ESA_A	-0.09	-0.11	-0.15	-0.06	-0.09	-0.10	0.56 / 0.58	0.49	0.47
8. ESA_B	-0.08	-0.10	-0.11	0.00	-0.04	-0.04	0.43	0.52 / 0.57	0.49
9. ESA_C	-0.11	-0.10	-0.15	-0.04	-0.06	-0.06	0.48	0.48	0.57 / 0.53

Note. Within each section we displayed the covariances for the control condition below the diagonal and for the intervention condition above the diagonal. The diagonal within each section displays the variances for the control condition on the left and the variances for the intervention condition on the right. Unstandardized parameter estimates. PA = positive affect parcels; SO = specific outcome (Opt, Grat, or SC); Mod = moderator (ESA or TG); Opt = optimism items; Grat = gratitude items; SC = self-compassion parcels; ESA = emotional self-awareness parcels; TG = trait gratitude parcels.

Chapter 5

Inducing Positive Affect and Positive Future Expectations Using the Best-Possible-Self Intervention: A Systematic Review and Meta-Analysis

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using the best-possible-self intervention: A systematic review and meta-analysis. *Journal of Positive Psychology*. Advance online publication.
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[3] Due to copyright issues, the authors' personal copy of the article rather than the version of record is included in this thesis.

Abstract

The best-possible-self intervention has been shown to effectively increase positive affect and optimism. Differences in timing and conceptualization of outcome assessments, however, complicate interpretations regarding the practical significance of these effects. To address this issue, we conducted a systematic literature search and included 34 randomized controlled trials into several meta-analyses. We coded the exact time of measurement and how outcomes were assessed. Results reveal small overall effects on positive affect (Hedge's $g = 0.28$, 95% CI [0.16, 0.41]) and optimism ($g = 0.21$, 95% CI [0.04, 0.38]). Effects on positive affect were strongest among studies that assessed momentary affect immediately after the intervention, whereas effects on optimism were only significant if conceptualized as positive future expectations rather than a general orientation in life. Descriptive results indicate no substantial follow-up effects. Thus, the best-possible-self intervention might be thought of as a mood/expectation induction procedure. Further development may lead to sustained effects.

Keywords: positive-psychological intervention, best-possible-self, meta-analysis, systematic review, well-being, happiness, positive affect, optimism

Traditionally, psychological intervention research has focused on alleviating human suffering, which has resulted in a range of powerful treatments for the psychologically distressed (e.g., Wampold & Imel, 2015). However, up until recently, psychology had little to offer for individuals that did not feel particularly distressed but wished to increase their happiness (Seligman & Csikszentmihalyi, 2000; Seligman, Steen, Park, & Peterson, 2005). One promising development that suits the needs of happiness seekers are positive-psychological interventions that aim to increase well-being through cultivating positive emotions, thoughts, and behaviors (Parks, Della Porta, Pierce, Zilca, & Lyubomirsky, 2012). Such interventions have generally been shown to effectively increase well-being (see Bolier et al., 2013; Sin & Lyubomirsky, 2009; C. A. White, Uttl, & Holder, 2019, for two independent meta-analyses and some controversy). In addition, there is accumulating evidence in support of one particularly prominent positive-psychological intervention, the best-possible-self (BPS) intervention (King, 2001). Specifically, results from two recent meta-analyses of randomized controlled trials show a moderate intervention effect on posttest differences in optimism (Hedge's $g = 0.64$; based on 10 studies; Malouff & Schutte, 2016), as well as small to moderate effects on pre- to posttest differences in optimism ($g = 0.33$, 95% CI [0.25, 0.42]) and positive affect ($g = 0.51$, 95% CI [0.26, 0.77]; 13 studies for each outcome; Carrillo et al., 2019). There was no significant effect on negative affect ($g = 0.19$, 95% CI [-0.33, 0.25]; 13 studies; Carrillo et al., 2019). These meta-analyses provide valuable insights into the effectiveness of the BPS intervention. They are, however, limited in four important ways: First, they do not examine the long-term effects of the BPS intervention (e.g., several weeks or months after the intervention). This might be partly because only few studies have investigated follow-up effects, which complicates statistically sound analyses. Nevertheless, a descriptive analysis would have been useful. Second, the meta-analyses do not differentiate between different times of posttest assessments (e.g., immediately after and several days after the intervention) and different conceptualizations of the same outcome (e.g., momentary affect and affect during the past week). Making such distinctions, however, is important because it provides additional insights into the practical and theoretical significance of the effects of the BPS intervention. Third, several outcomes, including life satisfaction, pessimism, and happiness were not separately considered. Rather, the authors decided to aggregate several outcomes into well-being composites, which is a widespread approach in positive-psychological intervention meta-analyses (Bolier et al., 2013; Sin & Lyubomirsky, 2009), but further complicates the interpretation of the results. Examining effects on individual outcomes, even if only descriptive, promises a clearer understanding of the effectiveness of the

BPS intervention. Fourth, moderator analyses yielded no consistent results, partially because of the small number of reviewed studies (13 and 10; Carrillo et al., 2019; Malouff & Schutte, 2016). It is, however, of great theoretical and practical relevance, for whom and under which conditions the effects of the BPS intervention are strongest (see Fritz & Lyubomirsky, 2018; Lyubomirsky, 2019; Lyubomirsky & Layous, 2013, 2014, for a deeper discussion). For example, initial evidence suggests that effects are larger among more motivated participants (Layous, Nelson, & Lyubomirsky, 2013; Lyubomirsky, Dickerhoof, Boehm, & Sheldon, 2011; Sheldon & Lyubomirsky, 2006) and participants from individualistic cultures (Boehm, Lyubomirsky, & Sheldon, 2011). In addition, one experimental study suggests that the BPS intervention is equally effective when administered online or in-person (Layous et al., 2013). However, none of these effects has been confirmed in a meta-analysis. Furthermore, we currently know little about appropriate dosing (e.g., single or repeated sessions), choice of themes (e.g., life in general or life domains), or whether adding an imagery exercise serves to increase effects (see Loveday, Lovell, & Jones, 2016, for a discussion). In this systematic review and meta-analysis, we want to go beyond previous reviews and meta-analyses in several important ways. Specifically, we provide an updated account of the effects of the BPS intervention on different well-being related outcomes that includes recently published studies and accounts for the specific time of outcome assessment as well as how outcomes were conceptualized. In addition, we include practitioner friendly metrics, describe follow-up effects, investigate moderators, compare effects against gratitude interventions, and provide guidance for future research.

5.1 Aims of the Present Study

The primary aim of this study was to examine the effects of the BPS intervention on different well-being related outcomes in mentally healthy adults relative to neutral control groups while accounting for the time of posttest assessment and how outcomes were conceptualized. Secondary objectives were to describe intervention effects at follow-up assessments and to test moderator effects. Specifically, we hypothesized:

(1) Participants in the BPS intervention condition will report higher positive affect and optimism compared with participants in the control conditions. Effect will (a) be larger if assessed at the final day of the intervention compared with several days later; and (b) be larger in studies that measured states (e.g., momentary affect) compared with more trait like variables (e.g., affect during the past week).

(2) The average size of the effects will depend on characteristics of the participants and features of

the activity. Specifically, effects will be larger among (a) more motivated participants; and (b)

participants from more individualistic societies, as well as in studies that (c) used more intense

interventions; and (d) used an imagery component.

5.2 Method

5.2.1 Data Sources

To comprehensively identify all relevant primary studies that instructed participants to write about

their best possible future, we conducted a literature search using the databases PsycINFO (EBSCO),

PsycARTICLES (EBSCO), ProQuest dissertations and theses (PQDT), and PubMed (NLM), as well as

reference lists of articles and books (as recommended by Lipsey & Wilson, 2001). We searched for

literature published in English and German language. No other limits were used. In addition, we

contacted experts in the field and asked them to identify sources that might still be missing. The following

keywords were used for the search: *best possible self, positive psychology intervention, positive writing,*

optimism intervention, optimism writing (see Appendix for the complete search strings). All available

studies up to February 2019 were included. A review protocol for this meta-analysis was pre-registered

using PROSPERO (No. CRD42019125305; as recommended by Moher, Liberati, Tetzlaff, & Altman,

2009).

5.2.2 Selection Criteria

We included all studies that fulfilled the following criteria:

(1) The study included some variation of the best-possible-self intervention developed by King

(2001). Specifically, the task involved writing about one's best possible future life. We included

studies that additionally instructed participants to visualize their best possible future. However,

studies that only used visualization were excluded because this type of intervention resembles

meditation and is outside the scope of this review. For the same reason, we excluded studies in

which participants received feedback on their writings, an intervention that closely resembles

psychotherapy (see Frattaroli, 2006, for a similar approach).

(2) The study must be a randomized experiment including an active control group. Studies that

solely relied on waitlist control groups were excluded. Participants in the comparison group must

have either written about a neutral topic (e.g., one's previous day or early memories), a traumatic

life event or participated in another positive-psychological intervention (e.g., writing a gratitude letter or gratitude lists). The BPS intervention must have been delivered separately. Portfolio study that administered the intervention together with other interventions were excluded if the design did not allow to determine the sole effect of the BPS intervention. Specifically, participants in the intervention condition were not receiving an additional treatment that the control participants were not receiving (except visualizing their best possible future life).

(3)   The study delivered the intervention to mentally healthy individuals (e.g., students, general population). We excluded studies that used clinical samples because such studies select participants based on clinical criteria (e.g., depressive symptoms scores) and this procedure likely restricts variance in outcomes related to psychological functioning. In addition, we excluded studies that delivered the intervention to physically impaired individuals if the instructions were tailored to the needs of the given sample such that low similarity with the original intervention remained (e.g., asking diabetes patients to write about their best possible HbA1c level; Gibson, Umeh, Newson, & Davies, 2018).

(4)   The study must contain at least one well-being related outcome that was measured after the completion of the intervention. The used measure must have been reliable and valid.

(5)   The study must provide enough information to calculate the effect size. If the necessary data were not reported, an attempt was made to obtain them from the authors.

5.2.3 Selection of Outcomes

We included outcome variables related to well-being. The most common variables were positive affect, negative affect, optimism, pessimism, life satisfaction, depressive symptoms, and happiness. In order to allow for more comprehensive conclusions regarding the effects of the BPS intervention, we did not aggregate outcomes into broader categories (e.g., combining affect and life satisfaction ratings for a subjective well-being composite; Bolier et al., 2013). When outcomes were reported at multiple times of measurement or when studies used multiple scales to assess a single outcome at a given time of measurement, we calculated a corresponding number of effect sizes. Again, we did not aggregate effects to allow for more comprehensive conclusions. For details on how dependencies within the data were handled see the statistical analysis section. Variables assessing physiological health (e.g., number of doctoral visits) and subjective ratings of the impact of the intervention (e.g., whether participants liked the

exercise) were excluded because they were rarely reported, and meta-analytically synthesizing effect sizes seemed inappropriate.

5.2.4 Assessment of Risk of Bias

We expected the included studies to differ in terms of risk of bias. Therefore, we systematically assessed risk of bias regarding random sequence generation, allocation concealment, blinding of participants and experimenters, missing outcome data, selective reporting of outcomes, and other sources of bias for each included study (see Higgins et al., 2011; Higgins et al., 2019, for a deeper discussion). For each category, two trained reviewers indicated either high or low risk of bias or, if insufficient information were provided, uncertain risk. Different assessments were discussed and solved with a third reviewer.

Random sequence allocation (selection bias). We assumed high risk of bias if participants were allocated to conditions based on non-random procedures such as participant's date of birth or dates of the experimental session. On the other hand, for example, computer-generated random sequences were assumed to involve a low risk of bias.

Allocation concealment (selection bias). We assumed high risk of bias if intervention allocations could have been foreseen before or during enrolment, for example, when open randomization lists were used. In studies that were conducted completely online, we assumed low risk of bias.

Blinding of participants and experimenters (performance bias). We assumed successful blinding if authors explicitly reported that participants and experimenters were blinded or if instructions were provided online, via film or audio recordings, or in written format only. Failure to blind experimenters was, for example, assumed when experimenters verbally administered the intervention because this procedure makes it impossible for the experimenter to remain blind to condition and might have affected the self-reports in the included studies (see Frattaroli, 2006, for a similar approach).

Blinding of outcome assessment (detection bias). The variables of interest in this review rely on self-reports and we assumed low risk of bias if participants were blind to condition assignment and if there were no hints that blinding was not maintained until data collection was completed.

Incomplete outcome data (attrition bias). We assumed high risk of bias if different attrition between groups exceeded 15%. In addition, we assumed high risk of bias if the overall attrition rate exceeded 20% unless reasons for missing data in the intervention and control conditions were reported, had no different implications in the compared groups, and were balanced across groups. If the numbers of

participants randomized into each intervention group were not clearly reported, the risk of bias remained unclear.

Selective reporting (reporting bias). We assumed selective reporting if outcomes were not reported at all or only for subgroups or not at all times of measurement. An attempt was made to obtain missing data from the authors. Low risk of bias was assumed if all main outcomes listed in the method section were fully reported.

Other bias. We assumed high risk of other bias if there were further issues that raised concerns about the possibility of bias. For example, high risk of bias was assumed if there was a baseline imbalance in outcomes between intervention and control conditions that was large enough to lead to an important exaggeration of effect estimates and was not explained by sequence generation (e.g., non-random procedures), lack of allocation concealment, or exclusion of participants. Otherwise we assumed low risk of bias.

5.2.5 Assessment of Publication Bias

Publication bias occurs when the availability of studies depends on the results (Rothstein, Sutton, & Borenstein, 2005). This can distort the results of meta-analyses. For example, the selective publication of studies with statistically significant positive results leads to an overestimation of pooled mean effect sizes. Although we expected limited risk of publication bias because our literature search encompassed unpublished studies, we nevertheless tested for it. The assessment was based on careful examinations of funnel plots (see Lipsey & Wilson, 2001; Terrin, Schmid, & Lau, 2005, for a deeper discussion). This approach was only used if there were enough effect sizes for a given outcome to ensure robust conclusions (> 10; see Page, Higgins, & Sterne, 2019, for an introduction). Other than in our hypothesis tests, we only considered the first effect size reported in each study in the funnel plots. This was done because the funnel plots were unable to account for the shared variance of effect sizes from the same study and no other practicable solution was available. In addition, we used Egger's regression analysis (Egger, Smith, Schneider, & Minder, 1997; Sterne, Becker, & Egger, 2005) to test funnel plot asymmetry and the trim-and-fill procedure (Duval & Tweedie, 2000a, 2000b) to estimate the number of missing effect sizes.

5.2.6 Selection and Coding of Moderator Variables

We expected the size of intervention effects to vary as a function of characteristics of the included studies. For example, we hypothesized that the difference between the BPS intervention and controls will be larger in studies that conceptualized affect as a state variable, asking participants how they feel "at the moment", than in studies that used a trait conceptualization, asking participants how they feel "in general". In addition, studies that assessed the outcome immediately after the intervention should report larger effects than studies with later assessments. Accordingly, we tested time of measurement and conceptualization of the outcome as moderators. Specifically, two trained raters coded the following moderator variables, whereby disagreements were discussed until a consensus was reached:

(1) Time of measurement was coded into one of three categories: immediate posttest (outcome was assessed during the final day of the intervention), posttest (between 1 and 6 days after the intervention), and follow-up (7 days or later). Follow-up assessments were not tested in our main analysis because only few studies reported them, however, we provided descriptive results of follow-up effects.

(2) Conceptualization of the outcome was coded using the instructions used and reported in the method sections of the included studies. We distinguished state (e.g., momentary positive affect), trait like (e.g., positive affect during the last week), and trait variables (e.g., habitual positive affect). If instructions were not reported by the authors, we used the instructions provided in the original articles of the used scales. We judged assessments of future expectations using the Future Expectations Scale (FEX; Peters, Vieler, & Lautenbacher, 2015) or the Subjective Probability Task (SPT; MacLeod, 1996) to be trait like, whereas traditional assessments of optimism using the Life Orientation Test (LOT-R; Glaesmer, Grande, Braehler, & Roth, 2011) were trait measures. In line with this approach, brief administrations of optimism interventions have been shown to produce larger effects if the FEX or SPT, rather than the LOT-R, was used (Malouff & Schutte, 2016).

(3) Intensity of the intervention was indicated by the number of laboratory, online, and homework writing sessions, and also took into account the average length of the sessions. The average length of sessions was based on the reported time that participants spent writing, excluding the time spent visualizing. Furthermore, we weighted the time spent on homework assignments by a factor of 0.75 to receive a more valid indicator. The factor was based on previous BPS studies showing that participants reported to carry out 3 in 4 assignments (Heekerens & Heinitz, 2019;

Heekerens, Heinitz, & Eid, 2019). For moderator analyses we used two dummy variables indicating that the intervention was brief (one session or a total writing time of less than 40 minutes), average (two to four sessions and 40 to 80 minutes as in the original paper; King, 2001), or extensive (more than four sessions or more than 80 minutes). The coding was done separately for each time of measurement. Thus a study that assessed positive affect immediately after a 30 minutes lab session followed by a week of daily 20 minutes writing exercises and a second assessment one day after the end on the full intervention would be considered brief at the first time of measurement (immediate posttest) and extensive at the second (posttest). We used average interventions as reference category in the main analysis.

(4)   Some studies instructed participants to visualize their best possible future before or after the writing session, typically for 5 minutes (see Peters, Flink, Boersma, & Linton, 2010, for an example). We coded whether studies included a mental imagery component or not.

(5)   In addition, we judged the cultural background of participants using Hofstede's (2011) individualism-collectivism dimension. If participants came from countries that scored 50 or higher on the individualism dimension (based on the culture compass tool; Hofstede, 2019), we supposed that the sample was individualistic.

(6)   Finally, we coded whether participants were asked to write about their future life in general or about specific life domains (Boehm et al., 2011), whether the intervention was delivered online or in person, and whether participants were compensated or not. We included both money and course credit as compensation.

5.2.7 Power Analysis and Expected Effect Sizes

We used a prior power analyses to determine how many effect sizes for a given outcome were required to justify the estimation of a pooled effect size with reasonable statistical power. We conservatively expected a pooled effect size of at least Hedge's $g = 0.30$ for well-being related outcomes in this meta-analysis that was based on one previous meta-analysis reporting Cohen's $d = 0.34$ for a combined effect size of positive interventions on posttest differences in subjective well-being (based on 28 studies; Bolier et al., 2013; also see Sin & Lyubomirsky, 2009). Results reveal that at least 15 effect sizes and an average sample size of 50 in each group (based on Bolier et al., 2013; Malouff & Schutte, 2016) are required to reach a statistical power of at least 0.80, given moderate heterogeneity in the data

(see Tiebel, 2018; Valentine, Pigott, & Rothstein, 2010, for details on how the analysis was performed). We assumed random-effect models.

5.2.8 Computation and Weighting of Effect Sizes

We calculated standardized effect sizes Hedge's g to indicate the difference between the BPS intervention and control conditions because the included studies used different scales to assess similar outcomes (e.g., the Positive and Negative Affect Schedule, Watson & Clark, 1988, or the Multidimensional State Mood Scale, Steyer, Schwenkmezger, Notz, & Eid, 1994, for positive affect). The index was calculated by subtracting the posttest mean of the control group from the posttest mean of the BPS intervention group and dividing this difference by the pooled standard deviation of both groups at posttest (Hedges, 1981; Morris & DeShon, 2002). An effect size of 0.5 shows that the mean of the BPS intervention group is half a standard deviation larger than the mean of the control group. As mentioned, for positive-psychological interventions effect sizes of 0.3 or larger have been reported (Bolier et al., 2013; Sin & Lyubomirsky, 2009). We did not calculate standardized mean changes because using mean differences allowed us to also include studies that did not report baseline scores of the outcomes. Another reason for this approach is that we did not expect significant differences between the baseline scores of the BPS intervention and control conditions because all included studies were randomized controlled trials. Another issue that we encountered was that three studies included in this review used cluster randomized controlled trials (Heekerens et al., 2019; Heekerens & Heinitz, 2019; Liau, Neihart, Teo, & Lo, 2016). Because in such trials the unit of analysis is different from the unit of allocation (Whiting-O'Keefe, Henke, & Simborg, 1984), which carries the risk of false positive conclusions due to artificially low standard errors, effect size estimates are potentially biased (Donner & Klar, 2002; I. R. White & Thomas, 2005). We carefully checked the articles for indications that results may be biased and particularly considered the reported intraclass correlations (I. R. White & Thomas, 2005). After concluding that the risk of bias due to cluster randomization in the three mentioned studies was low, we decided to calculate effect sizes based on the reported statistics without applying additional corrections. Finally, effect sizes in all our analyses were weighted with the inverse of the sampling variance (see Lipsey & Wilson, 2001; Morris & DeShon, 2002; Schmidt & Hunter, 2015, for the used formula and further information). Pooled mean effect sizes were calculated using the R package metafor (Viechtbauer, 2010).

5.2.9 Practical Relevance

One drawback of using standardized effect sizes is that they can be difficult to interpret when making practical decisions such as whether to apply or not to apply a positive-psychological intervention (Baguley, 2009; Morris & DeShon, 2002). In order to make the reported effects more accessible, we transformed the group means of the included outcomes to match a 0 to 100 scale and calculated the respective differences between BPS intervention and control groups (as recommended by Lind, 2014; Pek & Flora, 2018; using the R package scale; Wickham, 2018). A mean difference of 10 shows that the mean of the BPS intervention group is 10 points larger on a 0 to 100 scale than the mean of the control group.

5.2.10 Statistical Analysis

We used random-effect models to test our hypotheses because we expected the included studies to differ from one another in terms of a variety of unobserved variables such as implementation of the intervention and personality of participants (Borenstein, Hedges, Higgins, & Rothstein, 2010). Fixed-effect models were only used if the observed heterogeneity was very low ($I^2 < 40\%$; Higgins et al., 2019). Dependencies in the data, resulting from the fact that some effect sizes came from the same studies, were accounted for by applying robust estimates of the variance-covariance matrix (as implemented in the R package metafor; Viechtbauer, 2010). Mixed-effect models were used to test moderator effects. All analyses used R version 3.4.3 (R Core Team, 2017).

5.3 Results

5.3.1 Data Selection

The database search yielded 237 published articles (PsycINFO: 217, PsycARTICLES: 12, PubMed: 8) and 49 dissertations (see Figure 5.1 for the flow chart of literature search). After removing duplicates, the list contained 241 references. We included another 8 references that were identified through other sources. The final list included 249 references that were screened by two trained reviewers. In a first step, both reviewers read the titles and abstracts of the references. If one of the reviewers suggested that the study might fulfill the selection criteria of this meta-analysis, both reviewers read the full text of the reference in a second step to verify study eligibility. Conflicting judgments were discussed until a consensus was reached.
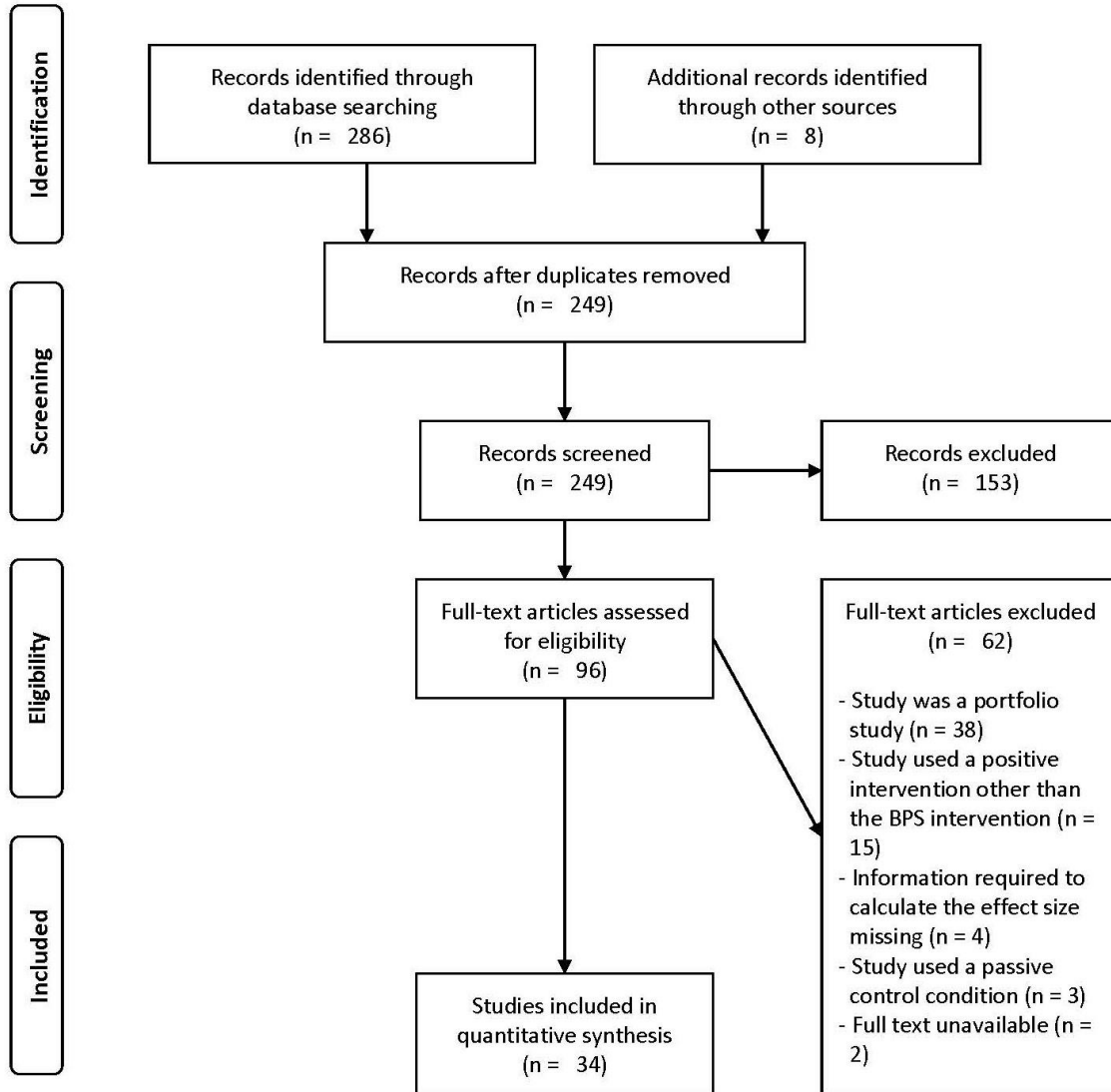
Figure 5.1. Flow chart of literature search.

5.3.2 Interrater Agreement

Regarding the coding of characteristics of the studies, including the information required to calculate effect sizes and to run moderator analyses, the average interrater agreement was 87.22% (Cohen's κ = .74; Cohen, 1960). Average agreement ranged from 61.90% (κ = .24) for calculation of the attrition rate at 3 months follow-up, to 88.23% (κ = .76) for mean age of participants, and up to 100.00% (κ = 1.00) for mean pessimism in the BPS intervention group at immediate posttest. For risk of bias assessment, the average interrater agreement was 79.65% (κ = .66). Specifically, average agreement was 66.67% (κ = .53) for blinding of personnel, 78.78% (κ = .67) for incomplete outcome data, each 81.81% (κ = .70) for allocation concealment, blinding of outcome assessors, selective outcome reporting, and other sources of bias, and 84.84% (κ = .73) for sequence generation. We conclude that the interrater reliability in this meta-analysis is satisfactory because the obtained kappa coefficients, except ratings for the calculation of attrition rates and the blinding of personnel, were close to the commonly applied criteria of .70 (McHugh, 2012). Different assessments and calculations were discussed. We took the utmost care to only include correct results and reliable assessments into our analyses.

5.3.3 Description of Included Studies

In the 34 included studies, a total of 1840 (M = 55.76, Range = 14 to 252) participants received the BPS intervention, 1835 (M = 55.61, Range = 13 to 253) an active control, and 787 (M = 78.70, Range = 15 to 249) either an expressive writing, self-compassion, or gratitude intervention. The average mean age of participants in the studies was 26.69 (Range = 17.83 to 51.08) years and 76.91% (Range = 53 to 100) were female. Table 5.1 reveals that six studies used a priori power analyses to determine sample sizes (also see White et al., 2019, for why this is important).

