Best possible future self writing: Effects on well-being, self-regulation, and related processes

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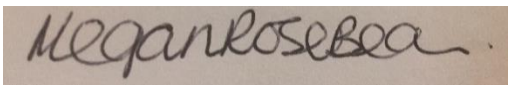

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Abstract

Best possible future self (BPFS) writing has consistently been shown to immediately increase positive affect and may elicit sustained improvements in well-being (e.g. Frein & Ponsler, 2014; King, 2001). It has been suggested that the well-being benefits of BPFS writing occur because the intervention increases self-regulation (King, 2001; 2002). This explanation is conceivable because of similarities between BPFS writing and future-oriented mental simulation, which has been found to benefit self-regulatory processes (e.g. Pham & Taylor, 1999). However, prior to the current research-programme, effects of mental simulation in comparison to writing about a BPFS had not been explored, and effects of BPFS writing on self-regulation had not been measured. The overarching aim of this thesis was to explore the suggestion that BPFS writing improves physical and psychological well-being through increasing self-regulation. In the first study BPFS writing bolstered self-regulation eight weeks following a single session but BPFS simulation did not, suggesting that they are not comparable processes. In the second study, the effect of BPFS writing on self-regulation was not replicated using four writing sessions. No sustained well-being benefits emerged in either study. It was suggested that the null findings in both studies may have arisen due to procedural characteristics, yet it is difficult to ascertain the effects that procedural variations may have on outcomes due to wide procedural heterogeneity throughout the literature. A systematic review was therefore conducted to explore the impact of BPFS writing on a range of physical and psychological outcome measures. Findings demonstrated that immediate increases in positive affect following BPFS writing are generalisable across procedural variations, but that longer-term benefits to well-being and cognitive processes, including self-regulation, appear limited. A contribution of this thesis has been the direct exploration of effects of BPFS writing on self-regulation, as well as a systematic review which provides the most comprehensive synthesis of evidence surrounding BPFS writing to date.

Table of Contents

List of Tables

List of Figures

List of abbreviations

This list describes the meaning of abbreviations used throughout the thesis:

Abbreviation	Meaning
AAS	Affect-Adjective Scale
AIDS	Acquired immunodeficiency virus
ABPM	Ambulatory blood pressure monitoring
ANS	Autonomic nervous system
ASQ	Attributional Style Questionnaire
BPFS	Best possible future self
BPFS-W	Best possible future self writing
BPS	British Psychological Society
DHP	Division of Health Psychology
CES-D	Centre for Epidemiologic Studies-Depression Scale
ConA	Concanavalin A
CONSORT	Consolidated Standards of Reporting Trials
CRD	Centre for Reviews and Dissemination
DASS-21	Depression, Anxiety and Stress Scale-21
DERS	Difficulties in Emotion-Regulation Scale
DES	Differential Emotions Scale
DV	Dependent variable
EBV	Epstein-Barr virus
ECPP	European Conference on Positive Psychology
EF(s)	Executive function(s)
EHPS	European Health Psychology Society
EPQ-N	Eysenck Personality Questionnaire- Neuroticism subscale
EW	Expressive writing
FES	Future Expectancies Scale
FOS	Future Orientation Scale
GSES	Generalised Self-Efficacy Scale
HIV	Human immunodeficiency virus
HPA axis	Hypothalamus-pituitary-adrenal axis
IV	Independent variable

LIWC	Linguistic Inquiry and Word Count
LOT	Life Orientation Test
LOT-R	Life Orientation Test- Revised
MCU	Medical care utilisation
NA	Negative affect
N-FEX	Negative future expectancies
NMI	Negative mood induction
OSPAN	Arithmetic operation-word memory span
PA	Positive affect
PANAS	Positive and Negative Affectivity Scale
PANAS-X	Positive and Negative Affectivity Scale- Expanded form
P-FEX	Positive future expectancies
PGIS	Personal Growth Initiative Scale
PHA	Phytohemagglutinin
PILL	Pennebaker Inventory of Limbic Languidness
PNS	Parasympathetic nervous system
PRISMA	Preferred Reporting Items for Systematic Reviews and Meta-Analyses
PSI	Physical Symptoms Inventory
ROB	Risk of bias
SD	Standard deviation
SESP	Society for Experimental Social Psychology
SHS	Subjective Happiness Scale
SNS	Sympathetic nervous system
SPANE	Scale of Positive and Negative Experience
SPT	Subjective Probability Test
SRQ	Self-Regulation Questionnaire
SSRQ	Short Self-Regulation Questionnaire
STADI	State-Trait-Anxiety-Depression Inventory
SWLS	Satisfaction with Life Scale
TOTE	Test-operate-test-exit

Chapter One

Introduction to the thesis

1.1 Background to the thesis

The physical and psychological well-being effects of writing about thoughts and feelings have been explored for over 30 years. In the first study of writing as a therapeutic intervention, Pennebaker and Beall (1986) found that when individuals wrote emotively about a personally traumatic experience their physical well-being increased four months later. Since then, numerous studies have been conducted to explore the effects of writing about trauma and they have shown that writing does, at least sometimes, benefit physical and psychological health (e.g. Hemenover, 2003; Smyth, Hockemeyer & Tulloch, 2008, but see also Sbarra, Boals, Mason, Larson & Mehl, 2013). However, the benefits of writing about trauma come at a cost. Consistently, it elicits immediate short-term residual distress (e.g. Kloss & Lisman, 2002; Schroder, Moran & Moser, 2018), which has been found to be unrelated to the subsequent benefits (Smyth, 1998). More recently, researchers have experimented with positively-valanced writing topics, which have been found to benefit well-being without the initial short-term distress (e.g. Burton & King, 2004; Wing, Schutte & Byrne, 2006). One of these topics is writing about a best possible future self (BPFS; King, 2001). This variation on Pennebaker and Beall's (1986) writing intervention consistently induces short-term positive affect (PA; e.g. Frein & Ponsler, 2014; Hanssen, Peters, Vlaeyen, Meevissen & Vancleef, 2013), and regularly yields beneficial changes in physical and psychological well-being (King, 2001; Peters, Meeviseen & Hanssen, 2013).

The effects of BPFS writing (BPFS-W) have been suggested to occur due to increases in self-regulation (e.g. King, 2001; 2002). This suggestion is conceivable due to similarities of BPFS-W to future-oriented mental simulation,

which benefits self-regulatory processes such as planning as well as goal performance (e.g. Pham & Taylor, 1999). However, the effects of BPFS-W in comparison to future-oriented simulation were yet to be explored, and self-regulation had not yet been directly measured as an outcome of the BPFS-W intervention. The first experimental study of the current thesis was designed to compare the effects of BPFS-W and future-oriented mental simulation on physical and psychological well-being, as well as on self-regulation.

1.2 Structure of the thesis

In Chapter Two the literature surrounding BPFS-W is reviewed. The chapter begins with an overview of the intervention's origins in expressive writing about trauma, followed by a discussion of the transition from trauma writing to positive writing including BPFS-W. The theories surrounding the mechanisms through which these interventions may impact well-being are also evaluated. In Section 2.2.3.1, King's (2001; 2002) self-regulation theory is presented. This leads into a review of the literature surrounding future-oriented mental simulation (Section 2.3); a self-regulatory activity suggested by King (2001) to be comparable to BPFS-W (e.g. Pham & Taylor, 1999). The aims and objectives of this thesis are presented in Chapter Three.

The findings of Study One are reported in Chapter Four. Literature suggests that mental simulation of the process goals towards an outcome is more effective than simulation of the outcome itself (e.g. Pham & Taylor, 1999; Taylor & Pham, 1999). The comparative effects of process and outcome BPFS simulation and BPFS-W on physical and psychological well-being, and on self-regulation and other potential mediators of effect, were explored in Study One. This was the first study to empirically compare mental simulation and writing, and the first to measure self-regulation as an outcome of BPFS-W. The results of Study One demonstrated no significant between-group difference in physical and psychological well-being, but BPFS-W did appear to improve self-regulation eight weeks post-writing, regardless of whether participants wrote about the process or the outcome. Future-oriented mental simulation did not improve self-regulation.

The lack of change in physical and psychological well-being following BPFS-W was surprising. It was suggested that this may be attributable to procedural differences between Study One and King's (2001) original BPFS-W study. For example, a single writing session was used in Study One, whereas King (2001) used four sessions. Given that Study One was the first BPFS-W study to include self-regulation as an outcome, it remained unknown whether the gains seen in self-regulation following a single writing session were generalisable to other procedures than the one used in Study One (e.g. King's (2001) protocol). Therefore, it also remained unknown whether the well-being and self-regulation benefits of BPFS-W required different procedural parameters to be promoted. The difference in effects of BPFS simulation and writing on self-regulation suggested that they are dissimilar in terms of the procedural parameters required to harvest optimum effects. For this reason, the focus of the thesis turns to BPFS-W alone from Chapter Five onwards.

In Chapter Five, Study Two of the thesis is presented. In this study, King's (2001) procedure was replicated as closely as possible, to explore the effects of four BPFS-W sessions on self-regulation and well-being. Findings were unexpected; there was no effect of BPFS-W on both well-being and self-regulation. The null findings may be attributable to remaining procedural differences between Study Two and King's (2001) study— as well as differences between Study Two and Study One— which may have lowered the efficacy of the intervention, such an online rather than laboratory setting.

Across Chapters Four and Five, it was suggested that it is difficult to compare and interpret inconsistencies in findings across BPFS studies due to wide variations in procedural factors such as the writing instructions used, the number, spacing and length of writing sessions, and the timing of follow-ups. Haase (2011) suggested that identification of patterns and inconsistencies across studies is more manageable when a systematic review is completed. Chapter Six presents a systematic review of the effects of BPFS-W on physical and psychological well-being, as well as on cognitive processes which may impact well-being, such as working-memory. Possible effects of procedural variations on intervention outcomes were also explored.

Finally, in Chapter Seven, the main findings from this thesis are summarised and limitations are outlined. The implications of the findings are discussed in the context of wider literature and suggestions for future research are made.

Chapter Two

Literature Review

2.1 Overview

The broad aim of this thesis was to investigate King's (2001; 2002) suggestion that writing about a best possible future self (BPFS) increases self-regulation and, in turn, benefits physical and psychological well-being. The current chapter has been written to review the literature surrounding BPFS writing (BPFS-W), beginning with its origins in expressive writing. The transition from traditional writing interventions centred around past trauma to writing about future life goals is described, and theories of mechanisms of effect are critically-evaluated. King's (2001) self-regulation theory of how BPFS-W brings about health benefits is then discussed, and the possibility that the intervention works in a similar way to mental simulation— an activity which has been found to benefit self-regulation (e.g. Pham & Taylor, 1999; Taylor & Armor, 1997, as cited by Taylor, Pham, Rivkin & Armor, 1998)— is introduced. Finally, literature surrounding the effects of mental simulation on self-regulation is discussed, along with the possible mechanisms through which these effects occur.

2.2 Writing interventions

2.2.1 The origins of writing interventions: Expressive writing

2.2.1.1 The first writing intervention study

Emotional expression— observable manifestations of emotions (Vogel, Wade & Hackler, 2008)— is associated with better physical and psychological health (Coates & Winston, 1987; Esterling, Antoni, Kumar & Schneiderman, 1990; Fawzy et al., 1993; Lieberman & Goldstein, 2006; Rachman, 1980). Non-expression, inhibition and repression of emotions are detrimental to health (Gross & Levenson, 1997; Larson & Chastain, 1990). According to Pennebaker

and Beall (1986), active inhibition of thoughts, feelings and behaviours over time is cumulatively stressful and is associated with low-level physiological work (see also Pennebaker, 1989, Pennebaker & Chung, 2007). It is these stress and arousal effects of inhibition which are thought to be damaging (Lepore, Greenberg, Bruno & Smyth, 2002). Individuals with lower emotional expression are more likely to suffer from psychiatric conditions such as anorexia nervosa (Espeset, Gulliksen, Nordbø, Skårderud & Holte, 2012), and have physical illness and lower life-satisfaction (Finkenauer & Rimé, 1998; Pennebaker & O'Heeron, 1984). Suppression of emotions is also associated with lowered immune-functioning (Petrie, Booth & Pennebaker, 1998), and physical diseases such as cancers (Kune, Kune, Watson & Bahnson, 1991; Shaffer, Graves, Swank & Pearson, 1987).

Given the association between inhibition and illness, it is unsurprising that many psychotherapies encourage open discussion of individuals' problems (Pennebaker, 1997; Smyth & Helm, 2003). However, emotional expression outside of a therapeutic environment is not always possible, because of perceptions that it is socially unacceptable, or absence of social support (Lepore, Silver, Wortman & Wayment, 1996; Wortman & Silver, 1989; Pennebaker & Beall, 1986; Smyth, Nazarian & Arigo, 2008). Written disclosure of feelings undercuts these barriers; it allows expression without a need for social interaction (McGihon, 1996; Smyth et al., 2008). It was upon this premise that writing interventions began to be investigated.

Early writing interventions focussed upon disclosure of trauma. The first study of the effects of writing about trauma was conducted by Pennebaker and Beall (1986). This study was designed to test their inhibition theory, that is, to explore whether the damaging effects of inhibition on health could be ameliorated by written disclosure. 46 students were allocated to four writing conditions: trauma-facts (factual details of an emotional experience), trauma-emotions (emotions surrounding the experience), trauma-combination (both facts and emotions) and control (benign topics, for example their shoes). Participants attended a laboratory to write for 15 minutes a day across four consecutive days. They engaged with the intervention well; they wrote about highly personal

experiences including sexual abuse, death of close loved-ones and drug abuse. The majority (54.6- 75.0%) of those in the trauma groups wrote about an experience that they had not shared before, suggesting that writing was an acceptable means of expression to them. Expressive writing (EW) about emotions surrounding personal trauma evoked an increase in negative mood immediately post-writing, as well as elevated systolic blood pressure. However, it resulted in reduced physical illness four months post-writing. When asked to describe how EW had affected them, participants reported that it had been beneficial; it had given them peace of mind, made thinking about their traumatic experience less painful, and generally made them feel better. Pennebaker and Beall's (1986) findings suggested that individuals may not need a therapist or social support network to reduce inhibition and enjoy the health benefits of emotional expression.

Since publication of Pennebaker and Beall's (1986) study, a plethora of investigations into the healing power of written disclosure has been conducted. Findings are discussed in the following sections of this chapter.

2.2.1.2 Short-term costs of long-term benefits

Pennebaker and Beall's (1986) findings suggested that writing about emotions surrounding a traumatic experience could be enough to reduce the health costs of inhibition. However, the benefits of EW are not immediately apparent. The increase in negative affect (NA) immediately following writing about trauma reported by Pennebaker and Beall (1986) has been replicated. Kloss and Lisman (2002) and Schroder, Moran and Moser (2018) found greater anxiety and anxious arousal immediately post-writing in EW participants relative to controls. Páez, Velasco & González (1999), Sharp and Hargrove (2004) and Smyth, True and Souto (2001) found significantly greater NA, and Burton and King (2008), Páez et al. (1999) and Smyth et al. (2001) found lower positive affect (PA). Páez et al. (1999) and Pennebaker, Kiecolt-Glaser and Glaser (1988) found greater physical symptoms immediately post-writing. The level of residual distress immediately post-writing is not predictive of subsequent health benefits, as demonstrated by the lack of a relationship between immediate distress and long-term outcomes found in a meta-analysis of effects of 13 EW

studies (Smyth, 1998). The immediate physiological reaction to the intervention, however, appears to be implicated in the long-term effects (Sloan & Marx, 2004a).

2.2.1.3 Physiological reactivity

Exposure to stressors exerts powerful effects on physiological systems (Kemeny, 2003). These effects are thought to have evolved to allow individuals to cope with threat; physiological systems required for an effective threat response are activated, and those which are not required are suppressed (Kemeny, 2003). Physiological arousal is controlled by the autonomic nervous system (ANS). The ANS is comprised of two distinct neurological networks; the parasympathetic nervous system (PNS) and the sympathetic nervous system (SNS). Activation of the ANS prepares the body for the fight or flight mechanism (Sadi, Finkelman & Rosenberg, 2013; Ziegler, 2012). The PNS exerts inhibitory effects and governs relaxation-related functions such as reductions of heart-rate (Levenson, 2014). The SNS exerts excitatory effects and governs arousal-related functions including elevations in pulse- and heart-rates and respiration (Kemeny, 2003; Levenson, 2014, Ziegler, 2012). It is these excitatory processes which are needed for fight or flight (Kemeny, 2003). The SNS overrides the PNS in the presence of physical or psychological stressors, resulting in cardiovascular reactivity (Applehans & Luecken, 2006; Loft et al., 2007; Ziegler, 2012).

EW about trauma has immediate physiological effects (e.g. Epstein, Sloan & Marx, 2005; Pennebaker & Beall, 1986; Sloan & Marx, 2004a). This is unsurprising, because it requires confrontation of threatening stimuli in the form of negative thoughts, feelings and memories. As previously discussed, Pennebaker and Beall (1986) found a significant increase in systolic blood pressure immediately following their first writing session, and a decrease in systolic blood pressure following further sessions. Similarly, Sloan and Marx (2004a) found a significantly greater increase in salivary-cortisol, a biomarker for stress, in EW participants than controls following the first writing session, but not at later sessions. They also found that increases in cortisol at the first session were associated with psychological well-being improvements at one-

month follow-up. This finding indicates a possible biological pathway through which EW may lead to health benefits.

2.2.1.4 Long-term psychological effects of EW

Short-term, EW generally results in transient distress and associated physiological arousal (e.g. Pennebaker & Beall, 1986). Long-term, it may have beneficial effects on psychological well-being, although results are inconsistent.

Several studies have demonstrated positive psychological effects of EW. In healthy students, it has been found to result in reduced depression (Sloan, Marx, Epstein & Dobbs, 2008), fewer intrusive thoughts (Klein & Boals, 2001), reduced anxiety and interpersonal sensitivity (Hemenover, 2003), improved mood (Pennebaker et al., 1988; Smyth, Hockemeyer & Tulloch, 2008), gains in post-traumatic growth (Smyth et al., 2008) and increased self-acceptance, personal growth and mastery (Hemenover, 2003). These benefits are generalisable to other populations. Soliday, Garofalo and Rogers (2004) found that adolescents who completed EW reported decreased NA and depressive symptoms, two and six weeks post-writing. Lepore and Greenberg (2002) found, in individuals who had recently experienced a romantic relationship break-down, a greater increase in tension and fatigue in control participants than EW participants two weeks post-writing, suggesting that EW buffered against negative effects of the break-up. EW has also been found to be beneficial for physically unwell and clinical groups. Craft, Davis and Paulson (2013) found that EW improved the quality of life of breast cancer survivors, and Bernard, Jackson and Jones (2006) found that it reduced severity of post-traumatic stress following first-episode psychosis, five weeks post-writing. Positive findings from distressed or unwell groups are important, as they suggest that writing interventions may have clinical applications.

Psychological well-being benefits of EW have been found across multiple populations, including clinical groups, healthy students, and individuals who have experienced a recent stressful situation. Nevertheless, EW does not always result in well-being gains. Kloss and Lisman (2002) and Niles, Byrne Haltom, Mulvenna, Lieberman & Stanton (2014) found no significant between-

group differences in anxiety and depression in healthy students and community members, nine and three months post-writing, respectively. Null findings appear to be as generalisable across populations as findings suggesting psychological benefits. Deters and Range (2003) and Giannotta, Settanni, Kliewer and Ciairano (2009) found no significant difference in post-traumatic stress symptoms between EW and control participants, in students who had experienced trauma and in adolescents, respectively. Rivkin, Gustafson, Weingartern and Chin (2006) found no effect on depression in human immunodeficiency virus (HIV) positive individuals, and in Lepore, Revenson, Roberts, Pranikoff and Davey's (2015) sample of colorectal cancer patients no change in depression or cancer-related quality of life was found. Importantly, Greenberg, Wortman and Stone (1996) found that EW was damaging to psychological well-being one month post-writing; female students with history of a severe trauma who wrote for 30 minutes reported significantly greater avoidant thoughts about the event and greater fatigue, relative to controls and those who wrote about an imaginary trauma. Sbarra, Boals, Mason, Larson and Mehl (2013) also found EW to be detrimental; in individuals searching for meaning in their marital separation, EW resulted in reduced emotional well-being nine months later, relative to a control task. It is unclear why inconsistencies in findings occur, although they may be attributable to differences in participant characteristics or intervention procedures across studies (e.g. Frattaroli, 2006; Reinhold, Bürkner & Holling, 2018; Smyth, 1998). It is clear, however, that sometimes the intervention is effective, but other times it is not. The evidence surrounding the effects of EW on physical health— particularly evidence from studies of immune-function— is more consistent.

2.2.1.5 Long-term physical effects of EW

Pennebaker and Beall's (1986) exploratory investigation suggested that EW is beneficial for physical health. Since then, multiple studies have been conducted to explore its sustained effects on physical health. Participants have demonstrated reduced health care visits for illness (e.g. Cameron & Nicholls, 1998; Pennebaker, Colder & Sharp, 1990; Pennebaker & Francis, 1996), sustained reductions in self-reported physical symptoms (e.g. Epstein et al., 2005; Park & Blumberg, 2002) and fewer self-reported sick days (e.g. Sheese,

Brown & Graziano, 2004; Smyth et al., 2001) relative to controls. It appears that the potential for physical health benefits of EW is generalisable across several populations. Significant effects have been found in healthy students (e.g. Pennebaker & Beall, 1986), as well as in clinical populations such as individuals with cancers (e.g. Henry, Schlegel, Talley, Molix & Battencourt, 2010), chronic pain (Norman, Lumley, Dooley & Diamond, 2004), asthma and arthritis (Smyth, Stone, Hurewitz & Kaell, 1999). Nevertheless, some studies have not yielded significant improvements in physical health outcomes across both healthy and clinical populations (e.g. Jensen-Johansen et al., 2018; Lu & Stanton, 2010; Niles et al., 2014). Moreover, Sheffield, Duncan, Thomson & Johal (2002) found that EW was detrimental to physical health at a three-week follow-up. As with contradictory results surrounding effects on psychological health, possible causes of inconsistency in findings regarding the effects of EW on physical health and moderators of effects are not yet fully-understood and continue to be explored (e.g. Baikie, 2008; Norman et al., 2004; O'Connor & Ashley, 2008; O'Connor, Walker, Hendrickx, Talbot & Schaefer, 2013; Rude & Haner, 2018; Sloan & Marx, 2018; Smyth & Pennebaker, 2008).

The measures most commonly used to assess physical health in EW studies are indirect. Records of health care visits for illness are not a clear indicator, as studies do not distinguish between visits for genuine illness and unnecessary visits (Pennebaker, et al., 1988). Equally, participants may have had physical symptoms for which they did not feel medical attention was required. Retrospective self-reports of physical symptoms and behaviours (i.e. self-reported sick-days) are also subjective. They depend upon the participant's memory and perception of their symptoms and behaviours (i.e., their 'self-theory'; Pennebaker, 2004) and are susceptible to demand characteristics (Pennebaker et al., 1988). The fragility of these measures is illustrated by the findings from Sheese et al.'s (2004) study; participants who wrote about trauma reported fewer post-writing sick-days relative to controls. However, groups did not differ in levels of self-reported physical symptoms (Sheese et al., 2004). This inconsistency in findings across measures intended to function as indicators of physical health demonstrates that these measures likely do not provide accurate reflections of symptoms and should be treated with caution.

More objective, robust evidence for the effects of EW about traumatic or stressful experiences on physical health can be drawn from a growing number of studies which have been undertaken to explore effects on immunological function (Pennebaker et al., 1988).

2.2.1.6 Effects of EW on immune-function

Stressful life experiences have been found to exert adverse effects on the immune system (Bartrop, Lazarus, Luckhurst, Kiloh & Penny, 1977; Kiecolt-Glaser et al., 1984; Marketon & Glaser, 2008). They reduce circulating levels of lymphocytes (immunological cells) and inhibit their functions (e.g. proliferation in response to a foreign substance), and reduce the rate of integrated immune responses, such as wound healing (Ader, 2001; Kemeny, 2003). The interaction between stressful events and immunity is mediated by the hypothalamus-pituitary-adrenal (HPA) axis and the sympathetic adreno-medullary nervous system (Lutgendorf & Costanzo, 2003). These networks are considered the peripheral limbs of the stress system, and are critical for effective immunoregulation (Chrousos, 1995; Elenkov, 2007; Elenkov, Wilder, Chrousos & Vizi, 2000; Ziemssen & Kern, 2007). Under chronic stress, both networks are vulnerable to dysregulation (Chrousos, 1995). Psychological and behavioural interventions can reduce the effects of chronic stress on immune-function (e.g. Kiecolt-Glaser, McGuire, Robles & Glaser, 2002; Robinson, Norton, Jarrett & Broadbent, 2017; Woods, Lowder, & Keylock, 2002). Modulation of the HPA axis and the sympathetic adreno-medullary nervous system through these interventions is thought to relieve allostatic load, caused by chronic stress, from the body and re-establish normal endocrine- and immune-function (Lutgendorf & Costanzo, 2003; Seeman & McEwan, 1996).

The first investigation of the immunological effects of EW was published by Pennebaker et al. (1988). The authors used two mitogens (phytohemagglutinin (PHA) and concanavalin A (ConA)) and assessed the proliferation of T-lymphocytes[1] at baseline, following the final of four 30-minute writing sessions, and at a six-week follow-up. Results demonstrated that EW participants had a

[1] Proliferation-rate of lymphocytes in response to mitogens (foreign substances) is an in-vitro representation of immune responses (Pennebaker et al., 1988).

significantly higher response to PHA immediately post-intervention and at follow-up than controls. Data from the response to ConA were available only at baseline and at the final writing session, due to a difficulty in ConA preparation. The ConA response pattern was comparable to that for PHA but failed to reach statistical significance. Pennebaker et al. (1988) also found that EW participants who had written about an experience that they had not disclosed before (thus had likely inhibited) demonstrated greater improvements in mitogen responses relative to those who wrote about an experience that they had discussed with others[2]. These findings suggest that written emotional disclosure improves immune-function and provide support for Pennebaker and Beall's (1986) and Pennebaker's (1988) theory that it does so by lowering inhibition.

The results of Pennebaker et al.'s (1988) study were the first to indicate that EW may enhance immune-function. Since then, multiple studies have been conducted which demonstrate its effects on immunity. Esterling, Antoni, Fletcher, Margulies and Schneiderman (1994) asked healthy Epstein-Barr virus (EBV) seropositive students to engage in writing or talking about a stressful personal experience, or a control topic. Results showed that there were lower EBV antibody titres in blood samples from the emotional disclosure group in comparison to controls at a one-week follow-up, indicating greater cellular immune control over latent EBV. Several studies have demonstrated faster wound healing following punch biopsy when participants had completed EW, in healthy students and university staff, healthy older adults and community members (Koschwanez et al., 2013; Robinson, Jarrett, Vedhara & Broadbent, 2017; Weinman, Ebrecht, Scott, Walburn & Dyson, 2008). Furthermore, Petrie, Booth, Pennebaker, Davison & Thomas (1995) found that EW resulted in better immune-function response to a hepatitis B vaccination, relative to controls. EW has also been demonstrated to benefit healing following surgery in participants high in alexithymia (but not in those low in alexithymia); Solano, Donati, Pecci, Persichetti & Colaci (2003) found that alexithymic participants who completed EW had shorter stays in hospital following papilloma resection, relative to non-

[2] Those who wrote about an undisclosed experience showed greater decreases in blood pressure from baseline to six-week follow-up relative to those who wrote about a disclosed experience (Pennebaker et al., 1988). Writing may have reduced the damaging, chronic physiological arousal thought to be caused by inhibition (Pennebaker, 1988; Pennebaker & Beall, 1986).

writing controls. This is an indirect marker of improved immune-function, given that hospital stay tends to end once the surgical wound has healed satisfactorily, indicated by an absence of blood in lavage fluid as well as an absence of symptoms and signs of infection (Solano et al., 2003). Importantly, it also appears that EW is beneficial for immune-function in clinical populations. Petrie, Fontanilla, Thomas, Booth and Pennebaker (2004) found that EW about a personal trauma resulted in an increase in CD4+ lymphocytes in individuals with HIV. This is important, because reductions in CD4+ lymphocytes are indicative of progression towards acquired immunodeficiency virus (AIDS).

Although findings are generally positive with regards to the effects of EW on immune-function, there is some inconsistency. Koschwanez et al. (2017) asked individuals undergoing laparoscopic bariatric surgery (gastric band) to write for 20 minutes a day over three consecutive days (two weeks pre-surgery) about either a traumatic experience or daily activities. During the surgical procedures, expanded polytetrafluoroethylene tubes were placed into laparoscopic port sites. 14 days later, the tubes were removed and examined for deposition of hydroxyproline (a substance in collagen and a robust biomarker of healing; Jorgensen, Sorensen, Kallehave, Schulze & Gottrup, 2001). Results demonstrated that EW patients had significantly less hydroxyproline deposition in their tubes than controls. They also had significantly lower levels of tumor necrosis factor alpha (a proinflammatory cytokine involved in regulation of wound repair; Werner & Grose, 2003) in wound fluid collected from a drain during the 24-hour period following surgery. These findings suggest that EW was not as beneficial as the control task for immune-function. Koschwanez et al. (2017) suggest that the control task may have allowed patients to plan how they were going to spend their time leading up to surgery, and that this may have made them feel more prepared.

Although inconsistency exists, it appears that generally EW enhances immune-function, at least for some people (e.g. Solano et al., 2003) or in some situations. Immune-function gains are a possible mechanism through which health benefits of EW occur.

2.2.2 Proposed mechanisms of the effects of EW on health

Overall, evidence surrounding the effects of EW is mixed. However, it does sometimes exert beneficial effects on health. Pennebaker and Beall's (1986) proposal that written emotive disclosure can lead to health benefits was therefore accurate. It was the inhibition theory by Pennebaker and Beall (1986) and Pennebaker (1988) which drove the beginnings of empirical investigation of the effects of EW. Much of the evidence logically suggests that lowering of inhibition could be a possible mechanism of effect; participants write about traumatic events, and in doing so they confront negative aspects of their life story which are usually actively avoided (Pennebaker, 1988). The consequence of this is an immediate increase in physiological arousal and NA, followed, at times, by long-term health benefits.

Some studies have yielded effects of EW which cannot be explained according to lowering of inhibition. If a release of inhibited, suppressed emotions is the mechanism through which writing about trauma benefits health, then it would be expected that individuals with high dispositional constraint would benefit more from the intervention than those low in dispositional constraint. However, Francis and Pennebaker (1992) found that participants with low dispositional constraint had greater reductions in sick days following writing about traumatic events than participants high in dispositional constraint. Similarly, Greenberg and Stone (1992) found that participants who wrote about a traumatic personal experience that they had not disclosed previously did not benefit from the intervention more than participants who wrote about a previously-disclosed trauma. If EW is as beneficial for participants who have not inhibited their feelings as it is for those who have, then it is likely that reduction of inhibition is not the only mechanism through which EW benefits health. It has been argued that the effects of these studies do not necessarily provide robust evidence against the inhibition theory. Sloan and Marx (2004b) suggest that an individual may state that they have disclosed to others about an experience, but that this does not mean that they have expressed deep emotions about it. They may have superficially mentioned it whilst inhibiting strong emotions. It is not possible to accurately measure emotional inhibition, because it is generally difficult for human beings to assess the extent to which they have actively held

back from discussing emotional experiences with others (Pennebaker & Chung, 2007). More convincing evidence against inhibition theory is provided by Greenberg et al.'s (1996) investigation. In this study, participants were allocated to three conditions: writing about a traumatic personal experience, writing about an imagined traumatic experience, and a control group. Results demonstrated that participants who had written about a traumatic experience had fewer health-centre visits relative to controls, irrespective of whether the traumatic experience was real or imaginary. Inhibition theory cannot explain benefits of EW about an imaginary trauma; if the participants did not experience it, then they can not have inhibited feelings surrounding it. Multiple alternative theories have been proposed as explanations for the effects of EW. Detailed discussions of these theories are beyond the scope of this chapter, however key propositions will be outlined (see Frattaroli (2006), Pennebaker and Chung (2007) and Sloan and Marx (2004b) for comprehensive accounts).

Some researchers suggest that writing about a negative event allows for the emotions associated with the event to be habituated, through a process of repeated exposure (Frattaroli, 2006; Pennebaker & Chung, 2007; Sloan & Marx, 2004b). If EW does elicit health benefits through exposure and habituation, then it would be expected to ameliorate symptoms typically associated with post-traumatic stress such as intrusive thoughts and avoidance (Sloan & Marx, 2004b). Some studies have demonstrated reductions in intrusive thoughts and avoidance (e.g. Klein & Boals, 2001; Schoutrop, Lange, Hanewald, Davidovich & Salomon, 2002), yet others have found either no effect (de Moor et al., 2002; Walker, Nail & Croyle, 1999; Stroebe, Stroebe, Schut, Zech & van den Bout, 2002) or (for avoidance only) adverse effects (Gidron, Peri, Connolly & Shalev, 1996; Greenberg et al., 1996; Smyth et al., 2001). These inconsistent findings suggest that habituation through repeated confrontation of emotions is unlikely to be a complete explanation as to how EW elicits health benefits. Furthermore, Krantz and Pennebaker (1995; as cited by Pennebaker & Chung, 2007) asked participants to express emotions through either body-movements only, or through body-movements followed by EW, for 10 minutes a day over three consecutive days. Although expressive movement alone was sufficient to increase subjective psychological well-being, physical health benefits were

observed only when movement was combined with EW. This suggests that repeated confrontation and expression of emotion is unlikely to be sufficient. Pennebaker and Chung (2007) suggest that the translation of emotions into language is important, and that beyond habituation and lowering of inhibition, change must occur at the cognitive level.

There is a small body of evidence to suggest that changes in cognitive processes occur following EW. Several studies have provided indirect evidence for improved cognitive function, such as increased academic grades amongst students who wrote about a stressful event (e.g. Cameron & Nicholls, 1998; Frattaroli, Thomas & Lyubomirsky, 2011; Lumley & Provenzano, 2003; Pennebaker et al., 1990). There is also direct evidence of cognitive gains. Klein and Boals (2001) found that students who wrote expressively about their thoughts and feelings about starting University had increased working-memory capacity at a seven-week follow-up (as measured by performance on an arithmetic operation-word memory span task; OSPAN; Turner & Engle, 1989). This was accompanied by a decrease in intrusive and avoidant thoughts about starting university. Klein and Boals (2001) suggest that the decrease in intrusive and avoidant thoughts (and possible increases in coherence of encoding and storage of the stressful memory) made a larger proportion of working-memory resources available for coping with stressors which would otherwise cause health problems. More recently, Kellogg, Mertz and Morgan (2010) found that EW was associated with greater improvements in OSPAN performance, relative to writing about a control topic. However, in this study, no effect of writing group was found on intrusive and avoidant thoughts. Kellogg et al. (2010) suggest that working-memory gains may occur following EW due to a reduced emotional cost of thoughts about a traumatic experience, rather than a reduction in frequency of intrusive or avoidant thoughts.

The above findings suggest that cognitive change may be a mechanism through which EW elicits health benefits. Pennebaker and Seagal (1999) posit that the process of putting an experience into words may help individuals to reorganise and restructure their emotional memories, to alter their thoughts about the experience and make sense of it. Several studies have supported this premise.

Pennebaker et al. (1990) asked participants who had found EW beneficial to explain why they thought that was. The majority stated that it allowed them greater insight into their experiences. Pennebaker (1993) explored these initial indices of cognitive change further, by collating the results of three previous written disclosure studies, and running the text generated by participants through a software programme intended for analysis of text (Linguistic Inquiry and Word Count; LIWC; Francis & Pennebaker, 1992; Tausczik & Pennebaker, 2010). The text analysis supported the qualitative findings from Pennebaker et al. (1990), insofar as participants who had benefited the most from EW had shown an increase in words suggestive of acknowledgement of causation (e.g. *because*, *cause* and *effect*) and words suggestive of insight (e.g. *know* and *consider*) across writing sessions. It appears that Pennebaker's (1993) findings are robust; the results of multiple studies have demonstrated an association between an increase in cognitive word-use across writing sessions and health improvements, across both self-reported and objective, immunological data (e.g. Pennebaker & Francis, 1996; Pennebaker, Mayne & Francis, 1997; Petrie et al., 1998). Furthermore, Ullrich and Lutgendorf (2002) asked participants to write about either their emotions surrounding a personal trauma, their emotions and cognitions surrounding a personal trauma (e.g. about how they had tried to make sense of it) or about a traumatic event that they saw in the media (control task), and found an increase in positive growth from trauma in the emotions and cognitions group only. It therefore appears that the mere confrontation and expression of emotions is not enough. Rather, change must occur at the cognitive level for health benefits of EW to be yielded; the individual must gain insight into their experience, make coherent sense of it, and integrate it into their self-schema (Pennebaker, 1993). In doing so, the traumatic memories become encoded and stored in a way that is more structured, organised and cohesive (Pennebaker et al., 1997; Smyth et al., 2001). This results in a reduction in consumption of cognitive resources by intrusive and avoidant thoughts about the experience (Pennebaker et al., 1997; Smyth et al., 2001), either through reduction in the frequency of such thoughts (Klein & Boals, 2001), or through dampening their negative emotional cost (Kellogg et al., 2010). This in turn leaves greater cognitive resources for coping with stressors, which results in a lower vulnerability to illness (Klein & Boals, 2001).

2.2.3 Positive writing and the self-regulation theory

The cognitive processing theory described above is a feasible explanation for most of the effects of writing on health described so far in this chapter. However, there are variations on the EW paradigm that cannot be explained through reduction of frequency or emotional cost of intrusive thoughts about trauma.

The traditional EW instructions encourage participants to write about traumatic, upsetting or stressful experiences (Pennebaker & Beall, 1986). In most studies, participants have been required to select a personal event to write about (e.g. Donnelly & Murray, 1991; Frayne & Wade, 2006; Greenberg & Stone, 1992; Kelley, Lumley & Leisen, 1997; Lumley & Provenzano, 2003; Park, Ayduk & Kross, 2016; Park & Blumberg, 2002; Petrie et al., 2004). In others, they have been asked to write about a specific event, such as a pet's serious illness (Hunt, Schloss, Moonat, Poulos & Wieland, 2007), sexual assault (Brown & Heimberg, 2001), a classmate's death (Margola, Facchin, Molgora & Revenson, 2010), natural disaster (Smyth, Anderson, Hockemeyer & Stone, 2002), loss of employment (Spera, Buhrfeind & Pennebaker, 1994), stressful experiences related to body-image (O'Connor et al., 2011) and a loved one's suicide (Kovac & Range, 2000). However, there are some studies which have deviated from this approach of writing about negative personal experiences and the deep thoughts and emotions connected to them.

The first study to deviate from writing about a personal traumatic experience was conducted by Greenberg et al. (1996). As discussed earlier in this chapter, Greenberg et al. (1996) found that writing about an imaginary trauma was associated with health benefits. A more common variation on the traditional EW task is writing about the perceived benefits of negative events. King and Miner (2000) and Stanton et al. (2002) found that writing about the perceived benefits of a traumatic event such as having cancer was as beneficial for health (in terms of reduced medical care use) as the more traditional written disclosure of deep emotions. Benefit-finding also improved the psychological well-being of individuals high in avoidance, although EW was more beneficial for individuals low in avoidance (Stanton et al., 2002). There is also evidence that traumatic

events need not feature in writing tasks at all for the benefits of writing to occur; writing about an intensely positive experience is effective. In this variation of the EW intervention, participants write about the happiest, most wonderful experience of their lives (see Burton & King, 2004). It has been found to result in lower physical illness (Burton & King, 2004; 2008), higher life-satisfaction (Wing, Schutte & Byrne, 2006) and lower dietary restraint (an element of disordered eating; Kupeli et al., 2018). Furthermore, writing about a positive personal experience immediately boosts PA (Kupeli et al., 2018; Burton & King, 2004). It appears, then, that the benefits of writing can be obtained without the short-term emotional cost associated with EW about trauma.

Any theory which depends upon confrontation or expression of negative emotions does not explain why positive writing elicits health benefits. Instead, the findings from these positively-oriented writing tasks have been explained using a self-regulation theory (King, 2001; 2002). This stance rests upon a simple definition of self-regulation as an individual's ability to attempt to behave in a way that will enable them to achieve their goals, to attend to feedback on the efficacy of their pursuits, and to respond to feedback by adjusting their behaviour if necessary (King, 2002). According to this conceptualisation, individuals adopt higher-order goals which they pursue by way of working and monitoring progress towards lower-order goals (King, Richards & Stemmerich, 1998). Therefore, self-regulation in this way is expected to be bolstered by activities which encourage clarity and accuracy of goal-identification, improve feedback-monitoring, and facilitate production of multiple strategies through which goals can be pursued (King, 2002). According to Carver and Scheier (1982), affective states function as feedback sources in self-regulation. Individuals experience positive emotions when their actual progress towards personally-salient goals mirrors or exceeds their expected progress, and negative emotions when it does not (King, 2002).

This affective feedback system may offer explanation as to why writing about a myriad of topics has health benefits. In learning about themselves and what is important to them, and in gaining understanding of their emotional reactions, individuals become better able to work towards their goals (King, 2002).