The number of writing sessions ranged from 1 to 8 (M = 2.73, Modus = 1) and time spent writing in each session ranged from 5 to 30 minutes (M = 16.42, Modus = 15). In eight studies participants were asked to continue the exercise at home 3 to 20 times (M = 9.63, Modus = 3) for 2 to 30 minutes (M = 8.13, Modus = 5). In addition, 17 studies used a mental imagery component to supplement the writing exercise. Regarding control conditions, 15 instructed participants to write about their past day or week, 10 about a typical day, two about early memories, two about activities for the following day, two about general life details, one about the layout of a place, and one about to-do lists. Attrition in all conditions ranged from 0.00% to 36.90% (M = 6.20) at immediate posttest, 0.00% to 54.43% (M = 20.83) at posttest, 3.00% to 72.03% (M = 35.50) at 30 days follow-up, 9.68% to 65.63% (M = 42.32) at 60 days

Table 5.1 Characteristics of Included Randomized Controlled Trials Examining the Effects of the Best-Possible-Self Intervention

Author (year)	Pub. type	Country	Population	Mean age (SD or range)	Female (%)	Session (number), duration	Homework (number), duration	Control group	Other intervention group	N (immediate post), attrition (%)	N (post), attrition (%)	N (follow-up post), attrition (%)	Outcome measures (conception)
King (2001)	Article	USA	Students	21.04 (3.15)	87.00%	4, 20min, imagery: no	-	Schedule for following day	Expressive writing	Ne = 21 Nc = 15 No = 19 0.00% Power analysis: no	Ne = 21 Nc = 15 No = 19 0.00%	-	PA: Mood Rating Scale (state) Optimism: LOT (trait) Life Satisfaction: SWLS (trait)
Sheldon (2006)	Article	USA	Students	-	74.63%	1, 20min, imagery: no	3, 5min	Typical day	Gratitude diary	Ne = 23 Nc = 23 No = 21, 4.29 % Power analysis: no	Ne = 23 Nc = 23 No = 21, 4.29 %	-	PA: PANAS (state) NA: PANAS (state)
Harrist (2007)	Article	USA	Students	21.00 (18 - 45)	66.67%	4, 20min, imagery: no	-	Schedule for following day	-	Ne = 19 Nc = 20, 0.00% Power analysis: no	-	-	PA: Mood Rating Scale (state) NA: Mood Rating Scale (state)
Peters (2010)	Article	Sweden	Students	29.60 (21 - 50)	62.20%	1, 15min, imagery: yes	-	Typical day	-	Ne = 44 Nc = 38, 0.00% Power analysis: no	-	-	PA: PANAS (stat) NA: PANAS (state) Optimism: SPT (trait like) Pessimism: SPT (trait like)
Shapira (2010)	Article	Canada	General population	34.00 (18 - 72)	82.00%	7, 10min, imagery: no	-	Early memories	Self-compassionate writing	Power analysis: no	Ne = 197 Nc = 146, 34.37%	30 days, Ne = 155 Nc = 191, 48.74% 90 days, Ne = 98 Nc = 135, 65.63% 180 days, Ne = 62 Nc = 95, 76.74%	Happiness: SHI (trait) Depression: CES-D (trait like)
Boehm (2011)	Article	USA	General population	35.62 (11.36)	53.00%	6, 10min, imagery: no	-	Past 7 days	Gratitude letter	Ne = 70 Nc = 69 No = 70, 5.00% Power analysis: no	-	30 days, Ne = 72 Nc = 70 No = 71, 3.00%	Life Satisfaction: SWLS (trait)
Lyubomirsky (2011)	Article	USA	General population	19.66 (2.91)	71.21%	8, 15min, imagery: no	-	Past 7 days	Gratitude letter	Power analysis: no	Ne = 110 Nc = 101 No = 104, 3.93%	180 days, Ne = 66 Nc = 71, No = 73 36.36%	PA: Mood Scale (trait like) NA: Mood Scale (trait like) Life Satisfaction: SWLS (trait) Happiness: SHS (trait)
Meevissen (2011)	Article	Netherlands	Students	23.50 (6.39)	93%	1, 20min, imagery: yes	14, 5min	Past 24 hrs.	-	Power analysis: no	Ne = 28 Nc = 23, 5.56%	-	PA: PANAS (trait like) NA: PANAS (trait like) Optimism: SPT, FEX (trait like) Pessimism: SPT, FEX (trait like)
Meevissen (2012)	Article	Netherlands	General population	-	100%	1, 20min, imagery: yes	-	-	-	Ne = 36 Nc = 34, 0.00% Power analysis: no	-	-	PA: BMIS (state) Optimism: FEX (trait like) Pessimism: FEX (trait like)

Study	Type	Country	Population	Age M (SD)	% female	Intervention		Instruction	Control	N	N	Follow-up	Outcomes
Boselie (2014)	Article	Netherlands	Students	21.90 (2.29)	78.38%	1, 15min, imagery: yes	-	Typical day	-	Ne = 38 Nc = 36, 0.00% Power analysis: no	-	-	PA: PANAS (state) NA: PANAS (state) Optimism: SPT, FEX (trait like) Pessimism: SPT, FEX (trait like)
Maddalena (2014)	Article	USA	Students	-	66.00%	3, 20min, imagery: no	-	Past 24 hrs.	Expressive writing	Power analysis: no	-	30 days, Ne = 24 Nc = 23 No = 20, 28.09%	
Renner (2014)	Article	Netherlands	Students	22.10 (3.94)	80.00%	1, 15min, imagery: yes	-	Typical day	-	Ne = 20 Nc = 20, 0.00% Power analysis: no	-	-	PA: PANAS (state) NA: PANAS (state)
Geschwind (2015)	Article	Belgium	Students	20.32 (1.97)	100.00%	1, 16min, imagery: yes	-	Typical day	-	Ne = 25 Nc = 25, 0.00% Power analysis: no	-	-	PA: mDES (state) NA: mDES (state)
Peters (2015)	Article	Germany	Students	23.50 (3.30)	57.14%	1, 15min, imagery: yes	-	Typical day	-	Ne = 28 Nc = 28, 0.00% Power analysis: no	-	-	Optimism: SPT, FEX (trait like) Pessimism: SPT, FEX (trait like) PA: PANAS (state) NA: PANAS (state)
Boselie (2016 - study 1)	Article	Netherlands	Students	21.35 (4.28)	79.01%	1, 15min, imagery: yes	-	Typical day	-	Ne = 41 Nc = 40, 0.00% Power analysis: no	-	-	PA: PANAS (state) NA: PANAS (state) Optimism: FEX (trait like) Pessimism: FEX (trait like)
Boselie (2016 - study 2)	Article	Netherlands	Students	21.84 (2.22)	73.77%	1, 15min, imagery: yes	-	Typical day	-	Ne = 32 Nc = 29, 6.15% Power analysis: no	-	-	PA: PANAS (state) NA: PANAS (state) Optimism: FEX (trait like) Pessimism: FEX (trait like)
Liau (2016)	Article	Singapore	Students	17.83 (1.12)	73.82%	2, 20min, imagery: no	-	Life details	-	Ne = 81 Nc = 81, 0.00% Power analysis: no	-	30 days, Ne = 81 Nc = 81, 15.18%	PA: PANAS (state) NA: PANAS (state) Optimism: LOT-R (trait) Life Satisfaction: BMSLS (trait)
Manthey (2016)	Article	Germany	General population	33.70 (9.60)	84.00%	8, 20min, imagery: no	-	To-do lists	Gratitude diary	Power analysis: no	Ne = 135 Nc = 150 No = 150, 34.68%	60 days, Ne = 102 Nc = 116 No = 104, 51.65%	PA: SPANE (trait like) NA: SPANE (trait like) Life Satisfaction: SWLS (trait)
Auyeung (2018)	Article	China	Students	22.82 (3.38)	73.00%	6, 10min, imagery: no	-	Past 24 hrs.	-	Power analysis: no	Ne = 48 Nc = 52, 28.06%	-	PA: PANAS (trait like) Depression: CES-D (trait like)
Carrillo (2018 - study 1)	Article	Spain	Students	21.76 (3.63)	76.79%	1, 15min, imagery: yes	6, 10min	Past 24 hrs.	BPS past, BPS present	Power analysis: yes	Ne = 27 Nc = 28 No (1) = 30 No (2) = 27, 21.43%	-	PA: PANAS (trait) NA: PANAS (trait) Optimism: LOT-R (trait) Life Satisfaction (trait) Happiness: HM (trait like)
Carrillo (2018 - study 2)	Article	Spain	General population	23.86 (6.25)	82.41%	7, 15min, imagery: yes	-	Past 24 hrs.	BPS past, BPS present	Power analysis: yes	Ne = 27 Nc = 25 No (1) = 28 No (2) = 28, 29.41%	-	PA: VAS (trait) NA: VAS (trait) Optimism: LOT-R (trait) Life Satisfaction (trait)

Table 5.1 Characteristics of Included Randomized Controlled Trials Examining the Effects of the Best-Possible-Self Intervention (Continued)

Study	Type	Country	Population	Age M (SD)	% female	Intervention	Control	Timeframe	Other PPI	N / Power (baseline)	N (post)	Follow-up	Measures
Molinari (2018)	Article	Spain	Fibromyalgia Syndrome patients	51.08 (10.54)	100.00%	1, 15min, imagery: yes	12, 5min	Past 24 hrs.	-	Power analysis: yes	Ne = 23 Nc = 28, 36.25%	30 days, Ne = 18 Nc = 17, 56.25% 90 days, Ne = 15 Nc = 13, 65.00%	PA: PANAS (state) NA: PANAS (state) Optimism: FEX (trait like), LOT-R (trait) Pessimism: FEX (trait like) Depression: BDI-II (trait like)
Heekerens (2019a)	Article	Germany	Students	24.43 (7.37)	80.47%	1, 30min, imagery: yes	2, 10min	Past 24 hrs.	-	Ne = 66 Nc = 62, 0.00% Power analysis: yes	Ne = 66 Nc = 62, 9.22%	-	PA: PANAS (trait like) NA: PANAS (trait like) Optimism: CIT (trait, based on LOT) Life Satisfaction: CIT (trait, based on SWLS)
Heekerens (2019b)	Article	Germany	Students	22.35 (5.04)	78.72%	1, 20min, imagery: yes	3, 20min	Past 24 hrs.	-	Ne = 92 Nc = 96, 6.00% Power analysis: yes	Ne = 90 Nc = 93, 8.50%	-	PA: PANAS (state) NA: PANAS (state) Optimism: FEX (trait like) Pessimism: FEX (trait like) Life Satisfaction: SWLS (trait)
Paulmichl (2019)	Master's thesis	Germany	Students	24.32 (6.19)	77.19%	1, 15min, imagery: no	-	Past 24 hrs.	-	Ne = 78 Nc = 93 36.90% Power analysis: no	-	-	PA: PANAS (state) NA: PANAS (state) Optimism: CPC-12 (state, based on LOT)
Heekerens (submitted)	Article	Germany	General population	43.26 (12.67)	57.20%	1, 15min, imagery: no	-	Past 24 hrs.	Gratitude letter, self-compassionate writing	Ne = 110 Nc = 106 No (1) = 105 No (2) = 104 2.30% Power analysis: yes	-	-	PA: MDBF (state), Optimism: LOT-R (state)

Note. We defined state measures as assessments using instructions such as "at the moment" whereas trait like measures used "during the past days or weeks" and trait measures used "usually" or "generally". Optimism assessments that asked participants to rate the likelihood of positive and negative future events were trait like measures. BDI-II = revised version of the Beck Depression Inventory; BMIS = Brief Mood Introspection Scale; BMSLS = Brief Multidimensional Student's Life Satisfaction Scale; CES-D = Center for Epidemiologic Studies Depression Scale; CIT = Comprehensive Inventory of Thriving; CPC-12 = Compound PsyCap Scale; HM = Fordyce Happiness Measure; mDES = modified Differential Emotion Scale; LOT = Life Orientation Test; LOT-R = Life Orientation Test Revised; PANAS = Positive and Negative Affect Schedule; MDBF = Multidimensional State Mood Scale; SHI = Steen Happiness Index; SHS = Subjective Happiness Scale; SPANE = Scale of Positive and Negative Experiences; SPT = Subjective Probability Task; SWLS = Satisfaction with Life Scale; VAS = Visual Analogue Scale; SD = standard deviation; N = number of participants; Ne = total number of participants in the best-possible-self intervention condition; Nc = number of participants in the control condition; No = number of participants in other positive-psychological intervention condition.

follow-up, 25.93% to 78.64% ($M = 56.52$) at 90 days follow-up, and 36.36% to 85.52% ($M = 66.21$) at 180 days follow-up. Out of 34 included studies, eight came from the Netherlands, eight from Germany, seven from the USA, four from Spain, two from Singapore, one from Sweden, one from Canada, one from Belgium, one from England, and one from China. Participants were mostly students (24 studies) or came from the general population (nine studies). One study recruited Fibromyalgia Syndrome patients (Molinari, Garcia-Palacios, Enrique, Comella, & Botella, 2018). Regarding outcomes, most studies assessed positive affect (30 studies with 44 effect sizes), negative affect (26 studies with 37 effect sizes), optimism (20 studies with 33 effect sizes), pessimism (11 studies with 16 effect sizes), and life satisfaction (12 studies with 21 effect sizes). In addition, six studies (15 effect sizes) assessed depressive symptoms and four studies (10 effect sizes) assessed happiness. Positive affect was predominantly conceptualized as a state variable asking participants how they feel "at the moment" (27 effect sizes) or a trait-like variable asking participants how they felt "during the past day/week" (12 effect sizes). For negative affect, 25 effect sizes came from state and eight from trait-like assessments. Affect was typically assessed using the Positive and Negative Affect Schedule (PANAS; 21 studies; Watson & Clark, 1988). Some studies applied the Mood Rating Scale (two studies; Diener & Emmons, 1984), the Multidimensional State Mood Scale (MDBF; two studies; Steyer et al., 1994), and others used the Brief Mood Introspection Scale (BMIS; one study; Mayer & Gaschke, 1988), the modified Differential Emotion Scale (mDES; one study; Catalino, Algoe, & Fredrickson, 2014), the Scale of Positive and Negative Experiences (SPANE; one study; Diener et al., 2009), a mood scale developed by Barrett and Russel (1998; one study), or a visual analog scale (one study). Optimism was predominantly conceptualized as a trait-like variable asking participants to estimate the likelihood of positive future events or experiences (18 effect sizes) using either the Subjective Probability Task (SPT; five studies; MacLeod, 1996), the Future Expectations Scale (FEX; nine studies; Hanssen, Peters, Vlaeyen, Meevissen, & Vancleef, 2013), or a subscale of the Comprehensive Inventory of Thriving (CIT; one study; Su, Tay, & Diener, 2014). Eleven effect sizes came from trait assessments using the original or revised version of the Life Orientation Test (eight studies; Scheier, Carver, & Bridges, 1994) and two effect sizes from two studies were state assessments asking participants to rate their momentary optimism. Some studies used more than one measure to assess optimism. Pessimism was assessed using the SPT (eight effect sizes in four studies) or FEX (eight effect sizes in seven studies). Life-satisfaction was assessed using the Satisfaction With Life Scale (SWLS; 16 effect sizes in eight studies; Diener, Emmons,

Larsen, & Griffin, 1985), the Temporal Satisfaction With Life Scale (two effect sizes in two studies;

Pavot, Diener, & Suh, 1998), and the Brief Multidimensional Student's Life Satisfaction Scale (BMSLS;

one effect size in one study; Seligson, Huebner, & Valois, 2005), all asking participants how they feel

about their life "in general". One study assessed life satisfaction as a state variable asking participants

how satisfied they are "at the moment" and one study asking participants how satisfied they were "during

the past two weeks". Depressive symptom scales typically asked participants about symptoms "during the

past two weeks" using the revised version of the Beck Depression Inventory (BDI-II; six effect sizes in

two studies; Beck & Steer, 1984), the Center for Epidemiologic Studies Depression Scale (CES-D; three

effect size in two study; Radloff, 1977), or the German General Depression Scale (ADS; four effect sizes

in one study; Meyer & Hautzinger, 2001). One study asked for depressive symptoms "at the moment"

using the State-Trait Anxiety-Depression Inventory (Renner, Hock, Bergner-Köther, & Laux, 2018).

Happiness was assessed using the Authentic Happiness Inventory (AHI; four effect sizes in one study;

Proyer, Gander, Wellenzohn, & Ruch, 2017), the Steen Happiness Index (SHI; three effect sizes in one

study; Seligman et al., 2005), the Subjective Happiness Scale (SHS; one effect size in one study;

Lyubomirsky & Lepper, 1999), and the Happiness Measure (one effect size in one study; Fordyce, 1988).

5.3.4 Test of Hypotheses

We tested main and moderation effects for positive affect and optimism. Full results for other

outcomes were only reported if the test was sufficiently powered (at least 15 effect sizes; see Tables 5.2

and 5.3). We excluded effect sizes for follow-up assessments in the test of hypotheses because they were

rarely reported, and data did not allow for robust conclusions. Descriptive follow-up results are provided

in the additional analysis section. Moderation effects were tested if there were at least 15 effect sizes for a

given outcome and at least five effect sizes in each category of the moderator (see Hedges & Pigott, 2004,

for a discussion). Keep in mind that we did not perform an a priori power analysis for our moderator

analyses and that, given the number of studies we found and the average within-study sample size in

those studies, power to detect what we believe to be meaningful effects was low (approximately .40;

Valentine et al., 2010). As such, perhaps the most reasonable conclusion to be drawn from moderator

analyses that did not reach statistical significance is that we currently have not enough information to

judge adequately whether this study characteristic has a meaningful effect. Finally, the tested moderators

were not independent of each other, which we believe is very important to keep in mind when interpreting

the results (see Lipsey, 2003, for further discussion).

Positive affect. According to our first hypothesis, we expected that participating in the BPS intervention increases positive affect and that the effect is larger if (a) assessed at the final day of the intervention rather than several days later; and (b) momentary affect was measured rather than affect during the past week. Our results support Hypothesis 1. Specifically, Figure 5.2 and Table 5.2 show that participants in the BPS intervention on average scored 0.28, 95% CI [0.16, 0.41], standard deviations higher on positive affect than participants in the control condition. The unweighted mean difference after transforming results to a 0 to 100 scale was 5.94 points (58.50 in the intervention vs. 52.56 in the control condition). There was considerable heterogeneity in the data, $Q(df = 31) = 76.07$, $p = .000$, $I^2 = 61.26\%$, which means that the effect sizes displayed in Figure 5.2 significantly differ from one another. Such variations can, for example, be explained by differences in the characteristics of the included studies. As we predicted in Hypotheses 1a and 1b, Table 5.3 shows that time of measurement and conception of the outcome were significant moderators of the effect on positive affect, $F(1,28) = 7.27$, $p = .012$, and $F(1,25) = 11.22$, $p = .003$, respectively. Specifically, the average effect at immediate posttest was 0.39, 95% CI [0.22, 0.55], whereas the average effect at posttest was 0.12, 95% CI [0.00, 0.24]. In addition, the average effect for state variables was 0.41, 95% CI [0.25, 0.58], whereas it was 0.09, 95% CI [-0.03, 0.20], for trait like variables. Trait variables were not tested because they were only assessed in three studies. Figure 5.2 reveals that time of measurement and conception of the outcome were interdependent. For example, 19 out of 20 studies that assessed positive affect immediately after the intervention also used a state measure, whereas only 2 out of 12 studies that assessed positive affect several days after the intervention used a state measure.

According to our second hypothesis, we expected that the effect on positive affect is larger among (a) more motivated participants; (b) participants from more individualistic cultures; (c) studies that used more intense interventions; and (d) studies that used an imagery component. Our results do not support Hypothesis 2a, 2c, and 2d. Hypothesis 2b could not be tested because only three studies used collectivistic samples. Specifically, regarding Hypotheses 2a, results in Table 5.3 reveal that compensation of participants was not a significant moderator, $F(1,23) = 0.00$, $p = .953$. Regarding Hypothesis 2c, results in Table 5.3 show a significant moderation effect for length of the intervention, $F(2,27) = 3.40$, $p = .048$. The effect, however, was not in the expected direction. Specifically, the average effect for brief interventions was 0.37, 95% CI [0.17, 0.57], whereas it was 0.32, 95% CI [0.05, 0.66], for average interventions and 0.10, 95% CI [-0.02, 0.23], for extensive interventions. One explanation for this

Author (year)	Intervention mean	sd	n	Control mean	sd	n	g [95% CI]
Immediate posttest (at day of the intervention)							
State (at the moment)							
Boselie (2014)	32.79	8.41	38	29.14	7.47	36	0.45 [−0.01, 0.92]
Boselie (2016 study 1)	33.53	6.70	41	27.73	7.05	40	0.84 [0.38, 1.29]
Boselie (2016 study 2)	29.42	5.61	32	27.06	5.90	29	0.41 [−0.10, 0.91]
Geschwind (2015)	4.34	0.86	25	3.28	1.31	25	0.94 [0.36, 1.53]
Hanssen (2013)	83.48	10.63	40	75.85	14.34	39	0.60 [0.15, 1.05]
Harrist (2007)	4.02	0.94	19	3.49	0.66	20	0.64 [−0.00, 1.29]
Heekerens (2019b)	3.39	0.71	92	3.07	0.73	96	0.44 [0.15, 0.73]
Heekerens (u. rev.)	3.48	0.88	110	3.29	0.85	106	0.22 [−0.05, 0.49]
Heimes (2013)	33.21	10.51	14	21.77	7.97	13	1.18 [0.36, 2.00]
King (2001)	1.26	1.48	19	1.37	1.61	16	−0.07 [−0.74, 0.60]
Liau (2016)	30.93	8.85	81	27.07	9.75	81	0.41 [0.10, 0.72]
Meevissen (2012)	4.11	0.39	37	3.92	0.39	35	0.48 [0.01, 0.95]
Ng (2016)	4.73	1.37	118	4.44	1.25	98	0.22 [−0.05, 0.49]
Paulmichl (2019)	3.23	0.71	78	3.11	0.76	93	0.16 [−0.14, 0.46]
Peters (2010)	3.46	0.74	43	2.52	0.93	37	1.12 [0.65, 1.59]
Peters (2015)	35.50	6.00	28	30.00	7.70	28	0.79 [0.24, 1.33]
Renner (2014)	64.39	12.29	20	62.00	20.78	20	0.14 [−0.48, 0.76]
Summerfield (2016)	29.93	8.59	15	30.13	8.19	15	−0.02 [−0.74, 0.69]
Titova (2017)	3.26	0.99	123	3.41	0.88	124	−0.16 [−0.41, 0.09]
Trait like (during the past day/s or week/s)							
Heekerens (2019a)	3.95	0.68	66	3.94	0.72	62	0.01 [−0.33, 0.36]
Effect size for immediate posttest (Q = 64.99, df = 30, p = .000; I^2 = 54.21%)							0.39 [0.22, 0.55]
Posttest (one to six days after the intervention)							
State (at the moment)							
Molinari (2018)	2.50	0.78	23	2.18	0.79	28	0.40 [−0.16, 0.96]
Sheldon (2006)	4.04	0.59	23	3.60	0.78	23	0.63 [0.03, 1.22]
Trait like (during the past day/s or week/s)							
Auyeung (2018)	28.60	6.91	48	27.52	5.65	52	0.17 [−0.22, 0.56]
Heekerens (2019a)	3.90	0.71	66	3.93	0.72	62	−0.04 [−0.39, 0.30]
Heekerens (2019b)	3.38	0.69	90	3.32	0.66	93	0.09 [−0.20, 0.38]
Lyubomirsky (2011)	3.05	0.95	110	3.15	0.94	101	−0.11 [−0.38, 0.16]
Manthey (2016)	21.30	4.10	135	21.10	4.20	150	0.05 [−0.18, 0.28]
Meevissen (2011)	3.52	0.67	28	3.12	0.77	23	0.55 [−0.01, 1.11]
Tempel (2016)	3.42	0.75	94	3.26	0.85	120	0.20 [−0.07, 0.47]
Trait (usually/generally)							
Carillo (2018 study 1)	34.82	6.00	27	34.07	7.69	28	0.11 [−0.42, 0.64]
Carillo (2018 study 2)	20.52	5.46	27	21.40	4.84	25	−0.17 [−0.71, 0.38]
Enrique (2018)	3.37	0.72	33	3.39	0.61	33	−0.03 [−0.51, 0.45]
Effect size for posttest (Q = 64.99, df = 30, p = .000; I^2 = 54.21%)							0.12 [0.00, 0.24]
Overall effect size (Q = 76.07, df = 31, p = .000; I^2 = 61.26%)							0.29 [0.17, 0.41]

Standardized effect size Hedge's *g*

Figure 5.2 Forest plot of the effects of the best-possible-self intervention on positive affect relative to neutral controls separate for time of assessment and conceptualization of the outcome. Positive effect sizes favor the intervention condition. Random effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

Table 5.2 Main Effects of the Best-Possible-Self Intervention Relative to Neutral Controls at Immediate Posttest and Posttest (Combined)

Outcome	n	k	Hedge's g [95% CI]	Mean difference on 0-100 scale	Heterogeneity	Model	Test for overall effect
Positive Affect	3494	32	0.29 [0.17, 0.41]	5.94 (58.50 vs. 52.56)	$Q(df = 31) = 76.07$, $p = .000$, $I^2 = 61.26\%$	RE	$t = 4.71$, $p = .000$
Negative Affect	2984	28	-0.06 [-0.16, 0.05]	-0.43 (18.98 vs. 19.41)	$Q(df = 27) = 36.37$, $p = .107$, $I^2 = 21.57\%$	FE	$t = -1.13$, $p = .270$
Optimism	1949	21	0.21 [0.04, 0.38]	2.21 (73.40 vs. 71.19)	$Q(df = 20) = 39.87$, $p = .005$, $I^2 = 50.95\%$	RE	$t = 2.68$, $p = .016$
Pessimism	1041	12	-0.40 [-0.57, -0.24]	-6.27 (26.22 vs. 32.49)	$Q(df = 11) = 15.36$, $p = .167$, $I^2 = 21.98\%$	FE	(underpowered)
Life Satisfaction	1669	12	0.00 [-0.09, 0.09]	1.18 (67.99 vs. 66.81)	$Q(df = 11) = 6.95$, $p = .803$, $I^2 = 0.00\%$	FE	(underpowered)
Depressive Symptoms	716	5	-0.09 [-0.23, 0.06]	-2.01 (22.92 vs. 24.93)	$Q(df = 4) = 3.24$, $p = .518$, $I^2 = 0.15\%$	FE	(underpowered)
Happiness	480	3	0.04 [-0.14, 0.22]	0.70 (55.91 vs. 55.21)	$Q(df = 2) = 0.66$, $p = .721$, $I^2 = 0.00\%$	FE	(underpowered)

Note. We calculated the mean difference by transforming all means to fit on a 0 to 100 scale and then subtracting the overall mean of the control from the overall mean of the intervention condition. Keep in mind that effect sizes were weighted, whereas mean differences were not. n = number of participants in both conditions; k = number of effect sizes; RE = random effects model; FE = fixed effects model; CI = confidence interval. In order to derive robust estimates, we used t tests in FE models comprising dependent effect sizes.

pattern is that studies that used extensive interventions were less likely to assess outcomes at immediate posttest than studies that used brief or average interventions (1 out of 10 vs. 19 out of 22). In addition, they were less likely to use state measures (2 out of 10 vs. 19 out of 22). Another explanation relates to a significant moderation effect that we did not predict, the effect of delivery format, $F(1,28) = 13.44$, $p = .001$. Specifically, the average effect for online interventions was 0.05, 95% CI [-0.06, 0.16], whereas it was 0.40, 95% CI [0.24, 0.56], for in-person administrations. Note that online studies were more likely to administer more intense interventions with 6 out of 9 online studies using an extensive intervention compared with 4 out of 23 in-person studies. Thus, it could be that the extensive interventions in our analysis turned out to be less effective because they were more likely to be delivered online. Finally, regarding Hypothesis 2d, results in Table 5.3 reveal no significant moderation effect of imagery component, $F(1,28) = 2.05$, $p = .164$.

Optimism. According to our first hypothesis, we expected that participating in the BPS intervention increases optimism and that the effect is larger if (a) assessed at the final day of the intervention rather than several days later; and (b) based on future expectations rather than life orientation. Our results support Hypothesis 1. Specifically, Figure 5.3 and Table 5.2 show that participants in the BPS intervention on average scored 0.21, 95% CI [0.04, 0.38], standard deviations higher on optimism than participants in the control condition. The unweighted mean difference after transforming results to a 0 to 100 scale was 2.21 points (73.40 in the intervention vs. 71.19 in the control condition). There was considerable heterogeneity in the data, $Q(df = 20) = 39.87$, $p = .005$, $I^2 = 50.95\%$, which may be explained by moderators. As we predicted in Hypotheses 2a and 2b, Table 5.3 shows that time of measurement and conception of the outcome were significant moderators of the effect on optimism, $F(1,15) = 12.02$, $p = .003$, and $F(1,12) = 55.64$, $p = .000$, respectively. Specifically, the average effect at immediate posttest was 0.36, 95% CI [0.18, 0.54], whereas it was -0.01, 95% CI [-0.18, 0.16], at posttest. In addition, the average effect for optimism conceptualized as future expectations (trait like) was 0.36, 95% CI [0.22, 0.50], whereas it was -0.14, 95% CI [-0.23, -0.04], for trait conceptualizations as a life orientation. We did not test assessments asking for participants momentary life orientation (state) because only three studies used this approach. Figure 5.3 reveals that time of measurement and conception of the outcome were interdependent. For example, 5 out of 7 studies that used a trait conceptualization also assessed positive affect several days after the intervention (posttest).

According to our second hypothesis, we expected that the effect on optimism is larger among (a)

Author (year)	Intervention			Control			g [95% CI]
	mean	sd	n	mean	sd	n	

Immediate posttest (at day of the intervention)

State (Life Orientation Test; at the moment)

Heekerens (u. rev.)	3.92	0.93	110	3.55	0.97	106	0.39 [0.12, 0.66]
Paulmichl (2019)	5.16	1.01	78	5.00	0.86	93	0.17 [−0.13, 0.47]
Peters (2015)	58.80	5.90	28	53.70	7.10	28	0.77 [0.23, 1.31]

Trait like (Future Expectation Test or Subjective Probability Task)

Boselie (2014)	56.08	5.91	38	53.47	6.77	36	0.41 [−0.05, 0.87]
Boselie (2016 study 1)	56.14	3.68	41	53.43	3.73	40	0.72 [0.27, 1.17]
Boselie (2016 study 2)	56.06	4.05	32	54.55	4.25	29	0.36 [−0.15, 0.87]
Hanssen (2013)	57.58	5.73	40	53.79	6.69	39	0.60 [0.15, 1.05]
Heekerens (2019b)	5.40	0.72	92	5.09	0.80	96	0.41 [0.12, 0.69]
Meevissen (2012)	5.60	0.47	21	5.29	0.79	34	0.44 [−0.11, 1.00]
Peters (2010)	5.71	0.68	43	5.33	0.68	38	0.55 [0.11, 1.00]

Trait (Life Orientation Test or Life Orientation Test Revised)

Heekerens (2019a)	4.01	0.69	66	4.17	0.70	62	−0.23 [−0.58, 0.12]
Peters (2013)	16.71	3.34	28	16.64	2.87	28	0.02 [−0.50, 0.55]

Effect size for immediate posttest (Q = 27.06, df = 19, p = .103; I^2 = 33.01%) — 0.36 [0.18, 0.54]

Posttest (one to six days after the intervention)

Trait like (Future Expectation Test or Subjective Probability Task)

Enrique (2018)	5.42	0.77	33	5.51	0.57	33	−0.13 [−0.61, 0.35]
Heekerens (2019b)	5.38	0.72	90	5.24	0.80	93	0.18 [−0.11, 0.47]
Meevissen (2011)	5.27	0.57	28	5.07	0.75	23	0.30 [−0.25, 0.85]
Molinari (2018)	4.44	0.99	23	4.29	1.20	28	0.13 [−0.42, 0.69]

Trait (Life Orientation Test or Life Orientation Test Revised)

Carillo (2018 study 1)	24.19	3.39	27	24.36	4.18	28	−0.04 [−0.57, 0.48]
Carillo (2018 study 2)	5.85	0.95	27	6.12	1.13	25	−0.26 [−0.80, 0.29]
Enrique (2018)	21.77	5.45	33	23.39	3.65	33	−0.35 [−0.83, 0.14]
Heekerens (2019a)	4.11	0.61	66	4.16	0.68	62	−0.08 [−0.42, 0.27]
Molinari (2018)	20.10	4.86	23	19.78	4.89	28	0.06 [−0.49, 0.62]

Effect size for posttest (Q = 27.06, df = 19, p = .103; I^2 = 33.01%) — −0.01 [−0.18, 0.16]

Overall effect size (Q = 39.87, df = 20, p = .005; I^2 = 50.95%) — 0.21 [0.04, 0.38]
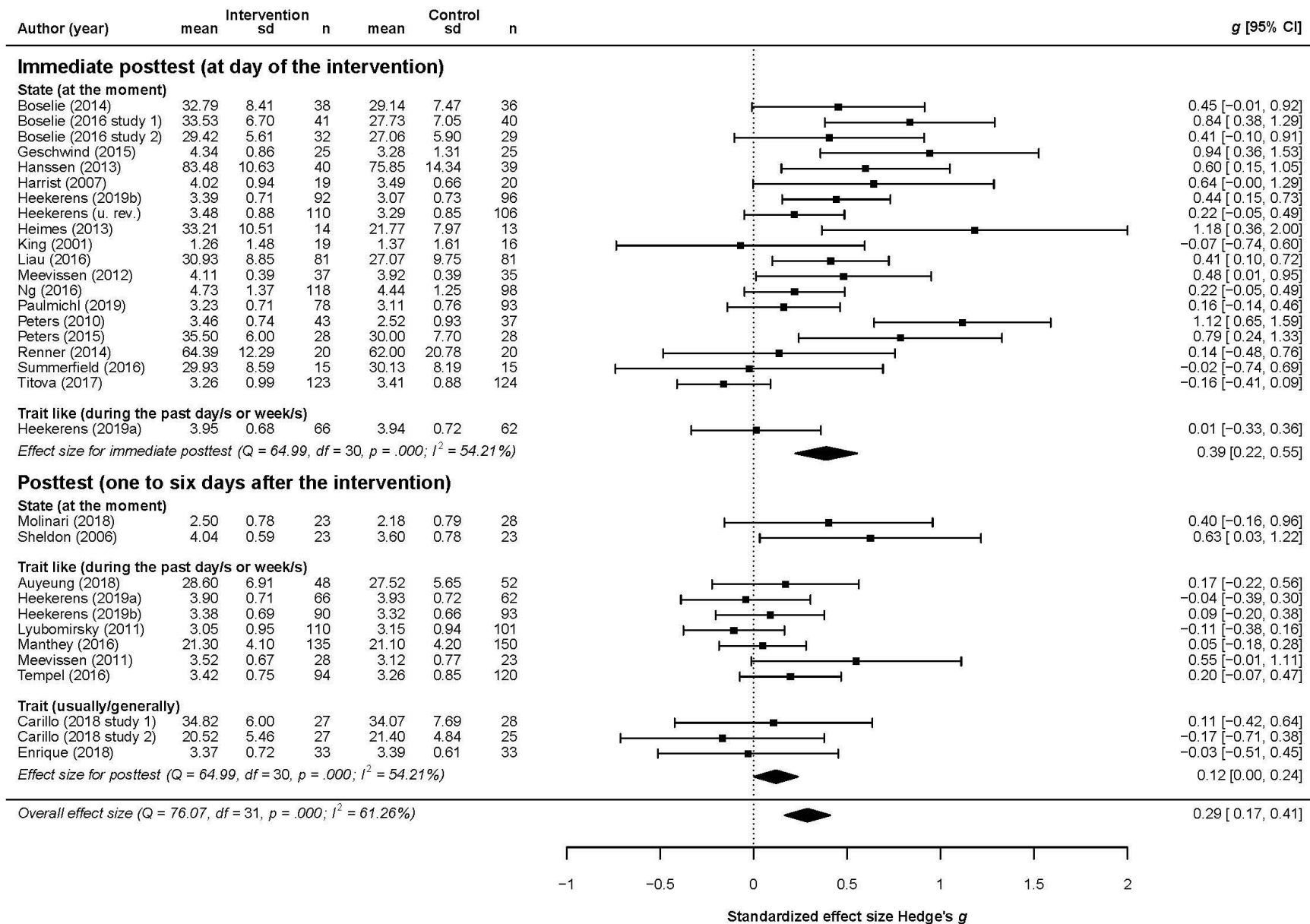
Standardized effect size Hedge's *g*
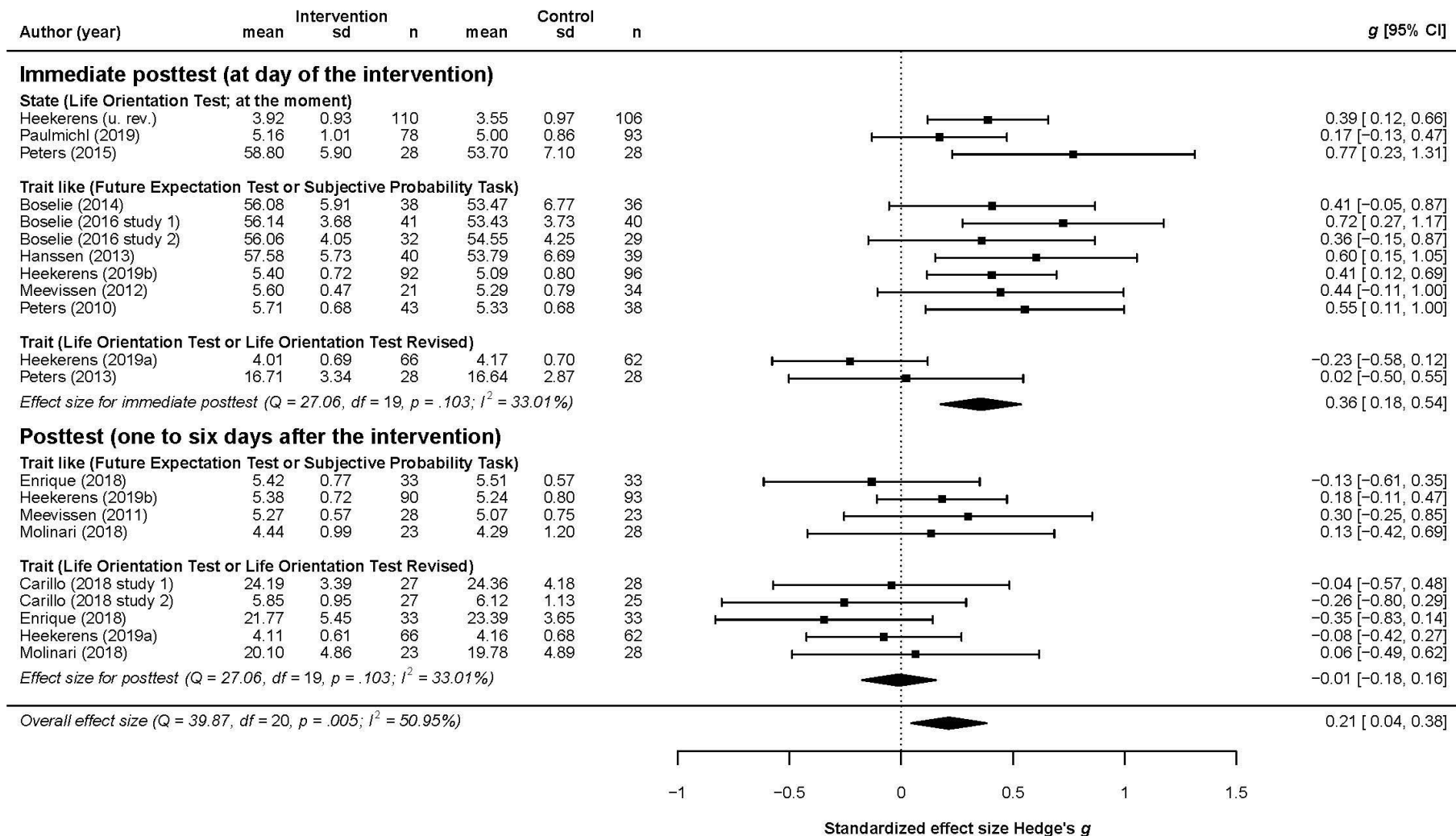
Figure 5.3 Forest plot of the effects of the best-possible-self intervention on optimism relative to neutral controls separate for time of assessment and conceptualization of the outcome. Positive effect sizes favor the intervention condition. Random effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

Table 5.3 Subgroup Analyses for Effects of the Best-Possible-Self Intervention Relative to Neutral Controls

Outcome	Moderator	Subgroup	k	Hedge's g [95% CI]	Mean difference on 0-100 scale	Test for subgroup differences
Positive Affect	Time of measurement	immediate posttest	20	0.39 [0.22, 0.55]	7.79 (59.55 vs. 51.76)	$F(1,28) = 7.27, p = .012$
		posttest	12	0.12 [0.00, 0.24]	2.85 (56.75 vs. 53.90)	
	Conception of outcome	state	21	0.41 [0.25, 0.58]	8.31 (58.61 vs. 50.30)	$F(1,25) = 11.22, p = .003$
		trait like	8	0.09 [-0.03, 0.20]	1.97 (59.86 vs. 57.89)	
		trait	3	(not tested)	(not tested)	
	Length of intervention	brief	17	0.37 [0.17, 0.57]	7.41 (64.06 vs. 56.61)	$F(2,27) = 3.40, p = .048$
		average	5	0.36 [0.05, 0.66]	8.21 (49.83 vs. 41.62)	
		extensive	10	0.10 [-0.02, 0.23]	2.23 (53.39 vs. 51.16)	
	Imagery component	yes	19	0.36 [0.17, 0.55]	6.13 (58.83 vs. 52.70)	$F(1,28) = 2.05, p = .164$
		no	13	0.20 [0.05, 0.34]	5.65 (58.02 vs. 52.37)	
	Delivery format	online	9	0.05 [-0.06, 0.16]	0.77 (54.04 vs. 53.27)	$F(1,28) = 13.44, p = .001$
		in person	23	0.40 [0.24, 0.56]	7.96 (60.25 vs. 52.29)	
	Culture	individualistic	29	(not tested)	(not tested)	(not tested)
		collectivistic	3	(not tested)	(not tested)	
	Theme	life in general	22	0.39 [0.22, 0.56]	7.75 (59.77 vs. 52.02)	$F(1,25) = 3.93, p = .058$
		life domains	10	0.18 [0.05, 0.31]	3.12 (55.55 vs. 52.43)	
	Compensation	yes	21	0.26 [0.12, 0.41]	4.76 (58.20 vs. 53.44)	$F(1,23) = 0.00, p = .953$
		no	11	0.28 [-0.08, 0.63]	5.85 (53.64 vs. 47.79)	
Negative Affect	Time of measurement	immediate posttest	18	-0.03 [-0.15, 0.10]	0.30 (15.99 vs. 15.69)	$F(1,24) = 2.12, p = .158$
		posttest	10	-0.10 [-0.21, 0.00]	-1.73 (24.36 vs. 26.09)	
	Conception of outcome	state	20	-0.07 [-0.19, 0.05]	-0.67 (13.49 vs. 14.16)	$F(1,22) = 0.12, p = .734$
		trait like	6	-0.04 [-0.22, 0.14]	-0.04 (38.00 vs. 38.04)	
		trait	2	(not tested)	(not tested)	
	Length of intervention	brief	12	-0.03 [-0.16, 0.09]	-1.30 (12.34 vs. 13.64)	$F(2,23) = 0.34, p = .723$
		average	7	-0.06 [-0.38, 0.27]	1.54 (31.85 vs. 30.31)	
		extensive	9	-0.09 [-0.17, -0.01]	-0.79 (17.83 vs. 18.62)	
	Imagery component	yes	18	-0.08 [-0.26, 0.10]	0.21 (19.20 vs. 18.99)	$F(1,24) = 0.28, p = .602$
		no	10	-0.03 [-0.13, 0.07]	-1.59 (18.57 vs. 20.16)	
	Delivery format	online	6	-0.02 [-0.12, 0.09]	0.15 (20.67 vs. 20.52)	$F(1,24) = 0.48, p = .494$
		in person	22	-0.08 [-0.23, 0.07]	-0.58 (18.52 vs. 19.10)	
	Culture	individualistic	26	(not tested)	(not tested)	(not tested)
		collectivistic	2	(not tested)	(not tested)	
	Theme	life in general	20	0.00 [-0.12, 0.13]	-0.03 (19.27 vs. 19.30)	$F(1,22) = 2.60, p = .121$
		life domains	8	-0.17 [-0.35, 0.01]	-1.18 (17.59 vs. 18.77)	
	Compensation	yes	19	-0.04 [-0.20, 0.11]	0.34 (20.05 vs. 19.71)	$F(1,20) = 0.29, p = .599$
		no	9	-0.09 [-0.22, 0.03]	-1.16 (15.47 vs. 16.63)	
Optimism	Time of measurement	immediate posttest	12	0.36 [0.18, 0.54]	4.31 (75.62 vs. 71.31)	$F(1,15) = 12.02, p = .003$
		posttest	9	-0.01 [-0.18, 0.16]	-0.58 (70.44 vs. 71.02)	
	Conception of outcome	state	3	(not tested)	(not tested)	
		trait like	11	0.36 [0.22, 0.50]	3.73 (73.95 vs. 70.22)	$F(1,12) = 55.64, p = .000$
		trait	7	-0.14 [-0.23, -0.04]	-2.22 (70.06 vs. 72.28)	
	Length of intervention	brief	11	0.36 [0.19, 0.53]	5.57 (78.05 vs. 72.48)	$F(1,14) = 9.06, p = .009$
		average	4	(not tested)	(not tested)	
		extensive	6	0.05 [-0.08, 0.19]	0.42 (68.87 vs. 68.45)	
	Imagery component	yes	19	(not tested)	(not tested)	(not tested)
		no	2	(not tested)	(not tested)	
	Delivery format	online	3	(not tested)	(not tested)	(not tested)
		in person	18	(not tested)	(not tested)	
	Culture	individualistic	21	(not tested)	(not tested)	(not tested)
		collectivistic	0	(not tested)	(not tested)	
	Theme	life in general	15	0.26 [0.04, 0.48]	2.90 (75.21 vs. 72.31)	$F(1,15) = 0.79, p = .388$
		life domains	6	0.10 [-0.20, 0.41]	0.49 (68.87 vs. 68.38)	
	Compensation	yes	11	0.24 [0.02, 0.46]	3.25 (74.59 vs. 71.34)	$F(1,12) = 1.89, p = .194$
		no	7	0.01 [-0.29, 0.30]	-0.47 (70.08 vs. 70.55)	

Note. We only reported moderator analyses if there were at least 15 effect sizes for a given outcome and at least 5 effect sizes for each category of the moderator. Effect sizes and confidence intervals were taken from random-effect models and corrected for dependencies between effect sizes. Mean differences were calculated by transforming all means to fit on a 0 to 100 scale and then subtracting the subgroup mean of the control from the subgroup mean of the intervention condition. Keep in mind that effect sizes were weighted, whereas mean differences were not. k = number of effect sizes; CI = confidence interval.

more motivated participants; (b) participants from more individualistic cultures; (c) studies that used more

intense interventions; and (d) studies that used an imagery component. Our results do not support

Hypothesis 2a and 2c. Hypothesis 2b and 2d could not be tested. Specifically, regarding hypotheses 2a,

results in Table 5.3 reveal that compensation of participants was not a significant moderator, $F(1,12) =$

1.89, $p = .194$. We did not test Hypothesis 2b because no study that assessed optimism used a

collectivistic sample. Regarding Hypothesis 2c, results in Table 5.3 show a significant moderation effect

for length of the intervention, $F(1,14) = 9.06$, $p = .009$. Again, the effect was not in the expected

direction. Specifically, results show that the average effect for brief interventions was 0.36, 95% CI [0.19,

0.53] compared with 0.05, 95% CI [-0.08, 0.19] for extensive interventions. As prior discussed, one

explanation for this pattern is that studies that used extensive interventions were less likely to use

immediate posttests compared with studies that used brief interventions (1 out of 6 vs. 9 out of 11). We

did not test average interventions because there were only four effect sizes in this category. Finally,

Hypothesis 2d was not tested because only two studies did not use an imagery component.

5.3.5 Additional Analyses

Other outcomes. In addition to positive affect and optimism, we examined effects on negative

affect, pessimism, life satisfaction, and happiness. Descriptive results indicate a decrease in pessimism

and no effects on the other outcomes. Specifically, Table 5.2 shows that the effect on pessimism was

-0.40, 95% CI [-0.57, -0.24], on negative affect -0.06, 95% CI [-0.16, 0.05], on depressive symptoms

-0.09, 95% CI [-0.23, 0.06], on life satisfaction -0.00 [-0.09, 0.09], and on happiness 0.04 [-0.14, 0.22]

(see Appendix for forest plots). Note that only the test of negative affect was sufficiently powered. We

used fixed-effect models and performed no moderator analyses because data were quite homogeneous (all

$I^2 < 40\%$; see Table 5.2).

Follow-up effects. Follow-up effects of the BPS intervention on the outcomes discussed prior are

shown in Figure 5.4. For positive affect effects seem to remain stable 30 days after the intervention and

disappear 60 days after the intervention. For optimism the plot shows no effects 30 days after the

intervention, but two effect sizes using the Life Orientation Test show higher optimism in the BPS

condition 60 days after the intervention. Given the substantial drop-out rates at follow-up assessments

(see description of included studies section), results should be interpreted with great care.

Comparison with gratitude interventions. Some of the included studies used a gratitude

intervention in addition to the BPS intervention and control condition. Wherever applicable, we compared

the effects of the gratitude intervention with the BPS intervention. We displayed the results in Figure 5.5, suggesting that the BPS intervention has a stronger effect on positive affect immediately after and up to a few days after the intervention (see Appendix for forest plots). Descriptive results also suggest that this effect may reverse approximately 30 days after the intervention. In addition, participants reported slightly lower levels of depressive symptoms after the gratitude compared with the BPS intervention in two studies. Be reminded that interpretations are preliminary and should be treated cautiously. We did not report comparisons with other interventions (e.g., expressive writing) because they were rarely reported (see Table 5.1).

5.3.6 Certainty of the Evidence

We assessed the certainty of the evidence following recommendations of the GRADE working group (Akl, Mustafa, Santesso, & Wiercioch, 2013). Specifically, we used the guideline development tool (GRADEpro, 2015) that assigns a level of certainty to each outcome under investigation using the categories "very low", "low", "moderate", and "high". All studies included in this meta-analysis used randomized controlled designs that are considered to result in highly certain results. However, this level of certainty can be called into question as a result of poor study quality, presence of publication bias, or presence of unexplained heterogeneity. The results of our judgments are displayed in Table 5.4. Assessments of the certainty of the evidence in part rely upon the risk of bias associated with the included studies (see Appendix for detailed results) and threat of publication bias (see Figure 5.6 for funnel plots). First, risk of bias generally differed between the included studies. We judged that risk of bias lowered confidence for the results for positive affect, optimism, pessimism, depressive symptoms, and happiness, but not negative affect and life satisfaction (see Table 5.4). The reason for this is that more than a third of the studies reporting positive affect, optimism, and pessimism did not effectively blind experimenters, which may have influenced the subjective outcomes in the observed direction (i.e., participants may have reported higher positive affect because experimenters expected this outcome and this expectation was somehow communicated to participants). In addition, most studies assessing depressive symptoms and happiness reported substantial overall drop-out rates (> 20% at posttest), which could have significantly biased results. Second, we conducted separate assessments of publication bias based on funnel plots for each outcome, if possible. Specifically, results from Egger's regression analysis reveal that the first funnel plot for positive affect depicted in Figure 5.6 was asymmetrical, $z = 2.74$, $p = .006$. Trim and fill results reveal that an estimated 6 ($SE = 3.65$) small and medium effect sizes were missing. After imputing

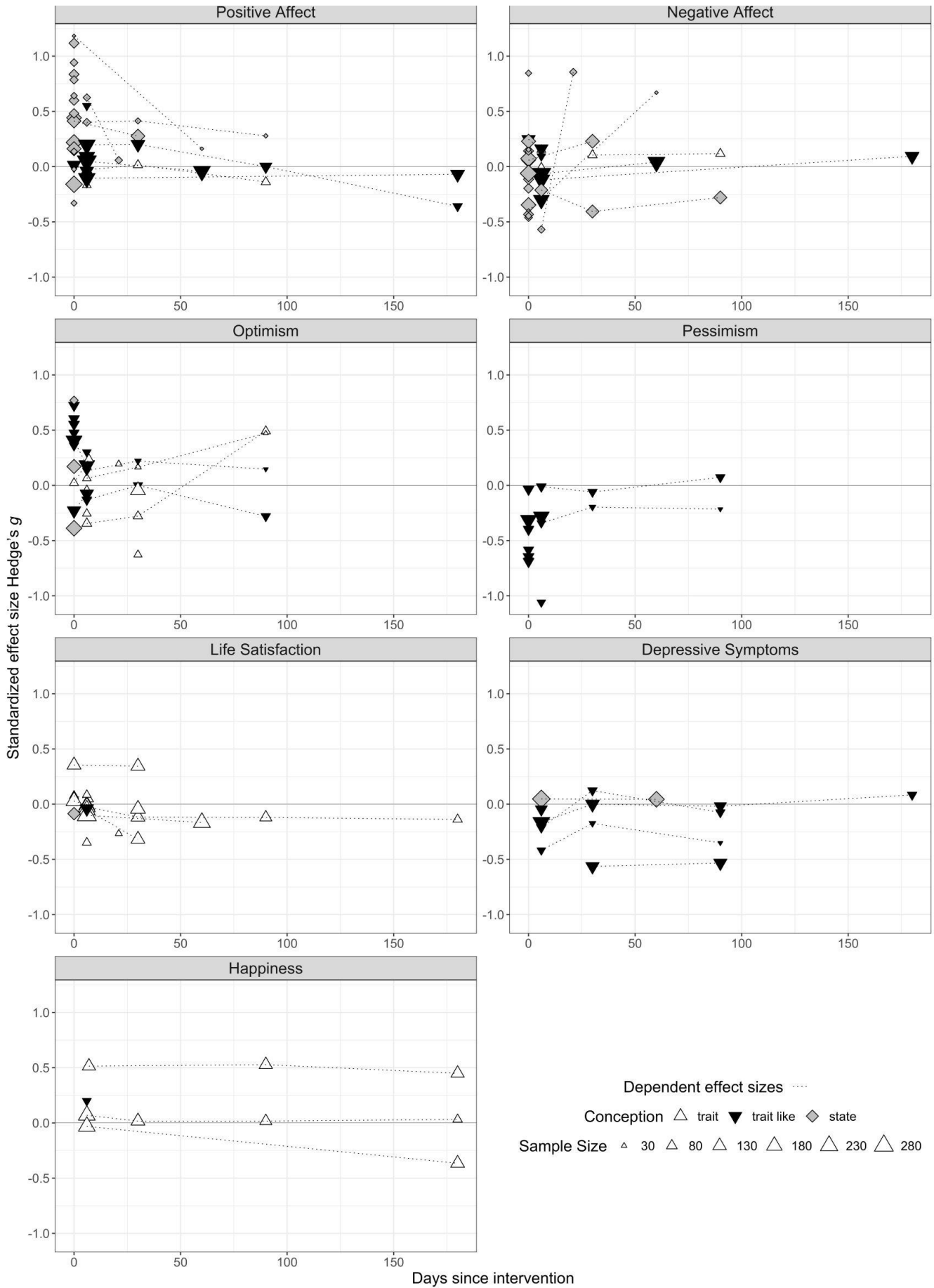**Figure 5.4** Descriptive effects of the best-possible-self intervention relative to neutral controls over time. Trait = "usually"; trait like = "during the past days/weeks"; state = "at the moment".
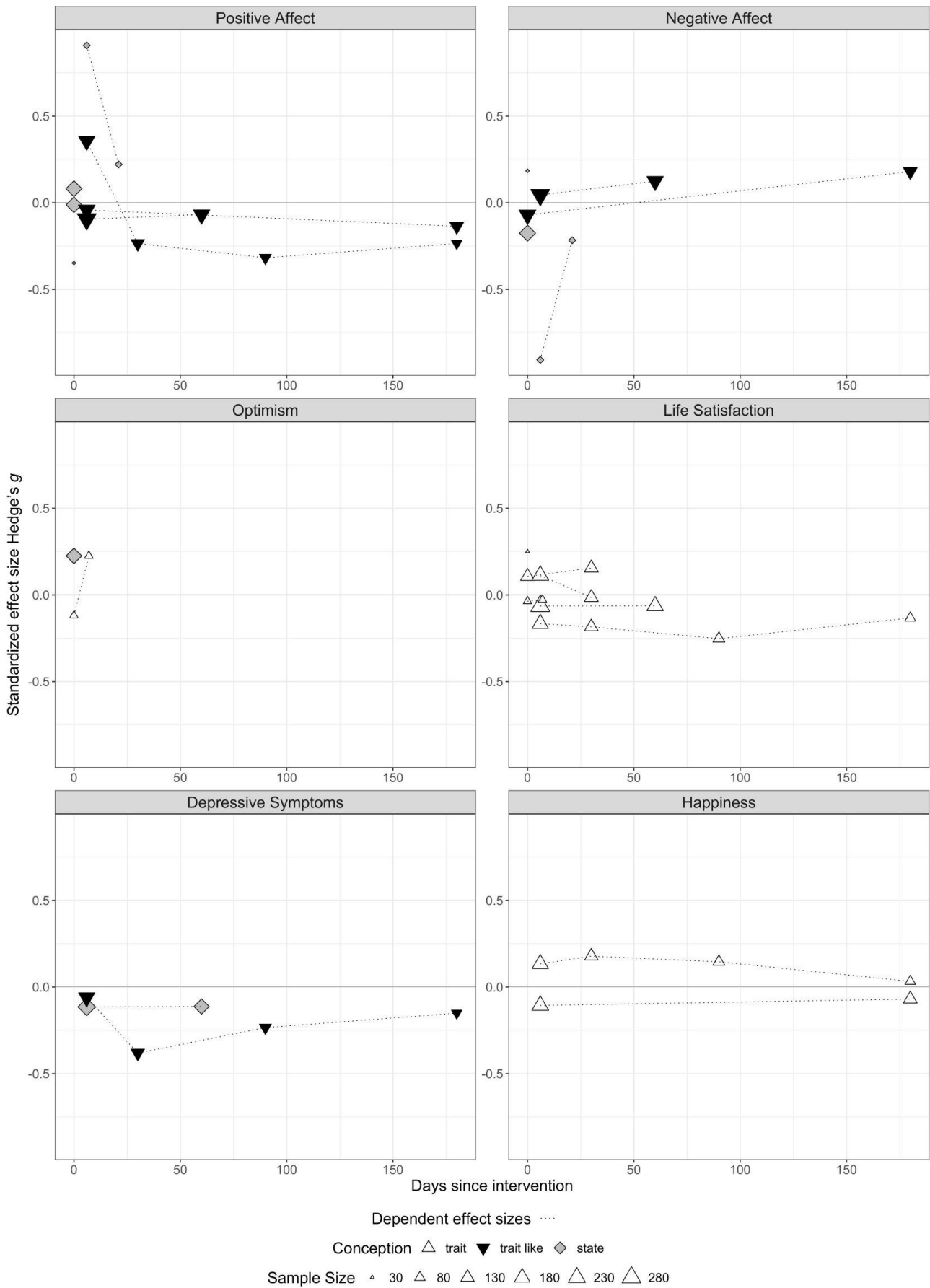
Figure 5.5 Descriptive effects of the best-possible-self intervention relative to gratitude interventions over time. Trait = "usually"; trait like = "during the past days/weeks"; state = "at the moment".

missing effect sizes the average effect was 0.17, 95% CI [0.02, 0.32]. Based on these results, we

concluded that the impact of the present publication bias was not severe, mainly because the finding that

the BPS intervention increases positive affect was not called into question. Regarding other outcomes,

results indicate no risk of publication bias. Specifically, for negative affect, results indicate no

asymmetry, $z = -0.15$, $p = .883$, and 1 ($SE = 3.19$) missing medium effect size. For optimism, results

indicate no asymmetry, $z = -0.25$, $p = .803$, and 0 ($SE = 2.57$) missing effect sizes. For pessimism, no

asymmetry, $z = -1.48$, $p = .139$, and 1 ($SE = 2.29$) missing small effect size. For life satisfaction, no

asymmetry, $z = 1.08$, $p = .279$, and 0 ($SE = 1.96$) missing effect sizes. We did not report funnel plots for

depressive symptoms and happiness because there were too few effect sizes for these outcomes (< 10;

Page et al., 2019). Furthermore, assessments of the certainty of the evidence were based on how precise

the overall effect size could be estimated. Specifically, our analysis was insufficiently powered to test

effect sizes for pessimism, depressive symptoms, life satisfaction, and happiness. We downgraded the

certainty of the evidence for these outcomes accordingly. Finally, notice that we did not downgrade the

certainty of the evidence due to considerable heterogeneity for positive affect and optimism because we

expected differences between studies and results from the moderator analyses discussed prior, at least

partially, explained these differences. Taken together, Table 5.4 shows that we judged the overall

certainty of the evidence for negative affect to be "high", whereas certainty was "moderate" for positive

affect and optimism, "low" for pessimism and life satisfaction, and "very low" for depressive symptoms

and happiness (see Akl et al., 2013, for a deeper discussion).

5.4 Discussion

The aim of this study was to comprehensively quantify the effects of the BPS intervention. In line

with our first hypothesis, results show that participating in the BPS intervention increases momentary

positive affect and positive future expectations at the day of the intervention. Other than predicted in our

second hypothesis, effects are not larger for more intense administrations of the intervention, among more

motivated participants, or in studies using an imagery component.