Negatively-valanced writing activities may alleviate the disruption to self-regulation caused by traumatic experiences (King, 2002), and help individuals to adjust to these experiences through gaining understanding and developing coping strategies (Cameron & Nicholls, 1998). Traumatic experiences can mean that higher-order goals must change, which has consequences for lower-order goals (King, 2002). Perhaps writing about traumatic experiences provides clarity around goals, for example by helping the individual to consider which lower-order goals are obsolete and should be abandoned (King, 2002). Abandonment of unattainable goals is related to improved subjective well-being and physical health (Heckhausen, Wrosch & Fleeson, 2001; Tunali & Power, 1993; Wrosch, Miller, Scheier & De Pontet, 2007). Negative emotions elicited by traumatic experiences can mask the affective feedback which is so critical in the maintenance of self-regulatory action; perhaps writing about traumatic experiences reduces the negative emotions which remain from them, and in doing so restores purity and strength of the affective feedback-loop (King, 2002). The more an individual's emotions depend upon their behaviour towards their goals, and the more informative their emotions are, the better they can self-regulate (Carver & Scheier, 1982; King, 2002). However, a self-regulation view of writing is not restricted to the confrontation or re-evaluation of traumatic experiences or negative emotions (King, 2002). Instead, King (2001) suggests that writing about any aspect of life experience may afford individuals the opportunity to learn about themselves. Both positive and negative topics may encourage integration of experiences into a wider context, whilst restricting them within the confines of written words, and thus simplifying them and making them more comprehensible (King, 2001). Both positive and negative topics may also help individuals to gain a sense of control over and understanding of their emotional life and their values (King, 2001). These processes improve self-awareness and self-regulation (King, 2001). In this view, writing about any salient experience could be expected to foster self-regulation, and through this bring about health benefits (King, 2001). Well-being benefits of effective self-regulation are well-documented; it is positively associated with happiness (Cheung, Gillebaart, Kroese & De Ridder, 2014), life-satisfaction (Hofmann, Luhmann, Fisher, Vohs & Baumeister, 2014) and psychological adjustment (Tangney, Baumeister & Boone, 2004), and negatively associated with general

distress (Bowlin & Baer, 2012). Psychological well-being, in turn, is associated with physical health (Diener & Chan, 2011; Nicholson, Kuper & Hemingway, 2006; Okun, Stock, Haring & Witter, 1984; Penninx, 2017).

It is possible that increases in self-regulation explain why writing about both positive and negative topics is associated with health improvements. The first writing task created with the intention of specifically targeting self-regulation was developed by Cameron and Nicholls (1998). In this study, students wrote about either their thoughts and feelings surrounding coming to university (e.g. about leaving their family behind, and about who they are or want to become), their thoughts and feelings as well as coping plans (to encourage self-regulation), or a control topic. They wrote for 20 minutes per week over three weeks. Results demonstrated significantly fewer health-centre visits following the disclosure and self-regulation conditions relative to controls. University adjustment decreased in the control and disclosure groups but was maintained in the self-regulation group; the self-regulatory writing task likely buffered against a decrease in adjustment. Furthermore, NA was found to increase over time in the control group, but not in the self-regulation or disclosure groups. Mood was most stable in the self-regulation group, again suggesting that the activity acted as a buffer. The results of this study provide indirect evidence for self-regulation as a mechanism of effect; by including instructions which directly encouraged self-regulation, the effects of writing about coming to university (a major, stressful life event; King, 2001) on health were increased.

2.2.3.1 Writing about best possible future selves

The self-regulatory view of writing activities was a cornerstone of the writing intervention explored in the current thesis. King (2001) investigated the effects of writing about a best possible future self (BPFS) on physical health and psychological well-being. This activity involves writing about a time in the future ‘‘when everything has gone as well as it possibly could’’ (King, 2001, p. 801). Participants write about having worked hard and reached all of their goals and are told to ‘‘think of this as the realisation of your life dreams’’ (King, 2001, p. 801). The BPFS-W instructions were designed to foster improvements in self-regulation, without participants experiencing negative emotions or confronting

traumatic or stressful experiences. King (2001) required participants (students) to write about either their BPFS, a past trauma, both their BPFS and a past trauma (combination group), or about their plans for the day (control group). All groups wrote for 20 minutes a day for four consecutive days (the combination group wrote about a past trauma for the first two days, and a BPFS for the second two days). A main effect of BPFS-W on immediate PA emerged, in that writing activities which included a BPFS resulted in higher PA immediately post-writing than writing activities which did not. Unsurprisingly, PA was significantly lower in the trauma-only group than in each of the other three groups. Furthermore, participants rated writing about trauma but not a BPFS as significantly more upsetting than the control task. Both were rated to be significantly more important than the control task. Participants perceived the BPFS topic as important, but it did not upset them or reduce their PA.

Long-term, there was significantly higher psychological well-being in participants who wrote about a BPFS in comparison to those who did not write about a BPFS, three weeks post-writing[3]. There was no significant benefit of writing about trauma. For physical health, both the trauma-only and BPFS-only groups made fewer health-centre visits in the five months post-writing relative to controls, when the number of visits made in the three months before writing were controlled for. Overall, King's (2001) findings demonstrate that BPFS-W is perceived by participants as being important, but unlike writing about trauma it does not result in temporary residual distress. Longer-term, it appears to be as effective as writing about trauma in terms of physical health outcomes, and more effective in terms of psychological well-being. King's (2001) findings have been replicated. BPFS-W consistently induces PA immediately (e.g. Frein & Ponsler, 2014; Hanssen, Peters, Vlaeyen, Meevissen & Vancleef, 2013), and often (although not always; e.g. Austenfeld, 2007; Austenfeld & Stanton, 2008) results in long-term, beneficial changes to well-being (e.g. Peters, Meevissen &

[3] Psychological well-being was measured using the Satisfaction with Life Scale (SWLS; Diener, Emmons, Larsen & Griffin, 1985) and the Life Orientation Test (LOT; Scheier & Carver, 1985). Scores on these measures were found to be highly correlated, so King (2001) analysed them as a composite (by averaging the standard scores). Scores from the individual scales are briefly mentioned in King's (2001) paper. Those who wrote about a BPFS had significantly higher LOT (optimism) scores in comparison to those who did not write about a BPFS, but there was no significant main effect of BPFS-W on SWLS (life-satisfaction) scores.

Hanssen, 2013; Shapira & Mongrain, 2010). These findings suggest that BPFS-W does indeed appear to be (at least at times) beneficial, and they demonstrate that NA and confrontation of traumatic memories are not necessary, active components of the mechanisms of writing interventions.

King (2001; 2002) explains the beneficial effects of BPFS-W in terms of self-regulation. King's (2001; 2002) approach is grounded in Control Theory (Carver & Scheier, 1982; 2012; Powers, 1973; as cited by Carver & Scheier, 2012). A comprehensive account of Control Theory is beyond the scope of the current chapter. However, the key principles are central to King's (2001; 2002) self-regulation theory of the effects of BPFS-W.

According to Control Theory, behaviour is regulated by way of a series of 'reference values' which exist in a motivational hierarchy of goal-directed actions (Carver & Scheier, 2012; Powers, 1973). The levels of this hierarchy range from lower-order, narrow, specific actions to higher-order, broad, abstract goals and values (Carver & Scheier, 2012). Goals positioned higher on the motivational hierarchy are pursued through regulation of action towards lower-order goals (Carver & Scheier, 2012; King et al., 1998). Reference values serve as standards to which current states are compared (Carver & Scheier, 2012). Discrepancies between these standards and current states are continually monitored to assess progress (King et al., 1998). Goal-directed action occurs if a discrepancy is found according to information generated by a negative feedback-loop (Carver & Scheier, 2012). The information from the loop includes affective responses to progress, for example feeling sad if actual progress is below expected progress (Carver & Scheier, 1982; King, 2002). The negative feedback-loop is best explained in terms of the 'test-operate-test-exit' (TOTE) unit, illustrated in Figure 2.1 (Miller, Galanter & Pribram, 1960, as cited by Carver & Scheier, 2012), using the example of an electrical room thermostat (Carver & Scheier, 2012). The thermostat detects the air temperature and compares it to the desired standard temperature (test stage). If a discrepancy exists, the thermostat acts to reduce the discrepancy; a furnace is heated or air conditioner is turned on (operate stage). The test and operate stages continue until the current state is congruent with the desired standard, at which point the

sequence ends (exit stage). Negative feedback-loops work in this way throughout the motivational hierarchy (Carver & Scheier, 2012; King, 2002). Standards in lower-order levels are specified by goals in higher-order feedback-loops (Carver & Scheier, 2012). To explain using the thermostat example above, an individual may have the higher-order goal of conserving energy, and as such may decide to set the thermostat to a lower desired temperature standard (Carver & Scheier, 2012).

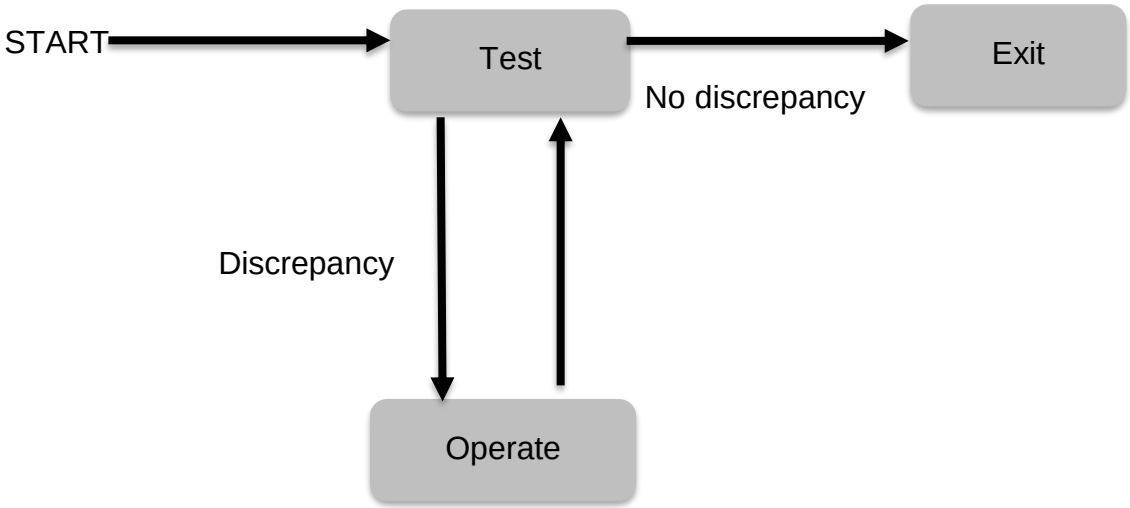


Figure 2.1: TOTE unit (Carver & Scheier, 2012; Miller et al., 1960; Reid, Vignali & Barker, 2015)

King (2002) suggests that activities which facilitate individuals in the identification of their goals, monitoring of feedback (e.g. affective responses; Emmons & Kaiser, 1996), and generation of goal-pursuit strategies should increase this type of self-regulation. Writing about a BPFS is likely to do this. Possible selves are part of the self-concept; what individuals wish to become, as well as what they fear becoming (Markus & Nurius, 1986; Markus & Wurf, 1987). They are derived from past representations of the self (as well as from cultural contexts and social comparisons) and are connected to the current self yet are distinct from these aspects of the self-concept (Markus & Nurius, 1986). Possible selves are vivid cognitive manifestations of enduring, self-relevant aspirations, fears and goals (Markus & Nurius, 1986). They are, as such, the connection between the self-concept and motivation and exist as standards against which the current self can be evaluated, as well as incentives for self-

regulated action (Markus & Nurius, 1986; Niedenthal, Setterlund & Wherry, 1992; Oyserman & Markus, 1990). According to King (2001), possible selves likely exist on a high level of the motivational hierarchy (see discussion of Control Theory above), and as such may be less often attended to in day-to-day life than standards positioned on lower levels. BPFS-W may therefore enable an individual to explore an area of their motivational life that has not previously been considered to a great degree (King, 2001). In doing so, BPFS-W may facilitate self-regulation by promoting clarity of individuals' life goals and priorities and by reducing goal-conflict (King, 2001; 2002). Self-regulatory processes, as well as physical and psychological wellbeing, are threatened by goal-conflict (Emmons & King, 1988; Gray, Ozer & Rosenthal, 2017; Stroebe, Mensink, Aarts, Schut & Kruglanski, 2008). Goal-conflict is particularly damaging to well-being when it occurs at the higher levels of the motivational hierarchy, where an individual's future self who has achieved all their life goals would be positioned (Russ, 2018). As such, it is quite possible that BPFS-W increases self-regulation and, in turn, improves health through this mechanism.

King (2002) stated that there are two conclusions which can be drawn with regards to the effects of writing interventions. ''First, expressive writing has health benefits. Second, no one really knows why'' (King, 2002, p. 119). This statement remains true today. The self-regulation theory of the effects of BPFS-W has been cited and endorsed by several authors since the publication of King's (2001) study (e.g. Frein & Ponsler, 2014; Layous, Nelson & Lyubomirsky, 2013; Sheldon & Lyubomirsky, 2006). However self-regulation has not, to the knowledge of the author, yet been directly measured as an outcome of the BPFS-W intervention. It is therefore unknown whether BPFS-W does increase self-regulation, and whether an increase in self-regulation mediates effects on physical and psychological health. BPFS-W is a promising intervention and is attractive because it is accessible and free from costs of trained health professionals and administrators (Pennebaker, 2004). In recent years it has begun to become accepted by mental health professionals and has been recommended both for use in clinical practice and in self-help resources (e.g. Greater Good in Action, n.d.; Niemiec, 2013; O'Hanlon & Bertolino, 2011). It is

therefore timely to broaden understanding of its effects and the mechanisms through which these effects occur.

King's (2001; 2002) self-regulation theory of the effects of BPFS-W is feasible. King (2001; 2002) draws a comparison between BPFS-W and a known self-regulatory activity; mental simulation of future events. Mental simulation of goals is beneficial for goal-directed action (an element or product of self-regulation; Carver & Scheier, 2012), and according to King (2001) it is quite possible that writing and simulation share therapeutic properties and mechanics and that BPFS-W may involve mental simulation.

2.3 Mental simulation

2.3.1 Definition

Mental simulation is defined as the imitative, cognitive representation of genuine or possible scenarios (Taylor & Pham, 1999). This may constitute a mental repetition of a past event, such as an argument, with the aim of working out how the conversation became heated (Pham & Taylor, 1999; Taylor et al., 1998). It may also be the construction of a future event, such as deciding how to approach a difficult conversation (Pham & Taylor, 1999; Taylor et al., 1998). To be clear, mental simulation is different to simply thinking about an event; it is conscious enactment, whereby the individual engages in self-projection into the imagined event sequence (Klein & Crandall, 1995; Waytz, Hershfield & Tamir, 2015). It has been suggested that future-oriented mental simulation is a self-regulatory process, which facilitates management of emotions and aids planning (Taylor et al., 1998; Taylor & Schneider, 1989). Projecting the self into positive end-states has been found to increase task-persistence and effort (Ruvolo & Markus, 1992), and improve behaviour in adolescents (Oyserman, Terry & Bybee, 2002).

Taylor and Pham (1996) present future-orientated mental simulation as a vehicle for translating cognition into action. They posit that when thoughts are transposed into concrete mental representations of reality, action consistent with that representation is likely to occur. There are intrinsic characteristics of

mental simulation which render it useful for envisioning future events and goals, planning how these goals will be materialised, and translating thoughts into behaviour (Hayes-Roth & Hayes-Roth, 1979; Taylor et al., 1998; Taylor & Pham, 1996; Taylor & Schneider, 1989). Mental simulation elevates an individual's perception that an event is true or likely, generates plans, and evokes emotional (and accompanying physiological) reactions, and in doing so encourages self-regulation and goal-directed action (Taylor & Pham, 1996; Taylor & Schneider, 1989). These features are discussed in the following paragraphs, and their proposed roles in facilitating the transition of imagined behaviours into authentic action are considered.

2.3.2. Features of mental simulation

2.3.2.1 Mental simulation increases perceived likelihood of events

The first feature of mental simulation which makes it useful for self-regulation is that it increases the perceived likelihood of events (Taylor & Pham, 1996). When an individual mentally-enacts an event sequence in a concrete, specific form, the event appears true (Taylor et al., 1998). For example, Garry, Manning, Loftus and Sherman's (1996) participants reported the likelihood that they had experienced events during childhood, such as becoming stuck in a tree. Two weeks later, they mentally-simulated some of the events, and again rated the likelihood that they had experienced them. Events that were initially rated as being unlikely to have occurred were rated as more likely to have occurred at the second time-point, if those events had been simulated. This effect did not occur for non-simulated events. Here, mental simulation altered participants' memories, suggesting that the impact of mental simulation on perceived likelihood is sufficiently powerful to exert a deleterious effect upon genuine representations of the past. Garry et al.'s (1996) findings are not unique; multiple studies have reported effects of mental simulation on memories (e.g. Goff & Roediger, 1998; Heaps & Nash, 1999; Mazzoni & Memon, 2003). It has been suggested that these studies do not necessarily evidence a quality of mental simulation, and that instead simulation probes inaccessible memories of events that did indeed happen, making them accessible (Mazzoni & Memon, 2003; Read & Lindsay, 2000). However, this appears unlikely to be a sufficient

explanation, given that perceived likelihood becomes inflated following mental simulation of future events.

A large amount of research effort has been invested into exploration of the impact of mental simulation on the perceived likelihood of future events (e.g. Anderson, 1983). For this reason, it has been used as a persuasive tool in advertising and health promotion (e.g. Green & Brock, 2000; Gregory, Cialdini & Carpenter, 1982; Jeong & Jang, 2016). For example, Jeong and Jang (2016) found that when participants imagined going to a fast-food restaurant, seeing a new healthy menu option and choosing that option, they reported greater intentions to purchase the healthy option. According to Taylor and Pham (1996) and Pham and Taylor (1999), this feature of mental simulation forms a bridge between thought and action. They suggest that mental simulation increases the perceived probability that an event will occur in future, and therefore— given that simulation by its very nature involves mental rehearsals of actions— primes individuals for action.

2.3.2.2 Mental simulation facilitates planning

The second characteristic of mental simulation which aids self-regulation is that it encourages the generation of plans through its organisation of sequences of actions (Taylor et al., 1998). Miller et al. (1960, as cited by Taylor et al., 1998) suggest that mental simulation helps individuals to assess whether their plans are viable and to screen them for errors and pitfalls. Hayes-Roth and Hayes-Roth's (1979) work describes how simulating how events will unfold provides information about the events including alternative options. For example, if an individual simulates their walk to a shop, the imagery may unveil additional opportunities such as other shops that they need to visit, or what action should be taken if a desired shop is closed (Taylor et al., 1998; Taylor & Schneider, 1989). Simulation of a chain of actions or behaviours aids individuals in the development of a plan for performing it efficiently (Hayes-Roth & Hayes-Roth, 1979; Taylor & Schneider, 1989).

The effects of mental simulation on planning can be attributed to their likeness to reality, insofar as they conform to the constraints of reality (Taylor et al.,

1998). They are as specific as reality; unlike abstract imagery they entwine information about specific social settings, roles and individuals (Taylor & Pham, 1996). Mentally-simulated events are imaginary, but they are not miraculous or implausible (Kahneman & Miller, 1986; Taylor et al., 1998). Downhill elements (those which remove unpredictable aspects and add predictable aspects of a scenario) are more likely to be present in mental simulation than uphill elements (those which add unlikely aspects; Kahneman & Tversky, 1979; Wells, Taylor & Turtle, 1987). These parameters of plausibility mean that mental simulation aids anticipation and planning of future events, insofar as imagined action-plans are not likely dependent upon the occurrence of unlikely scenarios (Taylor et al., 1998; Taylor & Schneider, 1989).

2.3.2.3 Mental simulation elicits emotions

Mental simulation is not a dry cognitive process (Ji, Heyes, MacLeod & Holmes, 2016; Taylor and Schneider, 1989). Its likeness to reality extends beyond parameters of plausibility to the emotional experience of events; during mental simulation, individuals assimilate their present affective state with that of the simulated experience (Larsen & Ketelaar, 1991; Morrow & Nolen-Hoeksema, 1990; Strack, Schwarz & Gschneidinger, 1985). This is a specific property of mental simulation, rather than a product of any thought-process surrounding an emotional event, object or scenario. Taylor and Schneider (1989) describe how affective states are influenced only by concrete construction of events in a time-ordered sequence rather than by abstract recall or thought (Strack et al., 1985). These emotional responses are accompanied by the physiological reactions which would occur if the event were truly experienced (e.g. Qualls, 1983). Simulation of emotions impacts heart-rate, blood pressure, and electrodermal activity (Acosta & Vila, 1990; Qualls, 1983; Roberts & Weerts, 1982; Taylor & Pham, 1996). This is thought to be implicated in the transitioning of thought to action (Renner, Murphy, Ji, Manly & Holmes, 2019; Taylor & Pham, 1996). Taylor and Schneider (1989) illustrate this by explaining that an individual would not begin to write a book if they thought it would be unsuccessful; an experiential vision of success (with anticipatory positive emotions and arousal) is thought to be critical in eliciting sufficient motivation to perform the lengthy sequence of actions involved.

2.3.3 Translation of thoughts and intentions into action

The characteristics of mental simulation discussed offer an explanation as to how it may bridge the gap between thought and action (Taylor & Pham, 1996). It increases perceived probability that an event will occur, aids planning, and elicits emotional and physiological reactions which enhance motivation and provide arousal to drive behaviour (Renner et al., 2019; Taylor & Pham, 1996). There is typically a gap between intentions and behaviours, in that intentions do not always translate into behaviours (Schwarzer, 2008; Sheeran, 2002). Intentions are more likely to translate into behaviours when a specific action-plan is generated (Gollwitzer & Sheeran, 2006). Mental simulation is therefore thought to spur goal-directed action because it enables identification of situational cues for action; it facilitates planning of a sequence of behaviours within a specific time and location context (Schwarzer, 2008). For example, mental simulation aids weight-loss (Marszał-Wiśniewska & Jarczewska-Gerc, 2016) and reduction of alcohol consumption (Conroy, Sparks & de Visser, 2015; Hagger, Lonsdale & Chatzisarantis, 2011; 2012). It is also used in cognitive-behavioural therapy (Taylor et al., 1998). Brownell, Marlatt, Lichtenstein and Wilson (1986) and Marlatt and Gordon (1985, as cited by Taylor et al., 1998) have demonstrated that mental rehearsal of events which render addicts vulnerable to relapse can help them to maintain abstinence during those events.

Theoretically, it appears quite possible that writing about the achievement of future goals might work in the same way as mental simulation. Writing about the realisation of life goals may, as King (2001) suggested, encourage individuals to attend to the higher-levels of their motivational hierarchy. This may make higher-order goals appear clearer and more achievable and may inform individuals of the plan that they must follow to reach these goals. Like mental simulation, BPFS-W elicits potent affective responses; perhaps these anticipatory positive feelings do spur goal-directed action at lower levels of the motivational hierarchy. The evidence surrounding the effects of mental simulation on self-regulation, however, paints a somewhat more complicated picture.

2.3.4 Types of mental simulation

There are several types of mental simulation, and they differ in their effectiveness in facilitating self-regulation. An obvious example of this is ruminative thought and worry, which can be maladaptive (e.g. Silver, Boon & Stones, 1983). Rumination is characterised by NA, which depletes self-regulation (Sirois, 2015; Thomsen, 2006). Imagination must be appropriately harnessed and mastered for simulation to be beneficial (Taylor et al., 1998). When considering simulations as interventions, they are distinguished most commonly into two broad categories; process simulations and outcome simulations (Pham & Taylor, 1999).

Outcome simulations are focussed on end-states and goal-achievement (Pham & Taylor, 1999). In the context of the motivational hierarchy discussed earlier (Section 2.2.3.1) outcome simulations involve higher-order goals. For example, a medical student may simulate becoming a successful surgeon, and visualise themselves in that role (Pham & Taylor, 1999). The positive emotions elicited by this self-projection may spur the student to study hard to achieve their goal (Taylor et al., 1998). This outcome simulation may also elevate the student's perceived likelihood of a future in which they are a surgeon; this may make the goal appear more attainable and may raise their self-efficacy (Pham & Taylor, 1999; Taylor & Pham, 1996). This could, in turn, boost the student's motivation to achieve the goal (Taylor et al., 1998). Outcome simulations have been heavily endorsed in self-help literature as a method through which goal-achievement can be increased (e.g. Dyer, 1989; Fanning, 1994). However, Taylor et al. (1998) suggest that the power of outcome simulations, in the absence of a specific plan of actions, to translate motivation into goal-directed action (such as studying hard for exams) is questionable.

Process simulations are not end-state focussed, and instead involve the sequence of actions which must be achieved to reach the higher-order end-state (Taylor & Pham, 1996). Process simulations involve generation of imagery of actions positioned lower in the motivational hierarchy. If the medical student discussed above were to generate a process simulation, they would imagine themselves studying hard, and submitting assignments (Pham & Taylor, 1999).

Process simulations are thought to facilitate goal-directed action by encouraging individuals to construct feasible, realistic and effective action-plans of the steps which must be taken to achieve a desired end-state (Pham & Taylor, 1999). It is this type of mental simulation which is typically found to be most effective in terms of goal-directed action (Taylor et al., 1998).

Before the literature surrounding the effects of process in comparison to outcome simulation is reviewed, it appears useful to outline Oettingen's (1996, 2012) work on optimistic future thinking and mental contrasting. Optimistic thinking has known benefits, such as positive effects on motivation, cognition and affect (Oettingen, 1996). However, optimistic fantasising about a positive end-state— without acknowledgement of the hard work and effortful action needed to reach it as well as the obstacles to be overcome— is thought to elicit an anticipatory experience of success which can be damaging to motivation and goal performance (Gollwitzer, Heckhausen & Ratajczak, 1990; Oettingen, 1996; Oettingen & Wadden, 1991). Sevincer, Busatta and Oettingen (2014) suggest that optimistic future thinking is effective when an individual compares a positive end-state with their current reality and considers the obstacles which exist between the current reality and the desired future outcome. Unlike when an individual merely fantasises about success, consideration of current reality results in acknowledgement that the positive end-state has not yet been realised, and that there are obstacles which must be overcome to reach it (Sevincer et al., 2014). This "mental contrasting" has been found to be a useful self-regulatory process which drives effortful action towards attaining goals; expectations of success are activated, and these expectations energise effortful goal-directed action (Kappes, Singmann & Oettingen, 2012; Oettingen, 2012; Oettingen et al., 2009; Oettingen, Pak & Schnetter, 2001; Sevincer & Oettingen, 2013). Considering this, it is unsurprising that outcome simulations are found to be less effective for goal performance than process simulations. Outcome simulations allow individuals to indulge freely in fantasies of goal success, whereas process simulations encourage consideration and planning of the actions which must be taken for goal success to be achieved.

Pham and Taylor (1997, as cited by Taylor et al., 1998 and Taylor & Pham, 1996) explored the efficacy of process in comparison to outcome simulations. In this landmark study, students preparing for midterm examinations were trained in one of three types of mental simulations; process, outcome, and combination. Process participants simulated studying hard to perform well. Outcome participants simulated receiving their desired grade and feeling pleased about it. The combination group simulated both the lower-order processes, and the higher-order outcome. Participants performed simulations every day for five to seven days leading up to their examination day. There was also a control group who monitored how many hours they invested in studying each day. Immediately following the first simulation session, outcome participants reported higher motivation to study than process and control participants. However, by the night before the examination, the effects of the process simulation became evident. In comparison to those in the other three groups, process participants reported lower anxiety and worry about the examination. Outcome participants still reported higher motivation than those in the process condition. This superior motivation did not translate into superior goal-directed action; outcome participants' increase in performance failed to reach statistical significance. Process (and combination) participants studied for an average of three hours longer than outcome and control participants and commenced their studying one and a half days earlier. They also received a marked net increase in their examination grade. The results of this study demonstrate that although outcome simulation was perceived to be motivating, it was process simulation which benefitted emotion-regulation and self-regulation of goal-directed action. This finding has been replicated. In a similar study, Taylor and Pham (1999) found that self-efficacy was higher following outcome relative to process simulation, and that outcome simulation also boosted motivation and confidence. However, these variables were not related to performance following outcome simulation. Although Taylor and Pham (1999) found that outcome simulation elicited performance gains relative to a control task, process simulation was superior.

More direct evidence of the superiority of process simulation was yielded by Taylor and Armor (1997; as cited by Taylor et al., 1998). In this study, the effects of process and outcome simulation on the planning fallacy were

examined. The planning fallacy is a self-regulatory dysfunction; the failure of an individual to accurately predict the resources required to complete a project (e.g. Buehler, Griffin & Ross, 1994; Kahneman & Tversky, 1979). Taylor and Armor (1997) recruited students with an assignment deadline in the next week and asked them to predict when they would begin work on their assignment and when they would complete it. They were assigned a process simulation (studying, collecting resources), an outcome simulation (feeling happy with their completed work) or a control task (self-monitoring). Participants from both the process and the outcome simulation conditions were more likely to begin and complete their work on time than controls. However, the process group were markedly more likely to complete on time than the outcome group. According to Taylor et al. (1998), the process simulation condition likely boosted problem-solving and planning skills required for timely completion of the assignment. Although these variables were not directly measured, this inference could be accurate; process simulation participants found the assignment to be significantly easier than did those in the outcome and control groups. Regardless of how process simulation reduced the planning fallacy, the fact that it did is evidence that process simulation is beneficial for self-regulation.

Pham and Taylor (1997), Taylor and Armor (1997; cited by Taylor et al., 1998) and Taylor & Pham (1999) demonstrated that process simulation is more effective than outcome simulation for regulation of goal-directed action. However, at this point the explanations for this effect were theoretical. Pham and Taylor (1999) replicated their original examination study, and this time attempted to illuminate the mechanisms through which process simulations benefit performance. Again, process participants spent longer studying, and achieved better grades; outcome participants' performance was worse than that of controls. Process simulation also reduced the discrepancy between participants' predictions of the number of hours that they would study, and the actual number of hours spent studying, whereas outcome and control participants' predictions were overestimated. Performance increases following process simulation appeared to have been mediated by the grade strived for the

day before the exam, reduced anxiety and increased planning[4]. From this, the authors drew two inferences. First, process simulation benefits emotion-regulation (reduced anxiety), which in turn gives rise to improved performance. Second, process simulation increases problem solving (planning), which stabilises aspiration (the grade strived for), which also gives rise to improved performance. Pham and Taylor (1999) therefore suggest that process simulation translates thoughts into action by boosting emotion-regulation and problem solving. These suggestions are conceivable, although it should be noted that neither emotion-regulation nor problem solving were measured directly and as such these inferences should be treated with caution. Nevertheless, Armitage and Reidy (2008; 2012) documented that process simulation (but not outcome simulation) lowers anxiety, thus it is quite possible that it is a reliable emotion-regulation strategy. Armitage and Reidy's (2008) work also suggests a further mechanism. Following simulation of the process involved in blood donation, participants reported increased self-efficacy[5] as well as higher intention to donate. This was not observed following outcome simulation. Furthermore, self-efficacy mediated the effects of process simulation on intention, thus it is possible that process simulation improves regulation of goal-directed action through increasing self-efficacy, too. However, in a later study (Armitage & Reidy, 2012), process simulation exerted no greater effect on self-efficacy than outcome simulation. The effects of process and outcome simulation on self-efficacy are therefore unclear. Further research is needed to identify the mechanisms through which process and outcome simulation benefit self-regulation. However, it is clear from findings that mental simulation benefits planning and performance that it is a useful self-regulatory activity. It is also clear that process simulation is a more effective self-regulatory activity than outcome simulation.

[4] Planning was measured using three questions about the extent to which participants had decided how to study including where/ when they might study, and whether they felt prepared and organised. Planning is an element of self-regulation (Hong & O'Neil, 2001), thus a mediating role of increased planning on performance gains following process simulation is strong evidence of its self-regulatory benefits.

[5] The authors measured perceived control (perception of the extent to which a behaviour is achievable) which is synonymous with self-efficacy (Ajzen, 1998; Armitage & Reidy, 2008).

2.4 Overall summary

BPFS-W can be beneficial for health and well-being (e.g. Shapira & Mongrain, 2010), and it has been proposed that this may be through increases in self-regulation (King, 2001; 2002). The effects of BPFS-W on self-regulation have not been measured, but it is possible that the intervention does impact self-regulation. This is because mental simulation of future goals has been found to benefit regulation of action and possibly emotions (e.g. Armitage & Reidy, 2008; Pham & Taylor, 1999). It is possible that writing about and simulation of future goals are essentially the same activity, or at least could be expected to share mechanisms of effect, as implied by King (2001). Therefore, although the mechanisms through which mental simulation benefits self-regulation of action are not yet clear, proposed mediators (such as increased emotion-regulation and self-efficacy) point to possible pathways through which BPFS-W may bolster self-regulation, and in turn, health. Nevertheless, King (2001) noted that BPFS-W is more comparable to outcome simulation than process simulation. Outcome simulation has been found to have some benefits to goal-directed action (Taylor & Armor, 1997, as cited by Taylor et al., 1998; Taylor & Pham, 1999), but findings are mixed (Pham & Taylor, 1999). Consistently, process simulation is found to be more beneficial for self-regulatory processes than outcome simulation (Pham & Taylor, 1997, as cited by Taylor & Pham, 1996; 1999; Taylor & Armor, 1997; Taylor & Pham, 1999).

The effects of writing about, in comparison to mental simulation of, future goals have not been investigated. Therefore, it is entirely possible at this point that simulation and writing are distinct in terms of their outcomes and mechanisms of effect. This said, it is possible that BPFS-W does not impact self-regulation at all, given that self-regulation has not yet been directly measured as an outcome of BPFS-W studies. It is also possible that mental simulation is less potent than writing in terms of well-being benefits, especially given that Krantz and Pennebaker (1995; as cited by Pennebaker & Chung, 2007) found that the translation of experience into language was critical for the physical health benefits of emotional expression to emerge, as discussed in Section 2.2.2.

Chapter Three

Aims and Objectives

3.1 Aims

The principle aim of this thesis was to investigate King's (2001; 2002) suggestion that writing about a best possible future self (BPFS) benefits physical and psychological well-being through increasing self-regulation. Although findings are mixed, multiple studies have suggested that BPFS writing (BPFS-W) elicits increases in well-being (Peters, Meevissen & Hanssen, 2013; Shapira & Mongrain, 2010; but see also Austenfeld, 2007; Austenfeld & Stanton, 2008). Several studies have cited King's (2001; 2002) self-regulation theory as a likely explanation for how the intervention may elicit well-being benefits (e.g. Frein & Ponsler, 2014; Sheldon & Lyubomirsky, 2006). However, prior to the commencement of the current programme of research, self-regulation had not been measured as an outcome of BPFS-W. In Chapters Four and Five, the findings of two experimental studies which included self-regulation as an outcome of BPFS-W are presented.

King (2001; 2002) also suggested that BPFS-W may be comparable to, or involve, mental simulation (see Chapter Two, Section 2.2.3.1). Mental simulation is defined as the imitative representation of genuine or hypothetical scenarios (Taylor & Pham, 1999) and has been found to increase goal-directed action and self-regulatory processes (e.g. Pham & Taylor, 1999; Taylor & Pham, 1999). Theoretically, it is conceivable that future-oriented simulation and BPFS-W are comparable activities; both require self-projection and generation of clear possible selves, which are known to energise action (Oyserman & Markus, 1990). This similarity lends credence to King's (2001; 2002) suggestion that BPFS-W bolsters self-regulation and through this benefits well-being. However, typically BPFS-W is akin to outcome simulations rather than process

simulations, and it is process simulations which have been found to be most beneficial for goal-directed action (e.g. Pham & Taylor, 1999). The comparative effects of mental simulation and writing about a BPFS had not been explored prior to the commencement of this research programme. The study presented in Chapter Four was conducted in-part to explore the differences between process-focussed and outcome-focussed BPFS writing and simulation tasks.

Interpretation of the findings of the studies presented in Chapters Four and Five was complicated by the procedural variations seen across BPFS-W studies, as well as a lack of research into the effects of procedural variations on intervention outcomes. It was decided that a systematic review (presented in Chapter Six) would facilitate identification of patterns and inconsistencies which could be missed when comparing individual studies and would therefore provide indication of how beneficial BPFS-W truly is across outcomes (Haase, 2011). It would also indicate whether findings are generalisable across procedural variations, or whether some variations do impact intervention efficacy (Boissel, Blanchard, Panak, Peyrieux & Sacks, 1989; Mulrow, 1994; O'Hagan, Matalon & Riesenberg, 2018).

3.2 Objectives

The main objectives of the current thesis were as follows:

i. To replicate the findings of previous studies which have found that future-oriented mental simulation is beneficial for self-regulatory processes

ii. To determine whether BPFS-W and simulation have comparable effects on well-being and self-regulation

iii. To determine whether BPFS-W benefits self-regulation

iv. To replicate the findings of previous studies which have found that BPFS-W is beneficial for physical and psychological well-being, and if replicated explore whether these benefits are mediated by gains in self-regulation

v. To determine whether writing about the process towards a BPFS or writing about the outcome of achieving it is most beneficial for well-being and self-regulation

vi. To evaluate all the available evidence surrounding BPFS-W interventions to determine:
    a. Whether the evidence suggests that BPFS-W truly does appear to benefit physical and psychological well-being as well as cognitive processes which may impact well-being
    b. Whether procedural variations across studies may impact the efficacy of BPFS-W interventions.

Chapter Four

Study One. Writing about and mentally-simulating best possible future selves or processes towards them: Effects on well-being and self-regulation.

4.1 Introduction

Writing about a best possible future self (BPFS) has consistently been shown to induce positive affect (PA) and at times result in long-term benefits to physical and psychological well-being (e.g. Hanssen, Peters, Vlaeyen, Meevissen & Vancleef, 2013; King, 2001; Peters, Meevissen & Hanssen, 2013; Shapira & Mongrain, 2010). As discussed in Chapter Two, King (2001) suggested that BPFS writing (BPFS-W) benefits health through increasing self-regulation, a process which facilitates goal-attainment through attention to feedback and adjustment of behaviour if necessary (King, 2002). This suggestion has been cited as a possible mechanism of effect multiple times (e.g. Frein & Ponsler, 2014; King, 2002; Sheldon & Lyubomirsky, 2006). However, the effects of BPFS-W on long-term self-regulation have not been measured.

It is conceivable that the health benefits of BPFS-W occur through increases in self-regulation; King (2001) draws a comparison between BPFS-W and future-oriented mental simulation, which has been found to directly benefit goal performance as well as goal-directed action and planning (e.g. Pham & Taylor, 1999), which is an important element of self-regulation (Hong & O'Niel, 2001). There are clear similarities between BPFS-W and future-oriented mental simulation; both involve self-projection into the future and generation of clear possible selves, which energise action (Oyserman & Markus, 1990). Therefore, it is possible that BPFS-W benefits self-regulation, too. However, the effects of

mental simulation in comparison to writing about a BPFS have not yet been investigated. Empirical comparison of simulation and writing would be beneficial in broadening understanding of both interventions in terms of their possible outcomes and mechanisms of effects.

The apparent similarity between future-oriented mental simulation and BPFS-W, and the known benefits of mental simulation for self-regulatory processes, lend credence to the suggestion that BPFS-W may elicit health benefits through increasing self-regulation. There are further mechanisms which have been suggested to be possible mediators of the effects of mental simulation on goal-directed action. Pham and Taylor (1999) found that reduced anxiety mediated academic performance gains following process simulation of studying effectively, and from this inferred that emotion-regulation (a type of self-regulation involving engagement in processes or behaviours that will change unpleasant or unhelpful emotional states; Gross, 2014), may be a mechanism through which process simulation exerts its effects. Armitage and Reidy (2008) found that increased self-efficacy mediated effects of process simulation on behavioural intention but found no significant between-group difference in self-efficacy following process and outcome simulation in a later study (Armitage & Reidy, 2012). Further research is needed before conclusions can be drawn with regards to the mechanisms through which process simulation energises action. First, although process simulation has consistently been found to have emotional benefits (Armitage & Reidy, 2008; 2012; Pham & Taylor, 1997, as cited by Taylor & Pham, 1996), this does not necessarily mean that the capacity for emotion-regulation increases. Second, the effects of the activity on self-efficacy are unclear; Armitage and Reidy (2012) used an outcome simulation as a control group and did not measure self-efficacy at baseline, thus it is possible that both process and outcome simulations benefitted self-efficacy, and it is equally possible that neither did. It would be useful for the effects of mental simulation on emotion-regulation and self-efficacy to be investigated further using direct measures and an appropriately-controlled experimental design to allow further insight into these potential mechanisms of effect. Of course, if these variables mediate the effects of mental simulation on goal-directed action, then they may be involved in the mechanics of BPFS-W, too.

Clarification of the mechanisms through which BPFS-W and mental simulation elicit benefits would be useful in terms of the application of both interventions. It would perhaps facilitate identification of individuals for whom the interventions are likely to be most effective. For example, if BPFS-W yields well-being effects through improving self-regulation, then it would be expected to bring about greater change in individuals with lower self-regulatory abilities. Furthermore, it may allow guidance of manipulations of task instructions and procedures to nurture the 'active processes' necessary for improvements in outcomes to be yielded.