5.4.1 Main Effects of the Best-Possible-Self Intervention

According to our results, the BPS intervention causes small increases in self-reported momentary positive

affect and positive future expectations immediately after the exercise. There were no effects on trait

conceptualizations of the outcomes, namely habitual affect and optimistic life orientation. Present effects

Table 5.4 Assessment of the Certainty of the Evidence Using the GRADE Approach

№ of studies	Study design	Risk of bias	Inconsistency	Indirectness	Imprecision	Other considerations	the BPS intervention	active control condition	Relative (95% CI)	Absolute (95% CI)	Certainty	Importance
Positive Affect												
30	randomised trials	serious a	not serious	not serious	not serious	none	1743	1751	-	SMD **0.29 SD higher** (0.17 higher to 0.41 higher)	⊕⊕⊕◯ MODERATE	CRITICAL
Negative Affect												
28	randomised trials	not serious	not serious	not serious	not serious	none	1504	1480	-	SMD **0.06 SD lower** (0.16 lower to 0.05 higher)	⊕⊕⊕⊕ HIGH	CRITICAL
Optimism												
17	randomised trials	serious b	not serious	not serious	not serious	none	967	982	-	SMD **0.21 SD higher** (0.05 higher to 0.38 higher)	⊕⊕⊕◯ MODERATE	IMPORTANT
Pessimism												
11	randomised trials	serious c	not serious	not serious	serious d	none	524	517	-	SMD **0.4 SD lower** (0.57 lower to 0.24 lower)	⊕⊕◯◯ LOW	IMPORTANT
Life Satisfaction												
10	randomised trials	not serious	not serious	not serious	very serious e	none	820	849	-	SMD **0 SD** (0.09 lower to 0.09 higher)	⊕⊕◯◯ LOW	CRITICAL
Depressive Symptoms												
5	randomised trials	serious f	not serious	not serious	very serious e	none	333	383	-	SMD **0.09 SD lower** (0.23 lower to 0.06 higher)	⊕◯◯◯ VERY LOW	NOT IMPORTANT
Happiness												
3	randomised trials	serious g	not serious	not serious	very serious e	none	231	249	-	SMD **0.04 SD higher** (0.14 lower to 0.22 higher)	⊕◯◯◯ VERY LOW	IMPORTANT

CI: Confidence interval; **SMD:** Standardised mean difference

Explanations

a. Experimenter not blinded in 11 out of 30 studies (8 unclear).
b. Experimenter not blinded in 10 out of 17 studies (3 unclear).
c. Experimenter not blinded in 6 out of 11 studies (3 unclear).
d. Test was insufficiently powered (less than 15 effect sizes; see power analysis section).
e. Test was insufficiently powered (less than 15 effect sizes; see power analysis section). Possibility of relevant benefit and harm after the intervention.
f. Substantial overall drop-out (> 20%) in 4 out of 5 studies.
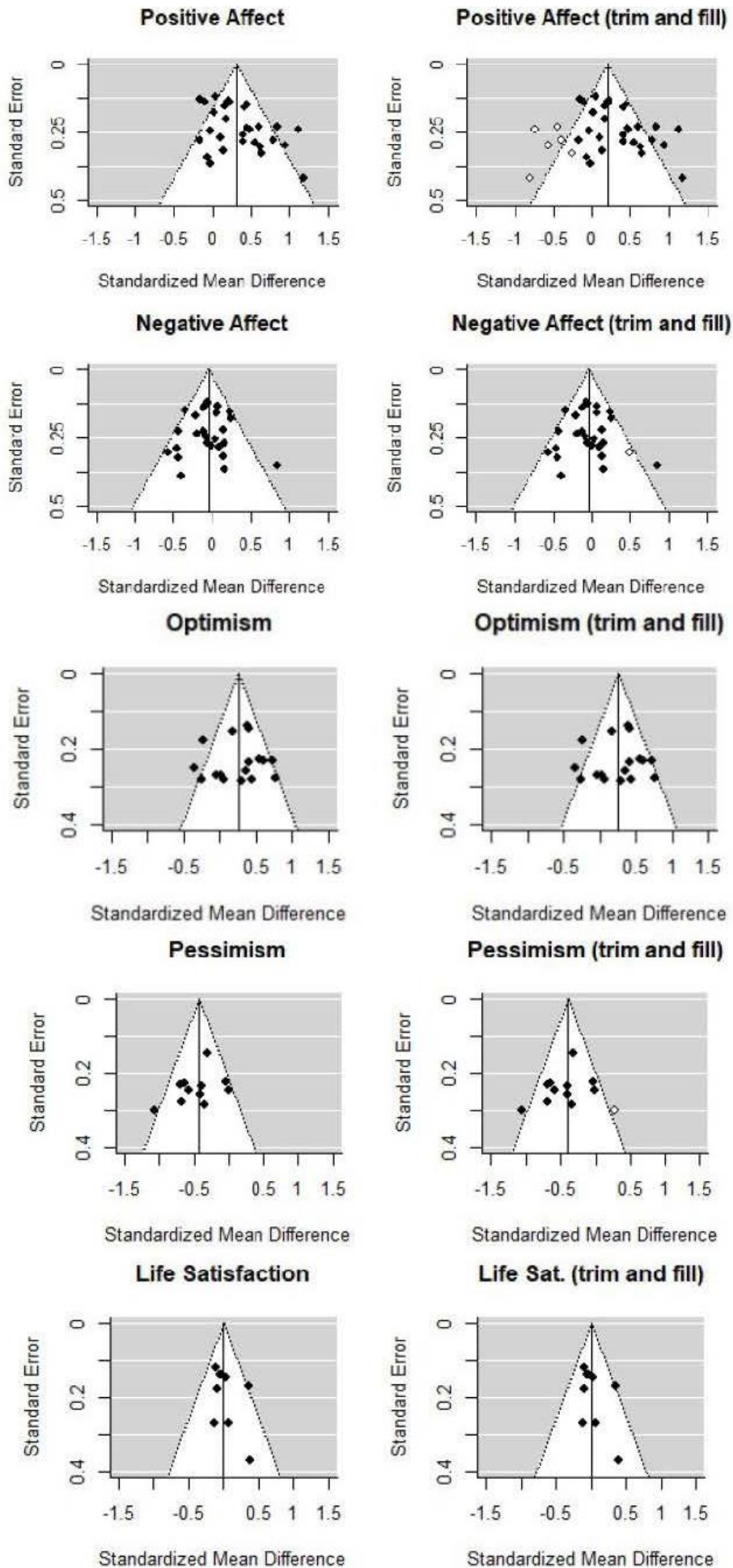g. Substantial overall drop-out (> 20%) in 2 out of 3 studies.

Figure 5.6 Funnel plots for comparison of the best-possible-self intervention with neutral controls.

of the intervention were strongest at the day of the intervention and effect sizes dwindled in the days after the intervention. This finding adds to existing meta-analyses (Carrillo et al., 2019; Malouff & Schutte, 2016) by showing that the effects of the BPS intervention might be more transient than previously assumed. From a theoretical perspective, the finding that the BPS intervention simultaneously affects positive affect and positive future expectations is in line with the process model of emotion regulation (Quoidbach, Mikolajczak, & Gross, 2015) that posits that inducing an optimistic outlook encourages positive emotions. Our data were, however, insufficient to determine whether the increase in positive future expectations was responsible for the increase in positive affect and this issue is still under debate (e.g., Heekerens et al., 2019). Another question, which has not been sufficiently addressed by previous reviews, is, whether the effects of the BPS intervention are meaningful to participants who wish to increase their well-being. Results from this meta-analysis suggest that participants' levels of positive affect will on average increase by approximately 7 points on a 0 to 100 scale immediately after the BPS intervention. For positive future expectations the increase is approximately 3 points. For both outcomes, effects decline over time and wash out approximately one week after the intervention. Based on these findings, our answer to the above question of whether effects are meaningful to happiness seekers is that it depends. Probably, the question is best answered in context, which can be provided by comparing the BPS intervention with procedures that result in similar effects. For example, the effect size we found for momentary positive affect is comparable with effects reported after experimentally inducing success (see Nummenmaa & Niemi, 2004, for a meta-analysis) or reading and reflecting on positive affirmations (see Westermann, Spies, Stahl, & Hesse, 1996, for a meta-analysis). Both procedures are typically used to induce positive affect in the laboratory. Hence, we suggest that researchers and practitioners might think of the BPS intervention as a mood and optimism induction procedure rather than a positive-psychological intervention, at least when the BPS intervention is administered on a single occasion or on three consecutive days for 20 minutes (e.g., King, 2001). One important difference between mood induction procedures and positive-psychological interventions is that the latter promise lasting changes in well-being (Bolier et al., 2013; Sin & Lyubomirsky, 2009), which our results do not support following the BPS intervention. Further developing the BPS intervention and integrating the BPS intervention into multiple component well-being programs may help to bolster effects (see Hendriks, Schotanus-Dijkstra, Hassankhan, Jong, & Bohlmeijer, 2019; Sheldon, Boehm, & Lyubomirsky, 2013, for a discussion and examples). Currently, if a participant wishes a temporary boost in happiness, that person is well advised

to give the BPS intervention a chance, especially as it is easy to implement and free of charge. If, however, lasting changes in happiness are the aim, more intense programs are required. Finally, other than one previous study, descriptive results of our meta-analysis suggest no effect on life satisfaction (Boehm et al., 2011).

5.4.2 Contextual and Person-Specific Moderators

Another question we asked was for whom and under which conditions effects of the BPS intervention are strongest. As mentioned, results do not support any of our moderator hypotheses. However, additional analysis results reveal that online administrations of the BPS intervention had no effect on positive affect. This finding is in sharp contrast to results from one experimental study that suggested that online and in-person administrations are equally effective (Layous et al., 2013). What puzzles us is that other well-being interventions have been convincingly shown to be effective when delivered through the internet (see Spijkerman, Pots, & Bohlmeijer, 2016, for the example of mindfulness-based interventions). One possible explanation for absent effects in online BPS trials is that the online interventions were poorly designed (Bolier & Abello, 2014). For example, designs may have failed to sufficiently engage users in the activity. In addition, online applications of the BPS intervention might only be effective for a subgroup of participants with specific needs, whereas in-person administrations are more accessible to the average participant (see Sanders, Schueller, Parks, & Howell, 2019, for preliminary evidence). Likewise, studies that compensated participants for doing the BPS intervention, which we used as a proxy for the degree of extrinsic motivation, were no less effective than those that did not. This finding contradicts earlier studies that highlight the importance of motivation as a moderating variable in BPS trials (Layous et al., 2013; Lyubomirsky et al., 2011). One explanation for the absent moderation effect is that receiving money or course credit does not interfere much with participants motivation to perform the exercise and hence other indicators of motivation might more successfully predict intervention success. Finally, results do not allow for firm conclusions regarding who might benefit most from doing the BPS intervention or how the exercise should be delivered to achieve optimal results. Oftentimes the interpretation of moderator analyses in our meta-analysis was complicated by the fact that certain study characteristics were more likely to appear together. For example, other than expected, more intense administrations of the BPS intervention did not result in larger effects. This finding can, at least in part, be explained by the fact that longer interventions were more likely to be delivered online and used less sensitive outcome measures (e.g., trait measures). Thus, we currently do

not know whether longer administrations were no more effective than brief administrations or whether small effects were caused by the poor designs of online BPS trials or by how the outcome was assessed. The same holds true when interpreting the subgroup effects of studies that used or not used an imagery component (Peters et al., 2010) and studies that compensated or not compensated for participation, which we used as a proxy for the extend, to which participants were extrinsically motivated.

5.4.3 Future Research

While our meta-analysis provides a detailed account of the effects of the BPS intervention on positive affect and optimism, a systematic review of the included studies reveals several partially unanswered questions and three important topics that we believe future research should address.

First, despite noticeable efforts to identify mediators of positive-psychological interventions (e.g., Fredrickson, Cohn, Coffey, Pek, & Finkel, 2008; Heekerens & Heinitz, 2019; Schotanus-Dijkstra, Pieterse, Drossaert, Walburg, & Bohlmeijer, 2019), we still know little about the underlying psychological mechanisms of the BPS intervention. Specifically, future research should seek to clarify whether effects on positive affect can be explained by induced optimistic thinking (as proposed by Quoidbach et al., 2015) or whether optimistic thinking is a result of the mood induction (more in line with Fredrickson, 2004). A third possibility is that the activation of positive self-relevant thoughts explains increases in both optimistic thinking and positive affect (Heekerens, Heinitz, Eid, & Merkle, submitted; Mongrain & Anselmo-Matthews, 2012). One way to test mediators is to administer the BPS intervention as part of larger programs that aim to increase well-being through teaching optimism or hope (Luthans, Avey, Avolio, Norman, & Combs, 2006) and then track the changes in positive affect, positive future expectations, and positive self-relevant thinking throughout the training period. At the end of the program researchers can test if changes in the potential mediators predict lasting increases in emotional well-being or other training effects. Embedding the BPS intervention in larger programs is important to ensure significant effects on outcomes several days or several weeks post implementation. Second, there is an urgent need and growing interest in understanding who generally profits from positive-psychological interventions (e.g., Lyubomirsky et al., 2011; Wang et al., 2017) and who is best served with which positive-psychological intervention (e.g., Schueller, 2010). Researchers should explicitly test how compensating participants in positive-psychological intervention trials affects motivation to participate and whether effects on well-being differ from participants who receive no compensation (also see Parks et al., 2012). In addition, we recommend that future studies clarify which personal characteristics, for

example, baseline level of emotional well-being, personality, or habitual explanatory style, successfully predict individual outcomes of the BPS intervention (Ng, 2016; Peters et al., 2015). Keep in mind, however, that the anticipated interaction effects are probably small and hence, moderator analyses will require large samples in order to be sufficiently powered. In addition, it remains largely unclear how cultural background influences the effects of the BPS intervention and future studies should also deliver the BPS intervention to participants from collectivistic cultures (see Hendriks, Warren et al., 2019, for a review and discussion). Third, researchers should systematically examine the effects of different doses of the BPS intervention. For example, researchers could test if differences emerge between groups writing for 10, 20, 30, or 40 minutes or whether results differ after a second or third administration of the intervention. Wherever applicable, researchers should seek to experimentally manipulate levels of the moderator in order to rule out alternative explanations and prevent statistical power issues (see Lyubomirsky et al., 2011, for an example). Finally, future studies should investigate effects on promising new outcomes such as experienced goal ambivalence (e.g., Heekerens et al., 2019) or meaning in life, and compare effects against other positive-psychological interventions or expressive writing to identify unique and shared effect patterns (e.g., Heekerens et al., submitted; King, 2001).

In general, researchers who use the BPS intervention should sufficiently blind experimenters (e.g., by using recorded instructions), explicitly state how the randomization sequence was generated (e.g., using a computer algorithm), and ensure allocation concealment to rule out potential risk of bias. In addition, a priori power analyses should be performed to determine required sample sizes. We recommend using the effect sizes reported in this study to inform such analyses. Longitudinal studies might use money incentives to lower drop-out rates. However, keep in mind that this likely interferes with participants intrinsic motivation to engage in the activity, which may result in smaller effects. Finally, future meta-analyses on the effects of positive psychological interventions should test how time of measurement and conceptualization of the outcome influence results. Existing meta-analysis (e.g., on the effects of gratitude interventions; Davis et al., 2016) could be reanalyzed against this background.

5.4.4 Limitations and Conclusion

Several limitations of our analyses should be mentioned. First, the certainty of the evidence for our main outcomes, positive affect and optimism, was only moderate. One reason for this was that experimenters were not blinded in many studies, which could have biased results. Although it is possible

to successfully blind experiments in some contexts (e.g., online studies or studies using recorded instructions), we cannot think of a practicable way to deal with this issue in studies that administer the intervention in-person. Second, the number of effect sizes for several outcomes and in many subgroups was too small to draw firm conclusions. Specifically, moderator analyses were typically insufficiently powered and non-significant results should be interpreted carefully as existing effects may have been overlooked. Third, we used posttest differences in outcomes to calculate effect sizes because this allowed us to include a maximum number of studies with a high quality. Although this approach seems appropriate given that only randomized controlled trials were included, which should rule out selection effects, effect sizes based on pretest-posttest differences could have explicitly controlled for this potential source of bias (Morris & DeShon, 2002). We did not choose to use pretest-posttest differences to calculate effect sizes because doing so would have reduced the number of studies in this meta-analysis from 34 to 16. Forth, in line with our inclusion criteria, studies included in this meta-analysis predominantly applied the BPS intervention to students and participants from the general population. Effects can and should not be generalized to other populations such as children (Owens & Patterson, 2013) and the psychologically distressed (Huffman et al., 2014).

In conclusion, this meta-analysis shows that the BPS intervention can be effective in inducing positive affect and positive future expectations. We hope that future research illuminates the effect mechanisms underlying the intervention and further develops best practice recommendations on how and to whom it should be delivered. Finally, it is currently unclear whether the BPS intervention might be a powerful component in more extensive well-being programs and a handy tool for coaches, consultants, and mental health experts.

5.5 References

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5.6 Appendix

A. Search Strings

Database: PsycINFO and PsycARTICLES (EBSCO)

String:

1. "best-possible-sel*" [TITLE/ABSTRACT]

2. "best possible sel*" [TITLE/ABSTRACT]

3. "positive psycholog* intervention*" [TITLE/ABSTRACT]

4. "positive writing" [TITLE/ABSTRACT]

5. "optimis* intervention*" [TITLE/ABSTRACT]

6. "optimis* writing" [TITLE/ABSTRACT]

7. #1 OR #2 OR #3 OR #4 OR #5 OR #6

8. randomized [TITLE/ABSTRACT]

9. randomly [TITLE/ABSTRACT]

10. trial [TITLE/ABSTRACT]

11. groups [TITLE/ABSTRACT]

12. conditions [TITLE/ABSTRACT]

13. #8 OR #9 OR #10 OR #11 OR #12

14. #7 AND #13

Date: 05/02/2019

Hits: 229 (PsycINFO: 217, PsycARTICLES: 12)

Database: ProQuest (PQDT) dissertations and theses

String:

1. "best-possible-sel*" [TITLE/ABSTRACT]

2. "best possible sel*" [TITLE/ABSTRACT]

3. "positive psycholog* intervention*" [TITLE/ABSTRACT]

4. "positive writing" [TITLE/ABSTRACT]

5. "optimis* intervention*" [TITLE/ABSTRACT]

6. "optimis* writing" [TITLE/ABSTRACT]

7. #1 OR #2 OR #3 OR #4 OR #5 OR #6

8. randomized [TITLE/ABSTRACT]

9. randomly [TITLE/ABSTRACT]

10. trial [TITLE/ABSTRACT]

11. groups [TITLE/ABSTRACT]

12. conditions [TITLE/ABSTRACT]

13. #8 OR #9 OR #10 OR #11 OR #12

14. #7 AND #13

Date: 04/02/2019

Hits: 49

Database: PubMed (NLM)

String:

1. "best-possible-sel*" [TITLE/ABSTRACT]

2. "best possible sel*" [TITLE/ABSTRACT]

3. "positive psycholog* intervention*" [TITLE/ABSTRACT]

4. "positive writing" [TITLE/ABSTRACT]

5. "optimis* intervention*" [TITLE/ABSTRACT]

6. "optimis* writing" [TITLE/ABSTRACT]

7. #1 OR #2 OR #3 OR #4 OR #5 OR #6

Date: 05/02/2019

Hits: 8

B. Excluded Studies

Table 5.A1 Characteristics of Excluded Randomized Controlled Trials Examining the Effects of the Best-Possible-Self Intervention

Author (year)	Publication type	Country	Population	Mean age (SD or range)	Female (%)	Session (number), duration	Homework (number), duration	Control group	Other intervention group	N (immediate post), attrition (%)	N (post), attrition (%)	N (follow-up post), attrition (%)	Outcome measures (conception)	Reason for exclusion
Austenfeld (2006)	Article	USA	Students	26.41 (4.04)	45.31%	3, 25 min, imagery: no	-	Past 24 hrs.	Expressive writing	-	-	90 days, Ne = 21 Nc = 21 No = 22	PA: PANAS-X, NA: PANAS-X, Depression: CES-D (trait like)	Information required to calculate effect size missing
Austenfeld (2008)	Article	USA	Students	19.00	69.84%	3, 20 min, imagery: no	-	Past 24 hrs.	Expressive writing	-	N = 63, 0.00%	-	PA: PANAS-X, NA: PANAS-X, Depression: CES-D (trait like)	Information required to calculate effect size missing
Murn (2013)	Doctoral Thesis	USA	Students	25.14 (5.68)	67.86%	3, 20 min, imagery: no	-	Past 24 hrs.	-	-	-	42 days, Ne = 14 Nc = 14, 0.00%	Self-esteem: RSE (trait)	Information required to calculate effect size missing
Odou (2013)	Article	Australia	General population	34.00 (13.99)	74.76%	7, without specified time, imagery: yes	-	Wait list	Three good things	-	Ne = 21 Nc = 30 No = 25, 64.00%	14 days, Ne = 14 Nc = 13 No = 11, 82.00%	PA: PANAS (trait like), NA: PANAS (trait like)	Used a passive control condition
Seear (2013)	Article	Australia	General population	34.00 (13.97)	75.36%	7, without specified time, imagery: yes	-	Wait list	Three good things	-	Ne = 21 Nc = 29, No = 26, 63.99%	14 days, Ne = 14 Nc = 12 No = 11, 82.46%	PA: PANAS (trait like), NA: PANAS (trait like)	Used a passive control condition
Waits (2017)	Doctoral Thesis	USA	Students	19.53 (3.12)	70.10%	3, 20 minutes, imagery: no	-	Early memories	Gratitude (three good things)	Ne = 38, Nc = 45, No = 61, 0.00%	-	28 days, Ne = 38, Nc = 45, No = 61, 0.00%	PA: PANAS (trait like), NA: PANAS (trait like)	Information required to calculate effect size missing
Gibson (2018)	Article	United Kingdom	Diabetes patients	48.66 (16.99)	76.00%	1, 10 min, imagery: yes	Use as often as desired	Wait list	-	-	-	28 days, N = 50 0.00%	PA: PANAS (trait like), NA: PANAS (trait like), Depression: HADS (trait like)	Used a passive control condition

Note. We defined state measures as assessments using instructions such as "at the moment", whereas trait like measures used "during the past days or weeks" and trait measures used "usually" or "generally". CES-D = Center for Epidemiologic Studies Depression Scale; HADS = Hospital Anxiety and Depression Scale; PANAS = Positive and Negative Affect Schedule; PANAS-X = Positive and Negative Affect Schedule Extended; RSE = Rosenberg Self-Esteem Scale; SD = standard deviation; N = number of participants; Ne = number of participants in the intervention condition; Nc = number of participants in the control condition.

C. Forest Plots

Author (year)	Intervention mean	sd	n	Control mean	sd	n	g [95% CI]
Immediate posttest (at day of the intervention)							
State (at the moment)							
Boselie (2014)	11.82	2.78	38	12.36	2.65	36	−0.20 [−0.65, 0.26]
Boselie (2016 study 1)	13.80	2.51	32	13.71	2.64	29	0.03 [−0.47, 0.54]
Boselie (2016 study 2)	13.09	3.54	41	12.58	3.58	40	0.14 [−0.29, 0.58]
Geschwind (2015)	1.17	0.17	25	1.32	0.42	25	−0.46 [−1.02, 0.10]
Hanssen (2013)	14.58	15.14	40	22.21	19.55	39	−0.43 [−0.88, 0.01]
Harrist (2007)	1.35	0.71	19	1.71	0.86	20	−0.45 [−1.08, 0.19]
Heekerens (2019b)	1.40	0.40	92	1.57	0.56	96	−0.35 [−0.63, −0.06]
Heimes (2013)	12.14	2.41	14	14.00	6.04	13	−0.40 [−1.16, 0.36]
King (2001)	2.36	0.91	21	1.61	0.80	15	0.85 [0.16, 1.54]
Liau (2016)	17.27	6.87	81	16.88	6.49	81	0.06 [−0.25, 0.37]
Ng (2016)	3.23	1.55	118	3.12	1.47	98	0.07 [−0.20, 0.34]
Paulmichl (2019)	1.63	0.65	78	1.49	0.57	93	0.23 [−0.07, 0.53]
Peters (2010)	1.25	0.33	43	1.29	0.36	37	−0.12 [−0.55, 0.32]
Peters (2015)	12.20	3.10	28	12.40	3.50	28	−0.06 [−0.58, 0.46]
Renner (2014)	13.53	12.45	20	11.90	11.29	20	0.13 [−0.49, 0.75]
Summerfield (2016)	16.13	10.61	15	14.67	7.68	15	0.15 [−0.56, 0.87]
Titova (2017)	1.73	0.81	123	1.78	0.82	124	−0.06 [−0.31, 0.19]
Trait like (during the past day/s or week/s)							
Heekerens (2019a)	3.65	0.71	66	3.47	0.74	62	0.25 [−0.10, 0.59]
Effect size for immediate posttest (Q = 35.27, df = 26, p = .106; I² = 26.29%)							−0.03 [−0.14, 0.09]
Posttest (one to six days after the intervention)							
State (at the moment)							
Molinari (2018)	2.01	1.06	71	2.23	1.00	71	−0.21 [−0.54, 0.12]
Sheldon (2006)	1.43	0.42	23	1.74	0.63	23	−0.57 [−1.16, 0.02]
Trait like (during the past day/s or week/s)							
Heekerens (2019a)	3.74	0.73	66	3.62	0.74	62	0.16 [−0.18, 0.51]
Heekerens (2019b)	1.76	0.68	90	1.96	0.64	93	−0.30 [−0.59, −0.01]
Lyubomirsky (2011)	1.89	0.93	110	2.01	1.07	101	−0.12 [−0.39, 0.15]
Manthey (2016)	15.30	4.60	135	15.60	4.60	150	−0.07 [−0.30, 0.17]
Meevissen (2011)	1.53	0.63	28	1.47	0.64	23	0.09 [−0.46, 0.64]
Trait (usually/generally)							
Carillo (2018 study 2)	8.37	4.16	27	8.40	4.57	25	−0.01 [−0.55, 0.54]
Carillo (2018 study 1)	16.74	4.58	27	16.04	4.48	28	0.15 [−0.38, 0.68]
Enrique (2018)	1.49	0.36	33	1.53	0.56	33	−0.08 [−0.57, 0.40]
Effect size for posttest (Q = 35.27, df = 26, p = .106; I² = 26.29%)							−0.10 [−0.20, −0.00]
Overall effect size (Q = 36.37, df = 27, p = .107; I² = 25.77%)							−0.06 [−0.16, 0.05]

Standardized effect size Hedge's *g*

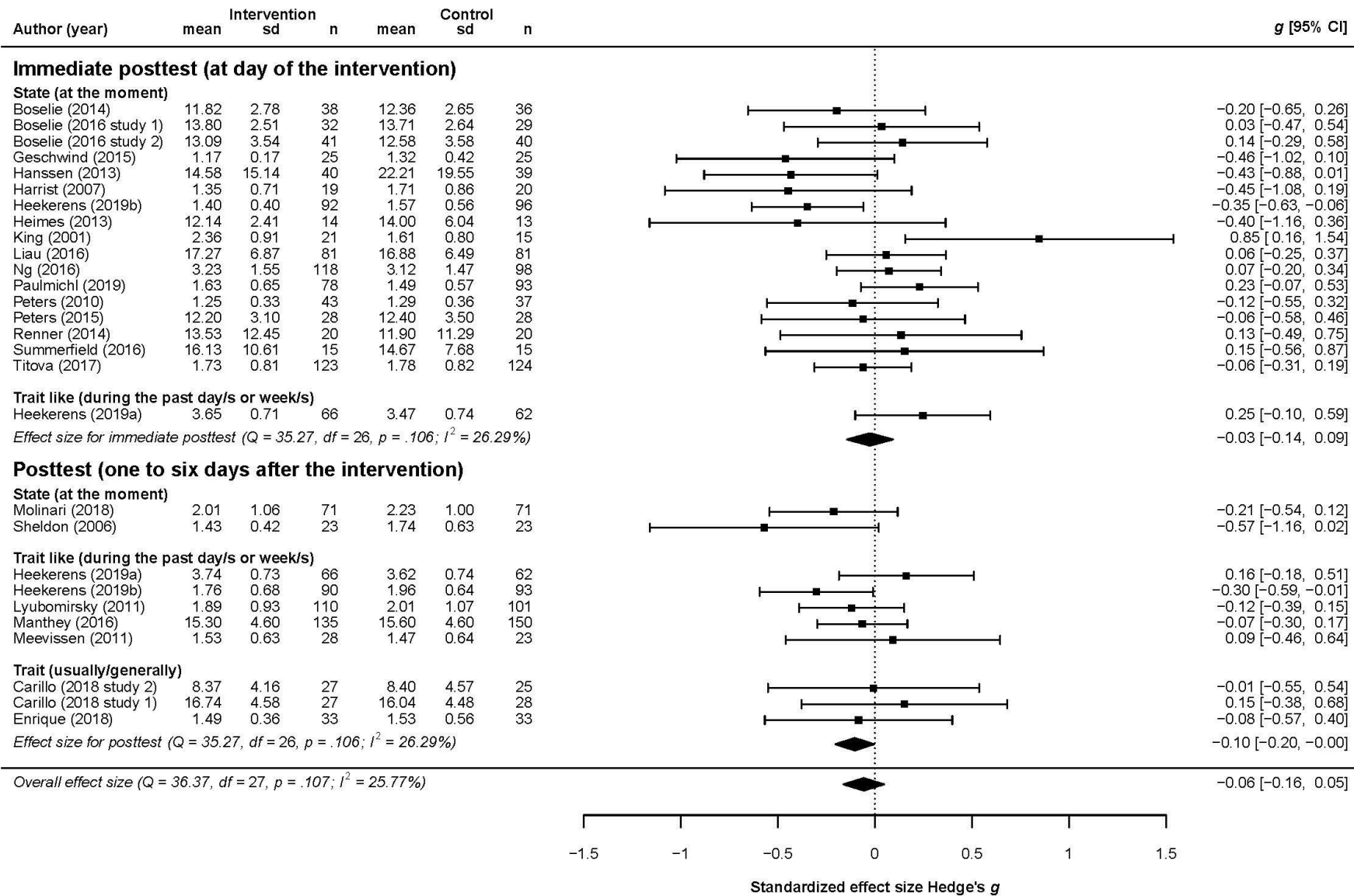**Figure 5.A1** Forest plot of the effects of the best-possible-self intervention on negative affect relative to neutral controls separate for time of assessment and conceptualization of the outcome. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

Author (year)	Intervention mean	sd	n	Control mean	sd	n		g [95% CI]

Immediate posttest (at day of the intervention)

Future Expectation Scale or Subjective Probability Task

Heekerens (2019b)	2.91	0.86	92	3.19	0.92	96		−0.31 [−0.60, −0.03]
Meevissen (2012)	2.62	0.79	36	3.12	0.90	34		−0.59 [−1.06, −0.11]
Hanssen (2013)	25.20	7.68	40	31.46	10.15	39		−0.69 [−1.14, −0.24]
Boselie (2014)	26.21	7.04	38	29.44	9.01	36		−0.40 [−0.86, 0.06]
Peters (2015)	21.20	7.60	28	26.50	7.80	28		−0.68 [−1.22, −0.14]
Boselie (2016 study 1)	25.30	6.13	41	29.14	5.63	40		−0.65 [−1.09, −0.20]
Boselie (2016 study 2)	27.12	4.47	32	29.00	4.70	29		−0.41 [−0.91, 0.10]
Peters (2010)	2.59	0.90	43	2.62	0.79	38		−0.03 [−0.47, 0.40]

Effect size for immediate posttest (Q = 14.87, df = 10, p = .137; I^2 = 32.74%)   −0.43 [−0.59, −0.27]

Posttest (one to six days after the intervention)