As discussed in Chapter Two, BPFS-W is not always found to be beneficial long-term (e.g. Austenfeld, 2007; Austenfeld & Stanton, 2008) and it is unclear under what conditions it is most effective. It is important to attempt to identify the procedural parameters within which the effects of mental simulation and BPFS-W are optimised, because they require no training of administrators and are as such highly accessible and cost-effective interventions (Taylor & Pham, 1996; Pennebaker, 2004). Most BPFS-W studies have adopted instructions identical or comparable to those used by King (2001), which guide participants to write about their lives in the future when all of their goals have been achieved. However, it is possible that changing the orientation of the writing topic from this outcome-focus to a process-focus may strengthen the therapeutic power of the intervention. Research surrounding the effects of mental simulation has suggested that process-focussed simulation is more effective than outcome simulation in bringing about positive changes in self-regulation (e.g. Pham & Taylor, 1999; Taylor & Armor, 1997, as cited by Taylor et al, 1998). Therefore, if mental simulation and writing about a BPFS are essentially the same activity, and if the health benefits of BPFS-W do occur through gains in self-regulation, then writing about the process towards a BPFS would be expected to be more powerful than writing about the outcome.

This possibility has been acknowledged in two studies (McGovern, 2004; Vaughn et al., 2003). Vaughn et al.'s (2003) participants completed one of three writing tasks in a single, 20-minute session. Outcome participants completed the standard BPFS-W task used by King (2001) and controls wrote about their

daily activities. The process condition was more structured; participants wrote for seven minutes about the outcome of reaching their BPFS, followed by a further two sets of seven minutes about what they could be doing in 10 and 20 years to help them to achieve it. The outcome condition was superior to the process condition in terms of psychological well-being four to seven weeks post-writing, and the process condition did not benefit well-being relative to the control condition. McGovern (2004) focussed on an academic BPFS and examined the effects of writing about the process towards this in comparison to writing about the outcome on self-efficacy for self-regulated learning and semester grades. Participants were asked to write for 20 minutes a day across four days about either the outcome of achieving a hoped-for semester grade, the actions to be completed to achieve it, or a neutral control topic. No significant main effect of condition on semester grades or self-efficacy was found two-weeks post-writing. It is difficult to draw firm conclusions with regards to the comparative efficacy of process and outcome BPFS-W from these studies because the process and outcome conditions differed greatly in structure, rendering fair comparisons impossible. Vaughn et al.'s (2003) process task was broken-down into three seven-minute sets, whereas their outcome task was not. It is possible that this additional structure reduced the efficacy of the process task; perhaps seven minutes was too short a time-period for participants to sufficiently engage with each sub-topic. Similarly, in McGovern's (2004) study, process participants received a new set of instructions daily, with each set directing them to write about different process goals. McGovern's (2004) outcome participants received the same instructions daily. Further research using more closely-matched designs is needed to clarify the comparative efficacy of process- and outcome-focussed BPFS-W.

4.2 Aims

There were two aims of the current study. The first was to test the hypothesis that there will be differential changes in physical and psychological well-being, self-regulation, emotion-regulation and self-efficacy as functions of task modality (writing or simulation) and task type (outcome, process and non-goal-related control). The second aim was to explore whether changes in any

physical and psychological well-being outcome variables may be mediated by self-regulation, emotion-regulation, or self-efficacy.

4.3 Method

4.3.1 Design

The present study employed a mixed-measures experimental design. There were two between-group independent variables (IVs). The first was 'modality' with two levels; writing and simulation. The second was 'task', with three levels; BPFS (outcome), the process of attaining a BPFS (process), and activities of the previous day (control). The within-group IV was outcome-assessment time-point. There were six independent groups: writing or simulating about a BPFS outcome, writing or simulating about a BPFS process, and writing or simulating about the activities of the previous day. The procedure consisted of a pre-manipulation (baseline) assessment, and three post-manipulation follow-up assessments, which took place one, four and eight weeks after the writing or simulation session. The dependent variables (DVs) measured at baseline and follow-up were physical and psychological well-being, generalised self-efficacy, self-regulation and emotion-regulation. Positive and negative affect (PA and NA) were measured immediately pre- and post-manipulation.

4.3.2 Power analysis

An *a priori* power analysis was conducted using G*Power (Faul, Erdfelder, Lang & Buchner, 2007) for the task (process versus outcome versus control) * modality (writing versus simulation) interaction. It was estimated that for a 3*2 ANCOVA— based on a medium effect size (f) of 0.25, numerator degrees of freedom of five, one covariate (baseline scores) and six independent groups— a sample of 211 participants was required to obtain the desired power level of 0.8 for the task * modality between-group interaction (actual power= .80).

It should be noted that whilst the above power calculation provides the desired sample size for the between-group interaction, the model used in the current study included a within-group variable (time), with three levels (one-, four- and eight-week follow-up). Therefore, the overall model is likely to have more power, thus a smaller sample than 211 is likely to be sufficient.

4.3.3 Participants

Participants were recruited using an advertisement displayed on Sheffield Hallam University Psychology Department's online research participation site (see Appendix A.2). Verbal recruitment was used in the University library. First year Undergraduate Psychology students were offered course credit for their participation, whilst other participants were offered a £5 high street shopping voucher. Participants were randomised to conditions using an online random order generator. 118 participants were recruited, 84 (71.2 %) were female, and the mean age of the sample was 24.14 (SD= 9.29) years. One hundred and thirteen (96%) were students.

4.3.4 Materials

The following questionnaires were used in the current investigation:

Positive and Negative Affectivity Scale (PANAS; Watson, Clark & Tellegen, 1988)

PA and NA were measured to enable assessment of whether participants engaged with the activities, because BPFS-W is consistently found to elicit immediate increases in PA (e.g. Frein & Ponsler, 2014; Hanssen et al., 2013). The state PANAS was employed to measure PA and NA immediately before and after the interventions. This instrument constitutes a list of 10 adjectives descriptive of negative moods (e.g. 'distressed' and 'jittery') and 10 items descriptive of positive moods (e.g. 'excited' and 'inspired'). Respondents are asked to indicate the extent to which each item relates to them in the present moment using a five-point Likert scale (1= 'very slightly/ not at all', 5= 'extremely'). For both subscales, a high score represents a high level of the respective affect, and possible scores range from 10 to 50. The state PANAS has been found to have a good level of internal reliability (α= .89 for PA, α= .85 for NA; Watson et al., 1988).

Depression, Anxiety and Stress Scale-21 (DASS-21; Lovibond & Lovibond, 1995).

To measure psychological well-being, the DASS-21 was used. The DASS-21 comprises three seven-item subscales, assessing depression, anxiety, and

stress. To illustrate, in the anxiety subscale, respondents are presented with items such as 'I was worried about situations in which I might panic and make a fool of myself'. Respondents reflect over the past week and indicate how often the item applied to them using a four-point Likert scale (0= 'did not apply to me at all', 3= 'applied to me very much or most of the time'). A score for each subscale is obtained by summation of each Likert scale answer, then multiplying the total by 2. Possible scores on each subscale range from 0 to 42. The DASS-21 has high internal consistency; Antony, Bieling, Cox, Enns and Swinson (1998) reported Cronbach's alphas of 0.94, 0.91 and 0.87 respectively for the depression, stress and anxiety subscales. The DASS-21 has also been found to correlate with other validated measures of depression, anxiety and stress; Osman et al. (2012) found significant correlations between scores on the DASS-21 and scores on the Perceived Stress Scale (Cohen, Kamarck & Mermelstein, 1983; r= .73, p<.001), the Beck Anxiety Inventory (Beck & Steer, 1990; r= .69, p<.001) and the Beck Depression Inventory II (Beck, Steer & Brown, 1996; r=.80, p<.001). It therefore has concurrent validity.

13-item Physical Symptoms Inventory (PSI; Kessler, Spector, Chang & Parr, 2008; Spector, 2018)

To assess physical health, the 13-item PSI was used. This measure includes items from the original 18-item PSI (Spector & Jex, 1998), following removal of five items which were not usually endorsed by respondents (Spector, 2018). It requires participants to reflect upon their physical health in the past month. Each item represents a common physical or somatic symptom, such as ‘eye strain’ and ‘dizziness’. Respondents are required to indicate how frequently they have experienced each symptom over the past month using a 5-point Likert scale (1= not at all’; 5= ‘every day’). Possible scores span 13-65, with higher scores indicating greater frequency of occurrence of physical symptoms. The 13-item PSI has been found to have a high level of internal reliability (α = .87; Kessler et al., 2008).

Short Self-Regulation Questionnaire (SSRQ; Carey, Neal & Collins, 2004)

The SSRQ was used to assess self-regulation. This is a 31-item self-report measure based on the original 63-item Self-Regulation Questionnaire (SRQ;

Brown, Miller & Lawendowski, 1999). It was designed to assess ability to regulate actions to reach future goals (Carey et al., 2004). Respondents give a rating of the extent they feel each item (for example ‘Once I have a goal, I can usually plan how to reach it’) applies to them using a five-point Likert scale (1= ‘strongly disagree’, 5 = ‘strongly agree’). Some items are reverse-scored, such as ‘I tend to keep doing the same thing, even when it doesn’t work’. Scores range from 31 to 155. A high score represents a high level of self-regulation.

The SSRQ is psychometrically sound. Carey et al. (2004) found a high level of internal consistency (α=.92), comparable to that of the original version (α =.91; Aubrey, Brown & Miller, 1994). Carey et al. (2004) also found no significant difference in SSRQ scores as a function of a range of demographic variables including age group, gender, ethnicity and social class, demonstrating construct validity. Furthermore, the SSRQ has concurrent validity; scores correlate significantly and strongly with scores on validated instruments intended to measure constructs related to self-regulation (Potgieter & Botha, 2009; e.g. Mindful Attention Awareness Scale; Brown & Ryan, 2003; r= .57).

Difficulties in Emotion-Regulation Scale (DERS; Gratz & Roemer, 2004)

Emotion-regulation was measured using the DERS. This measure consists of 36 items, such as ‘When I’m upset, I believe that wallowing in it is all I can do’. Respondents rate on a 5-point Likert scale how often each item applies to them (1 = 'almost never; 0-10%' of the time, 5 = 'almost always, 91-100%' of the time), and scores range from 36 to 180. High scores represent high levels of difficulties in emotion-regulation (reverse scoring is required on some items, such as 'I am clear about my feelings'). Gratz and Roemer (2004) have reported that the DERS has a high level of internal consistency (α= .93), as well as high concurrent validity in that scores are negatively and significantly correlated (r=.69) with a commonly-used measure of emotion-regulation (Generalized Expectancy for Negative Mood Regulation Scale; Catanzaro & Mearns, 1990). Finally, and of importance to the current study, the DERS has been found to have a high level of test-rest reliability over a period of eight weeks (r= .88; Gratz & Roemer, 2004).

Generalised Self-Efficacy Scale (GSES; Schwarzer & Jerusalem, 1995)

To measure self-efficacy, the GSES was used. In this scale, respondents are presented with 10 items relating to their perceptions of how effectively they cope in various situations, for example 'it is easy for me to stick to my aims and accomplish my goals', and 'when I am confronted with a problem, I can usually find several solutions'. Participants respond using a four-point Likert scale (1= 'not at all true', 4 = 'exactly true'). Possible scores range from 10 to 40. The higher the score, the higher the individual's level of perceived self-efficacy. This scale was selected due to its excellent psychometric properties. Weinman, Wright and Johnston (1995) report that high internal consistency has been found across five samples, with alpha levels ranging from 0.82 to 0.93, and Luszczynska, Scholz & Schwarzer (2005) found GSE scores to correlate significantly with scores from instruments intended to measure related social-cognitive constructs (e.g. positive outcome expectancies; Sniehotta, Scholz & Schwarzer, 2005; $r=.32$, $p<.05$). Furthermore, it was decided that a general measure of self-efficacy should be used due to expectation of a wide variety of self-generated goals in writing and simulation tasks, as suggested by Armitage and Reidy (2012).

In addition to the above measures, participants provided demographic information; their age, their gender, and whether they were currently a student.

4.3.5 Procedure

Prospective participants were invited to participate in an investigation exploring the impact of imagining life activities on health. If individuals were interested in taking part in the study, they arranged an appointment by sending an e-mail to the researcher. The procedure of the current investigation is summarised in Figure 4.1.

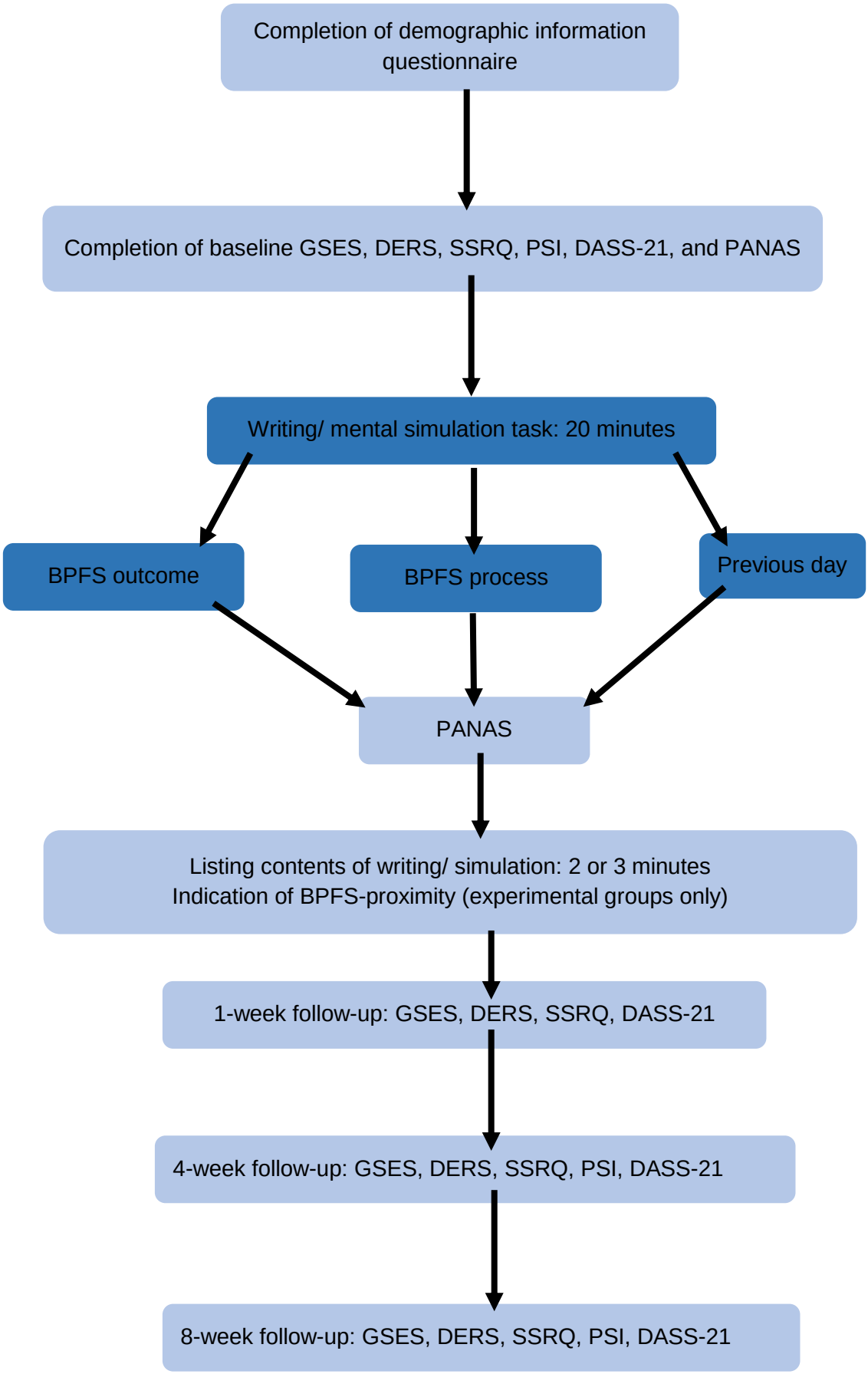


Figure 4.1: A flow diagram of the study procedure

On day one of the study, participants met with the researcher and read an information sheet (Appendix A.3) outlining what their participation would entail, then read and signed a consent form (Appendix A.4). They then completed a series of psychometric tests to measure their baseline physical health (PSI), psychological health (DASS-21), difficulties in emotion-regulation (DERS) and self-regulation (SSRQ), general self-efficacy (GSES) and PA and NA (PANAS). Participants were asked to take a moment to consider what their BPFS is or what happened in their previous day. They then wrote or mentally simulated for 20 minutes. Writing and simulation instructions were adapted from those of previous researchers, first in accordance with the demands of the present investigation, and second dependent on what task condition the participant was assigned.

Participants allocated writing/ simulation about a BPFS outcome were instructed with the following, based on writing instructions by King (2001) and simulation instructions by Pham and Taylor (1999):
We would like you to think/ write about your life in the future. 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 of your life dreams. It is very important that you visualise/ write about yourself actually reaching this best possible future self, and have that picture in your mind.

Participants allocated a writing or simulation task about the process they would have to complete to reach their BPFS were instructed with the following, adapted from Layous, Nelson and Lyubomirsky (2013) and Pham and Taylor (1999):
Now write down/ think about goals that you might want to attain that will help you achieve your best possible future self that you just thought about. Sometimes long-term goals seem overwhelming or out of your reach. But every journey begins with just a single step. Think/ write about taking little steps towards your long term best possible future self. A little step could be as simple as proactively seeking information you need or talking to someone who may be

able to guide you. It is very important that you visualise/ write about yourself actually completing each little step, and have that picture in your mind.

There is variation across BPFS intervention studies with regards to control group topics. Topics have included early memories (Shapira & Mongrain, 2010), details of the past seven days (Lyubomirsky, Dickerhoof, Boehm & Sheldon, 2011), and ordinary details of daily life (Sheldon & Lyubomirsky, 2006). The most frequently used control topics appear to be details of the previous day (e.g. Austenfeld, Paolo & Stanton, 2006; Layous et al., 2013) and plans for the next day (e.g. Harrist, Carlozzi, McGovern & Harrist, 2007; King, 2001). King (2001) suggested that writing about plans for the day may involve writing about lower-order, process goals. It was decided, given that participants writing/ simulating about the process of achieving their BPFS will focus on some lower-order goals, that this may confound findings. Hence the current study adapted Pham and Taylor's (1999) and Sheldon and Lyubomirsky's (2006) instructions to ask control participants to write/ mentally simulate about the details of their previous day, as detailed below:
What we would now like you to do is to think/ write about the details of your day yesterday. This may include particular classes or meetings, interactions with other people, what you had for lunch or what clothes you wore. Think/ write about as many details as you can. It is very important that you visualise/ write about yourself actually doing each activity, and have that picture in your mind/ write as though you are in that situation.

Upon completion of the writing/ simulation task, participants repeated the PANAS before being given a moment to write down the contents of their writing/ simulation in the form of a bullet-point list. Experimental group participants were then asked to indicate how far in the future they felt their BPFS was (hereon referred to as 'BPFS-proximity'). Finally, they were thanked and briefed about the next phase of the study. As illustrated in Figure 4.1, the next phase involved follow-up sessions, which took place one, four, and eight weeks post-intervention. Follow-ups were staggered in this way to explore for how long effects are maintained, and to capture effects which may have a latent onset. Participants were e-mailed on the day before each follow-up to remind them to

complete it. On the day of each follow-up participants were e-mailed a Qualtrics (online survey generator) link to online versions of the outcome measures[6]. Upon completion of the first two follow-up stages, participants were briefed online about the next stage of the study. Following the final follow-up, participants were thanked for their time and fully debriefed, including providing them with a debrief sheet (Appendix A.5).

4.3.6 Ethical considerations

The current investigation was designed and carried out in accordance with British Psychological Society (BPS) guidelines for conducting research involving human participants (Code of Ethics and Conduct; BPS, 2009). Details of the study were submitted to the faculty research ethics committee, who granted permission for it to be conducted. Ethics applications and approval are provided in Appendix A.6.

4.4 Results

4.4.1 Attrition

All 118 of the originally-recruited participants completed the first stage of the study; baseline measures, writing/ simulation and PANAS immediately post-intervention. One participant had omitted answers to half of the items on the DASS-21 at baseline. For this reason, all their data for the DASS-21 baseline was removed from analyses, yet their data for all other baseline measures remained. There was some attrition across the three follow-up phases. Seven of the 118 participants did not complete the one-week follow-up (111 participants completed the one-week follow-up, but one of these completed only the DERS). 23 participants did not complete the four-week follow-up (95 completed), and 29 did not complete the eight-week follow-up (89 completed). Only 82 participants completed all three follow-ups. The flow of participants through the study is shown in Figure 4.2.

Links to follow-ups were sent to participants one, four and eight weeks post-writing. However, some participants completed them after this date, and a

[6] The PSI was omitted at the one-week follow-up; it measures symptoms over the past month thus would be unlikely to detect changes in symptoms over one week.

minority completed them before (perhaps through student participants accessing links from peers). These participants were included in analyses, and ANOVAs were conducted to examine whether there were significant between-group differences in days between the intervention and each follow-up. There was no significant between-group difference in the mean number of days between completion of the intervention and the one-, four- and eight-week follow-ups (all Fs <1.991, all ps >.088). The group means of days[7] between baseline and each follow-up were 8.85 to 9.75 (one week), 29.93 to 31.06 (four weeks) and 55.45 to 59.68 (eight weeks).

[7] The mean number of days reported here includes the intervention day and the day on which each follow-up was completed.

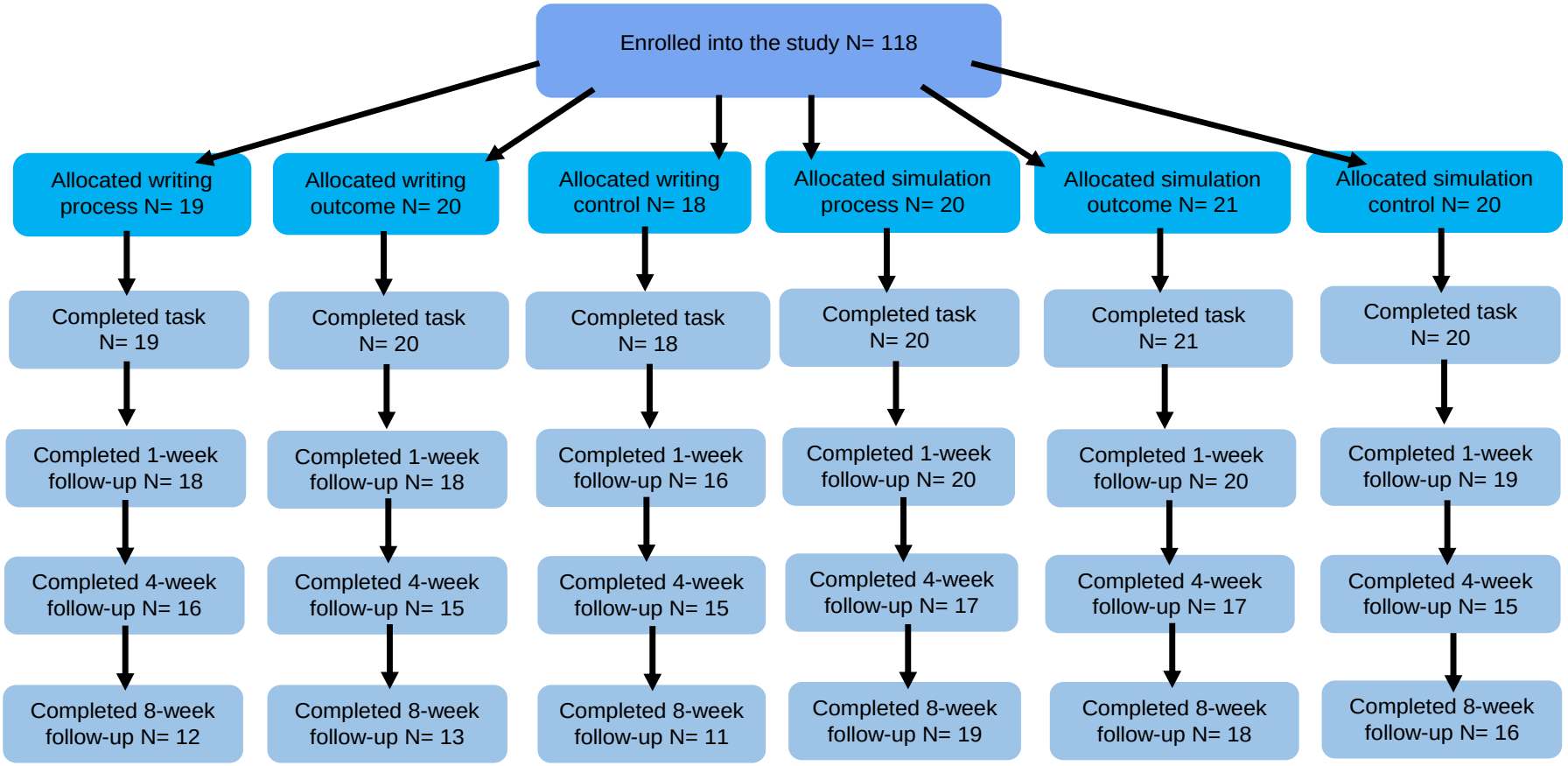


Figure 4.2: Diagram adapted from Consolidated Standards of Reporting Trials (CONSORT, 2010) showing participant-flow and attrition (Schulz, Altman & Moher, 2010).

4.4.2 Data preparation

4.4.2.1 Reliability analysis

Cronbach's alpha was calculated for all scales at baseline. The internal reliability of the PSI and the anxiety and NA scales was found to be acceptable (αs =.76, .67 and .73, respectively). The internal reliability of all other scales was found to be high (all αs ≥.82).

4.4.2.2 Missing data

Data were entered in SPSS and were screened for missing values. Several individual items were found to be missing. For BPFS-proximity, 4 values were missing. Values had also been omitted at baseline for GSES (n=1), PSI (n=1), depression (n=2), anxiety (n=1) and stress (n=1), positive (n=1) and negative (n=1) affect, DERS (n=7) and SSRQ (n=3). Immediately post-test there were missing values for PA (n=2). For the one-week follow-up, missing values were identified for the GSES (n=1), stress (n=1), DERS (n=3) and SSRQ (n=2). At the four-week follow-up, missing values were found on the PSI (n=1) and DERS (n=3), and at eight weeks they were found for depression (n=1) and the DERS (n=1).

Little's MCAR test was performed on each affected scale to decide the most appropriate method of dealing with the missing values. Where Little's MCAR test was non-significant, data were deemed to be missing completely at random, and expectation maximisation was used to estimate a value for each omission. Where Little's MCAR test was violated, stochastic regression was used.

4.4.2.3 Testing assumptions of parametric analyses

Data were examined to ascertain whether they were suitable for parametric analyses. Z scores were generated to screen the data for outliers. Univariate outliers were identified as any item with a z score greater than +/-3 standard deviations (SDs) from the mean (Stevens, 2002). Outliers were found for BPFS-proximity (n=3), and at baseline on the PSI (n=1), depression subscale (n=1), anxiety subscale (n=1), stress subscale (n=1) and NA subscale (n=2), and immediately post-test on the NA subscale (n=2). At the one-week follow-up,

outliers were identified for the SSRQ (n=1) and depression (n=1). At the four-week follow-up, outliers were found for the PSI (n=1), depression (n=1), anxiety (n=2) and stress (n=1).

Different transformations were required to correct the outliers in each DV, as shown in Table 4.1. Where a transformation was used for any time-point other than baseline on a DV, the other two time-points received the same transformation so that time-points could be compared using inferential statistics.

Table 4.1: Transformations used to correct outliers

DV	Transformation which corrected outliers
BPFS proximity	Tabachnick and Fidell (2001)
Negative affect pre-writing	Negative reciprocal
Negative affect post-writing	Negative reciprocal
Baseline PSI	Square root
Four-week follow-up PSI	Log
Baseline depression	Square root
One-week follow-up depression	Square root
Four-week follow-up depression	Square root
Baseline anxiety	Square root
Four-week follow-up anxiety	Square root
Baseline stress	Square root
Four-week follow-up stress	Square root
One-week follow-up SSRQ	Tabachnick and Fidell (2001)

As shown in Table 4.1, transformatons did not correct the outliers in BPFS-proximity and the SSRQ at one-week follow-up. These outliers were above the mean, thus were replaced with a value one higher than the highest score not identified as an outlier, as recommended by Tabachnick and Fidell (2001).

Normality of distributions of DVs were assessed according to Kim's (2013) suggestion of a distribution being significantly skewed if the Z score of the skewness value (skewness value/ standard error of skewness) is greater than 3.29 (equivalent alpha of .05) for medium-sized samples ($50< n < 300$). DVs were normally-distributed, other than the DERS at the one-week follow-up, in the process simulation group. Scattergraphs demonstrated no evidence of curvilinear relationships between any covariate and DV, thus the assumption of linearity was satisfied. To evaluate whether data were suitable for analysis using ANCOVA, the assumption of homogeneity of regression slopes was tested. There were no significant interactions between the covariates (baseline scores) and IVs (time-point, modality and task) for any DVs other than self-regulation (SSRQ), indicating that data for these DVs satisfied the assumption of homogeneity of regression slopes (all $Fs \leq 2.273$, all $ps \geq .112$). There was a significant interaction between modality and SSRQ baseline scores ($F(1, 74)= 8.294$, $p= .005$, $\eta_p^2= .101$). Interactions between SSRQ baseline scores and the other two IVs (task and time-point) were non-significant ($Fs= 1.416$ and 1.970, $ps= .249$ and $.148$, respectively). It should be noted that, on the most part, the assumption of homogeneity of variance was violated. For all DVs other than the DERS, the largest SD value was more than twice the value of the smallest SD value. This violation is unlikely to lower the accuracy of analyses, given that ANOVA and ANCOVA are usually accepted to be robust against heterogeneity of variance, especially when group sizes are relatively equal (Tabachnick & Fidell, 2013), but results should be treated with caution (Bradley, 1984)[8].

Mauchly's assumption of sphericity of a repeated-measures IV was also violated for the DERS ($X^2(2)= 18.743$, $p< .001$). For this reason, for all within-

[8] Homogeneity of variance was assessed using SDs generated from unadjusted data. This was because data for some DVs were differently transformed at follow-up time-points in comparison to baseline, due to different transformations being needed to correct outliers. It would, therefore, have been inaccurate to compare SDs from some baseline points to SDs from follow-ups. Violation of this assumption may therefore be a result of the presence of outliers.

participants main effects and interactions for the DERS, the Greenhouse-Geisser statistic has been reported. For all other DVs, data met Mauchly's assumption.

Following this cleaning and preparation of the data, they were deemed suitable for analysis using parametric statistical tests. Despite some violation of assumptions, ANCOVA was performed as it has been found to be robust against violations of its assumptions (Blanca, Alarcón, Arnau, Bono & Bendayan, 2017; Dancey & Reidy, 2007; Levy, 1980; Schmider, Ziegler, Danav, Bever & Bühner, 2010). However, results of analyses of DVs found to violate assumptions should be regarded with caution.

4.4.3 Checking adherence to task instructions

Adherence to intervention instructions was assessed by reading the content of the bullet lists generated by participants after the writing or simulation task— as well as the essays produced by participants allocated to writing conditions— and examining the extent to which the content was in line with the instructions given. Each participant's adherence was graded as complete adherence, partial adherence or no adherence. For some simulation participants— given that assessment could be made only using bullet lists— it was difficult to assess the level of adherence. These participants' adherence levels were graded as 'unclear'. Results from the adherence assessment are presented in Table 4.2.

Table 4.2: Adherence to task instructions across groups

	Adherence across groups					
	Writing Process (N= 19)	Writing Outcome (N=20)	Writing Control (N=18)	Simulation Process (N=20)	Simulation Outcome (N=21)	Simulation Control (N=20)
Complete adherence	17	18	18	17	21	20
Partial adherence	2	2	0	0	0	0
No adherence	0	0	0	0	0	0
Unclear	0	0	0	3	0	0

Across all conditions, adherence to instructions was deemed to be high. No participants failed to adhere completely.

Outcome

All simulation outcome participants were deemed to have adhered completely to the task instructions. 18 of the 20 writing outcome participants adhered completely. The remaining two participants partially adhered. One participant wrote about the outcome of their BPFS such as working as a nutritionist with elite athletes, but also wrote about some of the processes that they would need to go through to reach it, such as completing voluntary work. The other wrote about outcome goals such as working as a clinical psychologist but also included details of how having that higher-order goal impacted them currently. For example, they stated that it made them determined and hard-working. Commonly-occurring topics included in the essays and bullet lists generated by participants in BPFS outcome conditions included being in a healthy relationship with a romantic partner, being financially stable and having a successful, fulfilling career.

Process

17 of the 20 simulation process participants were deemed to have adhered fully to the task instructions. For the remaining three participants, it was difficult to assess adherence because the information provided about their imagery in the bullet lists was broad. The bullet lists generated from these participants included items such as 'productive', ‘no worries’, 'family', 'education' and 'wearing nice clothes’. 17 out of the 19 writing process participants were deemed to have adhered completely. The remaining two participants were deemed to have partially adhered. One had written about process goals such as asking a career advisor to help with decisions with regards to career options but had also written about some steps which had already been achieved, for example they had written that they had completed some courses designed to build confidence. The other participant deemed to have partially adhered had written about some process goals— such as writing up lecture notes and working on assignments to help them achieve their degree— but had also written about some positive

outcomes— such as going on frequent holidays— without mentioning the lower-order goals that they would need to achieve to reach those outcomes. Commonly-occurring topics included in the essays and bullet lists generated by participants in process conditions included talking to professionals such as University tutors and career advisors for guidance, working hard at university (for example by doing additional reading and attending lectures) and saving money (for example by going on fewer nights out).

Control

All control participants across both writing and simulation modalities had written or simulated only about the details of their previous day. Commonly-occurring topics included attending lectures, socialising and details around times of waking up and going to sleep.

4.4.4 Checking for between-group differences at baseline

Descriptive statistics for all demographic and outcome variables are presented in Table 4.3. All descriptive statistics presented in tables throughout this chapter are unadjusted; they represent the average scores prior to transformation for outlier correction. This was decided for ease of comparison due to some transformed variables being on different scales due to different transformations being necessary. Transformed data are used in inferential analyses and reported to illustrate simple effects.

Table 4.3: Means and SDs of age and outcome variables at baseline

	Group					
	Writing Process	Writing Outcome	Writing Control	Simulation Process	Simulation Outcome	Simulation Control
Age	25.84 (11.28)	25.30 (10.07)	22.06 (10.07)	21.10 (5.16)	23.81 (9.04)	26.65 (10.64)
BPFS-Proximity (years)	6.22 (4.14)	14.78 (13.65)	-	8.02 (8.95)	12.23 (7.52)	-
Physical symptoms	23.75 (5.19)	27.31 (9.83)	19.18 (3.79)	24.71 (4.92)	25.39 (6.30)	25.64 (6.02)
Depression	5.05 (5.01)	7.00 (8.86)	9.22 (9.97)	8.20 (7.25)	7.10 (7.50)	7.54 (8.60)
Anxiety	5.16 (4.34)	9.10 (7.12)	10.07 (9.70)	9.00 (5.21)	6.38 (6.47)	8.11 (4.92)
Stress	11.21 (9.74)	13.00 (10.25)	13.11 (9.23)	15.00 (8.19)	14.48 (10.27)	13.05 (7.28)
Negative affect	12.58 (2.43)	12.92 (3.25)	14.18 (6.40)	12.71 (2.89)	12.20 (2.91)	13.14 (3.44)
Positive affect	31.25 (6.68)	29.92 (7.39)	32.09 (3.62)	29.24 (5.29)	30.40 (6.51)	28.00 (7.41)
Emotion-regulation	78.10 (14.72)	82.65 (17.34)	72.64 (20.87)	82.53 (20.49)	75.67 (15.51)	84.20 (26.92)
Self-regulation	113.62 (10.77)	103.77 (17.03)	114.27 (21.05)	110.34 (17.09)	115.33 (12.16)	110.51 (15.19)
Self-efficacy	30.92 (3.94)	29.31 (5.96)	33.09 (4.04)	30.82 (3.63)	30.73 (3.79)	30.57 (4.57)

SDs are presented in parentheses in this and all subsequent tables.

BPFS-proximity

A 2*2 independent-measures ANOVA was conducted to explore whether there were significant between-group differences in participants' reported BPFS-proximity. Two IVs were included; modality (writing versus simulation) and task (process versus outcome). There was no significant main effect of modality ($F(1, 76)= .016$, $p= .901$, $\eta_p^2< .001$) and no significant modality * task interaction ($F(1, 76)= .552$, $p= .460$, $\eta_p^2= .007$). There was, however, a significant main effect of task ($F(1, 76)= 18.307$, $p< .001$, $\eta_p^2= .194$), in that BPFSs were rated to be more distal in the outcome in comparison to the process condition (mean numbers of years= 12.19 versus 6.77, respectively).

Pre-manipulation group differences on outcome variables

To explore whether there were any pre-manipulation between-group differences in outcome variables, a 2*3 independent-measures MANOVA was performed. Box's M was significant (p=.010), therefore the assumption of homogeneity of variance/ covariance matrices was violated. This is unlikely to be problematic given that group sizes at baseline were relatively equal, and Box's M is a conservative test of violations of the assumption of homogeneity of variance/covariance matrices (e.g. Tabachnick & Fidell, 2013).

The MANOVA demonstrated no significant multivariate difference in baseline scores as a function of modality (Pillai's trace= .076, $F(9, 103)= .947$, $p= .488$, $\eta p^2 =.076$) or task (Pillai's trace= .170, $F(18, 208)= 1.077$, $p= .377$, $\eta p^2 = .085$). There was also no modality * task interaction (Pillai's trace= .137, $F(18, 208)= .851$, $\eta p^2= .069$).

4.4.5 Immediate effects of writing/ simulation on PA and NA

The effects of modality (writing versus simulation) and task (process versus outcome versus control) on PA and NA immediately post-task was examined using two separate 3*2 ANCOVAs. Baseline (pre-test) levels were entered as a covariate to partial out their influence. Means and SDs across groups are presented in Table 4.4.

Table 4.4: Means and SDs of positive and negative affect scores as functions of modality and task

	Writing process	Writing outcome	Writing control	Simulation process	Simulation outcome	Simulation control
PA, pre-test	31.25 (6.68)	29.92 (7.39)	32.09 (3.62)	29.24 (5.29)	30.40 (6.51)	28.00 (7.41)
PA, post-test	34.17 (4.60)	33.31 (7.70)	31.77 (8.14)	29.65 (7.80)	35.20 (5.61)	27.21 (8.30)
NA, pre-test	12.58 (2.43)	12.92 (3.25)	14.18 (6.40)	12.71 (2.89)	12.20 (2.91)	13.14 (3.44)
NA, post-test	12.08 (2.15)	10.92 (1.04)	15.45 (8.36)	12.12 (2.83)	11.07 (1.53)	11.64 (2.06)

Analyses indicated no significant main effects of modality ($F(1,111)=.001$, $p=.977$, $\eta_p^2< .001$) or task ($F(2,111)= 1.473$, $p= .234$, $\eta_p^2= .026$) on NA, and no significant modality * task interaction ($F(2,111)= 1.649$, $p= .197$, $\eta_p^2= .029$).

The ANCOVA for PA indicated no significant main effect of modality ($F(1,111)= 1.581$, $p= .211$, $\eta_p^2= .014$) and no significant modality * task interaction ($F(2,111)=.958$, $p= .377$, $\eta_p^2= .017$). However, there was a significant main effect of task ($F(2,111)= 7.167$, $p=.001$, $\eta_p^2= .114$). Pairwise comparisons were undertaken to explore this further. It should be noted that where results from a pairwise comparison have been reported throughout this thesis, no Bonferroni adjustment has been applied. Perneger (1998) suggests that consideration of how many comparisons are performed is irrelevant to the interpretation of each individual finding, and that each finding should be interpreted using knowledge with regards to whether it is plausible. Although many researchers use the Bonferroni adjustment with the intention of reducing the risk of type one error, it substantially reduces statistical power and increases the type two error rate (Perneger, 1998; Nakagawa, 2004). There are two reasons why a type two error presents a greater risk than a type one error in the current study. First, the sample size is relatively small and as such type two errors are already a possibility. Second, the current study is the first to explore the comparative effects of process and outcome BPFS writing and mental simulation, and the first to measure self-regulation as an outcome and possible mechanism of BPFS-W. Therefore, given the exploratory— rather than confirmatory— nature of this research, type two errors could be argued to pose greater risks than type one errors. Type two errors could lead not only to failure to detect important effects, but also to possible differences in the effects of writing and mental simulation tasks being missed. Perhaps more conservative thresholds for significance should be applied if and when research in this area reaches a confirmatory stage. Pairwise comparisons indicated significantly greater PA in the outcome condition than in the control condition immediately post-task ($p< .001$, means= 33.21 versus 27.95), and significantly greater PA in the process condition than in the control condition immediately post-task ($p= .030$, means=

31.03 versus 27.95). There was no significant difference in PA between outcome and process conditions (p= .116, means= 33.21 versus 31.03[9]).

4.4.6 Long-term effects

4.4.6.1 Psychological and physical well-being

The effects of modality (writing versus simulation), task (outcome versus process versus control) and time-point (one-week versus four-week versus eight-week follow-up) on physical symptoms[10], depression, anxiety and stress were explored using ANCOVAs, with baseline scores entered as covariates. The unadjusted means and SDs as a function of group and time-point are presented in Table 4.5.

[9] Means reported to illustrate pairwise comparisons following ANCOVAs throughout this chapter are adjusted for the influence of the covariate, therefore they are different values to those presented in Tables 4.2, 4.3, 4.4 and 4.5.

[10] Given that physical symptoms were not measured one week post-intervention, the time-point IV has only two levels in analyses of physical symptoms.

Table 4.5: Means and SDs of all well-being outcomes at follow-ups

	Writing process			Writing outcome			Writing control			Simulation process			Simulation outcome			Simulation control		
	1 week	4 weeks	8 weeks	1 week	4 Weeks	8 Weeks	1 Week	4 weeks	8 weeks	1 week	4 Weeks	8 weeks	1 week	4 weeks	8 weeks	1 week	4 weeks	8 weeks
Dep.	6.22 (6.13)	6.00 (6.32)	6.67 (6.73)	6.67 (7.33)	5.07 (5.28)	7.08 (7.51)	6.25 (7.86)	6.53 (10.60)	6.73 (8.31)	7.90 (6.91)	9.88 (7.43)	8.32 (6.61)	8.50 (9.40)	8.00 (7.21)	6.33 (7.49)	7.26 (10.52)	8.80 (10.44)	7.00 (8.61)
Anx.	5.00 (4.13)	5.25 (5.56)	5.33 (5.55)	5.75 (7.48)	5.20 (5.85)	5.45 (8.15)	4.00 (4.82)	5.87 (10.21)	5.45 (8.15)	6.10 (5.29)	4.82 (4.64)	7.05 (4.92)	8.00 (8.78)	8.21 (9.87)	6.22 (7.57)	5.89 (7.29)	5.47 (6.91)	5.88 (6.75)
Stre.	10.33 (7.40)	8.63 (7.00)	9.00 (8.07)	11.05 (8.17)	8.13 (5.52)	11.84 (6.19)	10.25 (10.66)	8.53 (10.49)	9.27 (8.45)	14.20 (6.19)	12.00 (7.51)	12.74 (8.87)	15.00 (9.61)	13.29 (9.64)	12.11 (8.69)	11.16 (8.04)	11.07 (7.78)	10.25 (8.13)
Phys	-	21.50 (5.11)	21.50 (5.11)	-	24.31 (7.63)	24.31 (7.34)	-	19.09 (3.72)	20.00 (4.40)	-	23.53 (4.93)	24.41 (6.67)	-	23.81 (7.37)	23.88 (6.53)	-	21.93 (5.20)	22.57 (6.62)

Dep.= depression, Anx.= anxiety, Stre.= stress, Phys.= physical symptoms.

Psychological well-being

Depression: The 3*2*(3) ANCOVA revealed no significant main effect of time-point ($F(2, 148)= 1.450$, $p=.238$, $\eta_p^2= .019$), modality ($F(1, 74)= 1.076$, $p= .303$, $\eta_p^2= .014$), or task ($F(2, 74)= 2.056$, $p= .135$, $\eta_p^2=.053$) on depression. There were also no significant modality * task ($F(2, 74)=.028$, $p=.972$, $\eta_p^2=.001$), time-point * modality ($F(2, 1448)= .951$, $p=.389$, $\eta_p^2=.013$), time-point * task ($F(4, 148)= .416$, $p= .797$, $\eta_p^2= .011$) and time-point * modality * task ($F(4, 148)= .238$, $p= .916$, $\eta_p^2= .006$) interactions.

Anxiety: There was no significant main effect of time-point ($F(2,148)= .445$, $p= .642$, $\eta_p^2= .006$), modality ($F(1,74)= .659$, $p= .419$, $\eta_p^2= .009$) or task ($F(2,74)= 2.652$, $p= .077$, $\eta_p^2= .067$) on anxiety. The modality * task ($F(2, 74)= .748$, $p= .477$, $\eta_p^2= .020$), time-point * modality ($F(2, 74)= .115$, $p= .892$, $\eta_p^2= .002$), time-point * task ($F(4, 74)= .407$, $p= .804$, $\eta_p^2= .011$), and time-point * modality * task ($F(4, 148)= 1.577$, $p= .183$, $\eta_p^2= .041$) interactions were also non-significant.

Stress: There was no significant main effect of time-point ($F(2, 148)= .824$, $p= .441$, $\eta_p^2= .011$), modality ($F(1,74)= 2.460$, $p= .121$, $\eta_p^2= .032$), or task ($F(2, 74)= 1.130$, $p= .329$, $\eta_p^2= .030$) on stress. There were also no significant modality * task ($F(2, 74)= .270$, $p= .764$, $\eta_p^2= .007$), time-point * modality ($F(2, 148)= 2.147$, $p= .120$, $\eta_p^2= .028$), time-point * task ($F(4, 148)= .621$, $p= .649$, $\eta_p^2= .016$), and time-point * modality * task ($F(4, 148)= .697$, $p= .595$, $\eta_p^2= .018$) interactions.

Physical well-being (symptoms): There was no significant main effect of time-point ($F(1, 76)= 1.146$, $p= .288$, $\eta_p^2= .015$), modality ($F(1, 76)= .052$, $p= .820$, $\eta_p^2= .001$), or task ($F(2, 76)= .167$, $p= .847$, $\eta_p^2= .004$). There were also no modality * task ($F(2, 76)= 1.775$, $p= .176$, $\eta_p^2= .045$), time-point * modality ($F(1, 76)= .048$, $p= .828$, $\eta_p^2= .001$), time-point * task ($F(2, 76)= .022$, $p= .978$, $\eta_p^2= .001$) or time-point * modality * task ($F(2, 76)= .057$, $p= .944$, $\eta_p^2= .002$) interactions.

4.4.6.2 Self-regulation and self-efficacy

The effects of modality, task and time-point on self-efficacy, self-regulation and emotion-regulation were explored using ANCOVAs, with baseline scores entered as covariates. The unadjusted means and SDs as a function of group and time-point are presented in Table 4.6.

Table 4.6: Means and SDs of self-regulation, emotion-regulation and self-efficacy at follow-ups

	Writing process			Writing outcome			Writing control			Simulation process			Simulation outcome			Simulation control		
	1 week	4 weeks	8 weeks	1 Week	4 Weeks	8 weeks	1 Week	4 weeks	8 Weeks	1 Week	4 Weeks	8 weeks	1 week	4 weeks	8 weeks	1 week	4 weeks	8 Weeks
Self-eff.	33.00 (4.18)	32.67 (4.38)	32.50 (4.23)	29.92 (5.54)	30.15 (5.93)	30.62 (5.53)	32.36 (5.48)	33.00 (5.62)	32.82 (4.69)	30.71 (3.87)	30.59 (3.81)	30.88 (4.64)	30.93 (3.10)	30.67 (2.85)	31.93 (4.89)	30.42 (5.49)	31.57 (6.38)	31.07 (5.68)
S-reg.	118.17 (14.22)	117.58 (16.12)	121.08 (14.95)	106.31 (18.18)	108.08 (17.20)	109.31 (19.17)	115.55 (23.57)	114.82 (24.06)	113.18 (27.79)	110.12 (19.44)	110.82 (14.88)	106.59 (17.27)	118.20 (11.63)	119.80 (10.80)	120.00 (12.11)	115.21 (14.48)	117.79 (19.26)	116.79 (17.02)
E-reg.	71.83 (15.23)	72.17 (19.04)	71.42 (21.98)	80.23 (22.57)	74.57 (16.75)	75.83 (19.13)	67.45 (15.98)	71.06 (21.04)	72.27 (23.35)	81.53 (23.78)	83.37 (27.00)	83.18 (24.85)	76.00 (15.58)	76.47 (17.90)	70.93 (17.60)	85.05 (24.05)	81.07 (24.79)	80.93 (25.99)

Self-eff.= self-efficacy, S-reg.= self-regulation, E-reg.= difficulties in emotion-regulation

Generalised self-efficacy: There was no significant main effect of time-point ($F(2, 150)= .771$, $p= .465$, $\eta_p^2= .010$), modality ($F(1, 75)= .911$, $p= .343$, $\eta_p^2= .012$) or task ($F(2, 75)= .329$, $p= .720$, $\eta_p^2= .009$) on self-efficacy. There were also found to be no significant modality * task ($F(2, 75)= 1.185$, $p= .311$, $\eta_p^2= .031$), time-point * modality ($F(2, 150)= .195$, $p= .823$, $\eta_p^2= .003$), time-point * task ($F(4, 150)= 1.090$, $p= .364$, $\eta_p^2= .0281$) or time-point * modality * task ($F(4, 150)= .087$, $p= .986$, $\eta_p^2= .002$) interactions.

Difficulties in emotion-regulation: There was no significant main effect of time-point ($F(1.634, 122.573)= .783$, $p= .436$, $\eta_p^2= .010$), modality ($F(1, 75)= 3.366$, $p= .071$, $\eta_p^2= .043$), or task ($F(2, 75)= .101$, $p= .904$, $\eta_p^2= .003$) on difficulties in emotion-regulation. There were also no significant modality * task ($F(2, 75)= .412$, $p= .664$, $\eta_p^2= .011$), time-point * modality ($F(1.634, 122.573)= .391$, $p= .635$, $\eta_p^2= .005$), time-point * task ($F(3.269, 122.573)= .773$, $p= .522$, $\eta_p^2= .020$), or time-point * modality * task ($F(3.269, 122.573)= 1.248$, $p= .295$, $\eta_p^2= .032$) interactions.

Self-regulation: There was no significant main effect of time-point ($F(2, 150)= 1.872$, $p= .157$, $\eta_p^2= .024$), modality ($F(1, 75)= .044$, $p= .835$, $\eta_p^2= .024$) or task ($F(2, 75)= .115$, $p= .891$, $\eta_p^2= .003$) on self-regulation. There was also no significant time-point * modality ($F(2, 150)= 1.426$, $p= .244$, $\eta_p^2= .019$) or time-point * task ($F(4,150)= 1.549$, $p= .191$, $\eta_p^2= .040$) interactions. There was, however, a modality * task ($F(2, 75)= 3.094$, $p= .051$, $\eta_p^2= .076$) interaction that was approaching statistical significance, and a significant time-point * modality * task ($F(4, 150)= 2.659$, $p= .035$, $\eta_p^2= .066$) interaction.

To explore the three-way interaction further, three 2*3 ANCOVAs were performed to explore differences in self-regulation as a function of modality and task at each follow-up time-point separately.

Differences in self-regulation as a function of modality and task, at each follow-up time-point separately:

The 2*3 ANCOVA for the one-week follow-up indicated no main effect of modality ($F(1, 103)= 2.044$, $p= .156$, $\eta_p^2= .019$) or task ($F(2, 103)= .055$, $p= .946$, $\eta_p^2= .001$). There was also no significant modality * task interaction ($F(2, 103)= 1.026$, $p= .362$, $\eta_p^2= .020$). Similarly, the ANCOVA for the four-week follow-up indicated no main effect of modality ($F(1, 88)= 1.494$, $p= .225$, $\eta_p^2= .017$) or task ($F(2, 88)= .430$, $p= .652$, $\eta_p^2= .010$), and no significant modality * task interaction ($F(2, 88)= 2.041$, $p= .136$, $\eta_p^2= .044$).

At the eight-week follow-up there was no significant main effect of modality ($F(1, 82)= .613$, $p= .436$, $\eta_p^2= .007$) or task ($F(2, 82)= .191$, $p= .826$, $\eta_p^2= .005$). There was, however, a significant modality * task interaction ($F(2, 82)= 4.757$, $p= .011$, $\eta_p^2= .104$). This significant interaction was explored further using a series of ANCOVAs; two exploring the effect of task in writing and simulation modalities separately, and three exploring the effect of modality in process, outcome and control task types, separately.

The ANCOVA for writing indicated a significant effect of task type ($F(1, 32)= 3.332$, $p= .048$, $\eta_p^2= .172$). Least significant difference pairwise comparisons demonstrated no significant difference in self-regulation at the eight-week follow-up between writing outcome and writing process groups ($p= .896$, means= 116.75 versus 117.23). There was, however, significantly higher self-regulation in the writing outcome group in comparison to the writing control group ($p= .038$, means= 116.75 versus 108.59), and in the writing process group than in the writing control group ($p= .026$, means= 117.23 versus 108.59).

The ANCOVA for simulation also indicated a significant effect of task type on self-regulation ($F(2, 49)= 3.318$, $p= .045$, $\eta_p^2= .119$). Pairwise comparisons indicated no significant difference between simulation outcome and simulation process ($p= .093$, means= 115.54 versus 108.99) or simulation outcome and simulation control ($p= .417$, means= 115.54 versus 118.78) tasks. There was however significantly higher self-regulation in the simulation control group than in the simulation process group ($p= .016$, means= 118.78 versus 108.99).

The ANCOVA exploring differences in self-regulation as a function of modality in the outcome task group indicated no significant main effect of modality ($F(1, 28)= .338$, $p= .566$, $\eta_p^2= .012$). There was, however, a significant effect of modality in the process task group ($F(1, 28)= 5.660$, $p= .024$, $\eta_p^2= .168$), in that there was significantly higher self-regulation at the eight-week follow-up in the writing process condition than in the simulation process condition (means= 119.37 versus 108.19). There was also a significant effect of modality in the control task group ($F(1, 24)= 4.291$, $p= .049$, $\eta_p^2= .152$), in that there was significantly higher self-regulation at the eight-week follow-up in the simulation control group than in the writing control group (means 119.57 versus 111.81).

Given that there were no significant differences between groups in any well-being outcomes, mediation analyses were not performed.

See Appendix A.7 for SPSS outputs from main analyses.

4.5 Discussion

The aim of the current study was two-fold. First, it aimed to investigate whether there would be differences in physical and psychological well-being, self-regulation, emotion-regulation and self-efficacy as functions of modality (writing or simulation) and task type (outcome, process or control). Second, it aimed to explore whether any changes in well-being would be mediated by changes in self-regulation, emotion-regulation, or self-efficacy.

Examination of affect immediately following writing or simulation indicated that PA was significantly higher in both process and outcome conditions, relative to controls. Symptoms of depression, anxiety and stress did not differ as a function of either modality (writing versus mental simulation) or task (process versus outcome versus control) and did not change over the follow-up period. This pattern of non-significant findings was true for physical symptoms, too. There were also no differences in self-efficacy or emotion-regulation between groups, and no significant changes over time.

There was a general effect on self-regulation in the current study, which did not emerge until the eight-week follow-up. First, there was significantly higher self-regulation in both BPFS-W conditions relative to writing controls, but no significant difference between process and outcome writing tasks. Second, there were no significant differences between process and outcome simulation conditions, or between simulation outcome and control conditions. There was, however, significantly lower self-regulation in the process simulation condition, relative to simulation controls. Finally, process simulation participants reported significantly lower self-regulation relative to process writing participants, but there was no significant difference between simulation and writing outcome conditions.

The immediate effects of the BPFS tasks on PA were consistent with those reported by previous researchers (e.g. Hanssen et al., 2013; King, 2001; Sheldon & Lyubomirsky, 2006). Participants in all BPFS groups reported significantly greater PA immediately post-manipulation than controls, regardless of whether they simulated or wrote, or whether they focussed on the process towards their BPFS or the outcome of realising it. This finding suggests that BPFS participants engaged with the activities.

Although BPFS-W and simulation were found to bolster PA immediately post-intervention, it appears that this effect was transient given that there were no significant effects of BPFS-W or simulation on any physical and psychological well-being outcomes at the follow-ups. It is unclear why these null findings occurred, but they are not anomalous. BPFS-W has been found to have well-being benefits such as reducing symptoms of physical illness (King, 2001; Yogo & Fujihara, 2008), bolstering optimistic thoughts (Boselie, Vancleef, Smeets & Peters, 2014; Peters, Flink, Boersma & Linton, 2010), and improving life-satisfaction (Peters et al., 2013). However, previous literature has demonstrated inconsistency. For example, Austenfeld et al. (2006) found no difference between BPFS and control participants in self-reported physical symptoms or medical care utilisation at a follow-up three months post-writing. BPFS-W has also been found at times to have no effect on psychological well-being variables such as depressive symptoms (e.g. Austenfeld et al., 2006), life-satisfaction

(Liau, Neihart, Tee Teo & Lo, 2016) and stress (Troop, Chilcot, Hutchings & Varnaite, 2013) relative to control activities. It is possible that the null findings in the current study may have arisen as products of procedural factors. For example, the original BPFS-W procedure developed by King (2001) involves four 20-minute writing sessions, completed across four consecutive days. This procedure has commonly been replicated (or partially replicated using other numbers of multiple sessions over consecutive days or weeks) in studies conducted to explore the sustained effects of BPFS-W on well-being (e.g. Boehm, Lyubomirsky & Sheldon, 2011; Harrist et al., 2007; Murn, 2014; Shapira & Mongrain, 2011). However, in the current study, a single 20-minute writing session was administered. It is therefore possible that participants did not receive a sufficient 'dose' of BPFS-W for health benefits to be yielded. The current study is not the first to use a procedure markedly different to the original version, and some studies which have used variations on the original procedure have found well-being benefits. For example, Peters et al. (2013) used a single, 15-minute writing session, but found significantly greater life-satisfaction and optimistic explanatory-style in BPFS participants relative to controls, one week post-writing. Perhaps a single session is sufficient to bolster well-being more generally, but it is not enough to ameliorate symptoms of pathology, such as the physical symptoms and depression, anxiety and stress measured in the current study[11]. Peters et al. (2013) also used more specific writing instructions than those used in the current study; participants wrote about personal, professional and relational BPFSs, whereas in the current study the instructions were broad and open. Perhaps, then, a single writing session is sufficient only when specific instructions are used. The difficulty is that BPFS-W studies differ so greatly in procedural factors including the number, spacing and length of writing sessions, as well as the writing instructions used, the outcomes measured and the timing of follow-ups, that it is not possible to identify the procedural variations which are most effective. This makes comparisons of findings across studies difficult.

[11]The decision to measure anxiety, depression and stress rather than more general psychological well-being (which was measured by King, 2001; see Chapter Two, section 2.2.3.1) was made based on previous findings that mental simulation reduces anxiety, and that this reduced anxiety is possibly implicated in the mechanisms through which simulation benefits goal-directed action (Armitage & Reidy, 2008; 2012; Pham & Taylor, 1997; 1999).

Further research should be conducted to explore the effects of procedural variations on study outcomes, so that intervention effects can be optimised.

It is interesting that whilst physical and psychological well-being benefits were not yielded from BPFS-W in the current study self-regulation gains were found, regardless of whether participants wrote about the process towards or outcome of their BPFS. This finding suggests that BPFS-W may be a useful self-regulatory activity, and thus provides support for King's (2001; 2002) suggestion that the well-being effects sometimes found following BPFS-W occur because of increased self-regulation. However, it is unknown whether the gains in self-regulation yielded from a single session are generalisable to other procedures than the one used in the current study, e.g. that of King (2001), and therefore whether self-regulation and well-being benefits can both be fostered within the same procedural parameters. To explain, it is possible that self-regulation gains occur only when the specific procedure used in Study One is employed; a procedure which did not yield well-being benefits. It would be beneficial to closely replicate a BPFS-W procedure which has been found to yield well-being gains, such as the one used by King (2001), to examine its effects on self-regulation. If increases in both self-regulation and well-being occur following King's (2001) procedure of writing for 20 minutes a day over four consecutive days, then mediation analyses should be performed to determine whether self-regulation is implicated in the mechanisms of the BPFS-W intervention.

The finding that self-regulation was lower following process simulation relative to the control simulation is unexpected, given that process simulation has consistently been found to be beneficial for self-regulatory processes such as effective planning (e.g. Pham & Taylor, 1999) as well as for goal performance (e.g. Taylor & Pham, 1999). It is also surprising that the BPFS simulations did not benefit anxiety or emotion-regulation. Anxiety has been found to decrease following process (and to a lesser extent, outcome) simulation in multiple studies (e.g. Pham & Taylor, 1997, as cited by Taylor & Pham, 1996; Pham & Taylor, 1999), and this has been suggested to be indicative of gains in emotion-regulation (e.g. Armitage & Reidy, 2012; Pham & Taylor, 1997). It is possible that mental simulation of a BPFS is not effective because it is not truly mental

simulation. Typically, future-oriented mental simulation involves generation of imagery of a short-term, specific event. For example, Pham and Taylor's (1999) participants simulated the outcome of doing well in an examination, or the process of reaching that outcome, for five to seven days prior to the examination. Similarly, Armitage and Reidy (2012) asked participants to simulate the process or outcome of attending a dental consultation whilst they were waiting at the practice for their appointment. Participants in the current study, however, generated self-projections which were far further in the future than an immediately imminent event or an event in five to seven days; the mean BPFS-proximity for the outcome and process simulation conditions was 12.23 and 8.02 years, respectively. Imagery becomes increasingly abstract and decontextualized as the events it is centred upon increase in temporal distance (Gilbert & Wilson, 2007; Liberman & Trope, 2008; Szpunar, 2010; Trope & Liberman, 2010). Furthermore, an examination and a dental consultation are specific, concrete events. A BPFS on the other hand is broad; the bullet lists generated by participants in the current study demonstrated that they simulated multiple spheres of life, including their future career, family, physical appearance and health. Perhaps simulation of a BPFS is too broad and too distal to harness the features of mental simulation which are thought to facilitate goal-directed action, such as likeness to reality (Taylor et al., 1998; see Chapter Two, Section 2.3.2). Investigation of the effects of goal-proximity and specificity on efficacy of mental simulation tasks would be a useful avenue for further research. This would further knowledge of the flexibility of mental simulation in terms of its application for different types of goals. Given that BPFS-W was found to yield gains in self-regulation whereas BPFS simulation was not, the findings of the current study suggest that writing and simulation are dissimilar in terms of the procedural parameters required to harvest optimal effects.

4.6 Directions for further research

The positive impact of BPFS-W on self-regulation found in the current study provides support for King's (2001) suggestion that BPFS-W benefits well-being through increasing self-regulation. However, given that process simulation appeared to be detrimental to self-regulation, it appears that BPFS-W and simulation tasks require different methodological boundaries to be optimally

effective and are not comparable in terms of their mechanisms and effects. For this reason, the remainder of the thesis is focussed on BPFS-W alone.

Although the current study demonstrated a positive impact of BPFS-W on self-regulation, it yielded no effect of the intervention on physical and psychological well-being. This may have been a product of the intervention procedure used in the current study, in that a single session may have been too low a dose. The effects of BPFS-W on self-regulation are promising, however it is unknown whether self-regulation and well-being benefits can occur within the same procedural parameters, or whether they require different conditions. This was investigated in the second study of this research programme, using a partial replication of King's (2001) original BPFS-W intervention protocol.

Chapter Five

Study Two. Writing about a best possible future self: A partial replication of King (2001)

5.1 Introduction

King's (2001; 2002) self-regulation theory of how writing about a best possible future self (BPFS) may positively affect well-being, outlined in Chapter Two, has been cited as a possible mechanism of effect by multiple authors (e.g. Frein & Ponsler, 2014; Sheldon & Lyubomirsky, 2006) but was yet untested. Therefore, the effects of BPFS writing (BPFS-W) on self-regulation were explored for the first time in Study One. Results demonstrated that participants who wrote about a BPFS in a single, 20-minute session reported gains in self-regulation eight weeks post-writing, relative to those who wrote about the details of their previous day. This finding suggests that BPFS-W may be a useful self-regulatory activity. However, the null effects on sustained physical and psychological well-being prevented further exploration of the role of self-regulation as a mechanism of change.

The null findings were not altogether unexpected; although BPFS-W has been found to be beneficial for well-being (e.g. Harrist, Carlozzi, McGovern & Harrist, 2007; Shapira & Mongrain, 2010), findings are inconsistent (e.g. Austenfeld, Paolo & Stanton, 2006). It is unclear why BPFS-W was not found to impact well-being outcomes in Study One, but it was suggested that this may have been due to differences between the intervention procedure used in Study One and the original BPFS-W protocol developed by King (2001). For example, a single writing session was used in Study One, whereas four sessions were used in the original protocol, and typically studies conducted to investigate sustained effects

of BPFS-W on well-being involve multiple sessions (Boehm, Lyubomirsky & Sheldon, 2011; Murn, 2014; Shapira & Mongrain, 2011). Although some studies have yielded sustained well-being benefits following a single session (e.g. Peters, Meevissen & Hanssen, 2013), the procedures of these studies differ from Study One in other ways such as a more structured writing task. Therefore, perhaps a single session can be sufficient to induce beneficial change, but only when other procedural requirements are satisfied. It is generally difficult to compare findings across BPFS intervention studies because their procedures differ in multiple ways, including the number and spacing of writing sessions, the timing of follow-ups, the writing instructions used, and the outcomes measured. Therefore, it is difficult to identify what aspects of the procedure used in Study One may have rendered the BPFS intervention less effective for well-being.

It was suggested in Chapter Four that further research is needed to determine whether the self-regulation gains found following a single session of writing in Study One are generalisable to a BPFS-W intervention procedure which has already been found to yield sustained well-being benefits, such as the procedure used by King (2001). This would provide an indication of whether self-regulation and well-being benefits of BPFS-W can be fostered within the same intervention procedure, or whether they require different conditions.

5.2 Aims

The current study was a partial replication of King's (2001) investigation, with the addition of a measure of self-regulation. The first aim was to investigate whether the self-regulation gains found following a single session of writing in Study One are maintained when King's (2001) procedure of four 20-minute sessions over four consecutive days is used. The second aim was to explore whether any improvements in physical and psychological well-being following BPFS-W are mediated by gains in self-regulation.

5.3 Method

5.3.1 Design

The present investigation employed a mixed-measures experimental design. The between-group independent variable (IV) was 'task' and contained two levels: BPFS (experimental task) and plans for the day (control task). The repeated-measures IV was outcome assessment time-point. The procedure consisted of a pre-manipulation (baseline) assessment, and two follow-ups which occurred four and eight weeks following the fourth writing session. The dependent variables (DVs) measured at baseline and follow-up were physical symptoms, subjective psychological well-being, self-regulation, and future-orientation. Positive and negative affect (PA and NA) were measured immediately before and after each of the four writing tasks.

It should be noted that only the original outcome-focussed writing instructions used by King (2001) were administered in the current study, given that there were no differences in findings between the process and outcome writing tasks used in Study One.

5.3.2 Power analysis

An *a priori* power analysis was conducted using the G*Power software (Faul, Erdfelder, Lang & Buchner, 2007). It was estimated that for the main effect of group (BPFS versus control) — based on a medium effect size (f) of 0.25, one covariate (baseline scores) and two independent groups— a sample of 128 participants was required to obtain the desired power level of 0.8 (actual power= 0.80).

The above power calculation provides the desired sample size for the main between-group effect. However, the ANCOVA model used in the current study included a within-group variable (time), with two levels (four- and eight-week follow-up). Therefore, the overall model is likely to have more power and thus a smaller sample than 128 is likely to be necessary for a power level of 0.8 to be achieved.

5.3.3 Participants

Participants were recruited via an advertisement placed on Sheffield Hallam University Psychology Department's online research participation site as well as on social media (see Appendix A.8). Verbal recruitment was used in the University library. The study was open to all participants who met the eligibility criterion of having English as a first language. First year Undergraduate Psychology students were offered course credit for their time. Other participants were offered a £5 voucher. 59 participants were recruited into the investigation. 50 (84.7%) were female and 30 (50.8%) were current University students. The mean age of the sample was 28.37 (SD= 10.86) years.

5.3.4 Materials

Some of the measures used in Study One were used again in this study; the Short Self-Regulation Questionnaire (SSRQ; Carey, Neal & Collins, 2004), the 13-item Physical Symptoms Inventory (PSI; Kessler, Spector, Chang & Parr, 2008; Spector, 2018), and the state Positive and Negative Affectivity Scales (PANAS; Watson, Clark & Tellegen, 1988). The PANAS was administered as a manipulation-check due to the consistent increase in PA found following BPFS-W (e.g. Hanssen, Peters, Vlaeyen, Meevissen & Vancleef, 2013; Frein & Ponsler, 2014). See Chapter Four, Section 4.3.4 for details of these measures. In contrast to Study One, psychological well-being was assessed using the same instruments used by King (2001); the Satisfaction with Life Scale (Diener, Emmons, Larsen & Griffin, 1985) and the Life Orientation Test (Scheier & Carver, 1985).

<u>Satisfaction with Life Scale (SWLS; Diener et al., 1985)</u>

The SWLS was used to measure life-satisfaction. This measure consists of five items, such as 'In most ways my life is close to ideal'. Respondents are required to indicate to what extent they agree with each item, using a seven-point Likert scale (1 = 'strongly disagree', and 7 = 'strongly agree'). Possible scores range from 5 to 35, with high scores reflecting higher levels of reported life-satisfaction. The SWLS has been found by Diener et al. (1985) to have a high level of test-retest reliability over a two-month period (r= .82, a= .87). SWLS scores have been found to correlate with interviewers' estimates of participants'

life-satisfaction (r= .43; Diener et al., 1985), demonstrating an acceptable level of criterion validity, and item-total correlations have shown a high level of internal consistency (r= .61 to .81).

Life Orientation Test (LOT; Scheier & Carver, 1985)

The LOT is a self-report measure designed to assess individual differences in subjective optimism, in terms of generalised outcome expectancies. It consists of 12 items. Four of these are fillers (e.g. 'I enjoy my friends a lot'); intended to blur the instrument's purpose. The eight items which are included in an individual's score constitute four negatively-keyed items for example 'things never work out the way I want them to', and four positively-keyed items for example 'I always look on the bright side of things'. Respondents indicate the extent to which they believe each statement to be true using a five-point Likert scale (0= 'strongly disagree', 4= 'strongly agree'). Possible scores range from 0 to 32. Negatively-keyed items are reversed scored, therefore high scores represent high levels of optimism.

The LOT was revised by Scheier, Carver and Bridges (1994; LOT-R) in response to criticism of the scale. Smith, Pope, Rhodewalt and Poulton (1989) posited that effects attributable to optimism may exist as products of shared variance between trait anxiety and optimism; a suggestion which undermines not only the LOT but also optimism as a construct (Scheier et al., 1994). However, the original LOT was used in the present investigation because it was used by King (2001), and the current study was intended to mirror King's (2001) procedure as closely as possible. Both versions possess comparable and acceptable internal consistency (r = .76 for LOT and r = .78 for LOT-R), and test-retest reliability (r= .79 over an interval of four weeks for LOT; r= .68 over an interval of four months and r= .79 over an interval of 28 months for LOT-R). Scores on the LOT-R correlate highly with scores on the original LOT (r= .95; Scheier et al., 1994), suggesting that differences in optimism scores dependent on the version used would be negligible.

In King's (2001) study the SWLS and the LOT correlated significantly (r= .56, p< .001), and for this reason, King (2001) averaged the standard scores for the two

scales and used them as a composite measure of subjective psychological well-being. This procedure will be followed in the current investigation, to facilitate comparison of the results of the current study and those reported by King (2001).

Future Orientation Scale (FOS; Crespo, Jose, Kielpikowski & Pryor, 2013).
The FOS was used to measure future-orientation. A measure of future-orientation was included as King (2001; 2002) suggested that the BPFS-W task encourages individuals to focus on their higher-order, long-term future goals. This in turn may make them more able to regulate their behaviour towards realising those goals. It was thought that a measure of future-orientation might capture any change in participants' temporal horizons from focussing on lower-order events and goals to more distal goals. The FOS is a self-report measure containing four items, such as 'I am serious about working hard now so that I have a good future'. Respondents are required to indicate the extent to which they agree with each statement using a five-point Likert scale (1 = 'strongly disagree', 5 = 'strongly agree'). Possible scores range from 4 to 20, and a high score is indicative of high future-orientation. Crespo et al. (2013) report that the FOS has a high level of internal consistency (α= .74 to .81).

All scale instructions (apart from those for the PANAS) were modified to request responses in relation to the last month to prevent overlap across measurement time-points (baseline, four weeks and eight weeks).

5.3.5 Procedure

Prospective participants were invited to take part in an online study designed to investigate the effects of writing about life activities on thinking styles and health. The procedure for this study is summarised in Figure 5.1.

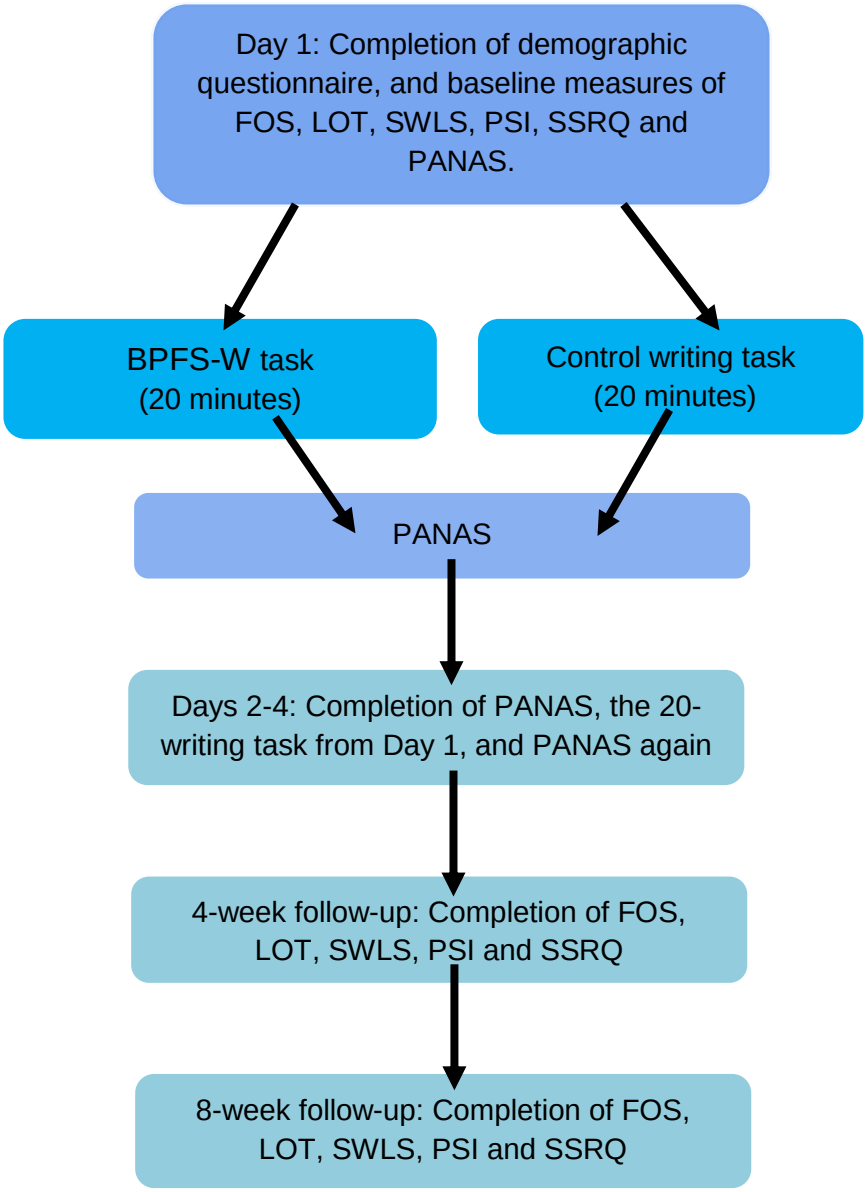


Figure 5.1: A flow diagram of the study procedure

Individuals wishing to take part were given a link to the first set of study materials (using Qualtrics). The first page of this online pack contained an information sheet (Appendix A.9), outlining what participation would involve, and advising participants about the nature of their voluntary participation including their right to withdraw. Participants were informed that they should only continue if English was their first language. Eligible participants were then presented with a consent form and typed an 'X' in a box to provide informed consent (Appendix A.10). Participants were unable to progress if they did not type the X and were informed that if they did not want to take part they should close the browser, and that no information about them would be recorded. Once participants had

provided informed consent, they filled in a demographic information questionnaire which asked about their age, gender, nationality, and whether they were a current student.

Participants were then provided with the instruments detailed above to measure their baseline positive and negative affect (PA and NA; PANAS), future-orientation (FOS), general subjective well-being (LOT and SWLS), physical symptoms (PSI), and self-regulation (SSRQ). They then typed for 20 minutes about their BPFS or plans for the day. An automatic timer was implemented so that participants could not progress from the task screen before 20 minutes had passed.

The BPFS-W instructions were taken from King (2001). Participants were provided with the following instructions:
Think about your life in the future. 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 of your life dreams. Now, write about what you imagined. Please write for 20 minutes.

The control instructions were adapted from Petrie, Booth, Pennebaker, Davison and Thomas (1995) and King (2001). Control participants were provided with the following instructions:
Write about your plans for the rest of the day in as much detail as possible. It is important that you write about your plans in a purely objective, descriptive way. This means that we would like you to write only about the facts, whilst avoiding writing about feelings and emotions as much as you can. Please write for 20 minutes.

Immediately upon completion of the 20-minute typing task, participants completed the PANAS again, then were asked to enter their e-mail address so that the researcher could forward the remaining links to them. It was made clear that participants' e-mail addresses would immediately and automatically be sent to a separate file in the Qualtrics programme, so it would not be possible to

connect their addresses with their data. Participants were then briefed about what would be expected of them at the next stage of the study.
The second, third and fourth links were sent to participants over the following three consecutive days. On the first page of each online pack, participants were reminded of the voluntary nature of their participation, then were given the PANAS and the same 20-minute typing task that they completed on the first day. They then completed the PANAS again and were briefed about the next stage of their participation. Four and eight weeks following the fourth writing day, participants were sent a link containing the same questionnaires that were completed at baseline, other than the PANAS. At the end of the eight-week follow-up, they were given a debrief sheet which was used to thank them for their time, and to provide them with more information about the aim of the investigation (see Appendix A.11).

5.3.6 Ethical considerations

The present investigation was designed and conducted in accordance with the British Psychological Society (BPS) guidelines for conducting research involving human participants (Code of Ethics and Conduct; BPS, 2009). Details of the study were submitted to the faculty research ethics committee at Sheffield Hallam University, who granted permission for it to be carried out. Please see Appendix A.12 for ethics proforma, data management plan and approval letter.

5.4 Results

5.4.1 Exclusion and Attrition

Only participants who completed the writing task four times across four consecutive days were included, to mirror King's (2001) procedure as closely as possible. To explore whether there were any differences in outcomes between individuals who completed four sessions across four consecutive days (completers) and those who did not (non-completers), two MANOVAs were performed for the BPFS and control groups separately. In these analyses, the IV was completion status (completers versus non-completers). The DVs were baseline scores for PA, NA, future-orientation, physical symptoms, self-regulation, life-satisfaction and optimism. Descriptive statistics for baseline scores in completers and non-completers are presented in Table 5.1.

Table 5.1: Means and SDs of baseline scores on outcome variables in completers and non-completers

	BPFS		Control	
	Completers	Noncompleters	Completers	Non-completers
Physical symptoms	23.39 (4.82)	25.91 (8.20)	25.73 (7.35)	26.09 (9.30)
PA	23.28 (6.06)	23.55 (9.28)	24.16 (9.35)	28.18 (10.59)
NA	14.28 (4.70)	15.36 (8.98)	15.05 (5.33)	15.55 (4.18)
Optimism	16.89 (2.89)	17.00 (7.68)	17.74 (4.25)	17.91 (7.66)
Life-satisfaction	19.50 (4.87)	23.45 (6.41)	21.32 (5.68)	22.27 (6.00)
Self-regulation	111.33 (12.69)	107.91 (19.19)	107.95 (13.11)	113.18 (15.01)
Future-orientation	17.06 (2.96)	16.81 (4.33)	16.21 (3.60)	15.00 (4.12)

Box's M was non-significant at the $p < .001$ level[12] for both experimental and control participants ($p = .029$ and .002, respectively), therefore data did not violate the assumption of homogeneity of variance/ covariance matrices and were suitable for MANOVA. The MANOVAs indicated no significant multivate difference in baseline scores between completers and non-completers in both the BPFS group (Pillai's trace= .288, $F(7, 21) = 1.211$, $p = .340$, $\eta p^2 = .288$) and the control group (Pillai's trace= .146, $F(7, 22) = .535$, $p = .799$, $\eta p^2 = .146$).