Future Expectation Scale or Subjective Probability Task

Heekerens (2019b)	2.92	0.91	90	3.19	0.99	93		−0.28 [−0.57, 0.01]
Meevissen (2011)	2.20	0.63	28	3.01	0.88	23		−1.06 [−1.65, −0.47]
Enrique (2018)	2.41	0.83	33	2.42	0.97	33		−0.01 [−0.49, 0.47]
Molinari (2018)	2.73	1.62	23	3.29	1.60	28		−0.34 [−0.90, 0.21]

Effect size for posttest (Q = 14.87, df = 10, p = .137; I^2 = 32.74%)   −0.34 [−0.59, −0.09]

Overall effect size (Q = 15.36, df = 11, p = .167; I^2 = 28.39%)   −0.40 [−0.57, −0.24]

Standardized effect size Hedge's *g*

Figure 5.A2 Forest plot of the effects of the best-possible-self intervention on pessimism relative to neutral controls separate for time of assessment. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.
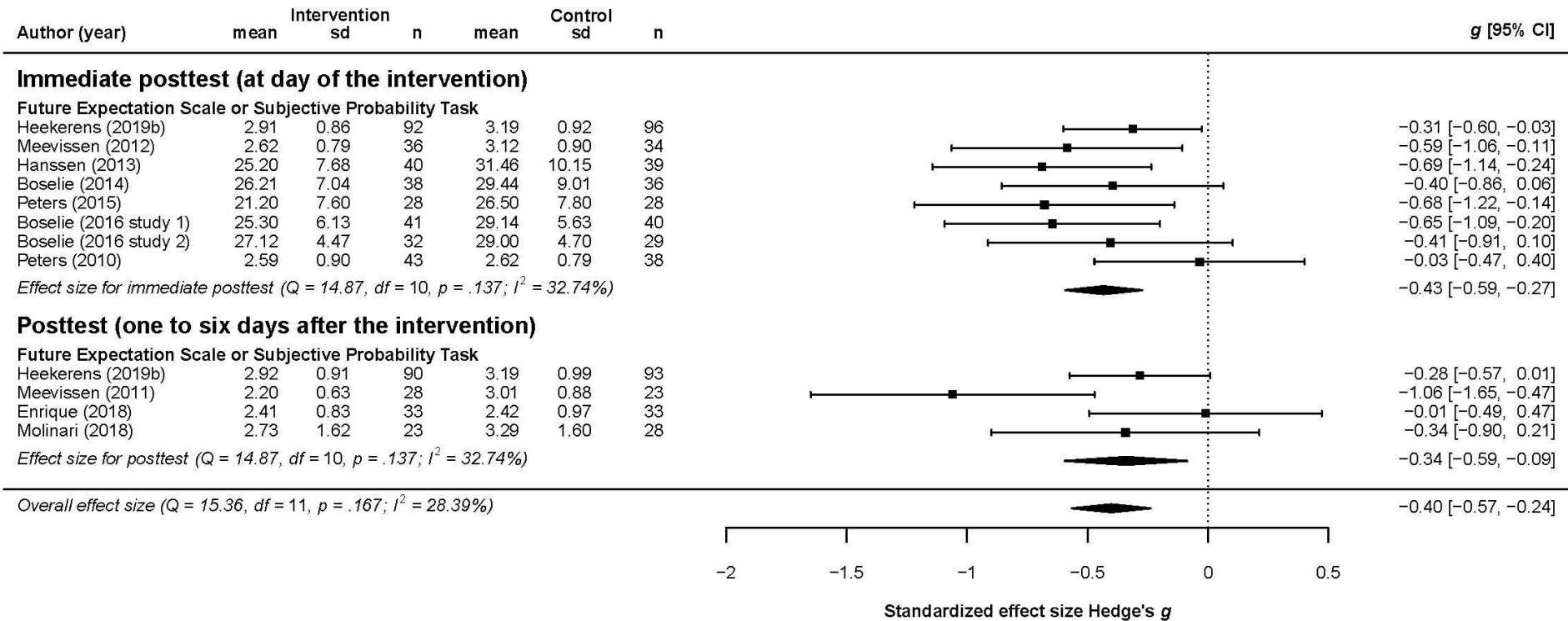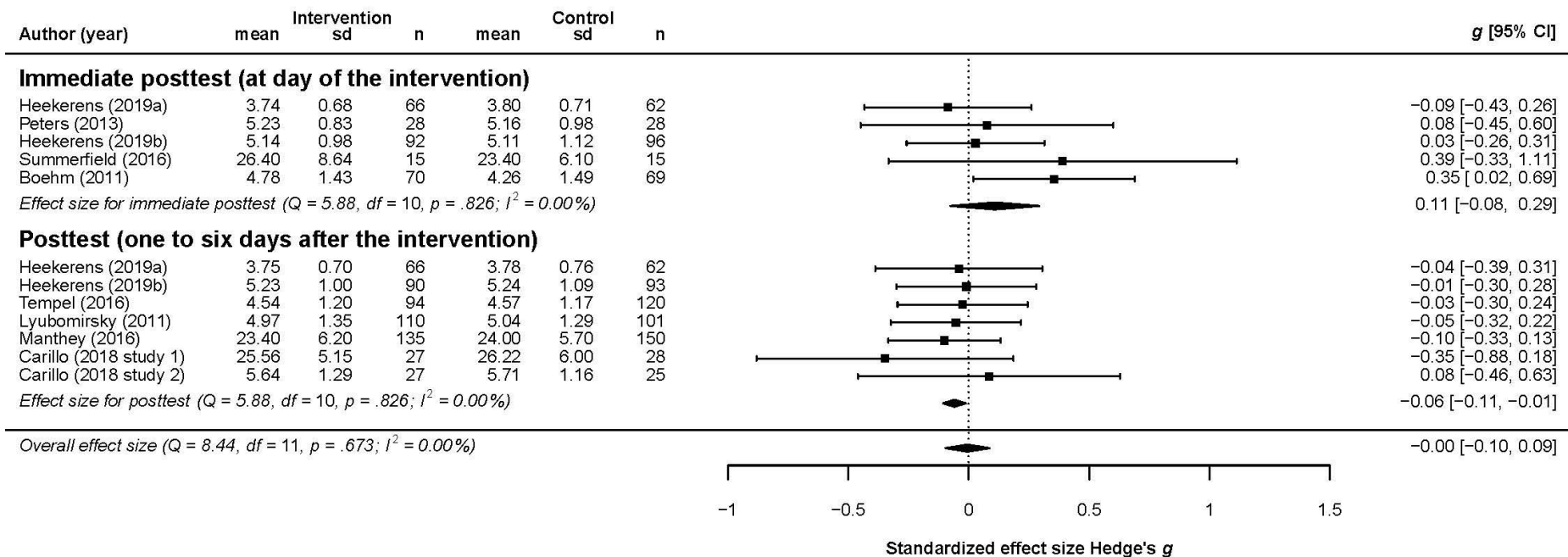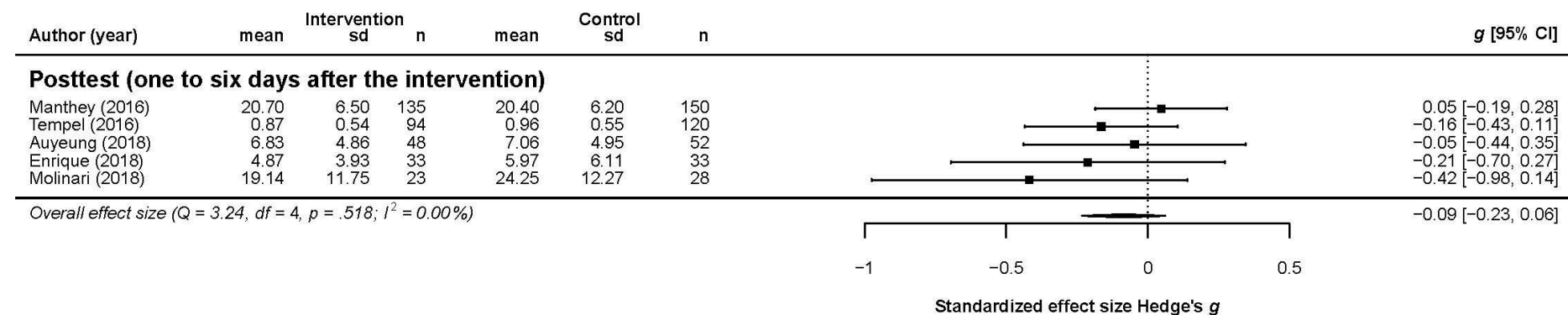
Author (year)	Intervention mean	sd	n	Control mean	sd	n		g [95% CI]
Immediate posttest (at day of the intervention)								
Heekerens (2019a)	3.74	0.68	66	3.80	0.71	62		−0.09 [−0.43, 0.26]
Peters (2013)	5.23	0.83	28	5.16	0.98	28		0.08 [−0.45, 0.60]
Heekerens (2019b)	5.14	0.98	92	5.11	1.12	96		0.03 [−0.26, 0.31]
Summerfield (2016)	26.40	8.64	15	23.40	6.10	15		0.39 [−0.33, 1.11]
Boehm (2011)	4.78	1.43	70	4.26	1.49	69		0.35 [0.02, 0.69]
Effect size for immediate posttest (Q = 5.88, df = 10, p = .826; I^2 = 0.00%)								0.11 [−0.08, 0.29]
Posttest (one to six days after the intervention)								
Heekerens (2019a)	3.75	0.70	66	3.78	0.76	62		−0.04 [−0.39, 0.31]
Heekerens (2019b)	5.23	1.00	90	5.24	1.09	93		−0.01 [−0.30, 0.28]
Tempel (2016)	4.54	1.20	94	4.57	1.17	120		−0.03 [−0.30, 0.24]
Lyubomirsky (2011)	4.97	1.35	110	5.04	1.29	101		−0.05 [−0.32, 0.22]
Manthey (2016)	23.40	6.20	135	24.00	5.70	150		−0.10 [−0.33, 0.13]
Carillo (2018 study 1)	25.56	5.15	27	26.22	6.00	28		−0.35 [−0.88, 0.18]
Carillo (2018 study 2)	5.64	1.29	27	5.71	1.16	25		0.08 [−0.46, 0.63]
Effect size for posttest (Q = 5.88, df = 10, p = .826; I^2 = 0.00%)								−0.06 [−0.11, −0.01]
Overall effect size (Q = 8.44, df = 11, p = .673; I^2 = 0.00%)								−0.00 [−0.10, 0.09]

Figure 5.A3 Forest plot of the effects of the best-possible-self intervention on life satisfaction relative to neutral controls separate for time of assessment. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.
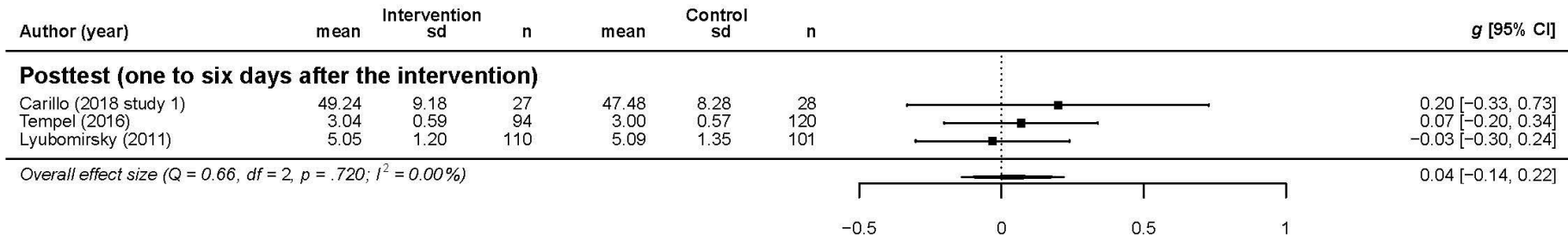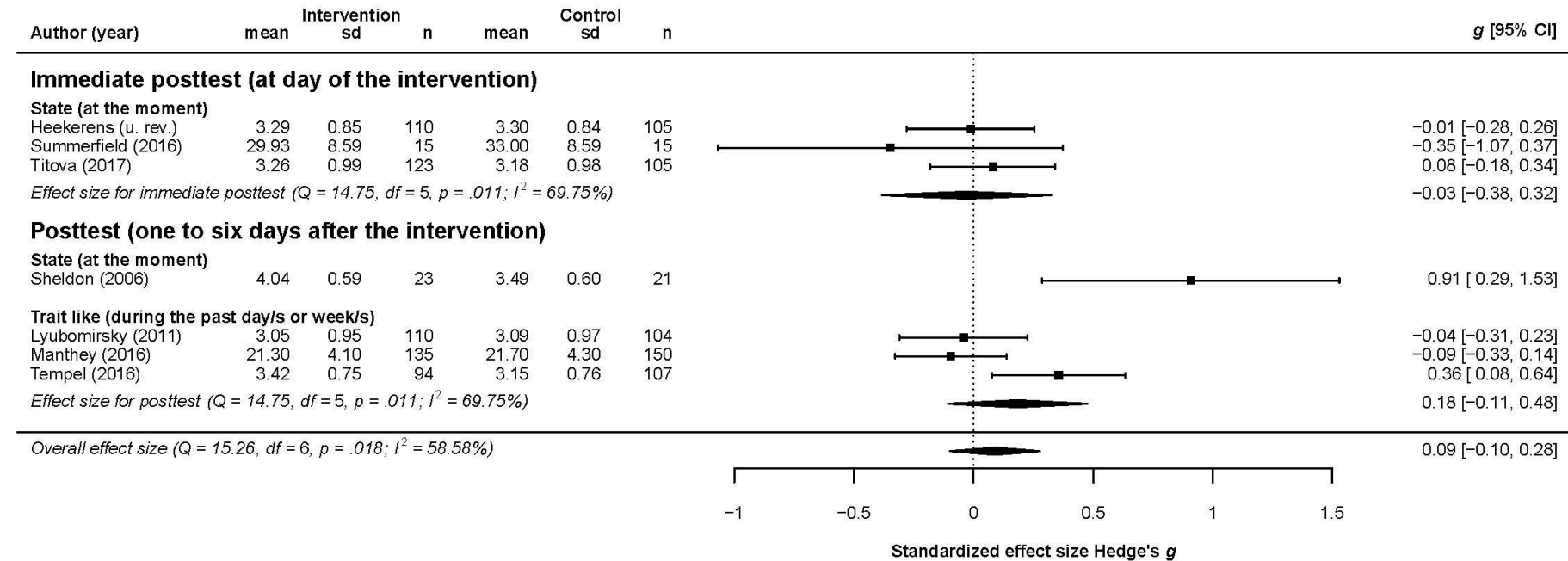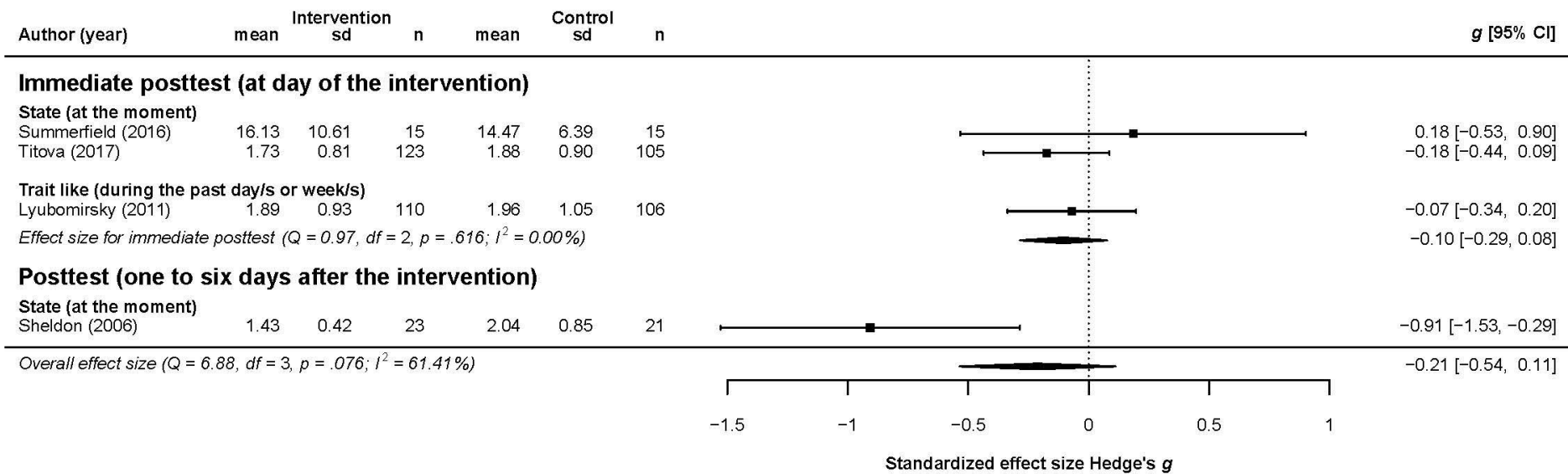
Author (year)	Intervention mean	sd	n	Control mean	sd	n		g [95% CI]
Posttest (one to six days after the intervention)								
Manthey (2016)	20.70	6.50	135	20.40	6.20	150		0.05 [−0.19, 0.28]
Tempel (2016)	0.87	0.54	94	0.96	0.55	120		−0.16 [−0.43, 0.11]
Auyeung (2018)	6.83	4.86	48	7.06	4.95	52		−0.05 [−0.44, 0.35]
Enrique (2018)	4.87	3.93	33	5.97	6.11	33		−0.21 [−0.70, 0.27]
Molinari (2018)	19.14	11.75	23	24.25	12.27	28		−0.42 [−0.98, 0.14]
Overall effect size (Q = 3.24, df = 4, p = .518; I^2 = 0.00%)								−0.09 [−0.23, 0.06]

Figure 5.A4 Forest plot of the effects of the best-possible-self intervention on depressive symptoms relative to neutral controls at posttest. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.
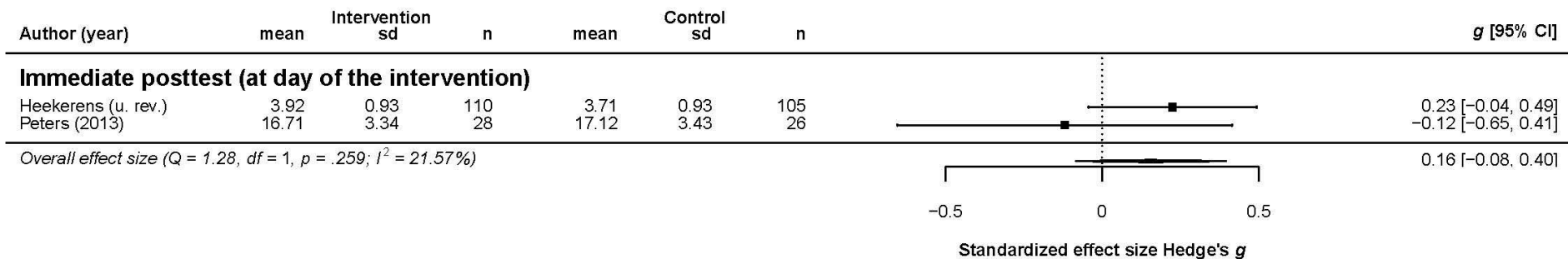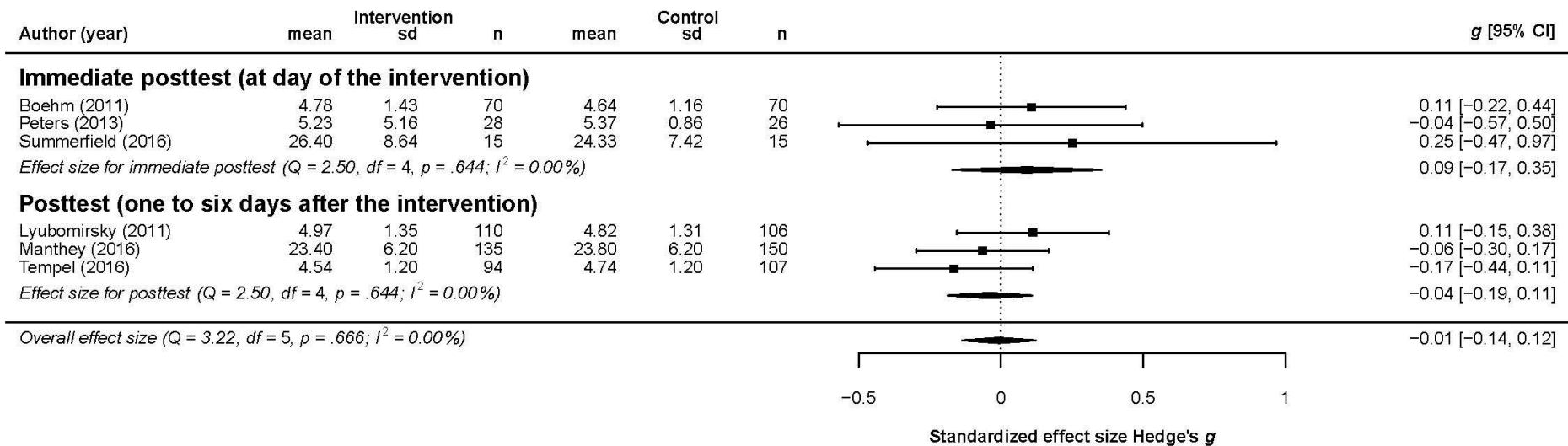
Author (year)	Intervention mean	sd	n	Control mean	sd	n		g [95% CI]
Posttest (one to six days after the intervention)								
Carillo (2018 study 1)	49.24	9.18	27	47.48	8.28	28		0.20 [−0.33, 0.73]
Tempel (2016)	3.04	0.59	94	3.00	0.57	120		0.07 [−0.20, 0.34]
Lyubomirsky (2011)	5.05	1.20	110	5.09	1.35	101		−0.03 [−0.30, 0.24]
Overall effect size (Q = 0.66, df = 2, p = .720; I² = 0.00%)								0.04 [−0.14, 0.22]

Figure 5.A5 Forest plot of the effects of the best-possible-self intervention on happiness relative to neutral controls at posttest. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.
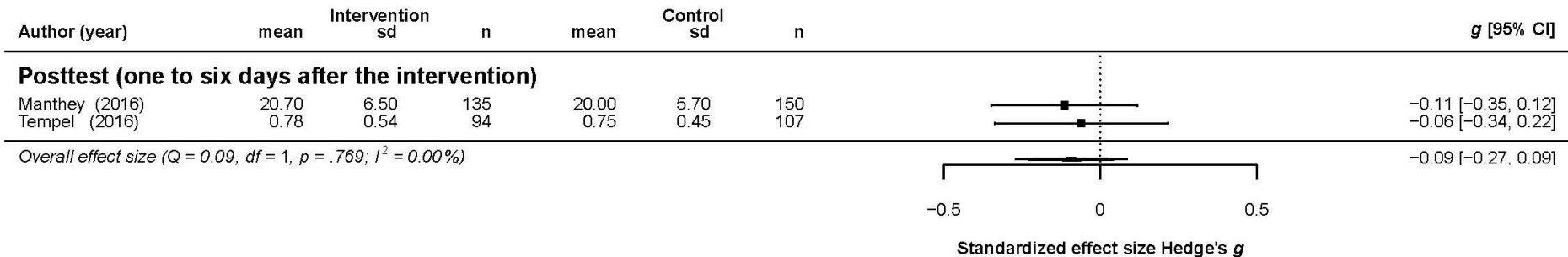
Author (year)	Intervention mean	sd	n	Control mean	sd	n		g [95% CI]
Immediate posttest (at day of the intervention)								
State (at the moment)								
Heekerens (u. rev.)	3.29	0.85	110	3.30	0.84	105		−0.01 [−0.28, 0.26]
Summerfield (2016)	29.93	8.59	15	33.00	8.59	15		−0.35 [−1.07, 0.37]
Titova (2017)	3.26	0.99	123	3.18	0.98	105		0.08 [−0.18, 0.34]
Effect size for immediate posttest (Q = 14.75, df = 5, p = .011; I² = 69.75%)								−0.03 [−0.38, 0.32]
Posttest (one to six days after the intervention)								
State (at the moment)								
Sheldon (2006)	4.04	0.59	23	3.49	0.60	21		0.91 [0.29, 1.53]
Trait like (during the past day/s or week/s)								
Lyubomirsky (2011)	3.05	0.95	110	3.09	0.97	104		−0.04 [−0.31, 0.23]
Manthey (2016)	21.30	4.10	135	21.70	4.30	150		−0.09 [−0.33, 0.14]
Tempel (2016)	3.42	0.75	94	3.15	0.76	107		0.36 [0.08, 0.64]
Effect size for posttest (Q = 14.75, df = 5, p = .011; I² = 69.75%)								0.18 [−0.11, 0.48]
Overall effect size (Q = 15.26, df = 6, p = .018; I² = 58.58%)								0.09 [−0.10, 0.28]

Standardized effect size Hedge's *g*

Figure 5.A6 Forest plot of the effects of the best-possible-self intervention on positive affect relative to gratitude interventions separate for time of assessment and conceptualization of the outcome. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

	Intervention			Control				
Author (year)	mean	sd	n	mean	sd	n		g [95% CI]
Immediate posttest (at day of the intervention)								
State (at the moment)								
Summerfield (2016)	16.13	10.61	15	14.47	6.39	15		0.18 [−0.53, 0.90]
Titova (2017)	1.73	0.81	123	1.88	0.90	105		−0.18 [−0.44, 0.09]
Trait like (during the past day/s or week/s)								
Lyubomirsky (2011)	1.89	0.93	110	1.96	1.05	106		−0.07 [−0.34, 0.20]
Effect size for immediate posttest (Q = 0.97, df = 2, p = .616; I^2 = 0.00%)								−0.10 [−0.29, 0.08]
Posttest (one to six days after the intervention)								
State (at the moment)								
Sheldon (2006)	1.43	0.42	23	2.04	0.85	21		−0.91 [−1.53, −0.29]
Overall effect size (Q = 6.88, df = 3, p = .076; I^2 = 61.41%)								−0.21 [−0.54, 0.11]

Figure 5.A7 Forest plot of the effects of the best-possible-self intervention on negative affect relative to gratitude interventions separate for time of assessment and conceptualization of the outcome. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

	Intervention			Control				
Author (year)	mean	sd	n	mean	sd	n		g [95% CI]
Immediate posttest (at day of the intervention)								
Heekerens (u. rev.)	3.92	0.93	110	3.71	0.93	105		0.23 [−0.04, 0.49]
Peters (2013)	16.71	3.34	28	17.12	3.43	26		−0.12 [−0.65, 0.41]
Overall effect size (Q = 1.28, df = 1, p = .259; I^2 = 21.57%)								0.16 [−0.08, 0.40]

Figure 5.A8 Forest plot of the effects of the best-possible-self intervention on optimism relative to gratitude interventions at immediate posttest. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

Author (year)	Intervention mean	Intervention sd	Intervention n	Control mean	Control sd	Control n	g [95% CI]
Immediate posttest (at day of the intervention)							
Boehm (2011)	4.78	1.43	70	4.64	1.16	70	0.11 [−0.22, 0.44]
Peters (2013)	5.23	5.16	28	5.37	0.86	26	−0.04 [−0.57, 0.50]
Summerfield (2016)	26.40	8.64	15	24.33	7.42	15	0.25 [−0.47, 0.97]
Effect size for immediate posttest (Q = 2.50, df = 4, p = .644; I² = 0.00%)							0.09 [−0.17, 0.35]
Posttest (one to six days after the intervention)							
Lyubomirsky (2011)	4.97	1.35	110	4.82	1.31	106	0.11 [−0.15, 0.38]
Manthey (2016)	23.40	6.20	135	23.80	6.20	150	−0.06 [−0.30, 0.17]
Tempel (2016)	4.54	1.20	94	4.74	1.20	107	−0.17 [−0.44, 0.11]
Effect size for posttest (Q = 2.50, df = 4, p = .644; I² = 0.00%)							−0.04 [−0.19, 0.11]
Overall effect size (Q = 3.22, df = 5, p = .666; I² = 0.00%)							−0.01 [−0.14, 0.12]

Standardized effect size Hedge's *g*

Figure 5.A9 Forest plot of the effects of the best-possible-self intervention on life satisfaction relative to gratitude interventions separate for time of assessment. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

Author (year)	Intervention mean	Intervention sd	Intervention n	Control mean	Control sd	Control n	g [95% CI]
Posttest (one to six days after the intervention)							
Manthey (2016)	20.70	6.50	135	20.00	5.70	150	−0.11 [−0.35, 0.12]
Tempel (2016)	0.78	0.54	94	0.75	0.45	107	−0.06 [−0.34, 0.22]
Overall effect size (Q = 0.09, df = 1, p = .769; I² = 0.00%)							−0.09 [−0.27, 0.09]

Standardized effect size Hedge's *g*

Figure 5.A10 Forest plot of the effects of the best-possible-self intervention on depressive symptoms relative to gratitude interventions at posttest. Positive effect sizes favor the intervention condition. Fixed effects model. SD = standard deviation; intervention = best-possible-self intervention; n = number of participants; CI = confidence interval.

D. Risk of Bias of Included Studies

Table 5.A2 Assessment of Risk of Bias for Randomized Controlled Trials Examining the Effects of the Best-Possible-Self Intervention

Author (year)	Random sequence generation	Allocation concealment	Blinding of experimenters	Blinding of outcome assessment	Incomplete outcome data	Selective outcome reporting	Other bias
King (2001)	unclear	unclear	unclear	low	low	low	low
Sheldon (2006)	unclear	unclear	high	low	low	low	low
Harrist (2007)	unclear	unclear	low	low	low	low	low
Peters (2010)	low	high	high	low	low	low	low
Shapira (2010)	low	low	low	low	high	low	low
Boehm (2011)	unclear	low	unclear	low	low	low	low
Lyubmirski (2011)	unclear	unclear	unclear	low	low follow-up: high	high	low
Meevissen (2011)	unclear	unclear	unclear	low	low	low	low
Meevissen (2012)	unclear	unclear	unclear	low	low	low	low
Hanssen (2013)	unclear	unclear	high	low	low	low	low
Heimes (2013)	low	low	low	low follow-up: unclear	low	low	low
Peters (2013)	low	low	high	low	low	low	low
Boselie (2014)	unclear	unclear	low	low	low	low	low
Maddalena (2014)	low	low	low	low	high	low	high
Renner (2014)	unclear	unclear	unclear	low	low	low	low
Geschwind (2015)	unclear	unclear	unclear	low	low	low	low
Peters (2015)	high	high	high	low	low	low	low
Boselie (2016 study 1)	unclear	unclear	high	low	low	low	low
Boselie (2016 study 2)	unclear	unclear	high	low	low	low	low
Liau (2016)	unclear	unclear	high	low follow-up: unclear	low	low	unclear
Manthey (2016)	low	low	low	low	high	low	low
Ng (2016)	unclear	unclear	unclear	unclear	low	low	low
Summerfield (2016)	low	high	low	low	high	low	low
Tempel (2016)	low	low	low	low	high	low	low
Titova (2017)	low	low	low	low	low	low	low
Auyeung (2018)	low	low	low	low	high	low	low
Carrillo (2018 study 1)	low	high	high	low	high	low	low
Carrillo (2018 study 2)	low	low	low	low	high	low	low
Molinari (2018)	low	low	high	low	high	low	high
Heekerens (2019a)	low	low	high	low	low	low	low
Heekerens (2019b)	low	low	low	low	low	low	low
Paulmichl (2019)	low	low	low	low	high	low	low
Heekerens (u. rev.)	low	low	high	low	low	low	low

Note. Assessments followed the criteria of the Coachrane Collaboration. For each category, two trained reviewers indicated either high or low risk or, if insufficient information were provided, uncertain risk.