Following exclusion of non-completers, 37 participants remained (mean age = 30.24 years, SD= 12.05). Of these, 31 (83.8%) were female, and 15 (40.5%) were current students. All considered their first language to be English. Two participants were American, all others were British. There was some attrition over the follow-up period. Four participants did not complete both the four- and eight-week follow-ups, and a further two did not complete the eight-week follow-up only. 31 completed both follow-ups. There was inconsistency for some

[12] Tabachnick and Fidell (2013) suggest that Box's M is overly conservative and therefore a criterion for significance of p<. 001 should be used in the interpretation of results from it.

participants in completing the follow-up measures at the specified time points (four and eight weeks). Analysis confirmed that there was no significant between-group difference in days between the fourth writing session and completion of the four-week follow-up ($t(31)= -1.588$, $p= .123$, BPFS mean =29.35 days[13], SD= .70, control mean= 29.88 days, SD= 1.15). There was also no significant between-group difference in days between the fourth writing session and completion of the eight-week follow-up ($t(29)= .041$, $p= .968$, BPFS mean= 57.88, SD= 1.72, control mean= 57.86, SD= 1.70). Therefore, all participants' data were included in follow-up analyses

The flow of participants through the study is shown in Figure 5.2.

[13] The mean number of days includes the day of the fourth writing session and the day each follow-up was completed.

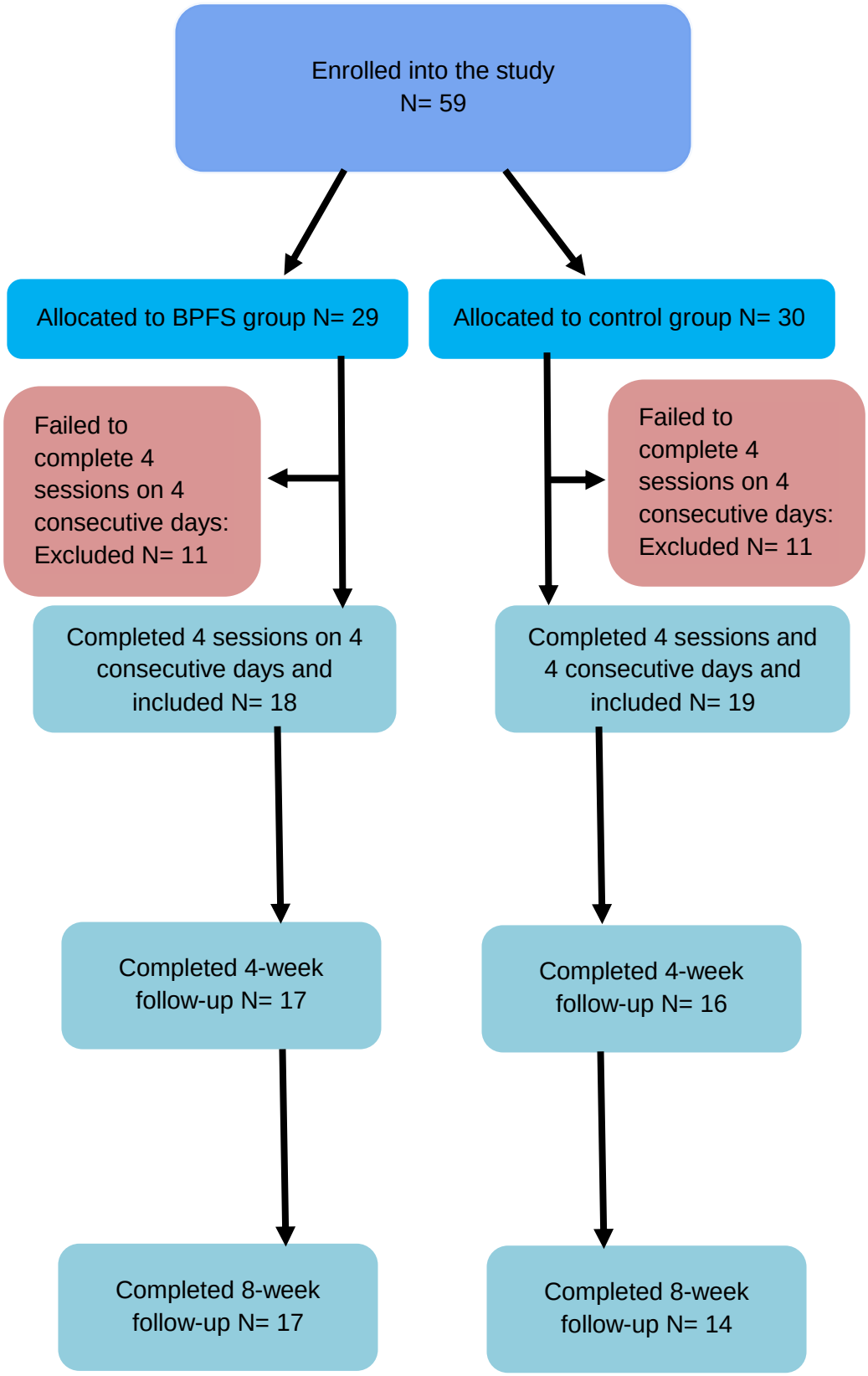


Figure 5.2: Diagram adapted from Consolidated Standards of Reporting Trials (CONSORT, 2010) showing participant flow and attrition (Schulz, Altman & Moher, 2010)

5.4.2 Data preparation

5.4.2.1 Reliability analysis

Cronbach's alpha was calculated for all scales at baseline. The internal reliability of the SWLS and the PSI was acceptable (αs= .67 and .74, respectively). The internal reliability of all other scales was high (all αs≥ .82).

5.4.2.2 Missing data analysis

Data were entered in SPSS and screened for missing values, of which there were none. Data were then examined to determine whether they were suitable for analyses using parametric statistics.

5.4.2.3 Testing assumptions of parametric analyses

Z scores were generated to screen the data for outliers; any z score greater than +/- 3 standard deviations (SDs) from the mean was considered an outlier, based on recommendations by Stevens (2002). Outliers were identified for NA at various time-points; Day 1 pre-writing, Day 3 pre- and post-writing, and Day 4 pre- and post-writing. These were addressed using negative reciprocal transformation, which successfully corrected them. Negative reciprocal transformation was then applied to the NA scores at each time-point to enable comparison of means using inferential statistics. Scattergraphs demonstrated no evidence of curvilinear relationships between any covariate and DV, therefore the assumption of linearity was met. Normality of distributions of DVs were assessed according to Kim's (2013) suggestion of a distribution being significantly skewed if the Z score of the skewness value (skewness value/ standard error of skewness) is greater than 1.96 (equivalent alpha of .05) for small samples (n < 50). All DVs were normally-distributed, other than future-orientation at the one- and two-weeks follow-ups in the BPFS group, and NA immediately following the third writing session in the control group (following transformation). These skewed variables are unlikely to cause bias, because ANOVA and ANCOVA have been found to be robust against violation of the assumption of normality (Blanca, Alarcón, Arnau, Bono & Bendayan, 2017; Levy, 1980; Schmider, Ziegler, Danav, Bever & Bühner, 2010). SDs for each DV across groups and time-points were consulted and given that the largest SD value for each DV was not greater than twice the value of the smallest, the

assumption of homogeneity of variance was satisfied, suggesting that the data were suitable for analysis using parametric statistics. To ascertain whether ANCOVA was an appropriate method of analysis, the homogeneity of regression slopes assumption was tested by generating a model that included the interactions between the covariate (baseline score) and IVs (time-point and group) for each ANCOVA. No significant interaction between any covariate and IV was indicated for any DV (all $Fs \leq 1.908$, all $ps \geq .179$). Following this preparation and cleaning of the data, it was decided that they were suitable for analysis using parametric tests and ANCOVA.

5.4.3 Checking adherence to task instructions

Adherence to intervention instructions was assessed by reading the content of the essays generated by participants in the BPFS and control tasks who had completed four sessions across four consecutive days and were included in analyses. Essays were examined to ascertain the extent to which the content was in line with the instructions given. Each participant's adherence was graded as complete adherence, partial adherence or no adherence. Minimum, maximum and mean word counts for each group on each of the four consecutive writing days were also examined. Results from the adherence assessment are presented in Table 5.2.

Table 5.2: Adherence to task instructions and essay word counts across groups

Adherence and word count for each day across groups								
	BPFS (N=18)				Control (N=19)			
	Day 1	Day 2	Day 3	Day 4	Day 1	Day 2	Day 3	Day 4
Complete adherence	18	18	18	17	18	19	19	18
Partial adherence	0	0	0	1	1	0	0	1
No adherence	0	0	0	0	0	0	0	0
Minimum words	102	68	66	52	34	66	31	62
Maximum words	851	761	901	921	607	407	642	436
Mean words	311.62	242.06	259.67	263.28	245.21	201.95	211.11	188.16

Adherence to task instructions was deemed to be high across the BPFS group and the control group. No participants failed to adhere completely.

BPFS

17 of the 18 BPFS participants adhered to task instructions on each of the four writing days. King (2001) listed common themes written about by participants; ‘‘job success, self-improvement, marriage and family, travel, home ownership, and so forth’’ (King, 2001, p. 802). These topics were also common themes in the essays produced by BPFS participants in the current study. A single participant was deemed to have only partially adhered on the fourth writing day (this individual had adhered completely on the other three days). Although the participant had written about their BPFS on the fourth day, they had included considerable detail about their current life and concerns without linking this detail into their future life goals.

Control

18 of the 19 control participants adhered to task instructions and wrote only about their plans for the rest of the day on each of the four writing days. A single participant was deemed to have partially adhered on days one and four only; they had completed the writing task late in the evening, so wrote about some plans for the following day in addition to their plans for the current day. Common topics in control participants' essays included doing housework, socialising, exercising and preparing meals.

The minimum, maximum and mean word counts demonstrate great variability across participants in the amount written on each writing day in both conditions. It should be noted that variability in the amount written does not mean variability in the time spent engaged with the writing task. The variability in number of words written by participants in the current study appears to be comparable to that of King's (2001) participants. Although King (2010) did not report the minimum, maximum and mean word counts, two example essays from BPFS participants were presented. These essays were 89 and 301 words long.

5.4.4 Checking for between-group differences at baseline

Descriptive statistics for demographic and outcome variables at baseline are presented in Table 5.3. As was the case in Chapter Four, all descriptive statistics presented in the tables in this chapter are unadjusted; they represent the average scores prior to transformation for outlier correction. Furthermore, although the standardised scores for the LOT and SWLS were averaged to provide a composite measure of psychological well-being, the unstandardised means and SDs for the LOT and SWL scales are presented in tables alongside the composite for ease of comparison and transparency.

Table 5.3: Means and SDs for age and outcome variables at baseline

	BPFS	Control
Age	27.50 (9.04)	32.84 (14.08)
Future-orientation	17.06 (2.96)	16.21 (3.60)
PA	23.28 (6.06)	24.16 (9.35)
NA	14.28 (4.70)	15.05 (5.33)
Physical symptoms	23.39 (4.82)	25.73 (7.35)
Self-regulation	111.33 (12.69)	107.95 (13.11)
Optimism	16.89 (2.89)	17.74 (4.25)
Life-satisfaction	19.50 (4.87)	21.32 (5.68)
Psychological well-being composite	-.15 (.73)	.14 (.99)

SDs are presented in parentheses in this and all subsequent tables.

Baseline differences in DVs

To explore whether there were any pre-manipulation between-group differences in PA, NA, psychological well-being, physical symptoms, future-orientation, and self-regulation, a one-way independent-measures MANOVA was performed. Box's M was non-significant at the $p < .001$ level ($p = .024$), therefore data did not violate the assumption of homogeneity of variance/ covariance matrices and were suitable for MANOVA. In this analysis, the IV was group (experimental versus control). The DVs were the baseline scores for PA, NA (following negative reciprocal transformation), physical symptoms, behavioural self-regulation, and psychological well-being (scores on the LOT and SWLS included separately, rather than the composite measure). All participants who completed the four consecutive writing days were included in this analysis, regardless of whether they completed the four- and eight-week follow-ups.

The MANOVA indicated no significant multivariate difference in baseline scores as a function of group (Pillai's trace= .095, $F(7, 29) = .435$, $p = .872$, $\eta p^2 = .095$)[14].

[14] Participants were not randomly allocated to groups due to an oversight. The null results of the MANOVA suggest this is unlikely to have caused selection bias.

5.4.5 Immediate effects of writing on PA and NA

To explore the immediate effects of BPFS-W on PA and NA, two 2*(2)*(4) factorial ANOVAs were performed. The between-group IV was group (BPFS versus control). The two within-group IVs were the writing day (one, two, three and four), and pre- versus post-writing. Means and SDs are presented in Table 5.4.

Table 5.4: Means and SDs of affect scores as functions of day and pre- or post-writing

	BPFS				**Control**			
	Day 1	Day 2	Day 3	Day 4	Day 1	Day 2	Day 3	Day 4
PA, pre-writing	23.28 (6.06)	21.44 (6.20)	22.00 (6.59)	19.11 (7.19)	24.16 (9.35)	24.32 (10.79)	21.74 (9.18)	22.11 (11.75)
PA, post-writing	25.72 (10.21)	23.50 (9.15)	24.39 (8.51)	24.44 (9.84)	22.84 (10.27)	23.11 (11.50)	20.53 (9.23)	21.37 (10.37)
NA, pre-writing	14.28 (4.70)	12.61 (3.11)	13.28 (4.14)	13.33 (3.24)	15.05 (5.33)	12.89 (3.28)	15.26 (6.61)	15.58 (2.87)
NA, post-writing	14.78 (5.99)	12.78 (2.96)	12.72 (2.74)	13.11 (2.87)	13.63 (3.45)	12.05 (2.63)	15.00 (7.52)	15.11 (7.39)

The ANOVA for PA indicated no significant main effect of day ($F(3, 105)= 1.855$, $p= .142$, $\eta p2= .050$), pre- or post-writing ($F1, 35)= 1.378$, $p= .248$, $\eta p^2= .038$), or group ($F(1, 35)= .032$, $p= .859$, $\eta p^2= .001$). The following interactions were also found to be non-significant: day * group, day * pre- or post- writing[15], and day * pre- or post-writing * group (all $Fs\leq 2.039$, all $ps\geq .113$, all $\eta p^2\leq.055$). There was, however, a significant pre- or post-writing * group interaction ($F1, 35)= 6.399$, $p= .016$, $\eta p^2= .155$).

To explore the significant pre- or post-writing * group interaction further, post-hoc analyses were conducted. First, two paired samples t-tests were conducted to explore differences in PA between pre- and post-writing time-points, in the BPFS and control groups separately. Second, two independent samples t-tests were conducted to explore differences between the BPFS and control groups, at each time point separately. In the BPFS group, there was no significant difference in PA between pre- (mean= 21.46, SD= 5.15) and post- (24.51, SD= 8.41) writing ($t(17)= -1.891$, $p= .076$, $d= .45$). In the control group, there was a significant decrease in PA from pre- (mean= 23.08, SD= 9.38) to post- (mean= 21.96, SD= 9.49) writing ($t(18)= 2.290$, $p= .034$, $d= .53$). There was no significant between-group difference in PA pre-writing ($t(28.240)= -.656$, $p= .517$, $d= .21$)[16] or post-writing ($t(35)= .864$, $p=.393$, $d= .28$).

The ANOVA for NA indicated no significant main effects of group ($F(1, 35)= .319$, $p= .576$, $\eta p^2= .009$) or pre- or post- writing ($F(1, 35)= .806$, $p= .375$, $\eta p^2= .023$). The following interactions were also non-significant: day * group, pre- or post- writing * group, day * pre- or post- writing, and day * pre- or post-writing * group (all $Fs\leq 1.121$, all $ps\geq .344$, all $\eta p^2\leq.031$). However, there was a significant main effect of day ($F(3, 105)= 3.696$, $p= .014$, $\eta p^2= .096$). Least significant difference pairwise comparisons demonstrated significantly lower NA on day two in comparison to days one and four (ps= .001 and .017, respectively). All other comparisons were non-significant (all $ps\geq .071$).

[15] The Greenhouse-Geisser statistic was used here, as Mauchley's test of sphericity for the day * pre- or post- writing repeated-measures interaction was significant (p= .002), therefore sphericity could not be assumed.

[16] Levene's Test for Equality of Variance was violated (p= .004), thus equal variances could not be assumed.

5.4.6 Long-term effects

5.4.6.1 Psychological and Physical Well-being

The effects of group (BPFS versus control) and time-point (four weeks versus eight weeks) on physical symptoms and psychological well-being were explored using separate ANCOVAS. Baseline scores were included as covariates to partial out their influence. Unadjusted means and SDs for outcome measures as a function of group and time-point are presented in Table 5.5.

Table 5.5: Means and SDs of well-being outcomes at follow-ups

	BPFS		Control	
	4 weeks	8 weeks	4 weeks	8 weeks
Physical symptoms	23.94 (4.24)	22.94 (5.87)	25.71 (7.86)	25.00 (7.03)
Optimism	17.24 (2.66)	17.12 (2.85)	17.31 (4.32)	17.50 (4.13)
Life-satisfaction	19.41 (6.28)	21.59 (6.50)	21.31 (5.84)	22.86 (5.83)
Psychological well-being (composite)	-.08 (.67)	-.07 (.76)	.09 (.10)	.09 (.10)

Psychological well-being: There was no significant main effect of time-point ($F(1, 28)= .078$, $p= .782$, $\eta p^2= .003$) or group ($F(1, 28)= .012$, $p= .912$, $\eta p^2< .001$) on psychological well-being, and no significant time-point * group interaction $F(1, 28)= .066$, $p= .799$, $\eta p^2= .002$)[17].

Physical symptoms: The ANCOVA for physical symptoms indicated no significant main effect of time-point ($F(1, 28)= .006$, $p= .939$, $\eta p^2< .001$) or group ($F(1, 28)= .584$, $p= .451$, $\eta p^2= .020$), and no significant time-point * group interaction ($F(1,28)= .065$, $p= .801$, $\eta p^2= .002$).

[17] Separate ANCOVAs for the LOT (optimism) and the SWLS (life-satisfaction) revealed no significant main effects or interactions (LOT: all Fs $\leq .838$, all ps $\geq .373$; SWLS: all Fs ≤ 1.043, all ps $\geq .316$. The correlation between baseline LOT and SWLS scores was comparable to that reported by King (2001) ($r= .54$, $p= .001$).

5.4.6.2 Self-regulation and Future-orientation

The effects of group and time-point (four weeks versus eight weeks) on future-orientation and self-regulation were explored using ANCOVAS. Baseline scores for each were included as covariates. Unadjusted means and SDs for outcome measures as a function of group and time-point are presented in Table 5.6.

Table 5.6: Means and SDs of self-regulation and future-orientation at follow-ups

	BPFS		Control	
	4 weeks	8 weeks	4 weeks	8 weeks
Self-regulation	108.24 (17.44)	109.88 (14.43)	105.57 (16.52)	107.64 (18.70)
Future-orientation	16.18 (3.52)	15.41 (3.66)	15.71 (3.38)	15.37 (3.54)

Future-orientation: There was no significant main effect of time-point ($F(1, 28)= .006$, $p= .941$, $\eta p^2< .001$) or group ($F(1, 28)= .795$, $p= .380$, $\eta p^2= .028$) on future-orientation, and no no significant time-point * group interaction $F(1, 28)= .160$, $p= .692$, $\eta p^2= .006$).

Self-regulation: There was no significant main effect of time-point $F(1, 28)= .373$, $p= .546$, $\eta p^2= .013$) or group $F(1, 28)= .086$, $p= .772$, $\eta p^2= .003$) on self-regulation, and no significant time-point * group interaction $F(1, 28)= .010$, $p= .920$, $\eta p^2< .001$).

Given that there were no significant main effects of group and no group * time interactions for any physical and psychological well-being variables or self-regulation and future-orientation, no mediation analyses were performed. Pleases see Appendix A.13 for SPSS outputs from main analyses.

5.5 Discussion

There were two aims of the current study. The first aim was to closely replicate King's (2001) BPFS-W procedure with the addition of a measure of self-regulation. This was to investigate whether the gains in self-regulation found in Study One following a single BPFS-W session are maintained when a procedure which has been found to benefit well-being is used. The second aim was to investigate whether any changes in physical and psychological well-being following writing are mediated by changes in self-regulation.

Results demonstrated that there was no immediate effect of BPFS-W on PA, although there was a decrease from pre- to post-writing in the control group. There was no significant main effect of group (BPFS versus control) on physical and psychological well-being, and no changes over time. BPFS-W also did not impact future-orientation or self-regulation.

The decrease in PA from pre- to post-writing in the control group is surprising, because the control task was intended to be non-emotive. It is unclear why this effect occurred, but it may have arisen from participants finding writing about plans for the day boring. Troop, Chilcot, Hutchings and Varnaite (2013) stated that they conducted a pilot study of the effects of BPFS-W (Winn & Troop, 2002) and their control group expressed feelings of boredom following writing about a trivial topic. However, PA did not change as a function of writing day in the current study. If the decrease from pre- to post-writing was a product of boredom, then PA would perhaps also be expected to reduce across the four days due to increasing boredom across the days.

The finding that BPFS-W did not increase PA is as surprising as the decrease in PA in the control group; multiple studies have demonstrated an increase in PA immediately post-writing (e.g. Frein & Ponsler, 2014; King, 2001; Peters et al., 2010). There are two possible explanations for this null effect. First, it is possible that the lack of change in PA is attributable to the administration of the writing task online. Although BPFS-W tasks are typically administered in a laboratory or at least in-person (e.g. King, 2001; Ng, 2016; Peters et al., 2010), an online setting was used due to time-constraints. Online administration is

more convenient than face-to-face data collection, because a larger number of potential participants can be reached in a shorter amount of time, and participation is not restricted by individuals' location or availability (Best & Krueger, 2004; Birnbaum, 2004). Nevertheless, there are costs of elimination of a laboratory to experimental control, including the experimenter's capacity to verify that participants interpret instructions as intended and ensure that they adhere to instructions and complete the study without distractions (Sheese, Brown & Graziano, 2004). It is therefore possible that participants did not engage with the intervention sufficiently for PA to be increased. However, given the consistent increase in PA immediately following BPFS-W found across the literature, it is more plausible that the null findings are a result of the analysis being underpowered. The p value is approaching significance, and the Cohen's d value indicates a close to moderate effect size. Indeed, the descriptive statistics are indicative of an increase in PA from pre- to post-writing across all sessions.

The lack of change in PA in the BPFS group and the drop in PA in the control group were not the only unexpected findings in the current study. Surprisingly, given that the procedure was closely based on that of King (2001), there were no significant effects of BPFS-W on any longer-term physical and psychological well-being outcomes. This could be attributable to poor engagement due to the internet-mediated design, as previously discussed. However, it is possible that the inconsistency in findings surrounding physical health is attributable to more salient procedural differences between the current study and that of King (2001).

Two important differences between King's (2001) study and the current investigation are the use of self-report versus healthcare utilisation and the timing of follow-ups. First, in the current study, self-reported physical health was measured at four and eight weeks post-writing. In contrast, King (2001) measured post-writing physical health by examining records of health-centres for visits for illness across five months following the intervention. Perhaps, therefore, there was an effect of BPFS-W in the current study, but this was not detectable until after the eight-week follow-up had taken place. Physical health

effects have been detected sooner than eight weeks post-writing in previous literature; Maddalena, Saxey-Reese and Barnes (2014) found improved self-reported physical well-being one month post-writing. However, Maddalena et al.'s (2014) writing instructions were more structured than those used in the current study, thus it is possible that effects occur sooner than eight weeks following structured, but not open, BPFS-W tasks. Second, King's (2001) finding of a reduction in healthcare utilisation cannot be confidently interpreted as a proportionate reduction in physical symptoms. Some individuals have higher symptom thresholds for service use than others (van Loenen, van den Berg, Faber & Westert, 2015), and frequency of health-centre visits is mediated by multiple variables including neuroticism and loneliness (Ellaway, Wood & Macintyre, 1999; Jerram & Coleman, 1999). It is therefore possible that King's (2001) finding reflects a change in a variable other than physical symptoms. Further research is needed to determine the point at which possible changes in physical health following BPFS-W become measurable and when effects dissipate. It would also be useful for measures of immune function to be used to assess physical health, given that this is a more direct and objective indicator than self-report and records of healthcare utilisation. As discussed in Chapter Two (Section 2.2.1.6), measurement of immune function has provided robust evidence of the beneficial effects of writing about traumatic experiences on physical health.

The differences in findings surrounding physical and psychological well-being between King's (2001) findings and those of the current study may be attributable to procedural differences. However, it is also possible that they have occurred due to differences in participant characteristics. King's (2001) sample had a mean age of 21.04 years, and all participants were students. In the current study, the mean age of participants included in analyses was 30.24 years and fewer than half (40.5%) of them were students. It is possible that BPFS-W is a different experience for young students than it is for individuals in a later life-stage who are not enrolled in a university programme. Individuals who have succeeded in gaining a place at university may be more hopeful and optimistic about their BPFS than those who are not in this position (King, 2001). This could make BPFS-W more useful for them than for individuals whose

future may not seem as hope-filled (King, 2001). Furthermore, writing for a period of 20 minutes is an activity that students are likely more familiar with than those outside of university, therefore it is possible that the act of engaging in the writing activity is more natural to students. Empirical examination of the effects of BPFS-W in students in comparison to other groups would be a useful direction for future research, because this would provide insight into the individuals for whom the intervention is likely to be most beneficial.

Although the lack of change in physical and psychological well-being found in the current study could conceivably have arisen due to the factors outlined above, it is surprising given the similarity of its procedure to that of King (2001). It is also interesting that BPFS-W was not found to benefit self-regulation relative to controls, given that in Study One of this research programme self-regulation was found to be greater in BPFS-W groups relative to the control writing group, eight weeks post-writing. There are three plausible explanations for this null effect. First, it may be attributable to the potential lowered efficacy of the intervention in the current study due to its online administration as highlighted earlier in this chapter. Second, the participants in Study One were mainly students (98%) and their average age was 24.14 years. The participants in the current study were older and most were not students; as stated above it is possible that BPFS-W is less effective for older individuals who are not current University students. The third possible explanation is centred in King's (2001; 2002) self-regulation theory that BPFS-W likely enables individuals to explore goals which are positioned on a high level of their motivational hierarchy (see Chapter Two, Section 2.2.3.1). Goals on high levels of the hierarchy (higher-order goals) are less likely to be attended to regularly, thus examination of these goals may bring clarity to an individual's priorities, reduce goal-conflict, enable generation of goal-pursuit strategies and facilitate monitoring of feedback (King, 2002). Through these mechanisms, BPFS-W may increase self-regulation (King, 2001; 2002). In the current study, BPFS-W was not found to impact future-orientation. Scores on the FOS range from four to 20, and baseline scores in this study were 17.06 for the BPFS group, and 16.21 for controls. Given these high baseline scores, it is possible that participants were already focussed on their higher-order goals, clear of their priorities and goal-

pursuit strategies, monitoring feedback on their progress towards their higher-order goals and self-regulating effectively towards achieving them. This suggestion is theoretical and should be treated with caution, because the FOS has not undergone empirical validation. This means that scores on the FOS may not be reflective of future-orientation, or at least scores may not reproducibly demonstrate the true variability in future-orientation within and between participants (Streiner, Norman & Cairney, 2015). Therefore, it is possible that future-orientation did change as a product of BPFS-W, but that change was not detected by the FOS.

5.6 Directions for further research

It was discussed in Chapter Four and earlier in the current chapter that it is difficult to compare findings across BPFS studies due to marked variations in procedural factors such as the specific writing instructions used, the number, spacing and length of writing sessions, the timing of follow-ups and the specific outcomes measured. This renders interpretation of differences in findings near impossible. Given the complexity of the literature surrounding BPFS-W, a systematic review is a logical step forward; it would make the evidence more manageable and facilitate identification of patterns and inconsistencies which would likely be missed when comparing individual studies (Haase, 2011). Through integrating findings from multiple studies differing in measurement of outcomes, intervention administration procedures, study design and risk of bias, systematic reviews provide a holistic interpretative platform which cannot be created in any single experimental study (Dickersin & Berlin, 1992; Light & Pillemer, 1984; Mulrow, 1994). This may help to establish how beneficial BPFS-W truly is across outcomes, as well as whether findings are generalisable across procedural variations (Boissel, Blanchard, Panak, Peyrieux & Sacks, 1989; Mulrow, 1994; O'Hagan, Matalon & Riesenberg, 2018). It may facilitate identification of procedural factors— or combinations of factors— which appear to increase therapeutic power, those which do not influence efficacy, and those which are detrimental and should be abandoned (O'Hagan et al., 2018). Reviewing all the available literature will clarify the strength of the evidence surrounding the effects of BPFS-W, as well as whether inconsistent findings are best explained by procedural, quality or outcome measurement variations

(Knipschild, 1994; Mulrow, 1994). This will mean that conclusions will be more accurate, creating a springboard for better-informed decision-making with regards to future research directions including suggestions for improving the strength of the evidence (Mulrow, 1994; O'Hagan et al., 2018).

Chapter Six

Is writing about a best possible future self beneficial for physical and psychological well-being? A systematic review

6.1 Introduction

The effects of writing about a best possible future self (BPFS) on physical health and psychological well-being have been investigated multiple times since the first empirical examination of its effects on well-being was reported by King (2001). Some evidence has suggested that the intervention is beneficial for a wide range of outcomes, including decreasing symptoms of physical illness (King, 2001), bolstering positive affect (PA) and positive thoughts about the future (Peters, Flink, Boersma & Linton, 2010), increasing general psychological well-being (Vaughn et al., 2003) and dampening negative affect (NA; Odou & Vella-Brodrick, 2013).

The positive effects of BPFS writing (BPFS-W) have been promoted through academic narratives in peer-reviewed, published records. Boselie, Vancleef and Peters (2017) state that "previous research has proven the effectiveness" (p. 447) of the intervention in eliciting improvements in several well-being outcomes including PA. Boselie, Vancleef and Peters (2016) assert that "previous research has confirmed" (p. 26) its effectiveness in improving outcomes, including dampening of NA. Thus, the beneficial effects of BPFS-W appear to have become accepted. This is to the point that the activity has been employed as a reliable means of manipulating subjective well-being in studies which were not conducted with the primary aim of investigating its effects. Boselie, Vancleef, Smeets and Peters (2014) aimed to investigate whether induced optimism diminishes the deleterious effect of pain on executive functions (EFs).

This was based on two premises. The first was that coping with pain consumes self-regulatory resources, and therefore is detrimental to EF performance (Solberg Nes, Roach & Segerstrom, 2009). The second was that optimistic individuals show a tendency to demonstrate perseverance in pursuing a goal when experiencing pain and can better adapt to pain (Affleck et al., 2001; Brenes, Rapp, Rejeski & Miller, 2002). These benefits of optimism may be related to greater capacity for self-regulation and, in turn, EFs (Schmitz, Saile & Nilges, 1996; Wrosch & Scheier, 2003). In Boselie et al.'s (2014) study, BPFS-W was used to induce optimism to allow examination of the interactions between pain, optimism and EFs. Its effects, to be discussed later in the review, were measured only as a manipulation check and not as primary outcomes of the investigation.

The confidence in BPFS-W running through the academic narrative has served as a vehicle for its transition into public use. It has been adopted by writers and administrators of well-being resource websites. An adaptation is freely-available on the Greater Good in Action (n.d.) website, a collaborative online project from Hope Lab and the University of California, Berkely's Greater Good Science Centre, through which activities intended to enhance well-being can be accessed in the public domain. Its use is also endorsed on the website for Soaringwords (n.d.); a non-profit organisation which provides positive psychology-based activities intended to inspire chronically-unwell children and their families to actively self-heal. Similarly, in an article by Niemiec (2013) in 'Psychology Today', readers are encouraged to complete the exercise to improve their hope and well-being. Moreover, practitioners have been encouraged to use the intervention; Niemiec (2013) encourages therapists to use the exercise in treatment of clients. In O'Hanlon and Bertolino's (2011) resource book for psychological therapy clinicians (written to aid implementation of positive psychology interventions into clinical practice) use of BPFS-W during treatment is promoted to help clients orient to a positive future. These examples demonstrate the perceived therapeutic power of the intervention by psychologists and well-being practitioners.

Evidently, BPFS-W has become regarded as an activity which is beneficial for well-being. However, it is possible that this confidence in the intervention is to an extent unfounded or at least premature, as there is apparent inconsistency in the evidence surrounding its effects. Some studies have indeed found the activity beneficial in terms of self-reported reductions in symptoms of physical illness (e.g. Yogo & Fujihara, 2008), but others have not (e.g. Austenfeld, Paolo & Stanton, 2006). This inconsistency is also apparent for the intervention's effects on psychological outcomes; Hanssen, Peters, Vlaeyen, Meevissen & Vancleef (2013) found no significant between-group difference in change in NA immediately post-writing, whereas Odou and Vella-Brodrick (2013) found that NA was dampened. These examples demonstrate that BPFS-W is sometimes beneficial, but other times it is not.

As discussed in Chapters Four and Five, these contradictory findings are possibly products of procedural variations between studies. Using the above example, Odou and Vella-Brodrick (2013) used seven writing sessions, whereas Hanssen et al. (2013) used only one. Perhaps the intervention is more powerful in terms of reducing NA only when multiple writing sessions are used. Odou and Vella-Brodrick (2013) did not specify how long participants should have written for, whereas Hanssen et al. (2013) asked them to write for 15 minutes. Perhaps it is this temporal constraint which lowered the efficacy of Hanssen et al.'s (2013) version. These examples of inconsistency— both in terms of multiple differences in intervention administration and in terms of findings— appear typical of the BPFS literature. They render accurate interpretation of findings difficult, particularly as there appears to be limited evidence surrounding the impact of procedural, intervention administration variations. Therefore, conclusions with regards to the efficacy of BPFS-W cannot be drawn from the literature as it presently exists. It was for this reason that it was difficult to explain the null effects of BPFS-W on well-being in Studies One and Two of the current thesis. Sin and Lyubomirsky (2009) and Bolier et al. (2013) conducted systematic reviews and meta-analyses which suggested that procedural variations do impact effects of positive psychology-type interventions broadly. It is thus conceivable that they influence the outcomes of BPFS-W and effort should be invested into disentangling their effects.

The seemingly-sporadic procedural variations across studies are not the only feature of the BPFS intervention literature to make comparison and interpretation of findings difficult. Literature review-type introductions to academic journal articles often appear to be incomplete reflections of knowledge, and rationales for research appear fragile. For example, Liau, Neihart, Tee Teo & Lo (2016) state that their study was the first BPFS-W investigation to include measurement of depression as an outcome, yet depression had been measured previously by Austenfeld (2007) and Austenfeld and Stanton (2008), as well as by Austenfeld et al. (2006). There appears to be a lack of communication in the literature, likely caused by the wide range of terms that are used to describe BPFS-W. These include 'optimism manipulation' or 'optimism exercise' (Boselie et al., 2016; Shapira & Mongain, 2010), 'expressive writing about life goals' (Harrist, Carlozzi, McGovern & Harrist, 2007), and 'happiness-enhancing strategy' (Boehm, Lyubomirsky & Sheldon, 2011). This disjointedness of the evidence base renders it difficult to attempt to assess and evaluate the effects of the intervention overall by comparing individual studies.

Given that the evidence surrounding the effects of BPFS-W is complex and unclear, a systematic review is a necessary step forward. It will provide a more manageable, integrated interpretative platform from which stronger conclusions can be drawn (Mulrow, 1994). There is, to the knowledge of the researcher, one previous published systematic review of the BPFS literature. This work (Loveday, Lovell & Jones, 2016) aimed to critically examine the findings of BPFS-W studies. In addressing this aim, the authors discussed the methodological variations across studies, noting that the activity is successful when it is delivered both online and in-person. They suggested that its flexibility in terms of the delivery required for beneficial change demonstrates that it is robust. Loveday et al. (2016) stated that different ‘doses’ are administered across experiments, and described how some authors provide themes for participants to write about (e.g. best possible future personal life, professional life and relationship; Meevissen, Peters & Alberts, 2011; Peters, Meevissen & Hanssen, 2013), whilst others require writing about a small step to be achieved during the process of realisation of a BPFS (Layous, Nelson & Lyubomirsky,

2013). Critically, Loveday et al. (2016) highlighted that the authors who have contributed to the BPFS-W literature have regularly been less than forthcoming with their justifications for why they have used a modification of King's (2001) original protocol. Rather, modifications have been merely stated with neither reason nor explanation, and as such create complication in comparisons of findings across studies.

Loveday et al.'s (2016) review goes some way to suggest the current challenges in the interpretation of the BPFS-W literature. The review communicates that the lack of experimental effort invested in exploring the impact of modifications to King's (2001) protocol is problematic; it is impossible to determine the methodological parameters within which BPFS-W is likely to yield optimal improvements in outcomes. Nevertheless, it was decided that another systematic review is needed to facilitate identification of the potential impacts of these procedural modifications on outcomes, for three reasons.

The first two justifications for producing the current review stem from its differing focus from that of Loveday et al. (2016). First, Loveday et al. (2016) did not predominantly aim to determine the methodological factors which may impact the efficacy of BPFS-W interventions. Instead, Loveday et al.'s (2016) work serves as a scoping review, which sets the context for more rigorous address of the problem. Second, Loveday et al. (2016) did not focus only on writing interventions, and included any study which involved the participant generating a BPFS, including talking about it (Harrist et al., 2007) or drawing it (Owens & Patterson, 2013), and correlational studies designed to examine the association between types or importance of goals and well-being (e.g. Hill, Terrell, Arellano, Schuetz & Nagoshi, 2014; King & Smith, 2004). Loveday et al. (2016) also included portfolio studies, in which a BPFS activity is used alongside other tasks (e.g. D'raven, Moliver & Thompson, 2015). This prevents the pure effects of BPFS-W from being isolated.

The third justification for producing a second systematic review of the BPFS-W literature is that some of the systematic review methodology (and reporting) used in Loveday et al.'s (2016) work is not as suggested in the guidance

published by the Centre for Reviews and Dissemination (CRD; 2009) and by Moher, Liberati, Tetzlaff & Altman (2009) in the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) Statement. The authors did not produce a protocol prior to conducting the review, reducing transparency of the production process and subsequently confidence in the integrity of review findings (Butler, Hall & Copnall, 2016; Risenberg & Justice, 2014). They did not include a flow diagram to demonstrate the flow of information through search stages and did not provide figures for the number of studies rejected following database searches or reasons for exclusion. Loveday et al. (2016) also did not include details of how data from included records were extracted (e.g. whether one or more reviewers were involved, thus introducing the potential for error and bias; Edwards et al., 2002; Munn, Tufanaru & Aromataris, 2014). Few details of participants are provided, thus it is difficult to establish the generalisability of findings (Munn et al., 2014). Critically, Loveday et al. (2016) did not perform a risk of bias (ROB) assessment. ROB assessment is paramount in systematic reviews, because it allows assessment of the credibility of included studies; low quality studies have been found to produce both inflated and deflated estimates of effect (Kunz & Oxman, 1998; Schulz, Chalmers, Hayes & Altman, 1995). It enables consideration of how potential areas of bias may have influenced outcomes of individual studies when synthesising results, and therefore makes review conclusions more meaningful (Booth, Sutton & Papaioannou, 2016; Verhagen, de Vet, de Bie, Boers & van den Brandt, 2001).

Another area of methodology in Loveday et al.'s (2016) review which required development is the search strategy. Published guidance from the CRD (2009) recommends that search terms should retrieve as many potentially relevant studies as possible and should include all alternative terms for an intervention. In Loveday et al.'s (2016) database search the terms 'best possible self/ selves', 'positive psychological/ psychology interventions' and 'writing' were included (Loveday et al., 2016, p. 2). These did not successfully capture much of the BPFS-W literature, likely due to inconsistencies in the words used to describe the intervention, as previously discussed. This means Loveday et al.'s (2016) conclusions are not based on all of the available evidence, and as such may

provide a biased reflection of efficacy (Booth, Sutton & Papaioannou, 2016; Mulrow, 1994). Furthermore, Loveday et al. (2016) included only published, peer-reviewed journal articles and rejected records from theses and conference proceedings. This further restricts the papers included in the review and further reduces the meaningfulness of conclusions. Moreover, exclusion of grey literature may introduce publication bias, whereby studies are accepted or rejected from publication due to the direction or strength of effects (Gilbody & Song, 2000). Systematic reviews in which only published studies are included may therefore over-estimate the impacts of interventions (CRD, 2009).

6.2 Aims

The current systematic review was conducted to address two main aims. First, to establish whether the available evidence suggests that BPFS-W is beneficial for physical and psychological well-being of adults (including whether it impacts cognitive and process variables which may be related to well-being), relative to no-activity control or placebo task control conditions. Second, to establish whether procedural variations between studies, in the way that the intervention is administered or outcomes are measured, impact its efficacy.

6.3 Method

This review was conducted according to published guidance for completing systematic reviews in health care (CRD, 2009), and was reported according to recommendations in the PRISMA Statement (Moher et al., 2009; see Appendix A.14 for PRISMA checklist). Inclusion/ exclusion criteria and outcomes of interest were pre-specified in a protocol, which is registered on Prospero (identification number: CRD42017055651; see Appendix A.15 for original version). There were some deviations from this protocol. During the scoping search, it became apparent that it would be useful to include an additional outcome type (cognitive processes). The inclusion/ exclusion criteria were modified considering this and were tightened because of types of studies being identified in the scoping review which would have been included based on the original criteria but were not relevant to the current review. There were also some modifications which were necessary due to time-constraints. First, it was

stated in the protocol that if relevant information was missing from records then authors would be contacted. Instead, missing information or lack of clarity has been acknowledged throughout as transparently as possible. Second, it was specified that the first[18] and second reviewers would independently screen all records for eligibility. However, the second author independently screened 25% of records at both title/ abstract and full-text levels. Third, it was stated that two reviewers would independently extract data. Instead, the third reviewer thoroughly checked the first reviewer's extractions and flagged possible errors and omissions. Fourth, it was stated that two reviewers would independently assess ROB; assessment was instead discussed with the review team. There were other, minor, deviations which will be acknowledged throughout this section of the review.