Chapter 6

General Discussion and Conclusion

Positive-psychological interventions are increasingly used to lastingly improve well-being (see Hendriks, Warren et al., 2019, for a review). Effects, however, are typically small and decrease over time (e.g., Bolier et al., 2013), which may be resolved by further development. In order to effectively further develop positive-psychological interventions, three questions should be addressed that currently remain wholly or partially unanswered: First, what are the effects of positive-psychological interventions (e.g., practical significance, additional effects, etc.)? Second, how can we explain these effects (i.e., what are mediators)? Third, under which conditions and for whom do they work best (i.e., what are moderators)? Knowledge of mediators helps to increase intervention effects through adding more of identified active ingredients; knowledge of moderators helps to administer interventions in a way that encourages best results (contextual moderators) and through targeting interventions to those who benefit most (person-specific moderators). Due to limited resources, this thesis focuses on three popular positive-psychological interventions: the best-possible-self intervention (King, 2001), the gratitude letter exercise (Seligman, Steen, Park, & Peterson, 2005), and self-compassionate writing (Shapira & Mongrain, 2010). As shown in Chapter 1, findings and theories related to these interventions can be organized against the background of the positive activity model (Layous & Lyubomirsky, 2013). The model provides a conceptual framework to study the effects, mediators, and moderators of positive-psychological interventions, which incorporates different theoretical perspectives. Relevant well-being theories include, but are not limited to, cognitive theories such as the process model of emotion regulation (Gross, 1989; Quoidbach, Mikolajczak, & Gross, 2015), self-regulation theory (King, 2001), and the positive self-representations hypothesis (Mongrain & Anselmo-Matthews, 2012) as well as evolutionary theories such as the broaden-and-build theory (Fredrickson, 1998, 2001). There are, however, few studies that tested predictions derived from these theories in relation to the best-possible-self intervention, the gratitude letter exercise, or self-compassionate writing. Specifically, it remains unclear whether positive future expectations (process model of emotion regulation), goal ambivalence (self-regulation theory), or positive emotions (broaden-and-build theory) mediate the effects of the best-possible-self intervention. Another open question is whether the effect of the gratitude letter exercise on state gratitude and the effect of self-compassionate writing on state self-compassion are specific to these interventions, or whether other positive-psychological interventions (e.g., the best-possible-self intervention) provide similar benefits. In addition, to my best knowledge, no study has directly investigated the impact of the best-possible-self intervention, the gratitude letter exercise, or self-compassionate writing on positive self-relevant thoughts

(positive self-representations hypothesis). Moreover, a summary of the effects of the best-possible-self intervention on different indicators of well-being (e.g., positive affect, optimism, life satisfaction) that accounts for effects at different times of assessment (e.g., follow-up effects) and different conceptualizations of outcomes (e.g., state vs. trait optimism) is missing. Providing research on these topics is important to gain further insights into the processes underlying the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing, which may, in turn, help to further develop positive-psychological interventions. In addition, the positive activity model (Lyubomirsky & Layous, 2013) has been used to make predictions regarding which groups of participants benefit most from certain positive-psychological interventions (e.g., person-activity fit hypothesis; also see Lyubmirsky, 2007; Schueller, 2011). Although an increasing number of studies investigates personal characteristics of participants that may operate as moderators of intervention effects (e.g., Wellenzohn, Proyer, & Ruch, 2018), many questions remain partially or wholly unanswered. Specifically, one open question is whether the effects of the best-possible-self intervention on positive affect and state optimism are larger among participants who score low in emotional self-awareness compared with participants who score high in emotional self-awareness. In addition, it remains unclear whether the effects of self-compassionate writing are larger among participants higher in emotional self-awareness and whether the gratitude letter exercise provides better outcomes for participants higher in trait gratitude. It is important to address these questions in order to effectively target positive-psychological interventions to individuals who profit most. As shown in Table 6.1, and in line with the above reviewed open questions, the specific aims of this thesis were (1) to investigate goal ambivalence and positive future expectations as mechanisms of the effects of the best-possible-self intervention on positive affect; (2) to examine unique and shared effects of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing as well as to investigate emotional self-awareness and trait gratitude as moderators; and (3) to comprehensively examine the effects of the best-possible-self intervention considering the time of outcome assessment and outcome conceptualization as well as to investigate contextual moderators. To achieve these aims, three empirical studies were presented in Chapters 3, 4, and 5. Specifically, in Chapter 3, a longitudinal randomized controlled trial was used to investigate goal ambivalence and positive future expectations as mediators of subsequent increases in positive affect following the best-possible-self intervention. In Chapter 4 an online randomized controlled trial was used to examine the immediate effects of the best-possible-self intervention, the gratitude letter exercise, and

Table 6.1 Aims, Methods, and Findings of the Empirical Studies Discussed in this Thesis

Chapter	Aims	Methods	Findings
3	To investigate goal ambivalence and positive future expectations as mechanisms of the effects of the best-possible-self intervention on positive affect.	Longitudinal randomized controlled intervention trial with baseline, immediate posttest, and 1-week follow-up measures of positive affect, goal ambivalence, and positive future expectations. Mediation hypotheses were tested using two latent cross-lagged panel design models.	Participating in the best-possible-self intervention increased positive affect and reduced goal ambivalence up to one week later relative to an active control condition. Neither goal ambivalence nor positive future expectations at immediate posttest mediated the increase in positive affect in the week after the intervention. Additional analyses results indicate no intervention effects on life satisfaction, trait gratitude, and hope.
4	To examine unique and shared effects of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing. To investigate emotional self-awareness and trait gratitude as moderators.	Four groups online randomized controlled intervention trial with baseline assessment of moderator variables and immediate posttest measures of positive affect, optimism, gratitude, self-compassion, and current thoughts. Moderation hypotheses were tested using latent multiple group analyses.	Participants in the best-possible-self-intervention reported higher momentary optimism (but not gratitude) relative to an active control condition, even after controlling for positive affect. Participants in the gratitude letter exercise reported higher momentary gratitude (but not optimism) relative to an active control condition, even after controlling for positive affect. Participants in the best-possible-self intervention and gratitude letter exercise reported more positive self-relevant thoughts after the intervention relative to an active control condition. Self-compassionate writing showed no beneficial effects. Neither emotional self-awareness nor trait gratitude moderated the intervention effects.
5	To comprehensively examine the effects of the best-possible-self intervention considering the time of outcome assessment and outcome conceptualization. To investigate moderators of the best-possible-self intervention.	Systematic literature search that resulted in a total of 34 randomized controlled intervention trials. Assessment of risk of bias and examination of publication bias. Meta analyses were performed separately for each outcome variable. Moderator analyses were performed based on coding of the time of outcome assessment, how outcomes were conceptualized, and other relevant variables. For follow-up effects we provided descriptive results.	The best-possible-self intervention shows small effects on positive affect (Hedge's $g = 0.28$) and optimism ($g = 0.21$). Effects on positive affect are pronounced if measured immediately after the intervention ($g = 0.39$) or if conceptualized as momentary affect ($g = 0.41$), whereas effects several days after the intervention ($g = 0.12$) or if conceptualized as affect during the past days ($g = 0.09$) are negligible. Effects on optimism are pronounced if measured immediately after the intervention ($g = 0.36$) or if conceptualized as positive future expectations ($g = 0.36$), whereas effects several days after the intervention ($g = -0.01$) or if conceptualized as a general orientation in life ($g = -0.14$) are negligible. There was a moderate effect on negative future expectations ($g = -0.40$). Descriptive results indicate no effects on positive affect and optimism approximately one week after the implementation. There were no effects on negative affect, life satisfaction, depressive symptoms, and happiness. Administering the intervention online showed no effect on positive affect. Length of intervention, payment status of participants, inclusion of an imagery component, and choice of theme did not moderate intervention effects.

self-compassionate writing on state optimism, state gratitude, state self-compassion, positive affect, and positive self-relevant thoughts. Finally, Chapter 5 provided a meta-analysis and systematic review of the effects and contextual moderators of the best-possible-self intervention. Table 6.1 displays the specific methods and findings of the studies in Chapters 3, 4, and 5.

The following chapter starts with a discussion of the major findings of this thesis against the background of current theories and debates. A special focus is on (a) the effects of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing; (b) the underlying psychological mechanisms of these interventions (i.e., mediators of intervention effects); and (c) contextual and person-specific moderators of intervention effects. Afterwards, the generalizability of the findings and implications for the practical application of positive-psychological interventions are discussed. Finally, suggestions for future research are provided and a conclusion is drawn.

Three major findings emerged from the empirical studies in this thesis. First, the best-possible-self intervention, when administered as a stand-alone exercise, is insufficient to perpetuate lasting increases in well-being. Second, the best-possible-self intervention and the gratitude letter exercise show both specific and common effects that might serve as mediators. Third, online variations of the best-possible-self intervention are less effective than in-person administrations. In the following three paragraphs, a rationale is provided for each of these conclusions. In addition, other relevant findings regarding the effects, mediators, and moderators of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing are discussed. Other than in the introduction, findings regarding the three interventions are reviewed together in order to highlight overarching trends in the results and deepen current debates on positive-psychological interventions in general.

6.1 Implications for Effectiveness Research

Positive-psychological interventions aim to lastingly increase well-being and typically comprise brief exercises that are applied in a single session (e.g., Seligman et al., 2005), on a few consecutive days (e.g., King, 2001), or throughout the course of one week (e.g., Shapira & Mongrain, 2010). Their rationale is to teach individuals adaptive habits of thinking, behaving, and relating that are maintained after the initial intervention period, which presumably allows for permanent changes in well-being. Results from early studies suggest that the effects of the gratitude letter intervention last up to one month (Seligman et al., 2005), effects of the best-possible-self intervention last up to five months (King, 2001), and effects of self-compassionate writing last up to six months (Shapira & Mongrain, 2010). Some

researchers have reported effects of brief positive-psychological interventions after more than three years (Proyer, Wellenzohn, Gander, & Ruch, 2015).

6.1.1 Long-term Effects of the Best-Possible-Self Intervention

In contrast, findings from this thesis do not support the notion that all positive-psychological interventions provide long-term benefits. Specifically, the meta-analytic results discussed in Chapter 5, which include data reported in Chapters 3 and 4, show that the effects of the best-possible-self intervention on positive affect and state optimism disappear approximately one week after the intervention. In addition, there was no evidence for intervention effects on various other outcomes, including life satisfaction (Boehm, Lyubomirsky, & Sheldon, 2011) and depressive symptoms (Shapira & Mongrain, 2010; Yogo & Fujihara, 2008). These findings clearly question the merit of the best-possible-self intervention to lastingly increase well-being (King, 2001). Given that effects of the best-possible-self intervention probably last no longer than one week, the intervention's virtue as a positive alternative to expressive writing should also be reconsidered (although expressive writing may not be a good option neither as its effect on a psychological health composite was estimated at $d = 0.07$ at 1 to 15 months follow-up; Frattaroli, 2006; see Kállay, 2015; Reinhold, Bürkner, & Holling, 2018; Rude & Haner, 2018, for a deeper discussion). In line with our results, researchers who reanalyzed the data reported in two popular meta-analyses (Bolier et al., 2013; Sin & Lyubomirsky, 2009) concluded that the effects of positive-psychological interventions, including the best-possible-self intervention, have been overestimated in the past (White, Uttl, & Holder, 2019). Results from the reanalysis suggest diverging conclusions because small sample size bias and other shortcomings of previous meta-analyses were accounted for. In addition, reviewers and meta-analysts who comprehensively examined the effects of gratitude based positive-psychological interventions have drawn similarly sobering conclusions (Davis et al., 2016; Dickens, 2017; Wood, Froh, & Geraghty, 2010). Specifically, Dickens (2017) demonstrated that the unique benefits of gratitude interventions may have been overemphasized in the literature.

There are at least two explanations for why some positive-psychological interventions fail to perpetuate lasting increases in well-being. First, true to the motto "use-it-or-lose-it", researchers have argued that effects are only maintained if participants continue the exercise on their own and incorporate the skills that they presumably learned during the intervention into their daily lives (Schueller & Parks, 2014; see Lyubomirsky, Dickerhoof, Boehm, & Sheldon, 2011; Schotanus-Dijkstra, Pieterse, Drossaert, Walburg, & Bohlmeijer, 2019, for supporting evidence). Specifically, the hedonic adaptation prevention

model argues that prolonged intervention effects require that positive-psychological interventions initiate and perpetuate a continuous stream of positive experiences (Sheldon & Lyubomirsky, 2012; see Sheldon & Lyubomirsky, 2019, for a recent discussion). To prevent adaptation, recurring positive experiences need to vary and participants need to actively appreciate these experiences. The problem is that some current positive-psychological interventions (e.g., King, 2001; Seligman et al., 2005; Shapira & Mongrain, 2010) were developed as brief stand-alone interventions rather than techniques that individuals can incorporate into their everyday live. Thus, it will take deliberate effort to further develop the content, design, and structure of existing positive-psychological interventions to successfully motivate and enable individuals to make the transition from merely consuming one-time positive-psychological exercises to actively and effectively incorporating the happiness related skills that these interventions aim to cultivate into their daily routines (see Parks, 2014; Sheldon & Lyubomirsky, 2006a, for initial recommendations). One promising way to further develop positive-psychological interventions then is to offer participants positive activities that are more closely linked to daily routines and can be simply continued. For example, after the initial writing exercise, participants of the best-possible-self intervention could be asked to journal about details of their lives that are already close to how they ideally want to live in the future and to deliberately notice small positive changes in their lives. This should extend intervention effects because participants are offered novel positive experiences following the initial intervention. In order to prevent quick adaptations to these new experiences (see Sheldon & Lyubomirsky, 2012, for a deeper discussion), additional positive-psychological interventions might be used, specifically those that can easily be continued on a daily basis (e.g., meditation). For example, researchers have successfully implemented the best-possible-self exercise together with exercises designed to identify and use one's character strengths, doing acts of kindness, and loving-kindness meditation (see Schotanus-Dijkstra, Pieterse, Drossaert, Walburg, & Bohlmeijer, 2018, for an example). Results from one recent meta-analysis indicate that such multicomponent positive-psychological interventions increase subjective well-being (Hedge's $g = 0.27$, 95% CI [0.07, 0.48], 17 comparisons) and decrease depressive symptoms ($g = 0.45$, 95% CI [0.15, 0.76], 15 comparisons) at 1 to 12 months follow-up assessments (Hendriks, Schotanus-Dijkstra, Hassankhan, Jong, & Bohlmeijer, 2019). In comparison, the effect of cognitive-behavioral therapy to treat mild to moderate depression has been estimated at Cohen's $d = 0.82$ post intervention (Gloaguen, Cottraux, Cucherat, & Blackburn, 1998). Albeit the size of the effects is still small, multicomponent positive-psychological interventions seem to result in effects that are stable in the

medium term. Another reason for why some positive-psychological interventions do not result in

sustained effects, and that might also explain why the effects of multicomponent positive-psychological

interventions are generally smaller than the effects of cognitive-behavioral therapy, is that individuals

who participate in positive-psychological interventions trials are typically rather happy. Meta-analytic

results provided in Chapter 5 show that the average control participant of best-possible-self intervention

trials report levels of life satisfaction (68 on a 0 to 100 scale) that correspond to those found in a

representative sample of the German population (67 on a 0 to 100 scale; Glaesmer, Grande, Braehler, &

Roth, 2011; rescaled using the scales package in R; Wickham, 2018). Thus, participants in positive-

psychological intervention trials are indeed non-distressed. It may be difficult to make such generally

happy participants even happier because there is less potential for improvement (see Lyubomirsky

& Layous, 2013; Parks, Della-Porta, Pierce, Zilca, & Lyubomirsky, 2012; Smirnova & Parks, 2018, for a

deeper discussion). Such ceiling effects are known from practicing other skills, for example, running a

marathon: For most of us, it takes much more deliberate practice to move from running a four hours

marathon to running it at three hours compared with improving from seven to six hours. Likewise, it

might be more difficult to increase happiness from 5 to 6 on a 7 points scale than from 4 to 5.

All in all, findings from this thesis are enough to conclude that the best-possible-self intervention,

as it is currently applied by researchers, provides little benefits in the long run. This raises the question

whether the best-possible-self intervention should be labeled a positive-psychological intervention

because doing so implies that the exercise lastingly increase well-being (Sin & Lyubomirsky, 2009),

which is a requirement not met on the basis of available knowledge (also see Parks & Biswas-Diener,

2013; Schueller & Parks, 2014). The best-possible-self intervention is, however, well suited as a mood

and expectation induction procedure. Hence, it might be labeled accordingly. Generally, exercises that

have been demonstrated to merely provide short-term effects on unstable indicators of well-being (e.g.,

momentary affect) should be referred to "components of positive-psychological interventions" or

"mood/expectations induction procedures" instead of "positive-psychological interventions". This

terminology makes clear that stand-alone interventions are insufficient to lastingly increase well-being,

which helps to prevent misunderstandings both among researchers and practitioners. In addition, accurate

labels help to shift attention towards the level at which an intervention operates (e.g., brief intervention

with short-term effects vs. comprehensive intervention with medium-term effects). Using new labels

does, of course, not mean that incorporating the exercise into more comprehensive programs is

ineffective. Quite the contrary, as prior discussed, multicomponent programs are potentially well suited to lastingly increase well-being (Hendriks, Schotanus-Dijkstra et al., 2019; see Jarden, Rashid, Roache, & Lomas, 2019; Rashid & Seligman, 2018, for best practice guidelines). In addition, research on stand-alone interventions that are eventually included into comprehensive programs can still be useful to investigate underlying mechanisms and make recommendations for further developing these programs (e.g., Heekerens & Heinitz, 2019).

6.1.2 Minor Findings

There are two minor findings regarding the effects of positive-psychological interventions that are worth some discussion. First, results provided in Chapter 3 show no significant effect of the best-possible-self intervention on positive future expectations. One explanation for the absent effect is that participants with higher positive future expectation scores at baseline were more likely to end up in the intervention condition, which may have made it difficult to observe a large increase as a result of the intervention (also see Lyubomirsky & Layous, 2013). Consequently, the finding should not be overprized as a failed replication, specifically because the descriptive effect was in the expected direction. Meta-analytic results repeatedly confirmed that the best-possible-self intervention effectively increases state optimism (Malouff & Schutte, 2016; Carillo et al., 2019; Chapter 5). Second, results in Chapter 4 show no immediate effects of self-compassionate writing on positive affect or state self-compassion. This finding contradicts results from earlier studies that suggest an effect on self-compassion among anorexia nervosa patients (Kelly & Waring, 2018) and female athletes (Mosewich, Crocker, Kowalski, & DeLongis, 2013), as well as an effect on positive affect among female college students (Stern & Engeln, 2018, study 1). Results from this thesis are, however, in line with results from two studies that indicate *no* effects on positive affect among student samples comprising both males and females (Johnson & O'Brien, 2013, study 2; Wong & Mak, 2016). What stands out is that beneficial effects were exclusively reported in studies that used female samples. Thus, one explanation for the differing results is that self-compassionate writing is more effective for women compared with men. Descriptive results from the study discussed in Chapter 4 support this notion. Specifically, the posttest mean difference in positive affect between self-compassionate writing intervention and control participants was 0.11 points on a 5 points scale for women, 3.26 vs. 3.15, respectively, and -0.12, 3.35 vs. 3.47, for men. One meta-analysis on gender differences in self-compassion showed that men on average report higher levels of trait self-compassion than women (Yarnell et al., 2015). Yarnell and colleagues suggested that direct training in

self-compassion may be particularly helpful for women in order to learn how to better care for themselves instead of excessively caring for others, which might be more pronounced among women due to traditional role expectations. Another explanation for higher effects among female participants is that they prefer compassion-based interventions over other positive-psychological interventions (Schueller, 2010).

6.2 Implications for Well-Being Theories

Current theories suggest that positive-psychological interventions work through both specific mechanisms (e.g., adopting a grateful outlook after the gratitude letter exercise; Quoidbach et al., 2015) and common mechanisms (e.g., positive emotions or positive self-relevant thinking; Fredrickson, 1998, 2001; Mongrain & Anselmo-Matthews, 2012). Results from this thesis provide first evidence that the best-possible-self intervention and the gratitude letter exercise simultaneously affect both unique and shared outcomes. Specifically, results from Chapter 4 show that the best-possible-self intervention increases state optimism whereas the gratitude letter exercise increases state gratitude. Both interventions increased the number of positive self-relevant thoughts. This finding suggests that theories that seek to explain the underlying mechanisms of positive-psychological interventions should account for both specific and common factors. Current theories, however, emphasize either specific *or* common factors, which may be resolved by developing an integrative theoretical framework (e.g., based on Fredrickson, 2004; Mongrain & Anselmo-Matthews, 2012; Quoidbach et al., 2015). Theory development could also build on the positive activity model (Layous & Lyubomirsky, 2013), which principally incorporates both specific and common effect mechanisms because it uses broad labels to categorize mechanisms (e.g., positive cognition and positive emotions). One drawback of the model is that it provides only a rough conceptual framework designed to organize research on positive-psychological interventions rather than a detailed description of underlying mechanisms. The latter, however, is needed to derive testable predictions. In a second step, if evidence accumulates showing that the effects of positive-psychological interventions can be explained by specific and common mechanisms, further development should focus on both strengthening specific mechanisms of a given exercise (e.g., explicitly encouraging participants to adopt a grateful outlook while writing a gratitude letter) and extending general working principles (e.g., providing opportunities that allow participants to see themselves in a positive light, for example, as a grateful person). That being said, pinpointing specific mediators of intervention effects has proven difficult. Results from this thesis do not support reduced goal ambivalence or increased positive future expectations after the best-possible-self intervention as mediators of subsequent increases in positive

affect. This finding contradicts predictions derived from self-regulation theory (Kelly, Mansell, & Wood, 2015; King, 2001) and the process model of emotion regulation (Quoidbach et al., 2015). Specifically, the finding that positive future expectations immediately after the best-possible-self intervention do not predict increased positive affect in the week after the intervention conflicts with findings from one longitudinal mediation studies that identified state optimism as a promising mediator of positive-psychological interventions (Schotanus-Dijkstra et al., 2019). There are several differences between the studies that might explain the deviating results. First, Schotanus-Dijkstra and colleagues (2019) used a slightly distressed sample and assessed depressive symptoms and anxiety as outcomes. For one thing, as prior discussed, increasing well-being among underachievingly happy individuals should be easier (ceiling effect; Lyubomirsky & Layous, 2013). The resulting larger intervention effects should have made it easier to discover underlying mechanisms (unfortunately no effect sizes were reported for follow-up effects on depressive symptoms and anxiety; Schotanus-Dijkstra et al., 2019). In addition, it could be that inducing positive future expectations (state optimism) is more effective in dealing with maladaptive cognitions that lead to depressive symptoms than building adaptive cognitions that lead to positive emotions (also see Layous, Chancellor, Lyubomirsky, Wang, & Doraiswamy, 2011). Second, Schotanus-Dijkstra and colleagues (2019) applied a multicomponent positive-psychological intervention over the course of nine weeks rather than a brief stand-alone variant of the best-possible-self intervention. Thus, it could be that components other than writing about one's best possible future drove effects on state optimism or that increases in state optimism that translate into subsequent changes in well-being need longer intervention periods to build up (also see Fredrickson, Cohn, Coffey, Pek, & Finkel, 2008; Gander, Proyer, Hentz, & Ruch, 2019). However, results from this thesis are enough to conclude that positive future expectations are not supported as a mediator of the effect of the best-possible-self intervention on subsequent positive affect (Quoidbach et al., 2015). One alternative perspective on the process underlying the effects of the best-possible-self intervention is that the experiences of positive emotions during the intervention drives increases in positive future expectations (broaden-and-build theory; Fredrickson, 1998, 2001; also see Fredrickson et al., 2008). In contrast to this prediction, results from Chapter 4 show that increased positive affect immediately after the best-possible-self intervention does not mediate increased positive future expectations one week later. The finding conflicts with results from one earlier longitudinal mediation study that identified positive affect as a mediator of the effects of the best-possible-self intervention (Gibson, Umeh, Newson, & Davies, 2018). One explanation for the deviating

results is that Gibson and colleagues (2018) assessed self-care among diabetes as an outcome. The used behavioral self-report measure and clinical sample clearly differ from the future expectations measure and students sample used in Chapter 4. Thus, results may not be directly comparable.

6.3 Implications for Best Practice and Targeting Interventions

Methodology researchers have explained early on that "for any practical problem, there is some best group of treatments to use and some best allocation of persons to treatments" (Cronbach, 1957, p. 680). Positive-psychological interventions aim to solve the problem of lower than desired levels of well-being and it has been suggested that effects are ideal if features of the activity align with characteristics of participants (Lyubomirsky & Layous, 2013). Successfully allocating persons to treatments, however, requires that we know about contextual moderators (i.e., moderators related to features of the activity) and person-specific moderators (i.e., moderators related to characteristics of participants) of intervention effects.

6.3.1 Online Administrations of the Best-Possible-Self Intervention

Regarding contextual moderators, meta-analytic results from Chapter 5 suggest that online administrations of the best-possible-self intervention are less effective in increasing positive affect at posttest than in person administrations (Hedge's $g = 0.05$, 95% CI [-0.06, 0.16], 9 studies vs. $g = 0.40$, 95% CI [0.24, 0.56], 23 studies). This finding contradicts results from one study that randomly assigned participants to perform the intervention either online or in-person over the course of four weeks and reported that changes in positive affect from pretest to posttest were comparable in both groups (0.23, $SD = 1.17$, on a 7 points scale for in-person administration vs. 0.28, $SD = 0.88$, for online administration; Layous, Nelson, & Lyubomirsky, 2013). One explanation for the different results is that Layous and colleagues asked participants to actively take "baby steps" towards long-term goals that participants were requested to identify through their writing. This behavioral component might have driven the effects on positive affect and been equally effective for online and in-person administrations (see Mazzucchelli, Kane, & Rees, 2009, for a review of the effects of behavioral activation on mood). Generally, effective online administrations require user interfaces that are appropriate to engage participants in the activity (Diefenbach, 2018; Parks et al., 2012; Schueller & Parks, 2014). This prerequisite was probably not met by many of the online best-possible-self intervention trials in our meta-analysis (including the study by Layous et al., 2013; also see Sheldon & Lyubomirsky, 2012). Nevertheless, the finding points to the

potential limitations of online administrations of the best-possible-self intervention and, more generally, of online technologies to support well-being (e.g., digital coaching; Diefenbach, 2018). Further results from Chapter 5 do not support intensity of the administration of the best-possible-self intervention (length and number of sessions), incentivizing participants, inclusion of a brief imagery component before or after the writing session, and choice of theme (e.g., writing about one's future in general or regarding one's future family life) as moderators of the effects of the best-possible-self intervention. The interpretation of these findings, however, is complex because in our moderator analyses certain study features (e.g., use of more intense interventions) tended to appear together with other relevant features (e.g., use of trait instead of state outcome measures). These other features were difficult to account for in the analyses and might have biased the results. For example, it could be that more intense interventions were more effective. Effect sizes in studies that used comprehensive interventions, however, may have remained small because these studies also used less sensitive outcome measures compared with studies that administered less intense interventions (e.g., assessing habitual instead of momentary affect). Keeping this limitation in mind, findings from Chapter 5 challenge earlier ideas that longer administrations of the best-possible-self intervention are more effective (Bolier et al., 2013) or that the inclusion of an imagery component significantly increases intervention effects (Peters, Flink, Boersma, & Linton, 2010). The finding that different themes of the best-possible-self intervention yield similar effects is in line with previous research (Meevissen, Peters, & Alberts, 2011). In addition, the finding that incentivization status of participants made no difference for effects suggests that the moderating role of motivation might have been overestimated in the past (Dickerhoof, 2007; Lyubomirsky et al., 2011; Seear & Vella-Brodrick, 2013; Sheldon & Lyubomirsky, 2006b). However, incentivization status is, at best, an indirect measure of motivation and few studies have systematically investigated how incentives influence participants' motivation to engage in positive-psychological intervention trials (see Mitchell et al., 2013, for a deeper discussion). Thus, convincing evidence remains that motivation plays a key role in changing a person's happiness through intentional activities (Sheldon & Lyubomirsky, 2019).

6.3.2 Emotional Self-Awareness and Trait Gratitude are not Confirmed as Moderators

Regarding person-specific moderators, results from Chapter 4 neither support baseline emotional self-awareness as a moderator of the effects of the best-possible-self intervention or self-compassionate writing nor baseline trait gratitude as a moderator of the effects of the gratitude letter exercise. Thus, other than expected, the best-possible-self intervention seems to be equally effective in immediately increasing

positive affect and state optimism among participants who vary in their dispositional tendency to attend to own emotions (Austenfeld & Stanton, 2004). This finding contradicts results from earlier studies that found larger decreases in depressive symptoms at three months follow-up (Austenfeld, Paolo, & Stanton, 2006; Austenfeld & Stanton, 2008) and better self-reported physical health at one month follow-up (Maddalena, Reese, & Barnes, 2014) among participants who make few attempts to understand own emotions (low emotional processing) compared with participants who make many such attempts (high emotional processing). Probably differences between participants lower and higher in emotional processing are limited to outcomes related to negative affect and/or do not show until several weeks after the intervention. In addition, results from this thesis suggest that the effects of the gratitude letter exercise on positive affect and state gratitude are equal for individuals who tend to frequently and deeply experience feelings of appreciation and thankfulness and those who do less so (i.e., higher vs. lower levels of trait gratitude). This finding contradicts results from one study that reported larger posttest increases in life satisfaction (Rash, Matsuba, & Prkachin, 2011) and positive affect (Watkins, Woodward, Stone, & Kolts, 2003, study 4) for participants higher in trait gratitude. Specifically, our results do not support the notion that "grateful individuals are more likely to enjoy gratitude exercises" (Watkins et al., 2003, p. 447; also see Kaczmarek et al., 2015), although descriptive results were in the expected direction. Taken together, findings from our moderator analyses in Chapter 4 speak against the notion that certain types of activities yield better results for certain types of people (Cronbach, 1957; Lyubomirsky, 2007; Lyubomirsky & Layous, 2013; Schueller, 2011). Further discouraging evidence for the person-activity fit hypothesis comes from a recent randomized controlled trial that found that participants with higher levels of dispositional sense of humor did not benefit more from humor-based positive-psychological interventions compared with participants with lower levels of dispositional sense of humor (Wellenzohn, Proyer, & Ruch, 2018). On the other hand, extraversion has received some support as a moderator of the effects of strength-, humor-, and gratitude-based positive-psychological interventions (e.g., Ghielen, van Woerkom, & Christina Meyers, 2017; Senf & Liau, 2013; Wellenzohn et al., 2018). This is in line with the person-activity fit hypothesis because talkative and sociable individuals should have an easier time making enjoyable contact with others during the interventions (Lyubomirsky, 2007). Thus, some personal characteristics seem to successfully predict intervention success and hence could be used to target positive-psychological interventions to those who benefit most.