6.3.1 Inclusion/ exclusion criteria

Types of studies:

Studies with empirical/ experimental designs were eligible for inclusion. Other designs, such as correlational studies, were excluded. Study designs also had to have an appropriate placebo-control or 'no treatment' group, which could not be expected to elicit benefits in the outcomes relevant to this review. It also had to be possible to isolate the pure effects of BPFS-W on physical and psychological health. Therefore, portfolio studies (studies using one or more additional interventions with the same group of participants) and studies where participants were already receiving a psychological treatment were not eligible. If authors had analysed data for one or more outcomes in a way which rendered isolation of the effects of BPFS-W impossible (e.g. by combining BPFS-W participants' scores with those of another intervention group), descriptive statistics were consulted to ascertain whether they mirrored the patterns from the inferential analyses. If descriptive statistics for the BPFS and control groups were not available for one or more outcomes, then those outcomes (or the study, if they were unavailable for all outcomes) were excluded.

[18] The 'first reviewer' is the Ph.D. candidate. The 'second reviewer' is the candidate's Director of Studies, and the 'third reviewer' is the candidate's third supervisor. The titles 'second' and 'third' refer to the order in which tasks were performed. The 'review team' is the candidate and supervisory team.

Types of participants:

Only studies using adult participants were eligible for inclusion[19]. The term 'adult' refers to any sample which did not target children. Therefore, a study with some participants under 18 was eligible. This decision was made based on the knowledge that many studies of BPFS-W interventions utilise a student sample, and in some countries, students begin higher education below age 18. Any study with a mean age of below 16 was excluded. There were no restrictions on health statuses of participants; studies with clinical and non-clinical population samples were eligible.

Intervention:

The intervention reviewed was writing in prose about a BPFS. This could be writing by hand or typing online. It could be in any setting, including in groups, individually in a laboratory or at participants' homes. It could be purely writing or could be accompanied by mental imagery about a BPFS. The BPFS topic had to be comparable to the topic introduced by King (2001). This is to say that it had to be focussed on a positive future, and on life goals (a future that the participant would have some control over, rather than positive situations completely outside of their control).

Types of outcomes:

The inclusion criteria surrounding types of outcome were broad. Any outcome variables related to physical and psychological health and well-being were included, for example anxiety, optimism and physical symptoms. This included 'cognitive' or 'process' variables which could be expected to be associated with physical or psychological health, such as self-regulation and working-memory (hereon referred to as 'cognitive-process' outcomes). A detailed discussion with regards to how cognitive processes are related to well-being is beyond the scope of the current chapter. However, there is a large evidence base spanning several decades which demonstrates the association between cognitive processes and physical and psychological well-being (e.g. Balderston et al., 2017; Davis & Nolen-Hoeksema, 2000; Elliott, 1998; Kubzansky, Park,

[19] In the original protocol, it was specified that adults aged over 18 years would be included. This would have made criteria unnecessarily restrictive and would have resulted in exclusion of relevant studies.

Peterson, Vokonas & Sparrow, 2011; Phillips, Bull, Adams & Fraser, 2002; Sarason, 1984). It was therefore decided that it was important to include these outcomes, to ensure a holistic interpretation of the current evidence surrounding the effects of BPFS-W on well-being. Any type of measure was accepted, including self-reports, records of visits to health-centres, and physiological indicators.

Other criteria:

Studies had to be available in English, and there had to be sufficient information to discern whether they were eligible for inclusion. If insufficient information was reported, contact with authors for further details was attempted. Studies published between 2001 (year of publication of the first study of the effects of BPFS-W on health and well-being; King (2001)) and 2017 (the current year at the time that the searches were conducted) were eligible.

6.3.2 Searches

To retrieve all published and unpublished literature to answer the review questions as comprehensively as possible, a series of searches were conducted; an initial scoping search, a formal database search, a grey literature search, and finally reference list and citation searches.

Scoping search:

The reasons for conducting a scoping search were two-fold; first, to identify the breadth of the literature, and second to compile a list of terms used to describe BPFS-W to inform the database searches. The first reviewer was already familiar with the BPFS-W literature, and created a list of known studies which conformed to the predetermined inclusion/ exclusion criteria outlined earlier in this chapter. This list comprised 18 published journal articles and one unpublished conference abstract and hand-out. The first reviewer examined these records to identify the terms used to describe BPFS-W and found great disparity. For this reason, a reference list search was conducted to identify as many terms used to describe the intervention as possible, and therefore optimise the search terms used. Reference lists from the 19 already-known records (as well as from Loveday et al.'s (2016) review) were scanned for

further studies which pertained to BPFS-W. This yielded a further six records. The first reviewer studied the titles, abstracts and keyword sections of these 25 records and identified the terms used to describe BPFS-W.

Database search:

The database search was performed using a search strategy made up of the keywords identified in the scoping search. The search strategy was entered into Cochrane, MEDLINE, CINAHL, PsycInfo, and Scopus[20], and the search was limited to records published between 2001 and 2017. The search terms were piloted and modified to maximise the proportion of the 25 records found during the scoping search returned, to increase confidence in the search results. The final combinations of search terms returned 13 out of 25 records[21]. The database searches were run on 01/03/17 and 02/03/17. Final combinations of search terms are provided in Appendix A.16.

The search results from each database were downloaded into Refworks and duplicates were removed. The titles and abstracts of the remaining records were then screened to exclude any records which did not include BPFS-W. Any papers which pertained to BPFS-W were accepted for further screening, alongside papers which the reviewer was unsure about from the information in the abstract. For some records, abstracts were unavailable. These records were automatically included for further screening to ensure rigour throughout the review process.

To assess the reliability of the first reviewer's screening, 25% of the titles and abstracts were independently screened by another member of the review team. This second reviewer was blind to the first reviewer's decisions, and records were selected by generating a random order of all records in an Excel spreadsheet and then screening the first 25%. The second reviewer included no records that had been excluded by the first reviewer. However, the second

[20] In the protocol it was specified that MEDLINE, PsycInfo, Scopus, and Web of Science would be searched. Web of Science was replaced with Cochrane and CINAHL, based on advice from an information scientist.

[21] This was the most precise search strategy which could be generated with the resources available in a Ph.D. programme, because of the wide range of terms used to describe BPFS-W.

reviewer excluded 10 papers which had been included by the first reviewer (disagreements were resolved through discussion with the other members of the team). This gave an agreement level of 98.38%, therefore it was decided that no more than 25% of the title/ abstract screening needed to be replicated. This decision was especially justified given that the second reviewer did not include any papers which were excluded by the first author, thus suggesting that the first reviewer applied sufficient rigour during the screening process.

The remaining records were screened more thoroughly. The full texts of these records were retrieved and then screened for eligibility for inclusion against the predetermined inclusion/ exclusion criteria. If a potentially-relevant record was identified but the full text was not easily accessible, or it was not clear from the information provided whether the record was eligible for inclusion, then every effort was made to retrieve it or gain more information. Authors were contacted, and Sheffield Hallam University Library's document supply services were utilised. The second reviewer independently screened 25% of these records, using the same process as in the previous screening stage. There was a single disagreement, which was resolved through discussion with the other review team members. Given the agreement level of 95.00%, it was decided that sufficient rigour had been applied by the first reviewer and therefore no more than 25% of the full-text screening needed to be replicated. Studies excluded at the full-text screening, along with reasons for exclusion, are presented in Appendix A.17.

Electronic database searches are unlikely to be exhaustive in identifying all relevant records (Booth et al., 2016). This is particularly so when a concept is difficult to define (Garg et al., 2009). This is certainly the case with BPFS-W; a multitude of terms are used to describe the intervention. Indeed, there were some records identified during the scoping search which were not returned in the database search results (described henceforth as 'records identified through scoping search'). For this reason, additional searches were used to optimise the amount of relevant literature included in the review.

Grey literature search[22]:

To minimise publication bias, a grey literature search was conducted as suggested by Booth et al. (2016). McAuley, Tugwell and Moher (2000) randomly selected a sample of systematic reviews and found that only 33% of them included both grey literature and published studies. Published studies demonstrated larger intervention effects in comparison to grey literature studies; including only published literature in a systematic review can lead to inflated estimates of effect (McAuley et al., 2000). It was decided that the most effective way to search grey literature was to hand-search conference proceedings. The conference proceedings were selected by examining online profiles of the authors of the records included from previous searches and noting which conferences they had presented at. Journals that the records included from the database search were published in were also noted. The following proceedings were screened for studies pertaining to BPFS-W; British Psychological Society Division of Health Psychology (BPS DHP), European Health Psychology Society (EHPS), European Conference on Positive Psychology (ECPP), and Annual Meeting of the Society for Experimental Social Psychology (SESP). All years for the BPS DHP proceedings were searched from 2001 to 2016 (proceedings for 2017 were not yet published). For the EHPS proceedings, the same years were hand-searched other than 2002 and 2004; these were deemed unobtainable following attempts to access them both online and through the university's document-supply service. It was not possible to obtain full records of the ECPP and SESP proceedings. Therefore, the 'advanced scholar search' function on Google Scholar was used so that only records which were published in these proceedings would be returned.

Citation/ reference list search:

To ensure that the current review was as comprehensive as possible, two further search stages were conducted after all other searches had been completed. The first involved hand-searching the reference lists of studies included from the database and grey literature searches (other than those from

[22] It was specified in the protocol that ProQuest Dissertation Abstracts International would be screened. However, the author's institution did not have access to this database. Given that snowballing in the form of reference and citation searches were continued until saturation, it unlikely that this impacted the final body of included literature.

which the reference lists were searched during the scoping search). The second was a citation search. Citation searching has been demonstrated to be successful in identifying additional records to those found during database searches (Brettle & Long, 2001; Hinde & Spackman, 2015; McNally & Alborz, 2004; Papaioannou, Sutton, Carroll, Booth & Wong, 2010). The citation lists function on Google Scholar was used to find articles which had cited each of the included records. Titles and abstracts of records retrieved through this method were screened, and any which pertained to BPFS-W were explored further by assessing their eligibility against the inclusion/ exclusion criteria.

Citation and reference list searches were repeated with newly-identified eligible records until no more new records pertaining to BPFS-W were found, and saturation could be assumed.

6.3.3 Data extraction

An original extraction form was created and piloted on five records. The extracted data were then discussed in a meeting between the first and third reviewers and the form was amended to allow extraction of data which were missed using the original version. The first reviewer extracted data from each study independently, and the extracted data were checked for accuracy and completeness by the third reviewer. For each study, authors' names, date of publication, country, and publication type were extracted, along with participant information (sample size, average age, gender, ethnicity, and other characteristics such as whether participants were students). Procedural factors (e.g. number, length and spacing of writing sessions) and details of the intervention (writing instructions for all relevant experimental and control groups, whether writing tasks were supplemented with imagery) were then extracted. Finally, details of outcomes (including measurement instruments and when measurements were taken) and results were extracted. Potential areas of bias were noted. Please see the sample extraction form in Appendix A.18 for more information about the data extracted from each study.

6.3.4 ROB assessment

Assessment of quality and ROB began during data extraction. The first reviewer noted quality issues which were encountered throughout, such as low sample sizes and attrition. These observations informed construction of the ROB assessment form. A published ROB assessment form was also used to guide the production of the form (Viswanathan et al., 2012)[23]. The final form assessed selection bias, performance bias, and detection bias. ROB for each individual study was then assessed using this form. Three broad categories were used; low ROB, some ROB, and high ROB. Risk of attrition bias was also assessed, but it was difficult to categorise this in the same way as the other areas of risk. Therefore, this was recorded in the ROB narrative only.

For selection bias, assessments were made with regards to whether participants were randomly allocated to groups (and whether the randomisation strategy was appropriate), whether groups were equal at baseline in terms of demographic characteristics and baseline measures, and whether any baseline differences were dealt with appropriately (i.e. by using an ANCOVA with the baseline scores as the covariate to partial out their effects, or by using change scores). For performance bias, writing instructions were assessed in terms of the level of detail provided to each group as well as whether an equal dosage of writing had been ensured across groups. Detection bias was assessed according to whether measurement instruments had undergone empirical validation, whether equal follow-up times were ensured across groups, and whether the sample size allowed sufficient statistical power to detect the true outcome of analyses. For power analyses, desired sample sizes for a power level of 0.8 to be obtained for each design and analysis employed in the included studies were calculated using the G*Power 3.1 software (Faul, Erdfelder, Lang & Buchner, 2007). It was not possible to calculate power for all main effects and interactions due to time-constraints. Instead, power analyses were conducted for time * group interactions only (unless only main effects were reported, e.g. when change-scores were used). Therefore, interpretations of power in the current review should be treated with caution. Attrition bias was

[23] It was specified in the protocol that Cochrane's tool (Higgins & Green, 2011) would be used. This was changed following advice from an information scientist.

assessed according to whether there was a high level of attrition in the study, whether any attrition occurred in certain groups of participants more than in others (differential attrition), and whether any attrition was handled appropriately (e.g. by intention-to-treat analysis, as recommended by the CRD (2009)). ROB from between-group differences in outcomes at baseline (and controlling for them), validation of measures, power and attrition were assessed at the outcome level. All other areas of ROB were assessed at the study level. The results of the ROB assessment were used in the synthesis stage of the review; potential risks of bias were considered when comparing findings across studies, and when determining the likely overall effects of the BPFS-W intervention on each outcome. It is worth noting that this was done in relation to each outcome. This is to say that if it was stated in the synthesis that a study was of 'fair' quality, this means that it was of fair quality for that outcome. This is not necessarily a reflection of the quality of the whole study.

6.3.5 Data synthesis

Data from the included studies were analysed using narrative synthesis[24]. Data were first grouped into three broad outcome type categories; physical, psychological and cognitive-process outcomes. Within these three broad categories, outcomes were explored individually, apart from when two or more outcomes were closely linked (e.g. self-compassion, self-reassurance, and self-criticism). Syntheses for outcomes with larger and more complex evidence bodies were split into immediate and longer-term effects. It is worth mentioning here what is meant by 'immediate effects' and 'longer-term effects' in the context of this review. Immediate effects were classed as measures taken immediately following the end of the writing session phase of a participant's involvement in the study, whether this was immediately following a single session, or the final of several sessions administered over several days or weeks. Longer-term effects were classed as measures taken days, weeks or months after the writing phase.

[24] In the protocol it was specified that meta-analyses would be conducted where possible. This was not possible at this time, but the intention is to explore this in the future.

To achieve the first aim of the review (to examine whether BPFS-W is beneficial in terms of improving physical, psychological and cognitive-process outcomes), findings from all studies which included each outcome were compared. Where inconsistencies in findings were apparent, ROB was considered (for example, to explore the possibility that a null effect may have arisen due to underpowered analyses), as well as any procedural variations which may have altered intervention efficacy. Exploration of the possible effects of procedural variations went some way to address the second aim of the review (to examine whether procedural variations across BPFS-W studies affect the effectiveness of the intervention). A small number of studies included empirical investigation of the effects of procedural variations. These studies were grouped according to the procedural variation manipulated. Findings from within each group were compared, and areas of ROB were considered when drawing inferences from findings, to further address the second aim of the review.

6.3.6 Ethical considerations

An ethics checklist was completed for the current review and is provided, along with an approval letter, in Appendix A.19.

6.4 Results

6.4.1 Study selection

The records returned and included at each search stage are summerised in Figure 6.1. The database search returned 3,199 records. 758 were identified as duplicates and removed, leaving 2,441 records. 2,363 records were excluded at the title and abstract screening. For the remaining 78 records, the full texts were assessed, and a further 58 records were found to be ineligible for inclusion. 20 records from the database search satisfied the inclusion and exclusion criteria. The grey literature search returned 263 records, but only one was eligible for inclusion. The citation search returned 2,821 records, of which two were eligible. The reference list search returned a further eligible study which had not been previously-identified. A further 12 records were identified through the scoping search. The two BPFS-W studies produced earlier in this programme of research were also included. A total of 38 records and 37 studies were included

in the review. There are a greater number of records than studies because two of the studies were published in a single record, and a further two studies were published in two records each.

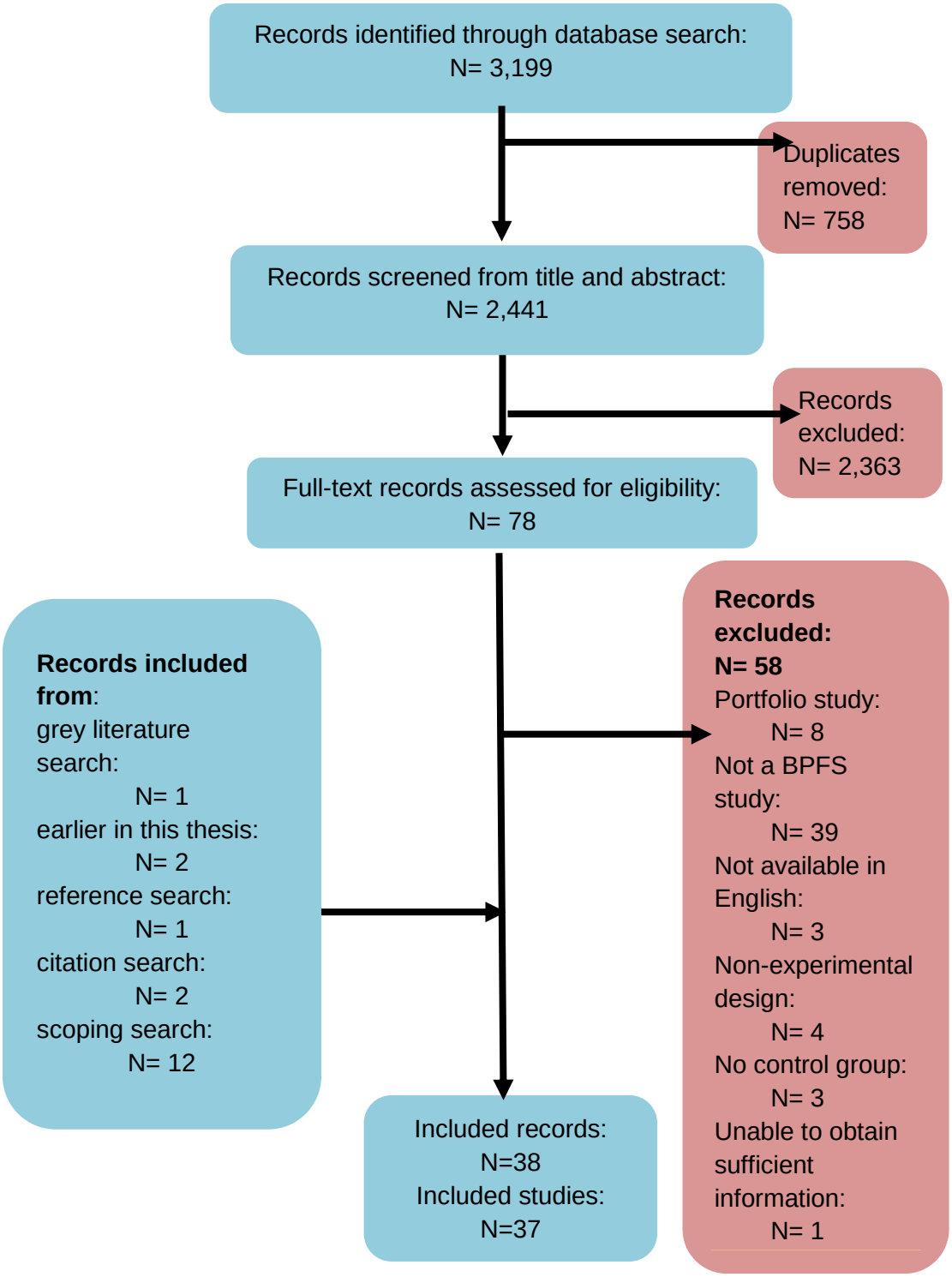


Figure 6.1: PRISMA flow diagram (Liberati et al., 2009) showing stages of searches and exclusion

6.4.2 Study characteristics[25]

Table 6.1 shows the publication characteristics of the included studies. Studies were undertaken in several countries including the UK (4), Japan (1), Singapore (2), Australia (1), Canada (1), Germany (2), Belgium (1) and Sweden (1). Most were from the USA (16) and the Netherlands (8). Most records were published journal articles (30). Others were student theses (4), unpublished papers (3), and a conference abstract and handout.

Table 6.1: Publication characteristics of included studies

Study authors	Date of publication	Country undertaken	Type of publication
Aborida	2016	USA	MSc thesis
Austenfeld; Austenfeld & Stanton	2007; 2008	USA	Ph.D. thesis; Journal article
Austenfeld, Paolo & Stanton	2006	USA	Journal article
Boehm, Lyubomirsky & Sheldon	2011	USA	Journal article
Boselie, Vancleef & Peters	2017	Netherlands	Journal article
Boselie, Vancleef & Peters (Study One (a))	2016	Netherlands	Journal article
Boselie, Vancleef & Peters (Study Two (b))	2016	Netherlands	Journal article
Boselie, Vancleef, Smeets & Peters	2014	Netherlands	Journal article
Frein & Ponsler	2014	USA	Journal article
Geschwind, Meulders, Peters, Vlaeyen & Meulders	2015	Belgium	Journal article
Hanssen, Peters. Vlaeyen, Meevissen & Vancleef	2013	Netherlands	Journal article
Harrist, Carlozzi, McGovern & Harrist	2007	USA	Journal article
King	2001	USA	Journal article
Layous, Nelson & Lyubomirsky	2013	USA	Journal article
Liau, Neihart, Tee Teo & Lo	2016	Singapore	Journal article

25 Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) presented a single study, as did Austenfeld (2007) and Austenfeld and Stanton (2008). Boselie et al. (2016) reported two separate studies within one record.

Study authors	Date of publication	Country undertaken	Type of publication
Lyubomirsky, Dickerhoof, Boehm & Sheldon	2011	USA	Journal article
Maddalena, Saxey-Reese & Barnes	2014	USA	Journal article
Manthey, Vehreschild & Renner	2016	Germany	Journal article
McGovern	2004	USA	Ph.D. thesis
Meevissen, Peters & Alberts	2011	Netherlands	Journal article
Murn	2014	USA	Ph.D. thesis
Nazarian & Smyth	2013	USA	Journal article
Ng	2016	Singapore	Journal article
Odou & Vella-Brodrick; Seear & Vella-Brodrick	2013; 2013	Australia	Journal article; Journal article
Peters, Flink, Boersma & Linton	2010	Sweden	Journal article
Peters, Meevissen & Hanssen	2013	Netherlands	Journal article
Peters, Vieler & Lautenbacher	2016	Germany	Journal article
Renner, Schwarz, Peters & Huibers	2014	Netherlands	Journal article
Shapira & Mongrain	2010	Canada	Journal article
Sheldon & Lyubomirsky	2006	USA	Journal article
Titova, Wagstaff & Parks	2017	USA	Journal article
Troop, Chilcot, Hutchings & Varnaite	2013	UK	Journal article
Vaughn, Abruzzo, Balliet, Merry, O'Rourke & Salpeter	2003	USA	Conference abstract and hand-out
Winn & Troop	2002	UK	Unpublished paper
Yogo & Fujihara	2008	Japan	Journal article
Ph.D. Study One	2015	UK	Unpublished paper
Ph.D. Study Two	2016	UK	Unpublished paper

6.4.3 Participant characteristics

Table 6.2 shows the demographic characteristics of the individuals who participated in the included studies.

The reader should assume that the figures presented in this table are for the final sample (following attrition) unless otherwise stated. Where these figures were not recorded by the study authors, pre-attrition figures have been provided. Please see the associated footnotes for further clarification.

Table 6.2: Participant characteristics in included studies

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
Aborida (2016)	69	38% 25-35; 32% 18-24 76% female	Unreported	37% worked more than 40 hours per week; 34% between 30 and 40 hours. 39% had completed 'some college'; 30% were bachelor's degree graduates. Some were students.
Austenfeld (2007); Austenfeld & Stanton (2008)	63	19.3 (1.19) 79.84% female	87.3% White, 1.6% African American, 6.3% Latino, 3.2% Asian, 1.6% other	100% students Participants included if they rated the stressfulness of their current situation as at least 3 (on 7-point scale; 1= not at all stressful, 7 = extremely stressful) and their perceived control over that situation as 5 or below (on 7-point scale; 1= no control at all, 7= complete control).
Austenfeld et al. (2006)	64	26.41 (4.04) 45% female	84% White, 2% African American, 2% Latino, 11% Asian, 2% other	100% students

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
Boehm et al. (2011)	220	25.62 (11.36) 52.72% female	49% Asian American, 51% Anglo American	19% had completed only high school, 56% a university degree, 25% a post-graduate degree.
Boselie et al. (2017)	61[26]	21.48 (2.47) 90.16% female	Unreported	100% students Participants with a chronic pain disorder, current pain, a heart or vascular condition, those who wore an electronic implant, were pregnant, had a diagnosis of a psychological illness in the last 3 months, or were taking anxiolytic/ antidepressant medication were excluded.
Boselie et al. (2016a)	81[27]	21.35 (4.28) 79.01% female	Unreported	100% students Participants with chronic pain conditions, cardiovascular disease, Raynaud disease or experiencing any current pain excluded.
Boselie et al. (2016b)	61[28]	21.84 (2.22) 73.77% female	Unreported	100% students Participants who had chronic pain conditions, current pain, were pregnant, had a heart or vascular condition, had an electronic implant, had been diagnosed with a psychological illness in the past three months, or were taking anxiolytic/ antidepressant medications excluded.

[26] 55 participants were included in the analyses surrounding set-shifting.

[27] Figures are for the full sample of 81. 79 included in working-memory analyses (mean age= 21.30 (4.33), 80.25% female).

[28] 58 were in analyses surrounding working-memory (mean age= 21.90 (2.20)); 3 were removed because they scored below chance (50%) on the working-memory task.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
Boselie et al. (2014)	74[29]	21.9 (2.29) 78.38% female	100% Dutch (native speakers)	100% students Participants with chronic pain disorders, current pain, with cardiovascular disease or Raynaud disease excluded.
Frein & Ponsler (2014)	39	20.6 7.69% female	Unreported	100% students
Geschwind et al. (2015) [30]	42	20.32 (1.97) 100% female	Unreported	74% students Participants who were pregnant, had respiratory, neurological or cardiovascular conditions, chronic pain or any minor or major illness excluded.
Hanssen et al. (2013)	79	22.59 (2.86) 81.01% female	Unreported	100% students
Harrist et al. (2007) [31]	75	21 (range= 18-45) 66.67% female	76% Euro American, 10% Native American, 4% Latino, 10% other	100% students

[29] 66 were included in working-memory analyses.
[30] Figures are for the full sample of 50 participants; a subset of 42 completed the third of three time-points.
[31] Figures are for the full sample of 75. There was no attrition, but health-centre records were available for only 68 participants.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
King (2001) [32]	81	21.04 (3.15) 81.18% female (2.35% unreported)	87% European American, 7% Hispanic, 3% African American, 3% Asian	100% students
Layous et al. (2013) [33]	119	19.10 (1.77) 71.76% female	30% Asian American, 19.3% Caucasian, 18% Hispanic/ Latino, 9.3% Black/ African American, 5.3% more than one ethnicity, 0.7% Hawaiian/ Pacific Islander, 4.7% other	100% students
Liau et al. (2016)	162	17.83 (1.12) 69.14% female	Unreported	100% students

[32] There are apparent errors in reporting of the total sample; King (2001) reported that 81 participants completed the study, but genders are provided for 85, and the sum of values provided for groups is 79. Although there was no attrition in terms of drop-out, health-centre records were available for a subset of 72 participants.
[33] Figures are for the full sample of 131, before 12 participants were excluded from analyses due to failing to complete follow-ups.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
Lyubomirsky et al. (2011) [34]	330	19.66 (2.91) 71.72% female	40% Asian, 20% Hispanic, 17% Caucasian, 5% African American, 5% Hawaiian/ Pacific Islander, 6% more than one ethnicity, 7% other	100% students
Maddalena et al. (2014)[35]	64	Age unreported 66% female	50% Caucasian, 17% Latino, 11% Asian, 8% Middle Eastern, 1% African, 1% Native American, 1% Pacific Islander, 9% unreported	100% students
Manthey et al. (2016) [36]	311	33.7 (9.6) 84.14% female	Unreported	28.3% students 6.9% educated to secondary school level, 52.4% to a higher education entrance qualification, 37.5% to university degree level, 3.2% to Ph.D.

[34] 330 were included, and details for these participants are recorded here. 319 remained at post-test, 214 remained at 6-month follow-up.

[35] Figures for all participants, including a trauma writing group and an additional control group. Analyses for effects of BPFS-W were conducted separately, with a different control group (N=34). Analyses for effects of spacing were conducted using the whole sample. Participants in BPFS-W versus control analyses had low levels of emotional processing.

[36] Figures for 435 participants who completed the baseline assessment, writing tasks, and post-test measures. 322 completed the follow-up. A further 3.5% were excluded. The sample size figure in this table is based on: 3.5% of 322= 11.27, and 322-11.27= 310.73.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
McGovern (2004)	46	Age unreported 69.57% female	Unreported	100% students
Meevissen et al. (2011) [37]	51	23.5 (6.39) 92.59% female	100% Dutch	Mostly students
Murn (2014)	28	25.14 (SD= 5.02) 67.86% female	78.6% White/ Caucasian, 5.35% Black/ African American/ African, 7.1% American Indian/ Native American/ Alaska Native, 7.1% Hispanic/ Latino.	100% students
Nazarian & Smyth (2013) [38]	195	30 (range= 18-74) 68.14% female	67.5% Caucasian, 8.5% African American, 6.5% Latino, 0.5% American Indian, 5.5% Asian, 9.5% other, 2.5% Biracial/ Mixed	50% students. Of non-students, 1% possessed less than high school qualifications, 13% had completed high school, 25% had completed some university, 29% possessed an undergraduate degree, and 31% had a graduate degree. Participants excluded if they had initiated therapy or medication for a psychological illness in the past 3 months, or if they were pregnant.

[37] Figures for the original sample of 54; the final sample included 51 participants. Three females were excluded due to failing to complete post-test measures.

[38] Figures for the 204 individuals recruited; 9 did not complete. The effects of 5 writing interventions were compared to control writing in this study. Analyses of the effects of BPFS-W (N= 65) relative to control writing were conducted completely separately from other analyses, but sample characteristics were reported only for the entire sample.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
Ng (2016)	216	28 (range= 20-61) 63.34% female (0.46% unreported)	Unreported.	100% (part-time) students.
Odou & Vella-Brodrick (2013); Seear & Vella-Brodrick (2013)	37[39]	33.97 (15.57) 75.68% female	Unreported (but 81.4% Australian residents)	27.6% students 41% in full-time work Mean years in education= 16
Peters et al. (2010) [40]	80	29.6 (range= 21-50) 62.20% female	96.34% Swedish Nationals	100% students
Peters et al. (2013)	82	22.8 (range= 18-65) 94.15% female	Unreported	98.70% had undergone university education or advanced professional training
Peters et al. (2016)	56	23.5 (3.3) 57.14% female	Unreported	100% students Participants with psychiatric, neurological or somatic conditions excluded.
Renner et al. (2014)	40	22.1 (range= 19-38) 80.00% female	Unreported	100% students
Shapira & Mongrain	197	34 (range= 18-72)	100% Canadian	Average annual income $30,000- $40,000

[39] 73 participants completed post-test measures, 37 participants remained at follow-up. Post-test and follow-up analysed separately for some outcomes.

[40] 81 participants completed the future-expectancies measure post-writing, 80 completed PA and NA measures post-writing. 82 completed the state optimism measure post-writing.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
(2010) [41]		81.54% female	(79.4% Caucasian)	
Sheldon & Lyubomirsky (2006)	67	Age unreported 74.63% female	85.07% Caucasian, 14.93% African American, Hispanic or Asian	100% students
Titova et al. (2017)	369	33.62 (11.42) 55.1% female	46.4% Anglo American, 20.2% Asian American, 33.4% Indian	Median yearly income= US $40,000- US $59,000 Education from 'some high school' to Ph.D.
Troop et al. (2013)	46	25.8 (9.3) 67.39% female	Unreported	100% students
Vaughn et al. (2003)	84	Both age and gender unreported	Unreported	100% students
Winn & Troop (2002)	34	29.1 (range= 19-42) 85.29% female	Unreported	100% students

[41] Figures for the 1002 individuals recruited. 79.7% dropped out. 203 and 197 completed follow-up depression and happiness measures, respectively. Completers were significantly older than drop-outs.

Study	Sample size (N)	Age (mean (SD; unless indicated otherwise)), and Gender	Race/ ethnicity	Other characteristics
Yogo & Fujihara (2008) [42]	83	Range= 18-19 71.15% female	100% Japanese	100% students
Ph.D. Study One (2015) [43]	82	24.14 (9.29) 71.2% female	Unreported	96% students
Ph.D. Study Two (2016) [44]	31	30.24 (12.05) 83.8% female	5.41% American, 94.59% British	40.5% students

[42] Figures for the 104 recruited; 21 did not complete the study.
[43] Figures for 118 who completed baseline measures, writing intervention and immediate post-writing PA and NA measures. 82 completed all time-points.
[44] Figures for 37 participants who completed the writing task and were included in analyses. 31 completed both follow-ups.

The participants in the included studies had the following demographic characteristics:

Age

Out of the 37 studies included, 19 had participant samples with a mean age in the range of 17 to 25 (e.g. Boselie et al., 2017; Ph.D. Study One). In 13 studies, the mean age of the participants was above 25 (e.g. Shapira & Mongrain, 2010; Winn & Troop, 2002). In Aborida's (2016) study, 38% of participants were aged 25 to 35 and 32% were aged 18 to 24. Ages of the remaining 30% of Aborida's (2016) participants were unreported. In four studies (Maddalena et al., 2014; McGovern, 2004; Sheldon & Lyubomirsky, 2006; Vaughn et al., 2013), the ages of participants were unreported altogether.

Gender

Most (34) studies had samples in which over 50% of participants were female (e.g. Geschwind et al., 2015; Murn, 2014; Ng, 2016). Two studies used predominantly male samples (Austenfeld et al., 2006; Frein & Ponsler, 2014). Vaughn et al. (2003) did not report the gender split of their participants.

Occupations and education

25 of the 37 studies had samples that consisted only of students (e.g. Frein & Ponsler, 2014; Maddalena et al., 2014), including Ng's (2016) participants who were part-time students. A further three studies had samples that consisted mainly of students (over 50%; Geschwind et al., 2015; Meevissen et al., 2011; Ph.D. Study One). Aborida's (2016) sample consisted of some students, and 71% of Aborida's (2016) participants worked at least 30 hours per week. 28.3% of Manthey et al.'s (2016) participants were students. Odou and Vella-Brodrick's (2013) and Seear and Vella-Brodrick's (2013) sample consisted of 27.6% students, whilst 41% were in full-time employment. 40.5% of the participants in Ph.D. Study Two were students.

The authors of some (7) studies reported participants' education level (e.g. Aborida, 2016; Boehm et al., 2011; Manthey et al., 2016). Participants across these studies were well-educated; education levels ranged from high school to Ph.D. degrees.

Race and ethnicity

In slightly over half (19) of the included studies, the authors did not report participants' racial and ethnic backgrounds (e.g. Boselie et al., 2017; Winn & Troop, 2002). In studies in which race and ethnicity were documented, a large proportion (13) used samples with participants from two or more ethnic and racial backgrounds (e.g. Layous et al., 2013; Titova et al., 2017; Ph.D. Study Two). All Meevissen et al.'s (2011) and Boselie et al.'s (2014) participants were Dutch, Shapira and Mongrain's (2010) participants were Canadian, and Yogo and Fujihara's (2008) participants were Japanese.

6.4.4: Results of individual studies

The following tables (6.3, 6.4 and 6.5) present the methodology and findings of all included studies. The studies have been split into three broad categories of outcomes: physical/ physiological, psychological and cognitive-process outcomes. Some studies appear in more than one table.

Key to Tables 6.3, 6.4 and 6.5

Shaded studies include manipulation of a procedural or methodological variable as an independent variable (IV; e.g. investigation of the effects of different writing instructions), or exploration of the effects of a naturally-occurring variation (e.g. number of writing sessions completed).	

Table 6.3: Effects of BPFS-W on physical and physiological health indicators

Study Authors	Writing instructions and imagery	Setting, number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Austenfeld (2007); Austenfeld & Stanton (2008)	**BPFS:** Standard, with addition of description of how participants would overcome an obstacle. **Control:** Daily activities (DAs). No imagery.	3 20-minute sessions, approx. 1 week apart in a semi-private cubicle	**Medical care utilisation (MCU):** Number of health-centre visits for illness (not injury). **Physical symptoms:** 9-item version of Pennebaker Inventory of Limbic Languidness (PILL; Pennebaker, 1982) **Blood pressure**: taken from arm at 3-minute intervals	MCU: 1 month pre- and post-writing PILL and blood pressure: Baseline and 1 month post-writing	Decrease in physical symptoms in BPFS and control groups. No significant between-group differences in MCU, physical symptoms, or blood pressure (when baseline controlled for).
Austenfeld et al. (2006)	**BPFS:** Personal and professional BPFS. Asked to describe how they would overcome an obstacle. **Control:** DAs No imagery.	3 25-mminute sessions. At least 1 week apart over max. period of 8 weeks, in a laboratory.	**MCU:** Number of visits health-centre for illness (not injury). **Physical symptoms**: 9 item version of PILL	MCU: 3-month period pre- and, 3 months post-writing Physical symptoms: Baseline and 3 months post-intervention	No significant effect on self-reported illness or use of MCU.

Study Authors	Writing instructions and imagery	Setting, number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Harrist et al. (2007)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 20-minute sessions, 4 consecutive days, in a laboratory.	**MCU:** Number of visits to health-centre for illness (not injury)	3-month period prior to the study, and the 3-month period following the study	There was also a modality IV (writing versus talking), which is beyond the scope of this review. Due to there being no significant modality * task (BPFS versus control) interaction, inferential statistics for isolated effects of BPFS-W versus control tasks on MCU were not reported by Harrist et al. (2007). Descriptive statistics suggest greater change from 3 months pre- to 3 months post-writing in BPFS-W participants in comparison to controls. The BPFS group decreased in MCU, and controls increased. These descriptive statistics corroborate outcomes of inferential analyses which demonstrated that BPFS (writing and talking) participants had fewer visits to a health professional 3 months post-writing in comparison to control (writing and talking) participants, when baseline levels were controlled for.
King (2001)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 20-minute sessions, 4 consecutive days, in a laboratory.	**MCU:** Number of visits to health-centre for illness (not injury)	3 months prior to the study, and then a period of 5 months following the intervention.	Significantly fewer visits in BPFS group post-intervention (controlling for pre-study visits) compared to controls. Significant decrease from pre- to post-intervention in BPFS group but not in controls.
Maddalena et al. (2014)	**BPFS:** Standard, with the addition of how they would overcome obstacles. **Control:** DAs. No imagery.	3 20-minute sessions. **One day condition**: 15 minutes apart, in a classroom. **Weekly condition:** once weekly, some in a	**Physical symptoms:** Physical symptoms scale (Reifman, Biernat & Lang, 1991)	Baseline and 1 month post-writing	BPFS improved and controls worsened in physical symptoms. No impact of spacing of writing sessions on effects of BPFS-W or control task.

Study Authors	Writing instructions and imagery	Setting, number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
		classroom and some at home.			
Nazarian & Smyth (2013)	**BPFS:** Standard. **Control:** DAs. No imagery.	3 20-minute sessions, once a week for 3 consecutive weeks, in a laboratory.	**Salivary-cortisol**	Pre- and 10 minutes post-each writing session	No significant main effect of group on change in salivary-cortisol.
Winn & Troop (2002)	**BPFS:** Standard. **Control:** Writing dispassionately about an object or event. No imagery.	3 20-minute sessions, 3 consecutive days. Baseline in a group session. Writing tasks and follow-up at home.	**Physical symptoms:** List of 7 common viral infections, rated on 5-point scale (frequency over past week)	Baseline and follow-up (8-12 weeks following participation).	No significant main effect of group or time on physical symptoms, and no significant group * time interaction
Yogo & Fujihara (2008)	**BPFS:** Standard. **Control:** DAs. No imagery.	3 20-minute sessions. Sessions appear to be over 2 weeks, laboratory.	**Physical symptoms:** 8-item version of PILL	Pre- and post-each writing session.	Decreased physical symptoms in BPFS group after each writing session. Effects relative to the control task (and effects of the control task) unreported.
Ph.D. Study One (2015)	**BPFS Outcome:** Standard **BPFS Process:** Writing about lower order, process goals towards reaching BPFS.	1 20-minute session. Laboratory for baseline measures and writing intervention; online follow-up	**Physical symptoms:** Physical Symptoms Inventory (PSI)- 13 item version (Kessler, Spector, Chang & Parr, 2008; Spector, 2018)	Baseline and 4- and 8-week follow-up	There was also a modality IV in this study (writing versus mental simulation). There was no significant modality * task (BPFS process versus BPFS outcome versus control) interaction effect on physical symptoms, thus inferential statistics for isolated effects of BPFS-W versus control tasks on physical symptoms were not reported. Descriptive statistics indicate general reductions in physical symptoms across all 3 writing groups. However,

Study Authors	Writing instructions and imagery	Setting, number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
	Control: DAs. No imagery[45].				reductions, and between-group differences in change, appear negligible. Descriptive statistics mirror inferential findings that there were no significant main effects of group or time-point and no significant task (BPFS process versus BPFS outcome versus control, across both writing and simulation modalities) * time interaction, when baseline levels of physical symptoms controlled for.
Ph.D. Study Two (2016)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 sessions, each 20 minutes long, 4 consecutive days, online.	**Physical symptoms:** PSI-13 item version	Baseline and 4- and 8-week follow-up	No significant main effect of group or time-point and no significant group * time interaction

[45] Participants were asked to take a few moments to consider what their BPFS is, but this was not a formal visualisation exercise.