6.4 Generalizability of the Findings

The generalizability of the findings of this thesis is limited in several ways and the most important of these limitations are discussed below.

First, findings regarding the effects of positive-psychological interventions predominantly used Western samples. Because culture has been proposed to influence the efficacy of positive-psychological interventions in different ways, for example through the social appropriateness of explicitly expressing gratitude or pursuing autonomy related goals, researchers should be careful when generalizing results to non-Western cultures (see Fritz & Lyubomirsky, 2018; Hendriks, Warren et al., 2019; Jarden et al., 2019, for a deeper discussion). Although delivering positive-psychological interventions to individuals from various cultures typically involves culturally adapting these interventions (e.g., the best-possible-self intervention), recent evidence suggests that some positive-psychological interventions that were developed within a Western cultural context can be readily administered in non-Western populations (e.g., life review exercises; see Hendriks et al., 2018, for a meta-analysis and further examples).

Second, the randomized controlled trials discussed in this thesis placed an emphasis on ensuring internal validity, including the systematic elimination of third variable influences in controlled settings, which might have limited the generalizability of the findings to real-world settings (Campbell, 1986). Because field experiments, for example evaluations of the US Army's positive psychology-based Comprehensive Soldier Fitness program (see Cornum, Matthews, & Seligman, 2011; Eidelson, Pilisuk, & Soldz, 2011, for a review and some controversy) come to partially different conclusions regarding the effectiveness of positive-psychological interventions (e.g., that comparably brief interventions can have lasting effects on well-being and resilience; Seligman, 2019), researchers and practitioners should be aware that effect sizes, underlying mechanisms, and moderators might differ when positive-psychological interventions are applied in organizations, clinical settings, or schools (also see Hone, Jarden, & Schofield, 2014). Generally, researchers should be cautious about generalizing findings from this thesis to multicomponent positive-psychological intervention trials because interactions between different components of the program probably yields unique effect patterns (see Hendriks, Schotanus-Dijkstra, Hassankhan, Jong, & Bohlmeijer, 2019, for a review). Findings should, however, be used to decide which positive-psychological exercises (e.g., writing about one's best possible future) fit into specific training programs (e.g., optimism training; Braun & Ziemke, 2019).

Third, all studies in this thesis used self-report measures as indicators of well-being and related constructs. Although the applied scales are typically well validated, self-report measures are prone to

several response biases, especially desirability bias, even if anonymity is guaranteed (see Heintzelman, Trent, & King, 2015, for experimental evidence). Thus, it could be that the mean values of well-being related constructs reported in this thesis are inflated because participants' motivation to respond in a valued manner was greater than their motivation to respond honestly. More so, the observed relationships between well-being related constructs (e.g., positive affect and optimism) may be partially due to the fact that both variables were measured using self-reports and are desirable themselves. In addition, systematic group differences in the social desirability of well-being reports following different interventions (e.g., positive-psychological interventions and active controls) cannot be ruled out. Alternative assessments of well-being, for example peer reports, cognitive tasks, and physiological measures, are available and might help to bypass the issue of social desirability (e.g., Rickard, Chin, & Vella-Brodrick, 2015; Yetton, Revord, Margolis, Lyubomirsky, & Seitz, 2019). However, self-reports still seem the most direct way to assess well-being and have been shown to be meaningful even though they are biased to some degree (see Pavot, 2008; Sandvik, Diener, & Seidlitz, 2009, for a discussion).

6.5 Implications for Positive-Psychological Practice

One important objective of positive-psychological intervention research is to promote a strong link between research and practice. This includes further developing practical applications based on sound evidence (Jarden et al., 2019). Evidence-based practice, however, always requires that practitioners strike a balance between the best available knowledge regarding intervention effects and the unique needs of individual clients, groups, or organizational structures (see American Psychological Association, 2006, for a deeper discussion). Findings from this thesis hold two important implications that practitioners who apply positive-psychological interventions should be aware of.

6.5.1 In Doubt, Make Modest Claims About Intervention Effects

Meta-analytic results from Chapter 5 show that the best-possible-self intervention, if administered as a stand-alone exercise, is insufficient to lastingly increase well-being. In addition, there were no benefits of self-compassionate writing in the study discussed in Chapter 4. All in all, evidence regarding the efficacy of brief compassion-based interventions remains controversial (Imrie & Troop, 2012; Johnson & O'Brien, 2013, study 2; Kelly & Waring, 2018; Mosewich et al., 2013; Stern & Engeln, 2018, study 1; Wong & Mak, 2016). Furthermore, meta-analysts recently concluded that the effects of brief gratitude-based interventions may have been overestimated in the past (Davis et al., 2016; Dickens,

2017). A reanalysis of earlier meta-analyses (Sin & Lyubomirksy, 2009; Bolier et al., 2013) on the effects of positive-psychological interventions comes to similar results (White et al., 2019). Practitioners who work with positive-psychological interventions should clearly communicate these limitations to readers of online blogs and self-help books, life coaching clients, corporate training participants, or whoever is the recipient of positive-psychological interventions. In line with this, members of a working group that has developed ethical guidelines for positive psychology practice, stressed the importance of giving accurate information when delivering positive-psychological interventions (Jarden et al., 2019). This includes being aware of the limitations of certain positive-psychological interventions and recognizing the boundaries of one's own expertise as a provider of such interventions. As explained prior, findings from this thesis are limited in so far as they are based on studies that opted to maximize internal validity at the expense of applicability of the results to real-world settings. Probably the effects of positive-psychological interventions that are administered as part of a corporate training, personal coaching, or psychotherapy differ from the effects observed in controlled settings (e.g., Niemiec, 2018; Rashid & Seligman, 2018). Practitioners should discuss this with clients, too. For example, practitioners, who apply the best-possible-self intervention, could explain that writing about one's best possible future has been demonstrated to increase momentary positive affect and induce positive future expectations in controlled settings. Other benefits, including long-term increases in well-being, have not been observed. However, they seem principally possible, given the intervention is embedded into a comprehensive personal change process (e.g., positive psychotherapy; Rashid & Seligman, 2018) or if the intervention is part of a well-structured multicomponent program (Hendriks, Schotanus-Dijkstra et al., 2019). Practitioners may then explain that the best-possible-self intervention provides just one of the many possibilities to potentially increase well-being. In any case, it should be made clear that there is currently no appropriate evidence supporting the claim that the best-possible-self intervention by itself causes sustained increases in well-being. More examples on how to communicate the results of positive-psychological intervention research in an accurate and responsible manner are provided on the website of the Greater Good Science Center of the University of California in Berkeley (Greater Good in Action, 2019).

6.5.2 Use the Best-Possible-Self Intervention in Career Counseling

Although many stand-alone positive-psychological interventions may not provide long term benefits, there are situations in which brief interventions can be fruitfully applied. Specifically, results

from Chapter 3 indicate that the best-possible-self intervention helps to reduce acute ambivalent feelings related to one's life goals ($d = 0.36$, 95% CI [0.15, 0.57]). In addition, descriptive meta-analytic evidence from Chapter 5 suggests that the best-possible-self intervention temporarily reduces negative future expectations at posttest (e.g., lower approval of statements such as "You will make a decision you regret", $g = 0.40$, 95% CI [0.24, 0.57], based on 12 studies). Both findings suggest that the best-possible-self intervention can provide short-term benefits for individuals who seek to deal with momentary indecisiveness or experience high levels of conflict regarding their life goals. Practitioners are encouraged to take up this new perspective. For example, career coaches traditionally support clients at times of increased uncertainty, such as when choosing a profession or making other important career decisions (see Hazen & Steckler, 2018, for an introduction). The best-possible-self intervention might be one tool to help clients deal with temporary distress and become aware of overarching life goals and personal values. Coaches can choose to ask clients to write about their best-possible-future as a homework assignment or encourage clients to visualize their best possible future during a coaching session. The intervention should, however, never be implemented without the possibility of discussing its content. In addition, coaches should make sure that personal insights that may result from the intervention are further processed to achieve optimal results (e.g., by developing action plans and helping clients commit to certain life goals at the expense of others). Results are preliminary and practitioners should do their best to keep up to date with new developments regarding the effects of the best-possible-self intervention (Jarden et al., 2019).

6.6 Suggestions for Future Research

Research on positive-psychological interventions has experiences a strong upturn in the past two decades and the annual number of published positive-psychological intervention trials has increased from three in 1998 (the year of Seligman's historical inaugural speech as president of the American Psychological Association) to nine in 2009 (the year of Sin and Lyubomirsky's first meta-analysis on the effectiveness of positive-psychological interventions) and up to 49 in 2016 (the last year for which data were provided; Hendriks et al., 2019). This development offers numerous benefits, including the availability of broad evidence to comprehensively judge the effectiveness of positive-psychological interventions and multiple perspectives on controversial topics in the field (e.g., Brown & Rohrer, 2019). Not all recent publications, however, succeed in providing valuable insights and opening new lines of

thoughts. The following suggestions are intended to point to worthwhile research topics and provide

hands-on recommendations on how to improve positive-psychological intervention research in the future.

6.6.1 Consider Definitions, Conceptualizations, and Timing of Outcome Assessment

Results from Chapter 5 have shown that it matters when and how outcomes in positive-

psychological intervention trials are measured (e.g., larger effects for state measures and assessments

immediately after the intervention compared with effects for trait measures and follow-up assessments).

In addition, effects vary depending on how well-being is defined (e.g., positive affect vs. life satisfaction).

Future researchers should carefully decide how to define well-being in the context of specific studies.

They should also consciously differentiate between outcome measures that capture short-term changes in

state variables (e.g., momentary grateful affect), medium-term changes in daily mood or patterns of

thinking, behaving, and relating (e.g., current expectations about the future or health behaviors), and long-

term changes in subjective well-being (i.e., habitual affect and life satisfaction) or well-being relevant

traits (e.g., trait optimism). Specifically, future meta-analyses should (a) focus on the effects of well-

defined and self-contained interventions that are comparable across studies (e.g., manualized self-help

interventions); (b) examine effects on various well-being related constructs (e.g., positive affect vs. life

satisfaction) separately; (c) provide descriptive and, if possible, inferential statistics on effects over time

(e.g., immediately after the intervention vs. several days/weeks/months later); and (d) investigate whether

different conceptualizations of the same outcome (e.g., state vs. trait) influence results. Ideally, effects are

compared against active control conditions that are suitable to account for expectation effects. Data from

existing meta-analyses (e.g., on the effects of performing acts of kindness; 27 studies; Curry et al., 2018)

should be reanalyzed against this background (e.g., the authors combine affect ratings, life satisfaction,

and happiness into a compound measure and do not report follow-up effects). Such reanalyzes are

important because making careful distinctions regarding when and how outcomes were assessed helps to

avoid misunderstandings when interpreting results, even if making these distinction means that some

inferential statistics cannot be applied. In addition, such analyses help to build a broad knowledge base

regarding the mechanisms of positive-psychological interventions, which is currently lacking (see Fritz

& Lyubomirsky, 2018, for a review). It is also important that researchers are aware that the aim of

positive-psychological interventions is to improve well-being in the long run (Sin & Lyubomirsky, 2009).

Appropriate designs and measures should be used to quantify the degree to which specific programs

achieve this aim (e.g., randomized controlled designs with several weeks follow-up assessments of

habitual affect and life satisfaction ratings; see Hendriks et al., 2018, for examples). Outcomes that capture short- and medium-term changes in well-being related outcomes (e.g., momentary affect) are better suited to explain the processes by which positive-psychological interventions unfold their effects. Ideally, short-, medium-, and long-term outcomes are combined within a single trial (e.g., using ambulatory assessment; Csikszentmihalyi & Hunter, 2003; Fahrenberg, Myrtek, Pawlik, & Perrez, 2007). One randomized controlled mediation study showed that practicing loving-kindness meditation for seven weeks increased daily experiences of positive emotions (short-term), which explained sustained increases in personal resources (medium-term; e.g., higher mindfulness) and life satisfaction (long-term; Fredrickson et al., 2008). The follow-up period was two weeks and should ideally be longer. However, otherwise future researchers are advised to model this approach. In addition, it is recommended to look at other recent studies with innovative designs (e.g., Heekerens & Heinitz, 2019; O'Connell, O'Shea, & Gallagher, 2018; Schotanus-Dijkstra et al., 2019). Finally, to address the issue of desirability bias in self-reports of well-being, intervention studies should quantify the amount of socially desirable responses and use appropriate statistical controls (see Perinelli & Gremigni, 2016, for an introduction). This is important to rule out the possibility that observed intervention and mediation effects are due to biases in outcome assessments.

6.6.2 Further Examine Mechanisms of Positive-Psychological Interventions

There are two sound ways to gain insights into the processes underlying positive-psychological intervention. First, researchers can evaluate comprehensive happiness programs that can be expected to result in medium-term increases in well-being (e.g., multi-component positive-psychological interventions; Hendriks, Schotanus-Dijkstra et al., 2019). During the intervention period changes in theoretically relevant constructs (e.g., changes cognitions; Quoidbach et al., 2015) can be tracked and related to the effects of the programs at follow-up. Ideally, control interventions that account for expectation effects are used (i.e., not waitlist controls as repeatedly done in past studies; Fredrickson et al., 2008; Schotanus-Dijkstra et al., 2019). Second, researchers can investigate underlying mechanisms by examining the short- and medium-term effects of stand-alone interventions or active principles (e.g., the best-possible-self intervention or writing a gratitude letter). Although longer studies that examined the effects of multi-component interventions clearly have greater potential to yield useful insights, they are usually cost-intensive and require a lot of logistical effort. Smaller studies are quicker to implement and can, if carefully designed, provide valuable insights into the active components of more comprehensive

programs. For example, results from Chapter 4 question whether self-compassionate writing by itself provides benefits (Shapira & Mongrain, 2010). Self-compassionate writing is part of the eight weeks long mindful self-compassion program, which has been suggested to lastingly increase well-being (see Neff & Germer, 2013, for two randomized controlled trials). If certain components of a program, for example self-compassionate writing as part of the mindfulness self-compassion program, are found to provide little benefit to most participants, it will increase the likelihood that other components of the program, for example placing one's hand on one's heart in times of stress or repeating a set of memorized self-compassion phrases (self-soothing touch; Neff & Germer, 2013), drive the overall success of a program. This way, research into the active components of larger programs can provide a sound basis for including certain exercises (e.g., self-soothing touch) at the expense of others (e.g., self-compassionate writing), which helps to further develop existing happiness programs.

6.6.3 Further Examine Person-Specific and Contextual Moderators

Despite considerable effort, the studies included in this thesis have yielded few insights regarding the moderators of the effects of positive-psychological interventions. For example, it remains unclear which personal characteristics of participants moderate the effects of the best-possible-self intervention on positive affect and state optimism. Future studies could examine personality, coping styles, and individual preferences as moderators of intervention effects (Layous & Lyubomirsky, 2013). One challenge with research on person-specific moderators of positive-psychological interventions is that such moderators can usually not be experimentally manipulated. An alternative is to match participants with certain characteristics to specific positive-psychological interventions and investigate whether the matched interventions outperform unmatched interventions (see Schueller, 2011, for a deeper discussion). In addition, current statistical methods to investigate moderation effects require large samples to be sufficiently powered, particularly if intervention and moderation effects are small (e.g., Cohen, 2003). Thus, future studies should investigate person-specific moderators using comprehensive happiness programs that are likely to result in medium-sized effects and use adequate sample sizes. In addition, researchers should systematically investigate contextual moderators of positive-psychological intervention (e.g., number of sessions, instructions, delivery format). Wherever applicable, studies should experimentally manipulate a contextual moderator of interest (e.g., investigating the effect of administering the best-possible-self intervention once vs. over the course of one week). Another option is to use meta-analytic procedures (i.e., comparing the effect sizes of studies that used various delivery

formats). For example, meta-analytic results from Chapter 5 indicate that studies that administered the best-possible-self intervention online show no significant effect on positive affect. One problem with this approach is that the analyses were unable to effectively control for various third variable influences. For example, it could be that characteristics other than delivery format (e.g., use of less sensitive outcome measures) better explain why online best-possible-self trials show no effects. This consideration is particularly important given that one experimental study showed that online and in-person administrations were equally effective (Layous et al., 2013). Knowledge about whether and under which circumstances the best-possible-self intervention can be effectively applied in online settings is much needed because the exercise is already widely used in digital formats (e.g., by the commercial platform Happify; Parks et al., 2018). Future studies from independent research groups should clarify this issue. In addition, experimental studies should investigate the effect of manipulating participants motivation to participate in the gratitude letter exercise and self-compassionate writing (e.g., using peer testimonials as done by Layous et al., 2013).

6.6.4 Investigate Effects in Diverse Samples

Results from a current review suggest that positive-psychological interventions have predominantly been examined in Western countries (103 out of 147 randomized controlled trials; Hendriks et al., 2019). Thus, current intervention trials typically comprise samples of comparably well-educated and by international standards wealthy individuals. However, results also show that the number of publications from non-Western countries has sharply increased in the past five years and that it might catch up with publications from Western countries soon (Hendriks et al., 2019). Nevertheless, future studies should examine the effects of positive-psychological interventions in more diverse samples. First, this includes further examining intervention effects among participants from non-Western cultures. One important task then is to culturally adapt positive-psychological interventions and develop happiness programs that fit the needs of individuals from collectivistic cultures and various economic and social backgrounds. This is important to avoid potential side effects. For example, Fritz and Lyubomirsky (2018) explained that positive-psychological interventions that appeal to individualistic values, such as building happiness by striving towards autonomy-related goals, might clash with the collectivistic and interdependent perspectives inherent in the culture of, for example, China. In line with this, two studies that use the best-possible-self intervention among Chinese students reported no effect on positive affect (Liau, 2016; Auyeung & Mo, 2018). The ethical guidelines for positive psychology practice also include

this point, requesting practitioners and researchers to administer positive-psychological interventions in a culturally sensitive way (Jarden et al., 2019). Thus, future studies should further develop the best-possible-self intervention to better suit the needs of individuals from more collectivistic cultures (e.g., by highlighting goal pursuits that align with collectivistic cultures such as establishing better relationships with others). Generally, happiness interventions need to be carefully evaluated within new contexts before being applied on a large scale. Some activities, however, might be considered culture-free (e.g., life review) and yet others might even be more effective in non-Western countries (e.g., practicing forgiveness; see Hendriks et al., 2018; Hendriks, Warren et al., 2019, for a deeper discussion). Second, positive-psychological interventions should be further examined using clinical samples. Results from a recent meta-analysis of positive-psychological intervention among patients with psychiatric or somatic disorders indicate small posttest effects on a composite measure of affect ratings, life satisfaction, hope, optimism, and other well-being related constructs ($g = 0.28$, 95% CI [0.07, 0.48], 33 comparisons), depressive symptoms ($g = 0.26$, 95% CI [0.09, 0.45], 26 comparisons), and anxiety ($g = 0.47$, 95% CI [0.23, 0.71], 14 comparisons; Chakhssi, Kraiss, Sommers-Spijkerman, & Bohlmeijer, 2018). Subgroup analyses show that the effects are driven by studies that used interventions that were administered over the course of at least eight weeks (e.g., positive psychotherapy; Rashid & Seligman, 2018). Effects were maintained at 8 to 12 weeks follow-up assessments ($g = 0.41$ for well-being, $g = 0.21$ for depressive symptoms, and $g = 0.35$ for anxiety; Chakhssi et al., 2018). Notably, the effect on the well-being composite at posttest was larger for interventions that were guided by a trained therapist ($g = 0.36$, 95% CI [0.16, 0.62], 26 comparisons), whereas studies that applied positive-psychological interventions without guidance show no effect ($g = -0.12$, 95% CI [-0.43, 0.20], 7 comparisons). A similar effect emerged for depressive symptoms. All in all, current findings suggest that positive-psychological interventions are feasible in clinical settings (Chakhssi et al., 2018; also see Moskowitz, 2010). It, however, remains unclear whether positive-psychological interventions are superior to established approaches (e.g., cognitive-behavioral therapy). In addition, little is known about whether current state of the art treatments benefit from incorporating positive psychology principles. Future researchers are encouraged to investigate these questions. For example, researchers have developed a positive affect treatment to increase reward sensitivity among patients who suffer from anhedonia (Craske, Meuret, Ritz, Treanor, & Dour, 2016; also see Moskowitz, 2010). The treatment includes exercises that are similar to positive-psychological interventions (e.g., imagining positive future events; Craske et al., 2016). Thus,

one might think of the treatment as a multicomponent positive-psychological intervention program designed to fit the needs of distressed individuals. Preliminary evidence from one randomized controlled trial indicates that the 15 weeks long treatment substantially increases positive affect and decreases negative affect among patients suffering from depression and anxiety during the treatment period and results in larger decreases in depressive symptoms up to six months after the intervention (Cohen's $d =$ 0.29; Craske et al., 2019). The treatment was compared against a specific form of cognitive behavioral therapy designed to deal with negative emotions (e.g., the comparison treatment included exercises on decreasing avoidance behavior and reducing threat appraisals but not behavioral activation towards rewarding activities). Given that the control condition was designed to help participants deal with distress, it is puzzling that the positive affect treatment was also more effective in reducing negative affect. Future studies should compare positive affect treatment against more rigorous controls (e.g., complete cognitive-behavioral therapy). In addition, mediation analyses could be used to investigate whether intervention effects at follow-up can be predicted by changes in theoretically proposed mechanisms (e.g., increased anticipation or motivation for reward; Craske et al., 2019) during the intervention period. Generally, it seems important to address questions regarding the theoretically assumed specificity of positive-psychological intervention effects in clinical settings because evidence shows that many changes resulting from psychotherapeutic treatments can be explained by common factors (e.g., alliance or empathy; Wampold, 2015).

6.7 Conclusion

Investigating the effects, mediators, and moderators of positive-psychological interventions remains an important objective in the field of positive psychology. This thesis has shown that the size and durability of the effects of the popular best-possible-self intervention may have been overestimated in the past. In addition, it has demonstrated that different positive-psychological interventions have both unique and shared effects, which should be considered when further developing current theories and practical applications. Finally, the challenge of successfully identifying moderators of the effects of positive-psychological interventions using traditional and meta-analytic methods has been clarified.

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Appendix

7.1 Abstract

Previous research has substantially contributed to further developing psychological interventions that aim to alleviate symptoms of psychopathology. In contrast, interventions that aim to build happiness are still in their infancy. Evidence from the past two decades indicates that positive-psychological interventions—relatively simple intentional activities that are designed to cultivate positive thoughts, emotions, and behaviors increase various indicators of well-being. Their effects, however, are typically small and decrease over time, which may be resolved by further developing them. Effective further development requires comprehensive knowledge of the effects, mediators, and moderators of positive-psychological interventions. In this context, three important questions remain wholly or partially unanswered: First, what are the effects of positive-psychological interventions (e.g., practical significance, additional effects, etc.)? Second, how do positive-psychological interventions operate (i.e., what are relevant mediators)? Third, for whom and under which conditions do positive-psychological interventions show greater effects (i.e., what are relevant moderators)? The aim of this thesis is to provide answers to these questions in order to help increasing the effectiveness of positive-psychological interventions. To allow for a detailed and nuanced discussion, this thesis focuses on three prominent positive-psychological interventions: the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing. Researchers proposed that these three interventions lastingly increase well-being through either specific mechanisms (e.g., adopting a grateful outlook) or common mechanisms (e.g., activation of positive emotions or positive self-relevant thoughts). In addition, it has been suggested that these interventions yield better results for certain types of people, largely depending on whether features of the activity fit characteristics of participants. Current evidence on this topic, however, is either weak or inconclusive and recommendations on how to further develop positive psychological interventions are rarely feasible.

To address this issue, three empirical studies were conducted. The aim of the first study was to investigate goal ambivalence and positive future expectations as mediators of the effect of the best-possible-self intervention on positive affect. Results from a longitudinal randomized controlled trial with baseline, immediate posttest, and 1-week follow-up assessments show that participating in the best-possible-self intervention increased positive affect and reduced goal ambivalence. Positive future expectations were not significantly affected. In addition, mediation analysis results from two latent cross-lagged panel design models indicate that neither goal ambivalence nor positive future expectations immediately after the intervention predicted increased positive affect in the following week. The aim of

the second study was to examine unique and shared effects of the best-possible-self intervention, the gratitude letter exercise, and self-compassionate writing. In addition, emotional self-awareness and trait gratitude were investigated as moderators. Results from an online randomized controlled trial with four groups show that participants in the best-possible-self intervention reported higher state optimism and more positive self-relevant thoughts immediately after the activity, whereas participants in the gratitude letter exercise reported higher state gratitude and more positive self-relevant thoughts. Self-compassionate writing showed no effects. Neither emotional self-awareness nor trait gratitude moderated the effects. The aims of the third study were to comprehensively examine the effects of the best-possible-self intervention and to investigate contextual moderators. Meta-analytic results based on 34 randomized controlled intervention trials that were identified through a systematic literature search show small effects on positive affect and optimism. Effects were pronounced if the outcome was measured immediately after the intervention relative to a few days later. In addition, effects were larger if positive affect was assessed asking participants how they feel "at the moment" rather than how they feel "in general" and if optimism was conceptualized as positive future expectations rather than a general orientation in life. Descriptive results indicate no effects approximately one week after the intervention. There were no significant effects on negative affect, life satisfaction, depressive symptoms, or happiness. Administering the best-possible-self intervention online showed no significant effect on positive affect.

In conclusion, effects of positive-psychological intervention may have been overestimated in the past. Specifically, the best-possible-self intervention, when administered as a stand-alone exercise, is insufficient to perpetuate lasting increases in well-being. Positive psychology practitioners who apply the intervention to clients should be aware of this limitation and must ensure that clients expectations are reasonable. The best-possible-self intervention may, however, be suitable to temporarily increase positive affect and optimism. In addition, the exercise has been shown to reduce ambivalent feelings regarding one's life goals, which may be particularly relevant for career coaching clients who face a transition period. Moreover, the best-possible-self intervention and the gratitude letter exercise show both unique and shared effects that might serve as mediators. Thus, further developing these interventions might start with both strengthening specific mechanisms (e.g., explicitly encouraging participants to adopt a grateful outlook while writing a gratitude letter) and extending general working principles (e.g., providing opportunities that allow participants to see themselves in a positive light, for example, as a grateful person). Current theoretical frameworks focus either on specific or common mechanisms and future

researchers should develop a framework that incorporates both types of mechanisms. In addition, online variations of the best-possible-self intervention seem to be less effective than in-person administrations. Alternative explanations for this observation could not be ruled out. Thus, future researchers should replicate this finding in more controlled settings. Finally, besides differences between online and in-person administrations, positive-psychological interventions seem to work equally well for different types of people and under varying circumstances. More research on the differential effects of positive-psychological interventions is needed to better understand when and how such interventions should be applied.

7.2 Zusammenfassung (Abstract in German)

Die psychologische Forschung hat bedeutsame Fortschritte in der Weiterentwicklung von Interventionen zur Milderung von psychopathologischen Symptomen gemacht. Gleichzeitig sind Interventionen, deren Ziel die Steigerung von Glück ist, häufig unterentwickelt. Studien der vergangenen zwei Jahrzehnte konnten zeigen, dass positiv-psychologische Interventionen – relativ einfache, intentionale Aktivitäten, deren Ziel der Aufbau positiver Gedanken, Gefühle und Emotionen ist, geeignet sind um verschiedene Indikatoren von Wohlbefinden zu steigern. Allerdings sind die Effekte dieser Interventionen meistens klein und nehmen über die Zeit hinweg ab, was durch eine Weiterentwicklung der Interventionen verbessert werden könnte. Effektive Weiterentwicklung setzt Wissen über die Effekte, Mediatoren und Moderatoren positiv-psychologischer Interventionen voraus. In diesem Zusammenhang bleiben drei wichtige Fragen vollständig oder teilweise unbeantwortet: Erstens, welche Effekte haben positiv-psychologische Interventionen (z.B., praktische Bedeutsamkeit, bislang unbekannte Effekte, usw.)? Zweitens, wie funktionieren positiv-psychologische Interventionen (i.a.W., was sind relevante Mediatoren)? Drittens, für wen und unter welchen Umständen sind positiv-psychologische Interventionen wirksam (i.a.W., was sind relevante Moderatoren)? Ziel der vorliegenden Arbeit ist es, Antworten auf diese Fragen zu formulieren, um eine Grundlage für die Weiterentwicklung positiv-psychologischer Interventionen zu schaffen. Um eine detaillierte und differenzierte Diskussion sicherzustellen, liegt der Fokus auf drei populären positiv-psychologische Interventionen: der Best-Possible-Self Intervention, dem Dankbarkeitsbrief und Schreiben, um Selbstmitgefühl aufzubauen. Forscher vermuten, dass die Effekte positiv-psychologischer Interventionen auf Wohlbefinden entweder durch spezifische Wirkmechanismen (z.B., Entwicklung einer dankbaren Lebenshaltung) oder allgemeine Wirkmechanismen (z.B., Aktivierung positiver Emotionen oder positiver selbst-relevanter Gedanken) erklärt werden können. Außerdem wurde vorgeschlagen, dass positiv-psychologische Interventionen für bestimmte Personengruppen bessere Effekte erzielen, sofern die Besonderheiten der jeweiligen Intervention zu bestimmten Merkmalen der Teilnehmenden passen. Aktuelle Evidenz in Bezug auf diese Hypothese ist entweder schwach oder widersprüchlich. Folglich sind zuverlässige Empfehlungen für die effektive Anwendung und Weiterentwicklung positiv-psychologischer Interventionen momentan kaum möglich.

Um dieses Problem anzugehen, wurden drei empirische Studien durchgeführt. Ziel der ersten Studie war Zielambivalenz und positive Zukunftserwartungen als Mediatoren für die Effekte der Best-Possible-Self Intervention auf positiven Affekt zu untersuchen. Ergebnisse aus der randomisiert kontrollierten Längsschnittstudie mit Prätestmessung, unmittelbarer Posttestmessung, und einwöchiger

Folgemessung zeigen, dass Teilnehmende in der Best-Possible-Self Intervention einen stärkeren Anstieg in positivem Affekt und eine stärkere Abnahme in Zielambivalenz berichten als Teilnehmende in der Kontrollgruppe. Es gab keinen signifikanten Effekt auf positive Zukunftserwartungen. Darüber hinaus zeigen Mediationsergebnisse aus zwei latenten Cross-Lagged Panel Design Modellen, dass weder Zielambivalenz noch positive Zukunftserwartungen direkt nach der Intervention positiven Affekt in der Woche nach der Intervention vorhersagten. Ziel der zweiten Studie war die Untersuchung der spezifischen und unspezifischen Effekte der Best-Possible-Self Intevention, des Dankbarkeitsbriefs und von Schreiben, um Selbstmitgefühl aufzubauen. Außerdem wurden emotionale Selbstaufmerksamkeit und dispositionale Dankbarkeit als Moderatoren untersucht. Ergebnisse einer randomisiert kontrollierten Onlinestudie mit vier Gruppen zeigen, dass Teilnehmende in der Best-Possible-Self Intervention höheren momentanen Optimismus und mehr positive selbstrelevante Gedanken direkt nach der Übung berichten, wohingegen Teilnehmende, die einen Dankbarkeitsbrief schrieben, höhere momentane Dankbarkeit und mehr positive selbstrelevante Gedanken berichteten. Schreiben, um Selbstmitgefühl aufzubauen zeigte keinen Effekt. Weder emotionale Selbstaufmerksamkeit noch dispositionale Dankbarkeit moderierten die Effekte. Ziele der dritten Studie waren die umfassende Untersuchung der Effekte der Best-Possible-Self Intervention und die Erforschung von kontextbezogenen Moderatoren. Meta-analytische Befunde, basierend auf 34 randomisiert kontrollierten Wirksamkeitsstudien, die durch eine systematische Literatursuche identifiziert wurden, zeigen kleine Effekte auf positiven Affekt und Optimismus. Die Effekte waren größer, wenn die Endpunkte direkt nach der Intervention gemessen wurden, verglichen mit Messungen ein oder mehrere Tage später. Außerdem waren die Effekte größer, wenn Teilnehmende nach ihrem positiven Affekt „im Moment" gefragt wurden, verglichen mit Messungen, die nach positivem Affekt „im Allgemeinen" fragten. Der Effekt auf Optimismus war größer, wenn positive Zukunftserwartungen und nicht eine generelle optimistische Lebenshaltung erfasst wurden. Deskriptive Ergebnisse deuten darauf hin, dass die Effekte etwa eine Woche nach der Intervention verschwinden. Es gab keine signifikanten Effekte auf negativen Affekt, Lebenszufriedenheit, depressive Symptomatik oder Glück. Onlineanwendungen der Intervention zeigten keinen signifikanten Effekt auf positiven Affekt.

Zusammenfassen lässt sich festhalten, dass die Effekte positive-psychologischer Interventionen in der Vergangenheit überschätzt worden sein könnten. Insbesondere die Best-Possible-Self Intervention, wenn sie als eigenständige Übung durchgeführt wird, scheint unzureichend, um langfristige Verbesserungen im Wohlbefinden zu bewirken. Anwender der Positiven Psychologie, welche ihren

Klienten diese Intervention anbieten, sollten sich dieser Einschränkung bewusst sein und sicherstellen, dass ihre Klienten angemessene Erwartungen in Bezug auf die Wirkung der Intervention entwickeln. Die Best-Possible Self Intervention ist besser geeignet, um kurzfristige Anstiege in positivem Affekt und Optimismus hervorzurufen. Ergebnisse der vorliegenden Arbeit zeigten außerdem, dass die Übung ambivalente Gefühle in Bezug auf die eigenen Lebensziele reduzieren kann, was beispielsweise Teilnehmenden einer Karriereberatung helfen könnte, aktuelle berufliche Veränderungen effektiv zu bewältigen. Darüber hinaus konnten für die Best-Possible-Self Intervention und den Dankesbrief spezifische und unspezifische Effekte nachgewiesen werden, die jeweils als Mediatoren fungieren könnten. Die Weiterentwicklung positiv-psychologischer Interventionen sollte sich darauf konzentrieren, sowohl spezifische Wirkmechanismen zu stärken (z.B., Teilnehmende, die einen Dankbarkeitsbrief schreiben, werden explizit ermutigt eine dankbare Lebenshaltung zu kultivieren), als auch allgemeine Wirkmechanismen auszubauen (z.B., Teilnehmenden wird die Möglichkeit geboten, sich selber von einer positiven Seite zu sehen, beispielsweise als dankbare Person). Aktuelle Theorien fokussieren entweder auf spezifische oder allgemeine Wirkmechanismen und zukünftige Forschung sollte ein Rahmenmodell entwickeln, das beide Wirkprinzipien abbildet. Darüber hinaus zeigen die Ergebnisse dieser Arbeit, dass Onlineanwendungen der Best-Possible-Self Intervention weniger wirksam sind als in vivo Anwendungen. Allerdings konnte dieser Befund nicht gegen den Einfluss von Drittvariablen abgesichert werden und zukünftige Forschung sollte das Ergebnis unter kontrollierten Bedingungen replizieren. Abgesehen von Unterschieden zwischen online und in vivo Anwendungen, zeigen die Ergebnisse dieser Arbeit, dass positiv-psychologische Interventionen für verschiedene Personengruppen und unter unterschiedlichen Bedingungen wirksam sind. Nichtsdestotrotz sollten zukünftige Forschungsarbeiten die Frage nach differentiellen Effekten von positive-psychologischen Interventionen weiter beleuchten, auch um Empfehlungen für die Praxis abzuleiten.

7.4 Item-level Effects (Chapter 3)

Table 7.A1 Single Item Means and Standard Deviations for Goal Ambivalence in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (*n* = 92)	SD t0 (*n* = 92)	Mean t1 (*n* = 90)	SD t0 (*n* = 90)	Mean t2 (*n* = 89)	SD t2 (*n* = 89)	Mean t0 (*n* = 96)	SD t0 (*n* = 96)	Mean t1 (*n* = 86)	SD t1 (*n* = 86)	Mean t2 (*n* = 87)	SD t2 (*n* = 87)
… habe ich gemischte Gefühle.	4.05	1.71	3.77	1.55	3.66	1.51	4.09	1.66	4.20	1.65	4.08	1.61
… fühle ich mich im Konflikt.	3.25	1.73	2.83	1.40	2.99	1.56	3.21	1.60	3.30	1.50	3.32	1.64
… sind meine Gedanken sowohl positiv als auch negativ	4.13	1.76	3.92	1.72	4.10	1.71	4.26	1.66	4.38	1.63	4.45	1.46
… sind meine Überlegungen widersprüchlich	3.20	1.62	2.82	1.52	3.02	1.59	3.07	1.58	3.48	1.52	3.22	1.56
… bin ich hin- und hergerissen.	3.51	1.69	3.07	1.48	3.08	1.49	3.42	1.67	3.57	1.67	3.39	1.66
… bin ich unentschlossen.	3.45	1.69	3.06	1.65	3.11	1.59	3.21	1.64	3.27	1.66	3.21	1.59
… sagt mein Bauch etwas anderes als mein Kopf	2.92	1.58	2.62	1.61	2.90	1.52	3.24	1.77	3.19	1.64	3.05	1.59
… stehen meine Gefühle im Gegensatz zu meinen	2.41	1.34	2.18	1.34	2.36	1.46	2.59	1.44	2.67	1.56	2-59	1.47

Note. We used the following instructions: „Wenn ich an meine Lebensziele denke …". n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A2 Single Item Means and Standard Deviations for Positive Affect in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (n = 92)	SD t0 (n = 92)	Mean t1 (n = 90)	SD t0 (n = 90)	Mean t2 (n = 89)	SD t2 (n = 89)	Mean t0 (n = 96)	SD t0 (n = 96)	Mean t1 (n = 86)	SD t1 (n = 86)	Mean t2 (n = 87)	SD t2 (n = 87)
aktiv	3.48	0.82	3.12	0.96	3.57	0.94	3.45	0.88	2.99	0.91	3.52	0.98
interessiert	3.96	0.81	3.81	0.88	3.76	0.91	4.03	0.85	3.57	0.95	3.69	0.77
freudig erregt	3.32	1.01	3.26	1.11	3.48	0.94	3.16	1.03	2.78	1.12	3.39	0.98
stark	3.30	0.86	3.28	1.03	3.27	1.02	3.26	1.02	2.98	1.12	3.10	1.07
angeregt	3.11	0.93	3.37	1.11	3.21	1.08	3.26	0.99	1.24	0.67	3.18	1.00
stolz	3.02	0.97	3.17	1.06	2.94	1.03	2.97	1.07	2.72	1.22	2.92	1.08
begeistert	3.36	1.01	3.23	1.19	3.27	1.09	3.43	1.01	2.58	1.15	3.32	1.01
wach	3.36	0.83	3.32	1.02	3.28	0.75	3.34	0.82	3.24	0.92	3.14	0.88
entschlossen	3.46	1.01	3.52	1.01	3.43	1.03	3.51	0.96	3.35	1.13	3.37	0.98
aufmerksam	3.67	0.77	3.81	0.73	3.55	0.83	3.70	0.80	3.62	0.92	3.55	0.90

Note. We used the following instructions: t0 = „Im Allgemeinen fühle ich mich …"; t1 = „Im Moment fühle ich mich …"; t2 = „In der letzten Woche fühlte ich mich …". n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A3 Single Item Means and Standard Deviations for Negative Affect in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (*n* = 92)	SD t0 (*n* = 92)	Mean t1 (*n* = 90)	SD t0 (*n* = 90)	Mean t2 (*n* = 89)	SD t2 (*n* = 89)	Mean t0 (*n* = 96)	SD t0 (*n* = 96)	Mean t1 (*n* = 86)	SD t1 (*n* = 86)	Mean t2 (*n* = 87)	SD t2 (*n* = 87)
bekümmert	2.20	1.04	1.79	0.88	2.11	1.12	2.31	1.09	1.98	0.98	2.36	1.03
verärgert	1.73	0.89	0.04	10.57	1.78	0.89	1.69	0.83	1.42	0.83	2.05	0.99
schuldig	1.50	0.93	1.18	0.41	1.45	0.87	1.60	0.83	1.41	0.76	1.57	0.96
erschrocken	1.37	0.75	1.16	0.42	1.31	0.67	1.46	0.78	1.16	0.48	1.49	0.90
feindselig	1.35	0.60	1.08	0.27	1.39	0.81	1.35	0.63	1.24	0.67	1.37	0.63
gereizt	2.04	0.98	1.34	0.64	2.04	1.02	2.01	0.84	1.72	0.98	2.33	1.05
beschämt	1.48	0.86	1.19	0.47	1.40	0.77	1.52	0.74	1.31	0.74	1.60	0.87
nervös	2.24	0.87	1.63	0.92	2.00	1.01	2.32	1.01	1.70	0.98	2.43	1.13
durcheinander	2.29	1.13	1.83	1.06	2.30	1.28	2.19	0.97	2.05	1.15	2.43	1.21
ängstlich	1.99	0.94	1.63	0.93	1.82	1.07	2.36	1.11	1.70	1.11	1.97	1.02

Note. We used the following instructions: t0 = „Im Allgemeinen fühle ich mich …"; t1 = „Im Moment fühle ich mich …"; t2 = „In der letzten Woche fühlte ich mich …". n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A4 Single Item Means and Standard Deviations for Gratitude in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (n = 92)	SD t0 (n = 92)	Mean t1 (n = 90)	SD t0 (n = 90)	Mean t2 (n = 89)	SD t2 (n = 89)	Mean t0 (n = 96)	SD t0 (n = 96)	Mean t1 (n = 86)	SD t1 (n = 86)	Mean t2 (n = 87)	SD t2 (n = 87)
Ich habe so vieles im Leben, wofür ich dankbar sein kann.	6.25	0.89	6.36	0.80	6.82	0.77	6.25	0.91	6.35	0.93	6.40	0.81
Müsste ich alles aufschreiben, wo für ich je dankbar war, dann würde das eine sehr lange Liste ergeben.	5.71	1.24	5.90	1.12	5.89	1.39	5.77	1.20	6.13	1.04	5.94	1.27
Wenn ich mir die Welt ansehe, dann kann ich nicht viel erkennen, wofür ich dankbar sein könnte. (R)	1.99	1.11	1.90	1.13	1.93	1.41	1.78	1.12	0.83	10.98	2.33	1.82
Ich empfinde vielen verschiedenen Menschen gegenüber Dankbarkeit.	5.45	1.37	5.69	1.15	5.82	1.17	5.84	1.11	5.80	1.42	5.89	1.10
Mit zunehmendem Alter kann ich Menschen, Erlebnisse oder Augenblicke besser wertschätzen, die Teil meiner Lebensgeschichte waren.	5.79	1.25	5.94	0.96	5.73	1.06	5.65	1.23	5.93	1.00	5.55	1.32
Es kann sehr viel Zeit vergehen, bis ich jemandem oder für etwas dankbar bin. (R)	2.55	1.304	2.91	1.56	3.02	1.57	2.38	1.28	2.77	1.55	2.69	1.44

Note. No specific instructions were used. R = reverse coded item; n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A5 Single Item Means and Standard Deviations for Hope in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (n = 92)	SD t0 (n = 92)	Mean t1 (n = 90)	SD t0 (n = 90)	Mean t2 (n = 89)	SD t2 (n = 89)	Mean t0 (n = 96)	SD t0 (n = 96)	Mean t1 (n = 86)	SD t1 (n = 86)	Mean t2 (n = 86)	SD t2 (n = 86)
Sollte ich mich in einer Zwickmühle befinden, würden mir viele Auswege einfallen. (path)	3.95	0.97	4.28	0.96	4.16	1.04	3.90	1.14	4.05	0.99	4.10	1.01
Momentan verfolge ich meine Ziele mit Elan. (agency)	4.38	1.13	4.50	1.00	4.49	1.09	4.32	1.01	4.44	1.00	4.30	1.16
Für jegliche Probleme, die sich mir momentan stellen, gibt es zahlreiche Lösungen. (path)	3.91	1.13	3.92	1.00	4.03	1.15	3.85	1.15	3.99	1.05	3.86	1.23
Im Moment betrachte ich mich als recht erfolgreich. (agency)	4.18	1.03	4.34	1.04	4.43	1.04	4.04	1.05	4.15	1.11	4.14	0.93
Mir fallen viele Strategien ein, um meine derzeitigen Ziele zu erreichen. (path)	4.17	0.96	4.21	0.93	4.22	1.01	4.06	1.03	4.24	1.05	4.14	0.98
Im Moment erreiche ich die Ziele, die ich mir selbst gesteckt habe. (agency)	4.35	1.10	4.50	1.09	4.40	1.10	4.15	1.05	4.26	1.08	4.28	1.02

Note. Participants were instructed to answer the questions in view of their current life situation ("Wähle anhand der unten aufgeführten Skala die Option aus, die am besten beschreibt wie du dich aktuell siehst"). Path = pathways subscale; agency = agency subscale; n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A6 Single Item Means and Standard Deviations for Positive Future Expectations in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (*n* = 92)	SD t0 (*n* = 92)	Mean t1 (*n* = 90)	SD t0 (*n* = 90)	Mean t2 (*n* = 89)	SD t2 (*n* = 89)	Mean t0 (*n* = 96)	SD t0 (*n* = 96)	Mean t1 (*n* = 86)	SD t1 (*n* = 86)	Mean t2 (*n* = 87)	SD t2 (*n* = 87)
Du wirst in deinem Studium/Beruf gut abschneiden.	5.46	1.00	5.68	0.87	5.58	0.85	5.07	1.21	5.31	1.04	5.29	1.07
Du wirst erreichen, was du geplant hast.	5.43	1.22	4.34	11.06	5.58	1.06	5.03	1.24	5.11	1.16	5.33	1.17
Du wirst fit und gesund sein.	5.17	1.03	5.10	1.11	5.27	1.12	4.98	1.29	4.81	1.26	5.03	1.29
Du wirst viel Zufriedenheit aus deinem Beruf ziehen.	5.61	1.20	5.62	1.06	5.73	1.13	5.36	1.13	5.33	1.20	5.53	1.07
Du wirst deine Arbeitsbelastung leicht bewältigen.	4.47	1.31	4.89	1.27	4.48	1.34	4.21	1.32	4.36	1.43	4.52	1.41
Du wirst ein langes Leben haben.	5.14	1.24	5.18	1.15	5.13	1.14	4.94	1.19	3.74	11.26	5.15	1.08
Die Dinge werden sich so entwickeln, wie du es gehofft hast.	5.14	0.99	5.30	1.08	5.13	1.10	4.72	1.19	4.76	1.22	5.07	1.17
Du wirst dich lebendig und gut fühlen.	5.30	1.21	4.42	11.08	5.66	1.16	5.29	1.21	5.19	1.22	5.36	1.10
Du wirst gute und andauernde Freundschaften knüpfen.	5.38	1.27	5.66	1.16	5.63	1.20	5.35	1.38	5.72	1.30	5.61	1.10
Menschen, die dich treffen, werden dich mögen.	5.50	0.86	5.49	0.94	5.61	0.93	5.29	1.03	5.33	1.08	5.52	0.98

Note. We used the following instructions: "Wie wahrscheinlich ist es für dich, dass dir diese Ereignisse in Zukunft passieren werden?". n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A7 Single Item Means and Standard Deviations for Negative Future Expectations in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (n = 92)	SD t0 (n = 92)	Mean t1 (n = 90)	SD t0 (n = 90)	Mean t2 (n = 89)	SD t2 (n = 89)	Mean t0 (n = 96)	SD t0 (n = 96)	Mean t1 (n = 86)	SD t1 (n = 86)	Mean t2 (n = 87)	SD t2 (n = 87)
Du wirst gesundheitliche Probleme haben.	3.28	1.52	3.46	1.59	3.22	1.50	3.43	1.53	3.62	1.74	3.53	1.68
Du wirst unzufrieden mit dir selbst sein.	3.36	1.71	3.07	1.55	3.17	1.69	3.90	1.70	3.69	1.64	3.83	1.73
Du wirst ernsthaftes Unglück erfahren.	3.05	1.61	2.77	1.41	3.09	1.56	2.99	1.57	1.81	11.12	2.94	1.58
Die Leute werden dich stumpfsinnig und langweilig finden.	2.03	1.02	2.10	1.09	1.88	1.05	2.30	1.21	2.21	1.20	2.08	1.03
Die Leute werden denken, dass du ein Versager bzw. eine Versagerin bist.	1.89	1.17	1.71	0.88	1.75	0.93	2.15	1.27	1.83	1.14	2.05	1.22
Du wirst viele Fehler machen.	4.34	1.58	4.49	1.52	4.25	1.55	4.15	1.54	4.43	1.59	4.40	1.58
Du wirst mit deinem Studium / deiner Arbeit ernsthaft zeitlich zurückliegen.	2.82	1.46	2.71	1.36	2.69	1.30	3.00	1.62	2.90	1.52	2.89	1.46
Du wirst dich verbraucht fühlen und wenig Energie haben.	3.54	1.67	2.96	1.58	3.25	1.52	3.80	1.5	3.41	1.56	3.71	1.68
Du wirst viele unglückliche Momente erfahren.	3.28	1.61	3.32	1.58	3.43	1.66	3.64	1.63	3.81	1.71	3.53	1.59
Du wirst nicht fähig sein, deiner Verantwortung gerecht zu werden.	2.55	1.40	2.48	1.29	2.48	1.30	2.90	1.41	2.92	1.56	2.90	1.49

Note. We used the following instructions: "Wie wahrscheinlich ist es für dich, dass dir diese Ereignisse in Zukunft passieren werden?". n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

Table 7.A8 Single Item Means and Standard Deviations for Life Satisfaction in the Best-Possible-Self and Control Conditions at Pretest, Posttest, and Follow-up

Item text	BPS Intervention						Control					
	Mean t0 (n = 92)	SD t0 (n = 92)	Mean t1 (n = 90)	SD t0 (n = 90)	Mean t2 (n = 89)	SD t2 (n = 89)	Mean t0 (n = 96)	SD t0 (n = 96)	Mean t1 (n = 86)	SD t1 (n = 86)	Mean t2 (n = 87)	SD t2 (n = 87)
In den meisten Bereichen entspricht mein Leben meinen Idealvorstellungen.	3.54	1.08	3.69	0.86	3.65	0.94	3.38	1.03	3.50	1.07	3.66	1.04
Meine Lebensbedingungen sind ausgezeichnet.	3.96	0.90	4.03	0.87	4.12	0.86	3.94	0.87	5.10	0.92	4.25	0.77
Ich bin mit meinem Leben zufrieden.	3.87	0.73	3.87	0.82	4.00	0.80	3.88	0.85	3.84	0.87	3.97	0.87
Bisher habe ich die wesentlichen Dinge erreicht, die ich mir für mein Leben wünsche.	3.75	0.95	3.84	0.90	3.87	0.84	3.70	0.99	3.76	1.02	3.84	1.00
Wenn ich mein Leben noch einmal leben könnte, würde ich kaum etwas ändern.	3.16	1.20	3.37	1.16	3.45	1.07	3.25	1.19	3.50	1.15	3.41	1.12

Note. No specific instructions were used. n = number of participants; t0 = pretest; t1 = posttest; t2 = follow-up.

7.5 Item-level Effects (Chapter 4)

Table 7.A9 Single Item Means and Standard Deviations for Momentary Gratitude in the Intervention and Control Conditions at Pretest, Posttest, and Follow-up

Item text	Optimism condition (*n* = 110)		Gratitude condition (*n* = 105)		Self-compassion condition (*n* = 104)		Control condition (*n* = 106)		Total (*n* = 425)	
	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1
Im Moment empfinde ich einem Menschen gegenüber Dankbarkeit.	5.66	1.51	5.91	1.24	5.41	1.45	5.22	1.51	5.55	1.45
Im Moment kann ich einen Menschen gut wertschätzen, der Teil meiner Lebensgeschichte war.	5.89	1.31	6.13	1.13	5.68	1.37	5.68	1.33	5.85	1.30
Im Moment habe ich etwas im Leben, wofür ich dankbar sein kann.	6.18	1.13	6.08	1.13	5.94	1.31	5.86	1.26	6.02	1.21

Note. No specific instructions were used. t1 = immediate posttest; n = number of participants.

Table 7.A10 Single Item Means and Standard Deviations for Momentary Optimism in the Intervention and Control Conditions at Pretest, Posttest, and Follow-up

Item text	Optimism condition (n = 110)		Gratitude condition (n = 105)		Self-compassion condition (n = 104)		Control condition (n = 106)		Total (n = 425)	
	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1
Im Moment sehe ich meine Zukunft optimistisch.	3.89	1.10	3.87	1.0	3.70	1.13	3.67	0.99	3.78	1.06
Im Moment erwarte ich, dass mir mehr gute als schlechte Dinge widerfahren.	3.98	1.00	3.77	0.98	3.73	1.15	3.65	1.02	3.79	1.05
Im Moment erwarte ich das Beste von meinem Leben.	3.87	0.99	3.50	1.03	3.50	1.19	3.33	1.12	3.56	1.10

Note. No specific instructions were used. t1 = immediate posttest; n = number of participants.

Table 7.A11 Single Item Means and Standard Deviations for Momentary Self-Compassion in the Intervention and Control Conditions at Pretest, Posttest, and Follow-up

Item text	Optimism condition (n = 110)		Gratitude condition (n = 105)		Self-compassion condition (n = 104)		Control condition (n = 106)		Total (n = 425)	
	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1
Im Moment schenke ich mir selbst die Zuwendung und Einfühlsamkeit, die ich brauche.	3.63	1.06	3.44	1.01	3.39	1.01	3.38	1.10	3.46	1.05
Im Moment akzeptiere ich meine Fehler und Schwächen.	3.76	0.94	3.67	1.00	3.60	0.98	3.58	1.03	3.65	0.99
Im Moment missbillige und verurteile ich meine Fehler und Schwächen.	3.51	1.05	3.30	1.09	3.11	1.19	3.32	1.18	3.31	1.13
Im Moment bin ich intolerant und unduldsam gegenüber denjenigen Seiten meiner Persönlichkeit.	3.56	1.19	3.34	1.12	3.35	1.16	3.32	1.19	3.40	1.17
Im Moment versuche ich meine Fehler als Teil der menschlichen Natur zu sehen.	3.25	1.09	3.13	1.05	3.10	1.15	2.92	1.01	3.10	1.08
Im Moment kann ich mir vorstellen, dass die meisten Leute Gefühle der Unzulänglichkeit haben.	3.37	1.00	3.32	0.88	3.52	1.00	3.36	0.88	3.39	0.94
Im Moment denke ich, dass die meisten anderen Menschen wahrscheinlich glücklicher sind als ich.	3.58	1.22	3.30	1.15	3.43	1.23	3.50	1.14	3.46	1.19
Im Moment bin ich der Meinung, dass nur ich allein versage.	4.37	1.00	4.04	1.16	4.13	1.03	4.19	1.11	4.19	1.08
Im Moment achte ich darauf, was nicht in Ordnung ist.	2.86	1.05	2.87	1.07	2.69	1.05	2.83	1.09	2.81	1.06
Im Moment werde ich von Gefühlen der Unzulänglichkeit aufgezehrt.	3.90	1.17	3.69	1.24	3.70	1.27	3.80	1.08	3.77	1.19

Note. No specific instructions were used. t1 = immediate posttest; n = number of participants.

Table 7.A12 Single Item Means and Standard Deviations for Positive Affect in the Intervention and Control Conditions at Pretest, Posttest, and Follow-up

Item text	Optimism condition (n = 110)		Gratitude condition (n = 105)		Self-compassion condition (n = 104)		Control condition (n = 106)		Total (n = 425)	
	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1	Mean t1	SD t1
zufrieden	3.73	1.07	3.52	1.01	3.37	1.10	3.55	0.95	3.54	1.04
ausgeruht	2.96	1.16	3.06	1.17	3.02	1.16	2.78	1.20	2.96	1.17
ruhelos	3.49	1.28	3.48	1.35	3.42	1.16	3.41	1.34	3.45	1.28
schlecht	3.99	1.11	3.90	1.00	3.67	1.18	3.88	1.09	3.86	1.10
schlapp	3.37	1.37	3.28	1.31	3.13	1.20	3.08	1.30	3.22	1.30
gelassen	3.64	1.01	3.28	1.10	3.39	1.05	3.39	1.05	3.43	1.06
müde	2.75	1.27	2.88	1.21	2.82	1.27	2.58	1.18	2.75	1.23
gut	3.76	0.97	3.65	0.92	3.42	1.00	3.56	0.91	3.60	0.95
unruhig	3.70	1.19	3.71	1.19	3.64	1.12	3.59	1.32	3.66	1.20
munter	3.15	1.09	3.08	1.08	2.89	1.00	2.81	0.96	2.98	1.04
unwohl	3.95	1.09	3.93	1.10	3.69	1.20	3.78	1.18	3.84	1.14
entspannt	3.26	1.13	3.36	1.08	3.12	1.16	3.10	1.00	3.21	1.09

Note. We asked participants how they feel at the moment ("Im Moment fühle ich mich ..."). t1 = immediate posttest; n = number of participants.

7.6 List of Tables

7.7 List of Figures

7.8 Author Contributions

Chapter 3

Heekerens, J. B., Eid, M., & Heinitz, K. (2019). Dealing with conflict: Reducing goal ambivalence using the best-possible-self intervention. *Journal of Positive Psychology*. Advance online publication. doi: 10.1080/17439760.2019.1610479

Study concept:	Johannes B. Heekerens
Identification of the scientific problem:	Johannes B. Heekerens
Development of the research design:	Johannes B. Heekerens
Data collection:	Johannes B. Heekerens
Data analysis:	Johannes B. Heekerens & Michael Eid
Discussed the data:	Johannes B. Heekerens & Michael Eid
Wrote the paper:	Johannes B. Heekerens
Contributed comments/materials/analysis tools:	Kathrin Heinitz & Michael Eid

Chapter 4

Heekerens, J. B., Eid, M., Heinitz, K., & Merkle, B. (submitted). Deconstructing positive-psychological interventions: Differential effects of optimistic, grateful, and self-compassionate writing on well-being. *Journal of Positive Psychology*.

Study concept:	Johannes B. Heekerens
Identification of the scientific problem:	Johannes B. Heekerens
Development of the research design:	Johannes B. Heekerens & Belinda Merkle
Data collection:	Johannes B. Heekerens
Data analysis:	Johannes B. Heekerens & Michael Eid & Belinda Merkle
Discussed the data:	Johannes B. Heekerens & Michael Eid
Wrote the paper:	Johannes B. Heekerens
Contributed comments/materials/analysis tools:	Kathrin Heinitz & Michael Eid

Chapter 5

Heekerens, J. B., & Eid, M. (2020). Inducing positive affect and positive future expectations
using the best-possible-self intervention: A systematic review and meta-analysis. *Journal of
Positive Psychology*. Advance online publication. doi: 10.1080/17439760.2020.1716052

Study concept:	Johannes B. Heekerens
Identification of the scientific problem:	Johannes B. Heekerens
Development of the research design:	Johannes B. Heekerens
Literature search and coding:	Johannes B. Heekerens & research assistants
Data analysis:	Johannes B. Heekerens
Discussed the data:	Johannes B. Heekerens & Michael Eid
Wrote the paper:	Johannes B. Heekerens
Contributed comments/materials/analysis tools:	Michael Eid

7.9 Curriculum Vitae

Der Lebenslauf ist in der Online-Version aus Gründen des Datenschutzes nicht enthalten.

7.9 Curriculum Vitae

7.10 Eigenständigkeitserklärung

Hiermit erkläre ich, dass ich die vorliegende Dissertation selbstständig verfasst habe. Ich habe keine anderen als die aufgeführten Hilfsmittel verwendet. Die Arbeit wurde zu keinem anderen Zeitpunkt zu einem Promotionsverfahren angenommen oder abgelehnt.

Berlin, 16.04.2020

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(Johannes B. Heekerens)