Table 6.3 shows the methodological characteristics and results of the 10 studies which involved investigation of the effects of BPFS-W on physiological and physical health outcomes. The measures used as physical health indicators varied. Seven studies used questionnaires and asked participants to rate how regularly they experienced physical symptoms (e.g. Yogo & Fujihara, 2008). The authors of four studies (Austenfeld, 2007; Austenfeld & Stanton, 2008; Austenfeld et al., 2006; Harrist et al., 2007; King, 2001) accessed participants' health-centre records and used frequency of visits as an indicator of physical health. Physiological outcomes (salivary-cortisol and blood pressure) were measured in two studies (Austenfeld, 2007; Austenfeld & Stanton, 2008; Nazarian & Smyth, 2013).

Participants in six studies were given King's (2001) standard BPFS-W instructions (e.g. Winn & Troop, 2002). In Ph.D. Study One, writing instructions were manipulated as an IV; some participants were given the standard instructions, and others wrote about lower-order goals. In two studies (Austenfeld, 2007; Austenfeld & Stanton, 2008; Maddalena et al., 2014), the standard instructions were administered, with the addition of asking participants to write down how they would overcome obstacles to achieve their BPFS. Austenfeld et al. (2006) asked their participants to write about their personal and professional BPFS specifically as well as an obstacle.

Nazarian and Smyth (2013) and Yogo and Fujihara (2008) measured effects only immediately post-intervention. The majority (8 studies) had longer follow-ups, with some collecting responses as soon as four weeks post-intervention (e.g. Ph.D. Study One), and others collecting responses several months post-intervention (e.g. Harrist et al., 2007).

In six studies, writing sessions were administered in a laboratory (Austenfeld et al., 2006; Harrist et al., 2007; King, 2001; Nazarian & Smyth, 2013; Yogo & Fujihara, 2008; Ph.D. Study One). In one study (Austenfeld, 2007; Austenfeld & Stanton, 2008) a semi-private cubicle was used, whereas in Ph.D. Study Two participants wrote online. Maddalena et al. (2014) administered the intervention

in a classroom for some participants, whilst allowing others to write at home. All Winn and Troop's (2002) participants wrote at home.

Studies differed greatly in dosage and spacing of writing sessions. In three studies (Harrist et al., 2007; King, 2001; Ph.D. Study Two), King's (2001) original procedure of four 20-minute writing sessions over four days was used. In Ph.D. Study One, participants wrote for a single 20-minute session. Maddalena et al. (2014) included spacing as an IV and asked some participants to complete three sessions all in the same day, and others to write once a week for three consecutive weeks. The remaining five studies used three writing sessions with spacing ranging from three consecutive days (Winn & Troop, 2002), to three consecutive weeks (Nazarian & Smyth, 2013), to at least a week apart over eight weeks (Austenfeld et al., 2006).

None of these studies supplemented BPFS-W with mental imagery.

Table 6.4: Effects of BPFS-W on psychological well-being indicators

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Aborida (2016)	**BPFS:** 5 minutes about different BPFS sphere daily (career, personal interests, social life, health, romantic life). Then 3 minutes of process towards it. **Control:** DAs. No imagery.	5 8-minute sessions, over 5 consecutive days, online.	**PA and NA**: Positive and negative affect schedule (PANAS; (Watson, Clark & Tellegen, 1988) **Burn-out:** Copenhagen Burnout Inventory (Kristensen, Borritz, Villadsen & Christensen, 2005) **Job affective well-being:** Job Affective Wellbeing Scale (Van Katwyk, Fox, Spector & Kelloway, 2000).	Pre- first writing session and post-final writing session	No significant effect of condition on change in PA and NA, burn-out, or positive and negative job affective well-being.
Austenfeld (2007); Austenfeld & Stanton (2008)	**BPFS:** Standard, with addition of how they would overcome an obstacle. **Control:** DAs. No imagery.	3 20-minute sessions, approx. 1 week apart, in a semi-private cubicle.	**Depressive symptoms:** Centre for Epidemiologic Studies-Depression Scale (CES-D; Radloff, 1977) **Hostility:** Cook-Medley Hostility Scale Revised (Butcher, Dahlstrom, Graham, Tellegen & Kaemmer, 1989; Han, Weed,	All measured at baseline and 1-month follow-up	No significant main effect of condition on any outcome at 1-month follow-up (when baseline controlled for).

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
			Calhoun & Butcher, 1995) and cynicism, hypersensitivity, aggressive responding and social avoidance subscales. **Hostile affect**: Hostility subscale from PANAS-Expanded Form (PANAS-X; Watson & Clark, 1994)		
Austenfeld et al. (2006)	**BPFS:** Personal and professional BPFS, and asked to describe how they would overcome an obstacle. **Control:** DAs. No imagery.	3 25-minute sessions. At least 1 week apart over max. period of 8 weeks. In a laboratory.	**Depressive symptoms:** CES-D **Hostile, sad, fearful and guilty affect:** hostility, sadness, fear, and guilt subscales of PANAS-X.	Baseline and 3-month follow-up	No significant main effect of condition on any outcome at 3-month follow-up (when baseline controlled for)
Boehm et al. (2011)	**BPFS:** BPFS with regards to family, friends, romantic partner, career, health and hobbies (appears to be all spheres	6 10-minute sessions, once weekly for 6 weeks. Online.	**SWL:** Satisfaction with Life Scale (SWLS; Diener, Emmons, Larsen & Griffin, 1985)	Immediately pre- first writing session and post- final writing session, and at 1-month follow-up	BPFS group demonstrated greater increases in SWL over time than controls.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
	in each session). **Control:** DAs. No imagery.				
Boselie et al. (2017)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately post-writing.	1 15-minute session (plus 1 minute to think what to write), in a laboratory	**P-FEX and N-FEX**: Future-expectancies Scale (FES; adaptation of MacLeod's (1996) Subjective Probability Task (SPT) by Hanssen et al. (2013). **PA and NA**: PANAS	Pre- and post-writing	Higher P-FEX and lower N-FEX, and higher PA in BPFS group in comparison to controls (when pre-writing levels controlled for). No significant difference in NA.
Boselie et al. (2016a)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately post-writing.	1 15-minute session (plus 1 minute to think what to write), in a laboratory.	**P-FEX and N-FEX:** FES **PA and NA:** PANAS	Pre- and post-writing	Higher P-FEX and PA, and lower N-FEX in BPFS group in comparison to controls (when pre-writing levels controlled for). No significant between-group difference in NA.
Boselie et al. (2016b)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately post-writing.	1 15-minute session (plus 1 minute to think what to write), in a laboratory.	**P-FEX and N-FEX:** FES **PA and NA**: PANAS	Pre- and post-writing	Higher P-FEX and PA, and lower N-FEX in BPFS group in comparison to controls (when pre-writing levels controlled for). No between-group difference in NA.
Boselie et al. (2014)	**BPFS:** Standard. **Control:** DAs.	1 15-minute session (plus 1	**P-FEX and N-FEX**: FES	Pre- and post-writing	For P-FEX, N-FEX and PA, there was a significant time (pre- versus post-writing) * group (BPFS versus control)

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
	5 minutes of imagery immediately post-writing.	minute to think what to write), in a laboratory.	**PA and NA:** PANAS		interaction. There was an increase in P-FEX and decrease in N-FEX from pre- to post-writing in BPFS group but not in controls. There was a significant reduction in PA from pre- to post-writing in the control group but not in the BPFS group. Significant reduction in NA from pre- to post-writing in both groups, but no between-group difference.
Frein & Ponsler (2014)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 15-minute sessions, 4 consecutive days. Setting unreported.	**PA and NA**: PANAS	Pre- and post-each writing session	Average change score (post- minus pre-writing) demonstrated that there was a significantly greater increase in PA in the BPFS group in comparison to controls. There was no between-group difference in change in NA.
Geschwind et al. (2015)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately following writing.	1 session, 15 minutes long (plus 1 minute to think what to write), in a laboratory.	**PA and NA**: Modification of the Differential Emotions Scale (DES; Izard, Dougherty, Bloxom & Kotsch, 1974; cited by Geschwind et al., 2015). Modified version by Schaefer et al. (2003).	Pre- and post-writing	Significant time * group interaction for PA but not NA. Higher PA in BPFS-W group than controls post-writing (but not pre-writing).
Hanssen et al. (2013)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately	1 session, 15 minutes long (plus 1 minute to think what to write), in a	**P-FEX and N-FEX:** FES **PA and NA:** Visual analogue scales (VASs; 0-100); how	Pre- and post-writing	Greater increase in P-FEX and PA, and greater decrease in N-FEX in BPFS group in comparison to controls. No significant between-group difference in change in NA.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
	following writing.	laboratory.	positive and negative participants were feeling in that moment.		
Harrist et al. (2007)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 20-minute sessions, 4 consecutive days, in a laboratory.	**PA and NA:** Affect-Adjective Scale (AAS; Diener & Emmons, 1984)	Pre- and post-each writing session (averaged to give a single pre- and post-score for PA and NA, respectively)	There was also a modality IV (writing versus talking), which is beyond the scope of this review. Due to there being no significant modality * task (BPFS versus control) interaction, inferential statistics for isolated effects of BPFS-W versus control tasks on PA and NA were not reported by Harrist et al. (2007). Descriptive statistics indicate a greater change in PA and NA from pre-to-post-writing in the BPFS group than controls. In the BPFS group there was an increase in PA and a decrease in NA. In the control group there was a decrease in PA and no change in NA, from pre- to post-writing. These descriptive statistics corroborate the inferential findings that BPFS (writing and talking) participants had significantly higher post-test PA, and lower post-test NA, than control (writing and talking) participants (when baseline controlled for).
King (2001) [46]	**BPFS:** Standard. **Control:** DAs. No imagery.	4 20-minute sessions, 4 consecutive	**PA:** AAS (PA= average PA minus average NA)	Pre- and post-each writing session	No significant difference between BPFS (only) group and controls in PA (when baseline controlled for).

[46] For the benefit of continuity with the rest of the thesis, clarification is needed. There was a significant main effect of BPFS-W on PA, but this effect included participants from a combination group, which involved writing about both a BPFS and trauma, as well as participants who only wrote about a BPFS. King (2001) also measured the impact of BPFS-W on psychological wellbeing (a composite created from the Life Orientation Test (LOT; Scheier & Carver, 1985) and SWLS). However, effects of BPFS-W are reported by King (2001) as the effects of the BPFS-only and BPFS-trauma combination conditions combined (with no descriptive statistics for this variable reported). These details have not been presented here, as the review inclusion criteria specify that only data specific to BPFS-W (with no contamination from other interventions) may be included.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
		days. Laboratory.			
Layous et al. (2013)	**BPFS:** Writing about a different sphere of BPFS each session; academic, social, career, health, as well as about a process goal that they need to achieve to realise it. **Control:** DAs. No imagery.	4 15-minute sessions (10 minutes BPFS outcome, 5 minutes about a process goal), 4 consecutive weeks. **In-person condition:** Small groups (4-10) **Online condition:** online.	**PA:** AAS **Need-satisfaction:** Measure of autonomy, relatedness and competence as well as need-satisfaction composite (Sheldon, Elliot, Kim & Kasser, 2001).	Pre- first writing session and post- final writing session	Larger increases in PA from pre-to-post-writing in BPFS group in comparison to controls. No significant between-group difference in change in overall need-satisfaction, autonomy, competence or relatedness. No significant differences between online and in-person conditions.
Liau et al. (2016)	**BPFS:** Standard, with suggested domains (family, social life, work/ studies, recreational/ leisure activities, health). **Control:** DAs. No imagery.	2 writing sessions, which took ‘’around 20 minutes’’, 1 month apart, in a classroom.	**PA and NA:** PANAS **SWL:** Brief Multidimensional Students' Life Satisfaction Scale (Seligson, Huebner & Valois; 2003) **Optimism:** Life Orientation Test-Revised (LOT-R	PANAS (present moment) completed pre- and immediately post- each writing session. All other measures completed pre- first writing	Significant main effect of time on PA and NA, with both decreasing from pre- first session to post- first session. No time * group interaction for PA, but there was a significantly greater decrease in NA in BPFS group in comparison to controls. At the second writing session, there was a significant decrease in PA from pre-to-post-writing in both groups, but no time * group interaction. No significant main effect of time or time * group interaction for NA.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
			Scheier, Carver and Bridges, 1994) Optimism subscale **Depressive symptoms:** CES-D	session and post- second writing session.	There was a significant main effect of time for SWL, optimism and depression; there was a significant decrease in SWL and optimism and a significant increase in depressive symptoms from pre- first session to post-second session. However, there was no time * group interaction.
Lyubomirsky et al. (2011)	**BPFS:** Writing about a different BPFS sphere weekly; romantic life, educational attainment, hobbies, family life, career, social life, community involvement, health. **Control:** DAs. No imagery.	8 sessions, each lasting 15 minutes. Once a week for 8 consecutive weeks, online.	**Well-being composite, made up of: Unpleasant and pleasant affect:** rated 6 adjectives according to how often they had experienced them over past week; content, happy, pleased, miserable, unhappy, troubled **SWL:** SWLS **Happiness:** Subjective Happiness Scale (SHS; Lyubomirsky & Lepper, 1999).	All measured at baseline (1 week pre- and immediately pre-first writing session; combined for stable baseline measure), immediately after the eighth writing session, and at 6-month follow-up.	Inferential statistics for isolated effects of BPFS versus control unreported, as BPFS participants' data was grouped with data from participants who completed a gratitude task. No analyses were conducted to explore differences between BPFS and gratitude groups. Mean change-scores suggest that from baseline to post- eighth session, there was a decrease in well-being in BPFS and control groups, but the decrease was larger in the control group. From baseline to 6-month follow-up, there was an increase in well-being in the BPFS group, and a decrease in controls. A significant difference in well-being change between BPFS and control groups appears unlikely. In inferential analyses of effects of BPFS and gratitude conditions combined compared to controls, there was no significant difference at either time-point. The difference between the mean changes-scores for gratitude and BPFS (combined) and controls was non-significant and greater than the difference between the mean change-scores for BPFS and control groups.[47]

[47] Lyubomirsky et al. (2011) note that they also conducted a nine-month follow-up. Analyses are unreported; the authors state that the pattern of results was comparable, but that effects were weaker and some were non-significant. Little can be accurately inferred from this.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Manthey et al. (2016)	**BPFS:** A different sphere of BPFS each week; partnership and romantic life, hobbies, family, friendship and social relationship, clubs, networks, groups and community involvement, health, job and career, and a free topic. **Control:** Writing 5 important tasks for the week. No imagery.	8 sessions, once a week over 8 weeks. No timing reported. Online.	**PA and NA:** Scale of Positive and Negative Experience (SPANE; Diener et al., 2009). German translation. **SWL:** SWLS (German version; Glaesmer, Grande, Braehler & Roth, 2011). **Depression:** State-Trait-Anxiety-Depression Inventory (STADI; Laux, Hock, Bergner-Köther, Hodapp & Renner, 2013); State Euthymia (reverse-scored) and State Dysthymia subscales used to generate depression score.	All measured pre- first writing session and post- final writing session, and at 4-week follow-up	Inferential statistics for isolated effects of BPFS versus control tasks unreported, as BPFS participants' data was grouped with data from participants who completed a gratitude task. Descriptive statistics (mean scores at each time-point) suggest that there was a greater increase from pre- first session to post- final session, and pre- first session to follow-up, in the BPFS group in comparison to controls in SWL and PA. For NA and depression, there was a greater change in the BPFS group in comparison to controls. BPFS participants decreased in symptoms at both time-points. Control participants increased in symptoms from pre- first session to post- final session. Controls decreased from pre-first session to follow-up, but this decrease was smaller than that of the BPFS group. The patterns reported from the descriptive statistics mirror the inferential statistics performed to compare the effects of BPFS and gratitude groups (combined) to the control group. There were greater increases in SWL and PA, and significantly greater decreases in depression and NA from pre-to-post-writing, in the intervention groups in comparison to controls. There was no main effect of time (post- final writing session versus follow-up) and no time * group (intervention versus control) interaction, thus effects were maintained 1 month after the intervention.
Meevissen et al. (2011)	**BPFS:** Writing down aspects of BPFS across 3 spheres;	Single session, 20 minutes for writing the	**Optimism**: Life Orientation Test (LOT) Dutch translation. **P-FEX and N-FEX:**	Dispositional optimism: Baseline (3 days pre-writing) and	Immediately post-writing, significantly greater PA and P-FEX, and lower N-FEX, in BPFS participants than controls (when baseline controlled for). No significant between-group difference in NA.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
	personal, relational, professional, (starting each sentence with 'In the future I will'), followed by writing these statements in the form of a story. **Control**: DAs. 5 minutes of imagery immediately following writing, then 5 minutes daily for 2 weeks.	statements. No time given for writing the statements in the form of a story. Writing task and first imagery in the laboratory. Baseline measures, additional imagery, and follow-ups at home, other than the 2-week follow-up for PA, NA, P-FEX and N-FEX which were completed in the laboratory.	SPT **Optimistic explanatory-style:** Attributional Style Questionnaire (ASQ; Seligman, Abramson, Semmel & von Baeyer, 1979; Dutch version; Cohen, van den Bout, Kramer & Vilet, 1986) **PA and NA:** Shortened PANAS (Mackinnon et al., 1999). Dutch translation **Neuroticism:** Eysenck Personality Questionnaire-Neuroticism subscale, Dutch translation (EPQ-N; Eysenck, Eysenck, & Barrett, 1985; Eysenck & Eysenck, 1994), short form (Birley et al., 2006).	1- and 2-week follow-ups P-FEX and N-FEX: Immediately pre- and post-writing and imagery and 1- and 2-week follow-ups Optimistic explanatory-style: Baseline and 2-week follow-up PA and NA: Baseline, Immediately pre- and post-writing and imagery session, and 1- and 2-week follow-ups. Neuroticism: Baseline and assumed measurement at 1 and 2 weeks based on results	At 1- and 2-week follow-ups, there was no main effect of group (BPFS versus control) in P-FEX, neuroticism, or NA (when baseline controlled for). There was also no significant main effect of group on optimistic explanatory-style at the 2-week follow-up. There was, however, greater PA and optimism and lower N-FEX at 1 and 2-week follow-ups in BPFS participants than controls. Effects from time * group interactions are unreported, but are assumed significant due to the post-hoc, within-group analyses performed. Results from post- hoc analyses in this study should be treated with caution. This is particularly so for optimistic explanatory-style; effects on this variable were analysed with a one-way ANCOVA (baseline included as covariate), thus no interaction effect was analysed. Within-group analyses demonstrated that BPFS participants showed increases in optimism, optimistic explanatory-style and P-FEX, and decreases on N-FEX and NA from baseline to 1- and 2-week follow-up. No change in PA or neuroticism. Controls showed no change in optimism, optimistic explanatory-style, P-FEX and N-FEX or neuroticism. However, controls significantly decreased in NA and PA. (Appears BPFS activity buffered against decrease in PA). Further post-hoc analyses showed effects on PA, P-FEX and N-FEX occurred 1 week post-writing. Effects on optimism not apparent until 2 weeks post-writing.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
				section, but this is unreported.	
Murn (2014)	**BPFS:** Standard. **Control:** DAs. No imagery.	3 20-minute sessions, 3 consecutive days. Online (computer laboratory).	**Self-esteem:** Rosenberg Self-Esteem Scale (Rosenberg, 1965). **Body-comparison:** Appearance, muscularity, weight subscales of Body Comparison Scale (Fisher, Dunn & Thompson, 2002). **Body-esteem:** Appearance and functionality subscales (Franzoi, 1995) from Body-Esteem Scale (Franzoi & Shields, 1984), and body-esteem grand mean. **Self-compassion:** Self-kindness vs. self-judgement, common-humanity vs. isolation, mindfulness vs. over-identification subscales of Self Compassion Scale,	All pre- first writing session and post- final writing session, and 6-10-week follow-up.	Greater increase in self-esteem from pre- first writing session and post- final writing session, and from post- final writing session to follow-up, in the BPFS group in comparison to controls. No significant between-group difference over time in frequency of body-comparison and body-esteem. There was also no significant between-group difference in self-compassion over time.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
			and self-compassion grand mean (Neff, 2003a).		
Nazarian & Smyth (2013)	**BPFS:** Standard. **Control:** DAs. No imagery.	3 20-minute sessions, one weekly over 3 weeks. Laboratory.	**PA and NA:** PANAS	Pre- and post-each writing session	No significant time * group interaction for PA (based on averages of pre- and post-writing scores). Increase in PA marginally greater in BPFS-W group than controls (p=.05). No significant between-group difference in change in NA.
Ng (2016)	**BPFS:** Standard. **Control:** Writing about the details of a place they visited earlier. No imagery.	At least one session; asked to continue over the next 3 weeks. First session in a laboratory, others at home. No timing reported.	**Happiness:** SHS	Baseline and 3-week follow-up.	Ng (2016) included a main effect of BPFS-W in comparison to the control task on happiness as a hypothesis. However, this was not reported. It is assumed that this was non-significant, given that all other predictors included in a regression were significant and reported. Descriptive statistics for happiness for BPFS and control groups were split based on those above and below a mean neuroticism score. Numbers of participants above and below the mean were not reported, thus it is impossible to calculate the true group means. However, assuming there are relatively equal numbers above and below the mean, it is unlikely that there would be a significant difference between the BPFS group and controls in happiness, based on the descriptive statistics reported. This inference should be treated with caution.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Odou & Vella-Brodrick (2013); Seear & Vella-Brodrick (2013)	**BPFS:** Different sphere of BPFS (of their choice) each day. **Control:** No activity. BPFS group told to imagine BPFS pre-writing. No time-limits for imagery reported.	7 sessions. No time-limits reported, 7 consecutive days. Optional continuation for 2 weeks. Continuation (and reported time spent) included as IV. Online.	**PA and NA:** PANAS **Mental well-being:** Warwick-Edinburgh Mental Well-being Scale (Tennant et al., 2007).	Pre- first writing session and post- final writing session, as well as at 2-week follow-up.	No significant main effect of group in change in PA from baseline to post- final writing session.[48] No significant main effect of group on mental well-being at post-test (when baseline levels were controlled for). However, there was significantly lower NA after the final writing session in the BPFS group in comparison to controls (when baseline levels controlled for). No significant main effect of group on any outcome at 2-week follow-up (baseline levels were controlled for). No significant effect of whether participants had continued writing following the initial 7 days on PA, NA or mental well-being.
Peters et al. (2010)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery post-writing.	Single 15-minute session, plus 1 minute to think about what to write. In classroom.	**State optimism:** Two items asking how they feel about their future, and what their expectations for the coming week are **PA and NA:** PANAS-Short form **P-FEX and N-FEX:** SPT	PA, NA, P-FEX and N-FEX measured pre- and post-writing. State optimism measured post-writing only.	For PA, P-FEX and N-FEX, there was a significant time (pre- versus post-writing) * group (BPFS versus control) interaction. PA and P-FEX significantly increased after the BPFS task but not the control task. N-FEX significantly decreased in both the BPFS and control groups. There was a significantly greater decrease in N-FEX in the BPFS group in comparison to controls. For NA there was only a significant main effect of time; NA significantly decreased in both groups. There was no significant time * group interaction. No significant between-group differences in state optimism.

[48] Effects on PA analysed using ANOVA and change-scores, because the assumption of homogeneity of regression slopes was violated. NA and well-being analysed using ANCOVA.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Peters et al. (2013)	**BPFS:** Asked to think about how they would like to be remembered at the end of their lives, and write about BPFS across personal, relational, professional spheres. Then asked to select the 2 most important aspects from each sphere and write them as statements, starting with 'In the future I will…'. **Control:** DAs across 3 spheres. 5 minutes of imagery at end of session, then imagined one of the 6 statements (generated after writing) each day.	Single 15-minute session. 5 minutes about each sphere. Writing session, final imagery and 1-week follow-up in laboratory. Daily imagery at home. 2-week follow-up over e-mail.	**SWL:** SWLS **Optimism:** LOT-R **Optimistic explanatory-style:** ASQ	Baseline (the day before writing) and post- final imagery (1-week follow-up), and then at the 2-week follow-up (2 weeks after writing session).	Significantly greater increase in SWL from baseline to 1-week follow-up in the BPFS group in comparison to the control group. No significant differences between groups in change in SWL from baseline to 2-week follow-up. SWL significantly increased in a linear trend across time in the BPFS group but not in controls. Significantly greater increase in optimistic explanatory-style from baseline to 1-week and 2-week follow-up in the BPFS group in comparison to controls. There was a significant quadratic change in optimistic explanatory-style in the BPFS group but not controls; there was an increase from baseline to post-imagery, which reduced between 1-week and 2-week follow-up. No difference between BPFS and controls in change in optimism from baseline to 1-week follow-up (both groups increased in optimism), but there was a significant difference at 2-week follow-up; BPFS participants had a greater increase in optimism than controls. Optimism significantly increased in linear trend across time in BPFS but not control group.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Peters et al. (2016)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately following writing.	Single session, 15 minutes long, plus 1 minute of thinking about what to write. In laboratory.	**PA and NA:** PANAS (German version; Krohne, Egloff, Kohlmann & Tausch, 1996). **P-FEX and N-FEX:** FES-German translation.	Pre- and immediately post-writing, and 20 minutes later.	Significant time (pre- versus immediately post- versus 20 minutes post-writing) * group (BPFS versus control) interaction for P-FEX, N-FEX and PA. There was significantly higher PA immediately post-writing in comparison to pre-writing in both BPFS and control groups. The change in the BPFS group was significantly greater than the change in the control group. PA levels had returned to pre-writing levels 20 minutes later. Significantly greater P-FEX, and significantly lower N-FEX, both immediately following and 20 minutes post-writing in comparison to baseline in the BPFS group. There was no such difference in the control group. No significant change in NA over time, in both groups. No significant time * group interaction for NA.
Renner et al. (2014)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately following writing.	Single 15-minute session, laboratory.	**PA and NA:** PANAS, Dutch version (Engelen, De Peuter, Victoir, Van Diest, & Van den Bergh, 2006). **Specific moods:** Positive-negative, dull-glad, anxious-secure, happy-sad VASs. **Perfectionist and dependent attitudes:** Dysfunctional Attitude Scale-Revised (de Graaf, Roelofs & Huibers, 2009).	PANAS and VASs measured pre- and post- a negative mood induction (NMI), and post-writing. Perfectionist and dependent attitudes measured pre- and post-writing.	Greater increase in PA from pre- to post-writing in BPFS group in comparison to controls. No significant between-group difference in change in NA. Greater decrease in negative emotions on positive-negative, happy-sad, and dull-glad scales in BPFS group than controls. No significant between-group difference in change on anxious-secure scale or perfectionist attitudes. BPFS participants showed significantly (marginally, p=.05) less change in dependent attitudes in comparison to controls; means demonstrate BPFS participants increased and controls decreased in dependency.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Shapira & Mongrain (2010)	**BPFS:** Writing about BPFS with examples of what they might include (life in general, family, work life, social life). Told to imagine that they have resolved some current concerns. **Control:** Early memory. No imagery.	7 sessions, over 7 consecutive days. No time-limit reported. E-mailed 2 and 4 months post-writing to encourage continuation. Online.	**Depression:** CES-D **Happiness**: Steen Happiness Index (Seligman, Steen, Park & Peterson, 2005)	Pre- first writing session and post- final writing session, and at 1-month, 3-month, and 6-month follow-ups.	BPFS group reported significantly lower depression in comparison to controls at the 1-month and 3-month follow-ups (this had not yet emerged immediately post- final session, and the effect had dissipated by the 6-month follow-up). BPFS group reported greater happiness in comparison to controls immediately post-writing, and at 3-month and 6-month follow-ups. There was no significant between-group difference at the 1-month follow-up.
Sheldon & Lyubomirsky (2006)	**BPFS:** Standard. **Control:** DAs. No imagery.	Single session, timing unreported. Encouraged to write again over following 4 weeks. Laboratory (in group) for first session, then online.	**PA and NA:** PANAS	Immediately pre- and post-first writing session, and at 2- and 4-week follow-up (averaged to produce one follow-up score).	Significantly greater initial change in PA from pre- to post-first writing session in BPFS group in comparison to controls. BPFS group increased, and control group decreased, in PA. Significant decrease in NA from pre- to post- first writing session in both BPFS and control groups, but no significant between-group difference. The authors did not perform inferential analyses of the effect of condition on follow-up PA or NA, but it is possible to tentatively suggest that descriptive statistics indicate no significant differences between BPFS and control groups; PA had dropped in BPFS participants to a level comparable to controls. NA had risen in both groups at follow-up, but the between-group difference was smaller than the non-significant between-group difference immediately post-writing.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Titova et al. (2017)	**BPFS:** Standard. **Control:** DAs. No imagery.	Single session, 10-15 minutes long. Online.	**PA and NA:** General PA and NA, guilt, sadness, joviality, self-assurance and serenity subscales of the PANAS-X	Pre- and post-writing.	Significantly higher PA in BPFS group following writing in comparison to controls, but no significant main effect of group in NA (when baseline levels controlled for).
Troop et al. (2013)	**BPFS:** Standard. **Control:** Writing dispassionately about a book or a film. No imagery.	3 15-minute sessions, in one day with five minute breaks between sessions. In small groups.	**Stress:** Revised Hassles and Uplifts Scale (DeLongis, Folkman & Lazarus, 1988) **PA:** Activating, Relaxed and Safe/ Content PA subscales of Types of Positive Affect Scale (Gilbert et al., 2008) **Self-criticism/ self-reassurance:** Forms of Self-Criticizing/ Attacking and Self-Reassuring Scale (Gilbert, Clarke, Hempel, Miles & Irons, 2004).	Baseline and 2-week follow-up.	No significant effects or interactions of time and group on stress (on neither hassles nor uplifts). There was a significant effect of subscale; participants reported greater uplifts than hassles. Significant increase from baseline to follow-up in relaxed PA, but no difference between groups. No significant effects or interactions of time and group on activating PA or safe/ content PA. For self-criticism/ self-reassurance, there was a significant time (baseline vs. follow-up) * subscale (self-criticism vs. self-reassurance) * group (BPFS vs. control) interaction. Post-hoc analyses revealed a significant decrease in self-criticism in BPFS group but not in controls, and a significant decrease in self-reassurance in controls but no change in BPFS group.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Vaughn et al. (2003)	**BPFS Outcome:** Standard **BPFS Process:** First 7 minutes or so writing about BPFS followed by two sets of 7 minutes about what they could be doing in 10 and 20 years to achieve it. **Control:** DAs. 5 minutes of imagery immediately pre-writing.	Single session, 20 minutes (‘‘or so’’). Setting unreported.	**Psychological well-being:** Composite formed from average of scores from SWLS and Personal Growth Initiative Scale (PGIS) (Robitschek, 1998)	Outcomes appear to have been measured at a 4- to 7-week follow-up only.	Greater psychological well-being in BPFS outcome group in comparison to BPFS process and control groups (when levels of baseline optimism (LOT) controlled for).
Winn & Troop (2002)	**BPFS:** Standard. **Control:** Writing dispassionately about an object or event. No imagery.	3 20-minute sessions, 3 consecutive days. Baseline in a group session. Writing tasks and follow-up at home.	**PA and NA**: PANAS	Immediately pre- and post-each writing session.	Significant group * time interaction; PA increased from pre- to post-writing in BPFS group but not controls. NA decreased from pre- to post-writing in both groups; no significant main effect of group or group * time interaction.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
Yogo & Fujihara (2008)	**BPFS:** Standard. **Control:** DAs. No imagery.	3 20-minute sessions, appears to be over 2 weeks. Laboratory.	**Depressed/ anxious affect:** Multiple Mood Scale (MMS) Depression/ Anxiety subscale (Terasaki, Kishimoto & Kogo, 1992). **Hostile affect**: Multiple Mood Scale-Hostility subscale (Terasaki et al., 1992).	Pre- and post-each writing session.	BPFS group had decreased depressed/ anxious affect scores after each writing session. No significant change in hostile affect from pre- to post-writing in the BPFS group.[49] Effects on depressed/ anxious affect in the BPFS group relative to the control group (and effects of the control task) unreported.
Ph.D. Study One (2015)	**BPFS Outcome:** Standard **BPFS Process:** Writing about lower order, process goals towards reaching BPFS. **Control:** DAs. No imagery.	1 20-minute session. Laboratory for baseline measures, writing task, and immediate post-test affect; online follow-up.	**PA and NA:** PANAS **Depression, anxiety and stress**: Depression, anxiety and Stress subscales from the DASS-21	PA and NA measured at baseline (immediately pre-writing) and immediately post-writing. Depression, anxiety and stress measured at baseline and 1-, 4-, and 8-week follow-ups.	There was also a modality IV (writing versus mental simulation). Due to there being no significant modality * task (BPFS process versus BPFS outcome versus control) interaction on PA, NA, depression, anxiety and stress, inferential statistics for isolated effects of BPFS-W versus control were not reported. Descriptive statistics indicate reductions in NA in both BPFS-W groups, and an increase in NA in controls, from pre- to post-writing. All differences are small. There were larger changes in PA; there was an increase in PA in both BPFS groups, and a (smaller) decrease in controls. Descriptive statistics mirror inferential findings. There was no significant main effect of task (BPFS process versus BPFS outcome versus control, across both writing and simulation modalities) on NA, when baseline controlled for. There was a significant main effect of task for PA; BPFS outcome and process (writing and simulation)

[49] This lack of a significant change in hostile affect has been assumed, because Yogo and Fujihara (2008) appear not to have reported non-significant effects.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing tasks	Outcomes and Measures	Measurement time-points	Effects
					groups had significantly greater levels of PA than the control (writing and simulation) group, when baseline controlled for. In general, descriptives for depression, anxiety and stress specific to BPFS-W and control writing participants did not suggest large differences between groups in mean change-scores or large within-group changes over time. These patterns mirror inferential findings of no significant differences between BPFS and control (writing and simulation) groups, as well as no significant main effect of time, and no group * time interaction (when baseline levels controlled for).
Ph.D. Study Two (2016)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 20-minute sessions, 4 consecutive days. Online.	**PA and NA:** PANAS **Optimism:** LOT **SWL:** SWLS **Psychological well-being:** Composite formed from the LOT and the SWLS	PANAS measured pre- and post- each session. Psychological well-being measured at baseline (immediately pre- first writing session), and at 4- and 8-week follow-ups.	There was no significant main effect of group, and no main effects of the day of measurement or time (pre- versus post-writing) on PA. There was a significant time * group interaction. Post-hoc analyses indicated no significant between-group difference in PA either pre- or post-writing. However, there was significantly lower PA post-writing in comparison to pre-writing in the control group, but not in the BPFS group. For NA, there was no between-group difference, and no difference between pre- and post-writing. There was lower NA across both conditions on day 2 than days 1 and 4. No significant main effects of time or group on psychological well-being (or LOT and SWL), and no significant interaction (when baseline controlled for).

Table 6.4 shows the methodological characteristics and findings of the 35 studies which included investigation of the effects of BPFS-W on psychological well-being. Most studies measured general PA (26) and NA (23) (e.g. Aborida, 2016; Frein & Ponsler, 2014). Optimism (5) and related variables (positive and negative future-expectancies (8) and optimistic explanatory-style (2)) were also commonly-measured (e.g. Liau et al., 2016; Hanssen et al., 2013; Peters et al., 2013). Hostility was assessed in three studies (Austenfeld, 2007; Austenfeld & Stanton, 2008; Austenfeld et al., 2006; Yogo & Fujihara, 2008). Several studies included measurement of symptoms of psychological illness (e.g. Manthey et al., 2016; Shapira & Mongrain, 2010; Troop et al., 2013; Yogo & Fujihara, 2008), specifically depression (6), anxiety (2) and stress (2). Murn (2014) and Troop et al. (2013) measured self-compassion (and self-criticism/ self-reassurance). Burn-out and job affective well-being (Aborida, 2016), neuroticism (Meevissen et al., 2011), fear and guilt (Austenfeld et al., 2006), self-esteem, body-esteem and body-comparison (Murn, 2014), mental well-being (Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013), need-satisfaction (Layous et al., 2013) and perfectionist and dependent attitudes (Renner et al., 2014) were each measured in single studies. Austenfeld et al. (2006), Ng (2016), Renner et al. (2014) and Shapira and Mongrain (2010) measured happiness and happy and sad affects. The authors of five studies used a measure of life-satisfaction (e.g. Boehm et al., 2011). In the studies by Lyubomirsky et al. (2011) and Vaughn et al. (2003), as well as in Ph.D. Study Two, composites created from other measures of psychological well-being were used (although in Ph.D. Study Two, the impacts of the BPFS-W intervention on each of the variables included in the composite were analysed separately). All psychological well-being outcomes across all studies were measured using self-report questionnaires.

The majority (21) of studies administered King's (2001) original, standard writing instructions. In Austenfeld's (2007) and Austenfeld and Stanton's (2008) study, participants were given the standard instructions but were asked to also write about overcoming an obstacle. In the remaining studies, there was great variation in the writing instructions presented to participants. In eight studies, participants were told to write about specific areas of their BPFS (e.g. personal,

relational and professional; Aborida, 2016; Meevissen et al., 2011; Layous et al., 2013). Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) asked participants to write about a different sphere of their choice for each session. Liau et al. (2016) and Shapira and Mongrain (2010) gave participants prompts with regards to what areas of their future they might like to focus on but told them that they could write about whatever sphere that they wished. Three of these studies required inclusion of the process goals that participants would need to achieve or obstacles that they would need to overcome (Aborida, 2016; Austenfeld et al., 2006; Layous et al., 2013). In Ph.D. Study One writing instructions were manipulated as an IV as previously descibed. Vaughn et al. (2003) used a similar manipulation.

The majority (19) of studies investigated only the immediate effects of BPFS-W on psychological well-being outcomes (e.g. Aborida, 2016; Geschwind et al., 2015). Six studies investigated only longer-term effects (e.g. Ng, 2016; Vaughn et al., 2003). The remaining studies measured both the immediate and sustained impact of BPFS-W on psychological well-being (e.g. Lyubomirsky et al., 2011; Murn, 2014). In those studies which investigated sustained effects (14), the time-span ranged from one week (e.g. Ph.D. Study One) to six months post-writing (Lyubomirsky et al., 2011).

The authors of 17 studies administered the intervention to participants individually in a laboratory (although Meevissen et al. (2011) and Peters et al. (2013) also asked participants to complete mental imagery at home, and Ng (2016) asked participants to continue with the writing tasks at home). In nine studies, the intervention was online (e.g. Shapira & Mongrain, 2010; Titova et al., 2017). In Austenfeld's (2007) and Austenfeld and Stanton's (2008) study, BPFS-W was administered in a semi-private cubicle, and in Liau et al.'s (2016) and Peters et al.'s (2010) studies it was administered in a classroom. Winn and Troop's (2002) participants wrote at home. In Sheldon and Lyubomirsky's (2006) and Troop et al.'s (2013) studies, BPFS-W was administered in groups. Layous et al. (2013) investigated the impact of whether participants completed the writing task in-person or online. Frein and Ponsler (2014) and Vaughn et al. (2003) did not report the intervention setting.

Number, length and spacing of writing sessions varied greatly. King's (2001) protocol was administered in a minority of studies (Harrist et al., 2007; King, 2001; Ph.D. Study Two). A large proportion (14) required participants to complete a single writing session (e.g. Boselie et al., 2017; Geschwind et al., 2015), with the authors of two further studies administering a single formal session but encouraging participants to repeat it over the following four weeks (Ng, 2016; Sheldon & Lyubomirsky, 2006). In the remaining 16 studies, participants were required to complete between two (Liau et al., 2016) and eight writing sessions (Manthey et al., 2016). The length of sessions ranged from eight minutes (Aborida, 2016) to 25 minutes (Austenfeld et al., 2006). In six studies, the length of writing sessions was not reported (e.g. Manthey et al., 2016; Ng, 2016). The amount of time between sessions ranged from five minutes (Troop et al., 2013) to one month (Liau et al., 2016) across studies with multiple sessions.

In 13 studies BPFS-W was supplemented with mental imagery. In nine of these, the imagery lasted five minutes, was performed immediately post-writing, and involved imagining what participants had just written about (e.g. Geschwind et al., 2015; Hanssen et al., 2013). In Peters et al.'s (2013) and Meevissen et al.'s (2011) studies, participants completed five minutes of imagery immediately post-writing, and then continued with the imagery daily for one and two weeks, respectively. Conversely, in Odou and Vella-Brodrick's (2013) and Seear and Vella Brodrick's (2013), and Vaughn et al.'s (2003) studies, participants imagined their BPFS (or the control topic) immediately before writing.

Table 6.5: Effects of BPFS-W on cognitive-process outcomes

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing sessions	Outcomes and measures	Measurement time-points	Effects
Aborida (2016)	**BPFS:** 5 minutes of writing about different BPFS sphere daily (career, personal interests, social life, health, romantic life). Then 3 minutes of process towards it. **Control:** DAs. No imagery.	5 8-minute sessions, over 5 consecutive days, online.	**Work-related flow:** Work-Related Flow Inventory (Bakker, 2008)	Immediately pre first writing session, and immediately post final writing session.	No significant between-group difference in change in work-related flow from pre- first writing session to post- final writing session.
Boselie et al. (2017)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately post-writing.	1 15-minute session, (plus 1 minute to think what to write), in a laboratory	**Set-shifting:** Task Shifting Paradigm (e.g. Monsell, 2003)	Post-writing scores only included in analyses.	No significant main effect of group on set-shifting
Boselie et al. (2016a)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery	1 15-minute session, (plus 1 minute to think what to write), in a laboratory.	**Working-memory:** 2-back task (e.g. Jaeggi, Buschkuehl, Perrig & Meier, 2010)	Pre- and post-writing	Inferential analyses for the main effect of group on working-memory were not reported. Descriptive statistics suggest no significant between-group difference immediately post-writing. The difference between BPFS and control means was similar to a non-significant difference in means between

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing sessions	Outcomes and measures	Measurement time-points	Effects
	immediately post-writing.				levels of another IV (beyond the scope of the review).
Boselie et al. (2016b)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately post-writing.	1 15-minute session (plus 1 minute to think what to write), in a laboratory.	**Working-memory:** 2-back task	Only post-writing scores included in analyses.	No significant main effect of group on working-memory.
Boselie et al., 2014)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately post-writing.	1 15-minute session (plus 1 minute to think what to write), in a laboratory.	**Working-memory:** Arithmetic operation-word memory span task (OSPAN; Turner & Engle, 1989).	Only post-writing scores included in analyses.	Inferential analyses for the main effect of group on working-memory were not reported. However, the descriptive statistics demonstrate comparable means across groups. It appears unlikely that the difference in means was significant.
Layous et al. (2013)	**BPFS:** Writing about a different sphere of BPFS each session; academic, social, career, health, as well as about a process goal that they need to achieve to realise it.	4 15-minute sessions (10 minutes writing about BPFS outcome, 5 minutes about a process goal), over 4 consecutive weeks. **In-person condition:** Small groups (4-10 in each) **Online condition:** online	**Flow:** 5-item scale assessing the extent to which participants had experienced flow in the past week	Pre first and post final session	Significantly greater increases in flow from pre- first writing session to post- final writing session in the BPFS group in comparison to controls. No significant differences between those who wrote online and those who wrote in-person.

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing sessions	Outcomes and measures	Measurement time-points	Effects
	Control: DAs. No imagery.				
McGovern (2004)	**BPFS Outcome:** writing about getting their desired semester grade **BPFS Process:** writing about actions to be taken to achieve grade. **Control:** DAs (then a pair of shoes if they ran out of things to write). No imagery.	4 writing sessions, each at least 20 minutes long (but participants could write for longer if they wished), over 4 consecutive days, online.	**Self-efficacy for self-regulated learning:** Self-Efficacy for Self-Regulated Learning Scale (Gredler & Schwartz, 1997)	Baseline and 2-week follow-up	No significant time * group interaction.
Odou & Vella-Brodrick (2013); Seear & Vella-Brodrick (2013)	**BPFS:** Writing about a different sphere of BPFS (of their choice) each day. **Control:** No activity. Participants told to imagine BPFS before	7 sessions. No time-limits reported, 7 consecutive days. Option to continue activity for another 2 weeks. Continuation (as well as self-reported time spent on the task) was included as an IV.	**Mindfulness:** Mindful Attention Awareness Scale (Brown & Ryan, 2003).	Baseline, post-final writing session and 2-week follow-up.	The authors report a significant main effect of group; BPFS participants had significantly higher mindfulness in comparison to controls, despite no significant between-group difference at baseline. They also report that there was a significant main effect of time; 2-week follow-up scores were higher than baseline and post-test scores. However, the authors state that there was no significant time * group interaction. Although the authors report main effects of group and time, the non-significant interaction (and absence of descriptive statistics to aid in clarification) means that findings

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing sessions	Outcomes and measures	Measurement time-points	Effects
	writing about it. No time-limits for imagery reported.	Online.			should be treated with caution.
Peters et al. (2016)	**BPFS:** Standard. **Control:** DAs. 5 minutes of imagery immediately following writing.	Single 15-minute session, plus 1 minute of thinking about what to write, in a laboratory.	**Attentional preference to positive and negative faces:** Eye-tracker task; participants freely looked at positive and negative faces, relative fixation duration measured.	Pre- and immediately post-writing.	No significant main effect of group, and no group * time interaction.
Yogo & Fujihara (2008)	**BPFS:** Standard **Control:** DAs. No imagery.	3 20-minute sessions. Sessions appear to be over 2 weeks, in a laboratory.	**Working-memory:** OSPAN	Baseline (1 week pre- first writing session), and 1- and 5-week follow-ups.	A disclosure intervention (beyond the scope of this review) resulted in significantly greater working-memory than BPFS and control tasks 5 weeks post-writing. The effect of BPFS-W relative to the control task is unreported and is assumed non-significant on this basis. Descriptive statistics also suggest no significant differences across time or between-groups.
Ph.D. Study One (2015)	**BPFS Outcome:** Standard **BPFS Process:** Writing about lower order, process goals towards	1 20-minute session. In a laboratory for baseline measures, writing task, and immediate post-test affect; online follow-up	**Self-efficacy:** Generalised self-efficacy scale (Schwarzer & Jerusalem, 1995) **Self-regulation:** Short Self-Regulation	Baseline and 1-, 4-, and 8-week follow-ups.	There was also a modality IV in this study (writing versus mental simulation). Due to there being no significant modality * task (BPFS process versus BPFS outcome versus control) interaction effect on self-efficacy and emotion-regulation, inferential statistics for isolated effects of BPFS-W versus control tasks were not reported. Generally, descriptive statistics for self-efficacy and emotion-regulation specific to BPFS-W and control writing participants

Study Authors	Writing instructions and imagery	Number, duration and spacing of writing sessions	Outcomes and measures	Measurement time-points	Effects
	reaching BPFS. **Control:** DAs. No imagery.		Questionnaire (Carey, Neal & Collins, 2004). **Emotion-regulation:** Difficulties in Emotion Regulation Scale (Gratz & Roemer, 2004).		did not suggest large differences between groups or over time. The patterns in these descriptive statistics mirror the inferential results of no significant differences between BPFS and control (writing and simulation) groups, as well as no significant main effect of time, and no group * time interaction (when baseline controlled for). For self-regulation, there was a significant time * modality * task interaction, therefore inferential analyses specific to BPFS outcome, process and control writing were performed. Analyses revealed a significant main effect of task. There was significantly higher self-regulation in both BPFS-W groups in comparison to the control writing group, but no significant difference between process and outcome groups. This effect did not emerge until the 8-week follow-up.
Ph.D. Study Two (2016)	**BPFS:** Standard. **Control:** DAs. No imagery.	4 20-minute sessions, 4 consecutive days, online.	**Self-regulation:** Short Self-Regulation Questionnaire **Future-orientation:** Future Orientation Scale (Crespo, Jose, Kielikowski & Pryor, 2013).	Baseline and 4- and 8-week follow-ups.	No significant main effect of time or group on self-regulation or future-orientation, and no time * group interaction

Table 6.5 shows the methodological characteristics and findings of the 12 studies which included investigation of the effects of BPFS-W on cognitive processes which may impact physical or psychological health. The cognitive-process outcomes included; flow (Aborida, 2016; Layous et al., 2013), mindfulness (Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013), self-efficacy for self-regulated learning (McGovern, 2004), generalised self-efficacy (Ph.D. Study One), attentional-bias (Peters et al., 2016), working-memory (Boselie et al., 2014; Boselie et al., 2016a; Boselie et al., 2016b; Yogo & Fujihara, 2008), self-regulation (Ph.D. Study One; Ph.D. Study Two), set-shifting (Boselie et al., 2017), emotion-regulation (Ph.D. Study One) and future-orientation (Ph.D. Study Two). All outcomes were measured using self-report, apart from working-memory, set-shifting and attentional-bias which were assessed using cognitive tasks.

Writing instructions varied. King's (2001) standard instructions were used in seven studies (e.g. Yogo & Fujihara, 2008; Ph.D. Study Two). In Ph.D. Study One, a variation in writing instructions was included as an IV as described earlier. McGovern (2004) used a similar manipulation. Aborida (2016), Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) and Layous et al. (2013) asked participants to write about specific BPFS spheres.

Only immediate effects were measured in seven studies (e.g. Aborida, 2016). Sustained effects were measured by McGovern (2004), Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) and Yogo & Fujihara (2008), and in Ph.D. Studies One and Two, with follow-ups ranging from one to eight weeks post-writing. Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) measured both immediate and sustained effects.

Writing sessions were completed in a laboratory in seven studies (e.g. Yogo & Fujihara, 2008; Ph.D. Study One). Four were online (e.g. Aborida, 2016; McGovern, 2004). In Layous et al.'s (2013) study, setting was included as an IV as previously discussed.

There was no consistency in dosage and spacing of sessions. King's (2001) procedure was used in Ph.D. Study Two only. McGovern (2004) used four sessions over four consecutive days, but participants could write for longer than 20 minutes if they wished. Layous et al. (2013) also used four sessions, but these were 15 minutes long and spaced over four consecutive weeks. Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) asked participants to write each day for seven consecutive days; no time-limits were reported. Participants in six studies (e.g. Boselie et al., 2017; Peters et al., 2016; Ph.D. Study One) were asked to complete only one writing session (all for 15 minutes other than in Ph.D. Study One which required 20 minutes). Aborida (2015) used the most writing sessions; participants in this study were asked to complete five eight-minute sessions over five consecutive days). Yogo and Fujihara (2008) included three 20-minute sessions over two weeks.

Five studies (e.g. Boselie et al., 2017; Peters et al., 2016) supplemented BPFS-W with imagery, and asked participants to imagine what they had just written about for five minutes immediately post-writing. Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) asked participants to imagine their BPFS before writing about it.

6.4.5: Risk of bias (ROB) assessment results

The ROB assessment demonstrated that the studies included in the current review were generally of fair quality, but with some ROB. The ROB assessment results are provided in Table 6.6.

Key for ROB assessment table (Table 6.6)

	Low ROB
	Some ROB
	High ROB
U	Insufficient information to assess ROB/ unreported
N/A	Non-applicable

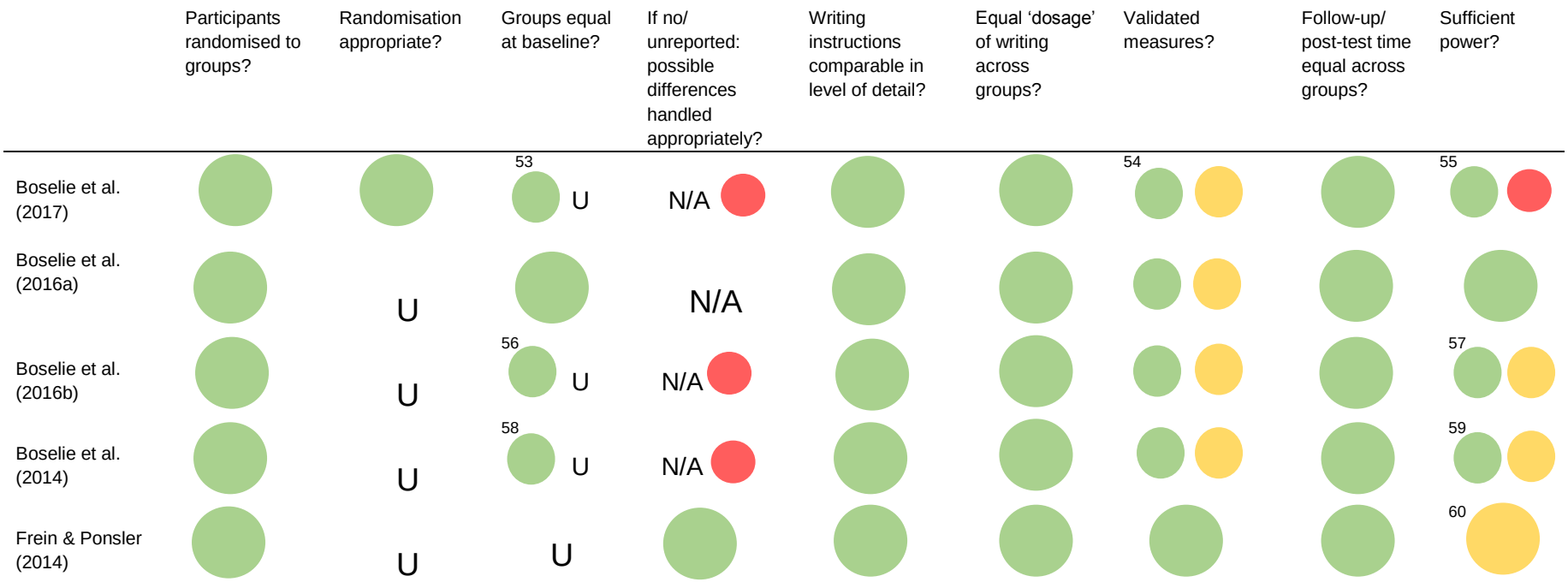
Table 6.6: ROB assessment for each included study

	Selection bias				Performance bias		Detection bias		
Study Author	Participants randomised to groups?	Randomisation appropriate?	Groups equal at baseline?	If no/ unreported: possible differences handled appropriately?	Writing instructions comparable in level of detail?	Equal 'dosage' of writing across groups?	Validated measures?	Follow-up/ post-test time equal across groups?	Sufficient power?
Aborida (2016)		U		N/A					50 
Austenfeld (2007); Austenfeld & Stanton (2008)				N/A			51 		
Austenfeld et al. (2006)		U		N/A			52 		
Boehm et al. (2011)		U		N/A					

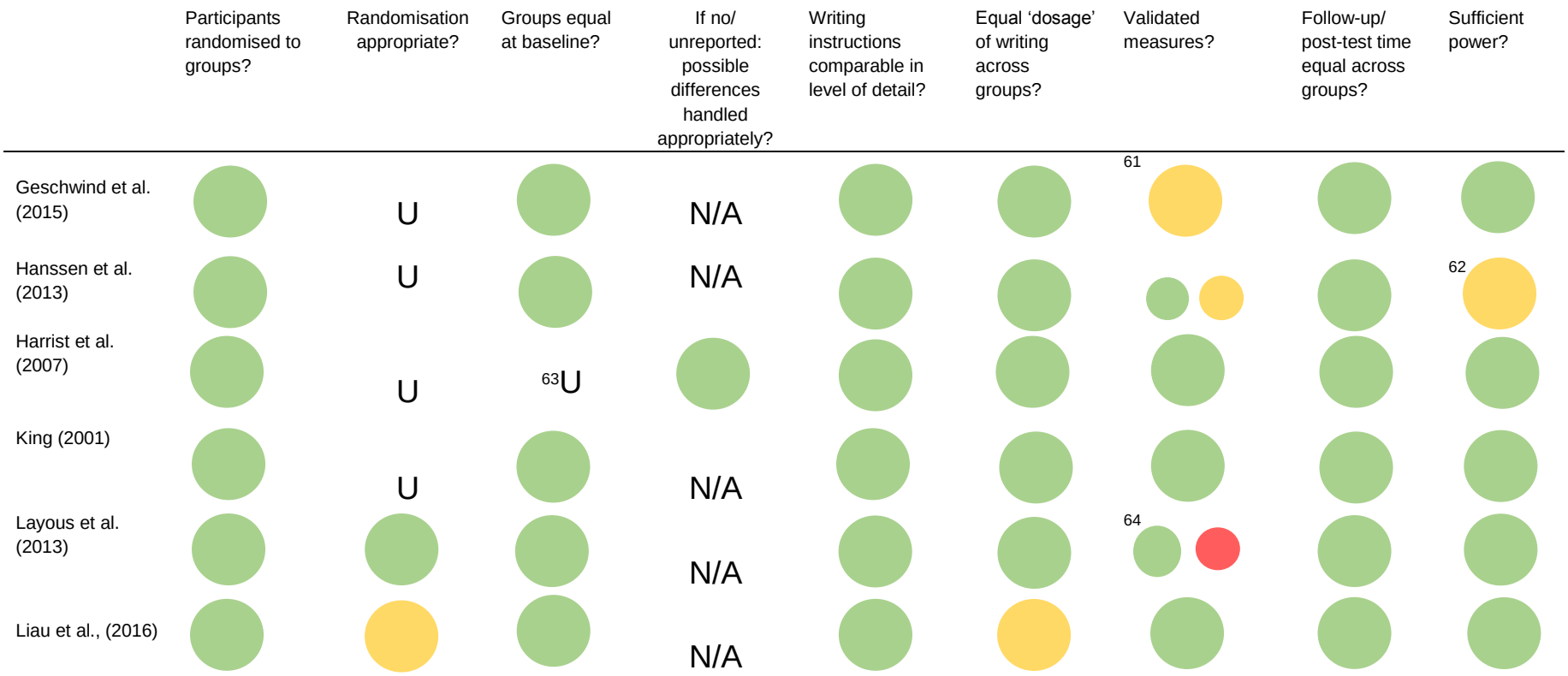
[50] Underpowered according to desired sample size estimates. For all outcomes other than NA, p values are very large and between-group differences in change scores appear negligible, thus risk of type two error is not high. For NA there is greater ROB; the p value is not approaching significance but is smaller than those for other outcomes, and descriptive statistics suggest the possibility of an effect had more individuals been recruited.

[51] Some ROB from 9-item PILL, a non-validated version of a validated measure. Some ROB from the blood pressure measurement. Blood pressure was averaged from two readings at one-minute intervals; such a small number of readings may yield inaccurate estimates of average blood pressure due to wide beat-to-beat variability (Pickering et al., 2005).

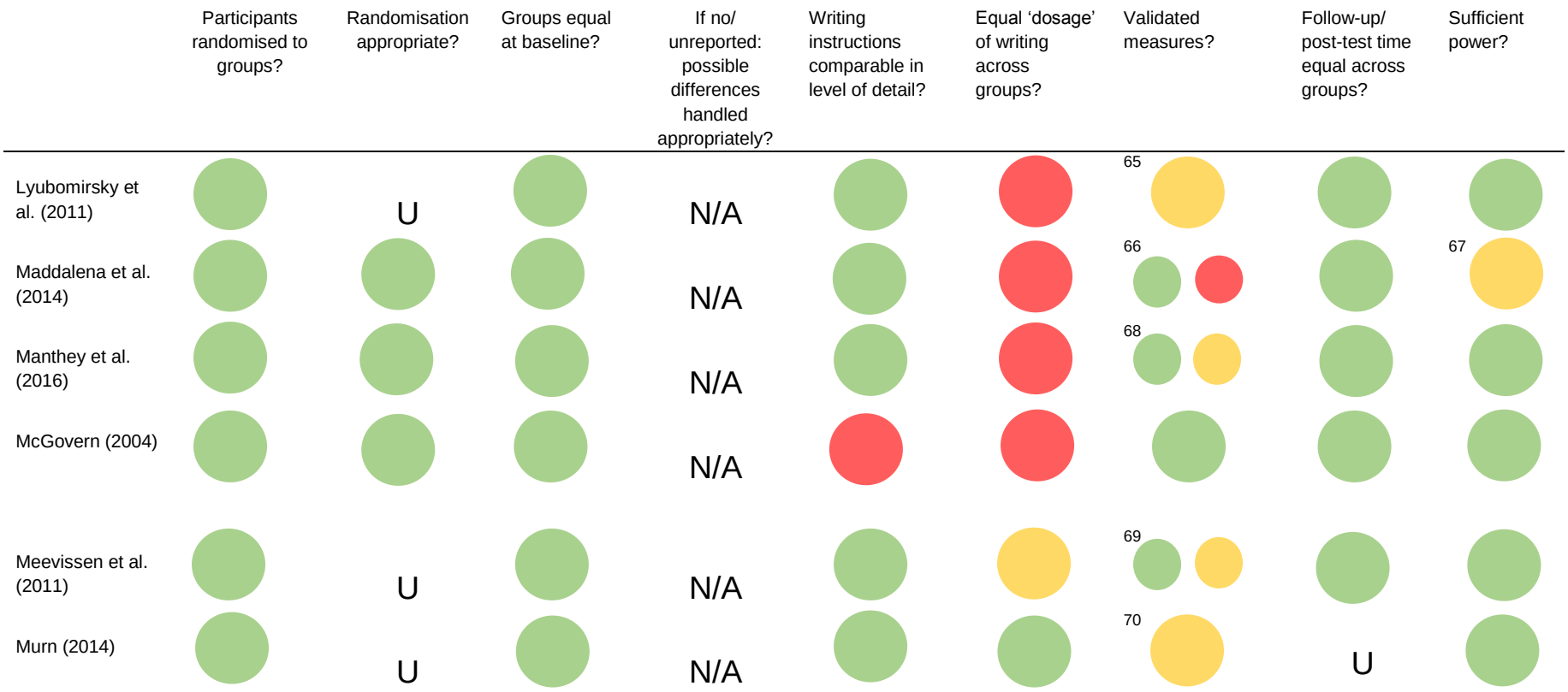
[52] Some ROB from 9-item PILL.



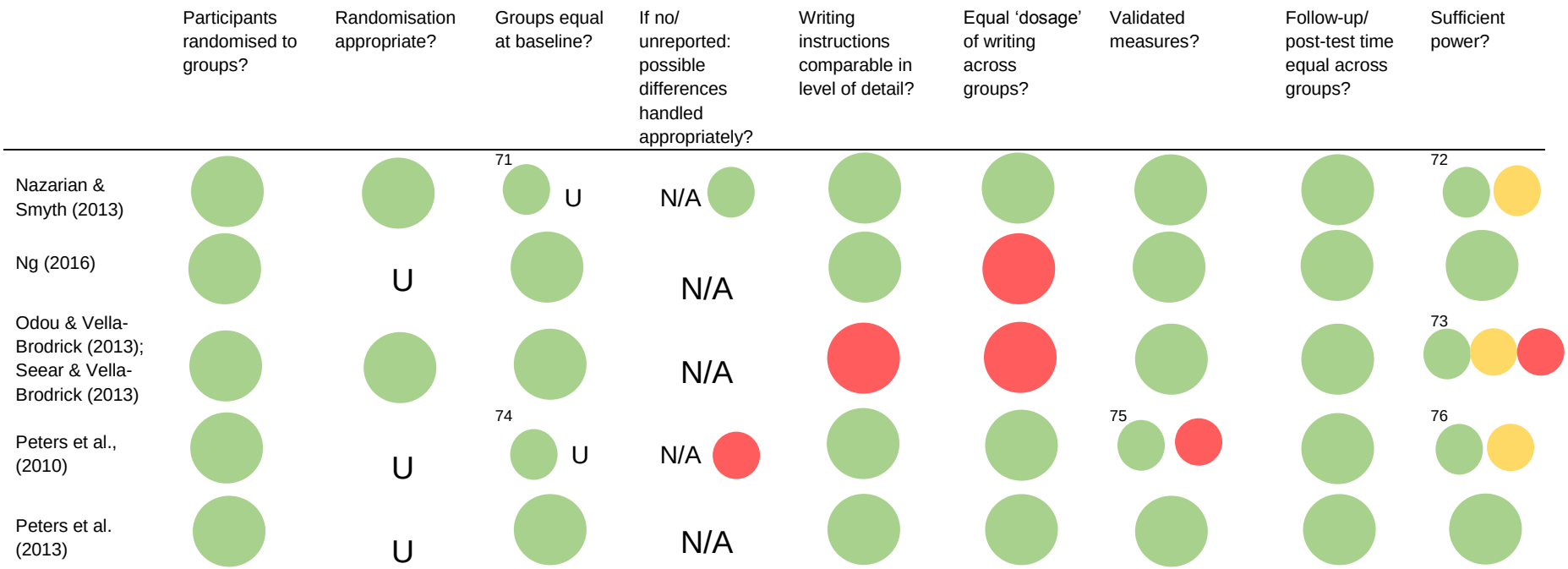
[53]Potential between-group differences not assessed or controlled for, for set-shifting only.
[54] Some ROB from FES (a non-validated measure adapted from the validated SPT). This also applies to Boselie et al's (2014; 2016a; 2016b), and Hanssen et al.'s (2013) studies.
[55] Set-shifting analyses underpowered according to desired sample size estimates; p values were not approaching significance, but descriptive statistics suggest a high risk of type two error in terms of the null effect.
[56] Potential between-group differences not assessed or controlled for, for working-memory only.
[57] Analyses surrounding working-memory were underpowered according to desired sample size estimates. The null effect does not appear to be at high risk of type two error; descriptive statistics suggest negligible between-group differences and the p value is large.
[58] Potential between-group differences not assessed or controlled for, for working-memory only. Unclear from inferential analyses whether P-FEX and N-FEX equal across groups at baseline, but from descriptive statistics differences appear unlikely.
[59] Working-memory analyses underpowered according to desired sample size estimates. Inferential statistics unreported but power ROB assessment is recorded for completeness.
[60] Underpowered according to estimates. ROB appears unlikely to be high as null effect p value is large and much greater than p values from significant effects.



[61] Some ROB from use of a non-validated modification (Schaefer et al., 2003) of the validated (but contentious; Boyle, 1984) DES (Izard et al., 1974).
[62] Results reported are from t-tests on change scores which are underpowered according to desired sample size estimates. However, the authors state that similar results were obtained from ANCOVAs which would not have been underpowered.
[63] Unreported for PA, NA and MCU. For PA and NA a baseline difference appears unlikely based on descriptives.
[64] High ROB from use of a non-validated scale intended to measure flow experience.



[65] Some ROB from composite comprising the SWLS and the SHI (validated measures) as well as a non-validated affect scale, intended to measure psychological wellbeing.
[66] High ROB from non-validated physical symptoms scale. Risk increased by inclusion in analyses only items found in preliminary analyses (of the same data) to be significantly impacted.
[67] Underpowered according to desired sample estimates but ROB appears not to be high as the effect size was small.
[68] Some ROB from non-validated German version of the SPANE.
[69] Some ROB from non-validated Dutch translation of EPQ-N short-form, shortened positive and negative affect scale, and LOT.
[70] All measures used were empirically-validated, however, Murn (2014) altered the Likert scale ranges.



[71] Possible baseline differences were not explored for NA only.

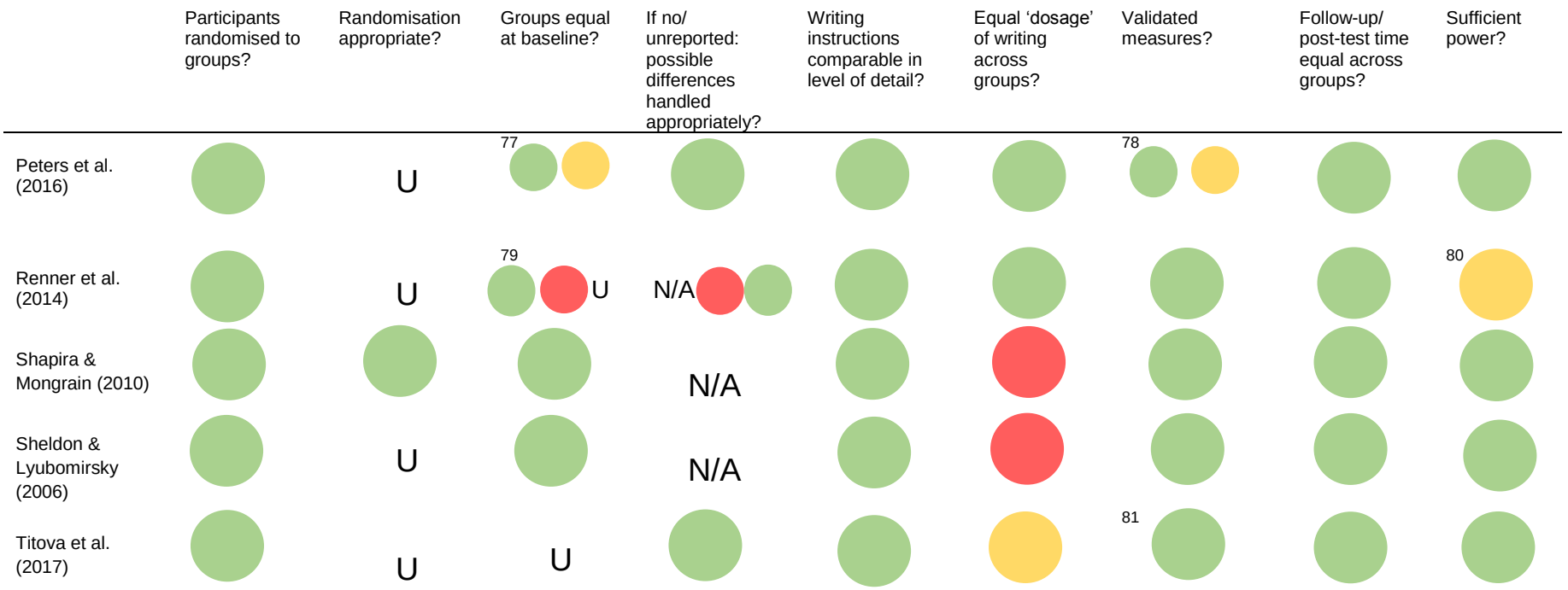
[72] One-way ANOVAs for PA, NA and salivary cortisol underpowered according to sample size estimates. Risk of type two error unlikely to be high; p values for null effects are large and effect sizes are small.

[73] PA analyses underpowered according to sample size estimates. ROB unlikely to be high post-writing; the p value is large and comparable to a null, powered, follow-up effect. Descriptives also suggest a null effect. Higher ROB for follow-up; p value is large but descriptives suggest greater between-group differences than at post-test. NA and well-being analyses at follow-up underpowered according to sample size estimates, but p values are large and descriptives suggest negligible between-group differences. Analyses of continuation effects were powered.

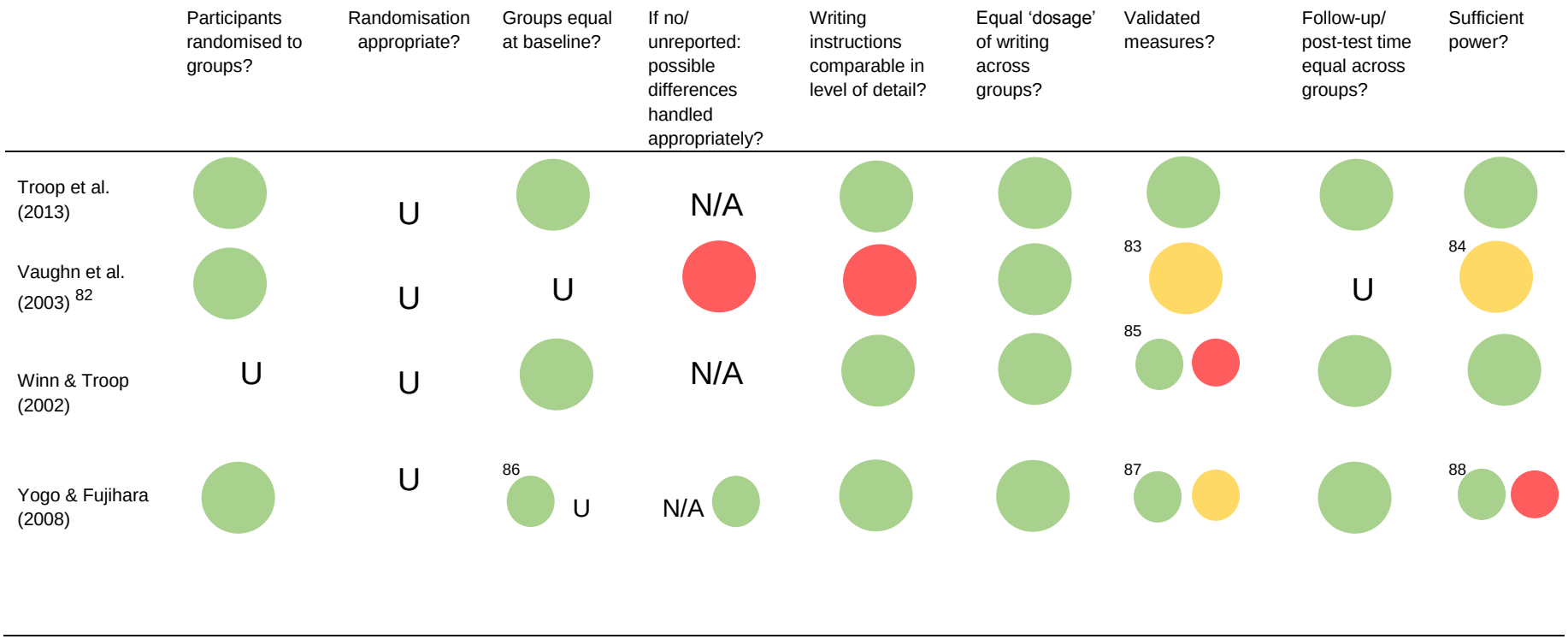
[74] No between-group differences in any outcome at baseline other than optimism, which was not measured at baseline (therefore differences could not be controlled for).

[75] High ROB from non-validated items intended to measure state optimism.

[76] State optimism (t-tests) were underpowered. Null effect appears not to be at high rsk of type two error; descriptive statistics suggest negligible between-group difference.



[77] No significant between-group baseline differences in any outcomes. There was a difference in dispositional optimism, measured as a moderator. This was controlled for.
[78] Some ROB from FES use, as well as FES being translated into German by the authors.
[79] There were no significant between-group differences in the bipolar-continuum scales or general PA and NA at baseline. However, there was an NMI pre-writing which elicited a greater increase in NA (measured on bipolar-continuum scales) in the BPFS group than controls. This was not controlled for in analyses, thus introduced a high ROB. The authors also did not assess whether there were baseline differences in perfectionism and dependency, but they did include baseline scores in analyses by using change scores.
[80] Underpowered according to desired sample estimates, but p values for null effects were large and descriptive statistics suggested negligible between-group differences.
[81] PANAS-X was used to measure affect. This is validated, but Titova et al. (2017) stated that they used the 42 items that comprise the general PA and NA, guilt, sadness, joviality, self-assurance and serenity subscales. There are a total of 48 items in these scales. It therefore appears that the authors stated that they used 42 items in error. The specific affect scales are categorised as basic PAs and NAs (Watson & Clark, 1994); thus, it is unlikely that use of them as a composite measure of PA and NA induces bias.



[82] Vaughn et al.'s (2003) study was available only as a conference abstract and hand-out, thus the information reported is limited. Poor quality should not be assumed based on the limited detail available.
[83] Some ROB from use of non-validated composite comprising SWLS and PGIS.
[84] Underpowered according to desired sample size estimates, but descriptive statistics suggest findings are not at high risk of type one or two errors.
[85] High ROB from the use of a non-validated scale intended to measure physical symptoms.
[86] No analyses conducted to explore or control for baseline between-group differences in any outcome other than working-memory. Unclear from inferential analyses whether there were baseline differences in working-memory but descriptives suggest no differences.
[87] Some ROB from non-validated 8-item version of the PILL.
[88] All analyses other than working-memory analyses underpowered according to estimates. Difficult to assess risk of type two error; null results (including descriptives) unreported.

	Participants randomised to groups?	Randomisation appropriate?	Groups equal at baseline?	If no/ unreported: possible differences handled appropriately?	Writing instructions comparable in level of detail?	Equal 'dosage' of writing across groups?	Validated measures?	Follow-up/ post-test time equal across groups?	Sufficient power?
Ph.D. Study One (2015)				N/A			[89] 		
Ph.D. Study Two (2016)		N/A		N/A			[90] 		[91] 

[89] Some ROB from 13-item PSI. This contains items from the validated PSI, minus five items which were not regularly-endorsed (Kessler et al., 2008; Spector, 2018).
[90] Some ROB from 13-item PSI and from use of a non-validated psychological well-being composite comprised of the LOT and SWLS (validated measures); although LOT and SWLS were also analysed separately. High ROB from non-validated measure of future-orientation.
[91] Affect analyses underpowered according to desired sample size estimates. The PA null finding may be a type two error due to a moderate effect size and large p value, but p values and effect sizes for NA suggest ROB from the low sample size is not high.

Selection bias

There were two potential risks of selection bias which were apparent across the included studies; problems with group allocation and randomisation, and failure of the allocation strategy to produce groups of participants with equivalent levels of relevant outcomes at baseline.

Although most (35) authors reported random allocation of participants to groups, many (25) did not include details of how this randomisation occurred, rendering it difficult to assess risk of selection bias. Of those who did report the randomisation strategy used (11), methods were appropriate in all but one study (Liau et al., 2016). For example, Manthey et al. (2016) and Shapira and Mongrain (2010) used automated randomisation by a computer programme, and Nazarian and Smyth (2013) used a computerised random numbers generator. Methods such as these allow chance a role in group allocation. They are less likely to introduce bias in comparison to the method used by Liau et al. (2016), which involved randomising classes of students to groups (although they did not report exactly how this was done). Although Liau et al. (2016) state that they used randomisation in group allocation, this strategy is not truly random. According to the CRD (2009), successful randomisation should result in groups that are balanced in terms of both known and unknown potential confounding variables. Randomisation of whole classes is unlikely to achieve this. Given that Liau et al. (2016) provide limited information with regards to participant characteristics it is impossible to infer what these confounding variables may be. For example, each class may have been studying a different module subject. This would introduce confounding variables, as subject preferences have been found to be related to personality traits, personal interests and demographic factors (e.g. Blackstone & Fulton, 1974; Colley & Comber, 2003; McKenzie & DaCosta, 1999; Rosenbloom, Ash, Dupont & Coder, 2008).

Despite the problems with randomisation strategies discussed above, for most studies (24) it appeared that the strategy used to allocate participants to groups was sufficient. The authors of these studies report that groups were equal prior to experimental manipulation on all outcome variables, therefore the risk of

selection bias is low. A further study (Peters et al., 2016) did not find any differences in any outcome variables at baseline but did find significant between-group differences in dispositional optimism, which had been measured as a potential moderator. This baseline inequality is unlikely to introduce high ROB into results, because it was appropriately controlled-for in analyses of effects of BPFS-W. Peters et al. (2016) performed ANCOVAs to partial out the influence of variations in dispositional optimism on between-group, post-writing differences in each outcome variable. The results of these ANCOVAs mirrored the findings of ANOVAs in which dispositional optimism was not controlled-for, suggesting that the baseline between-group differences in dispositional optimism did not bias the effects of the BPFS task relative to the control task.

In 11 studies, no indication was given of whether baseline differences were present for one or more outcomes. In some studies, the ROB from failure to assess baseline differences is unlikely to be high, because baseline scores were either controlled for in analyses using ANCOVA (e.g. Harrist et al., 2007; Titova et al., 2017), or were at least included in analyses (e.g. by using change scores; Nazarian & Smyth, 2013; Yogo & Fujihara, 2008)[92]. Renner et al.'s (2014) NA analyses were found to be at high ROB. Although Renner et al. (2014) found groups to have equal levels of NA at baseline, the BPFS-W group reported a greater increase in NA following a pre-writing NMI in comparison to controls. This was not controlled for in analyses and as such results should be treated with caution. It is particularly important to note the five studies in which baseline scores for one or more outcomes were not measured or included in analyses at all (e.g. Boselie et al., 2017; Peters et al., 2010). These studies were graded as having a high ROB. It is impossible to infer from them whether post-test scores are naturally-occurring, or whether they have arisen as products of experimental manipulation. If selection bias is present in these studies, it may lead to the detection of significant between-group differences at post-test even when neither group's score on a given outcome has changed.

[92] ANCOVA is typically preferable for reducing error variance and/ or adjusting post-test means according to pre-test differences. However, ANOVA of change-scores is also an acceptable means of analysis (Dimitrov & Rumrill, 2003).

Results concerning these outcomes should be treated with extreme caution due to this high ROB.

Selection bias is problematic because it reduces the confidence with which post-test between-group differences can be attributed to effects of the intervention condition relative to the control condition. Effects may be contaminated not only by naturally-occurring differences between groups, but also by potential differences in responsiveness to treatment.

Performance bias

Some risks of performance bias were found across several studies; differences in the level of structure and detail provided in writing instructions, and failure to ensure that all groups were subjected to the same 'dosage' of writing.

The authors of four studies employed writing instructions which included more detail and structure for some groups than others. For example, Vaughn et al.'s (2003) BPFS outcome and control groups wrote for 20 minutes about an ideal future or daily activities, respectively. Vaughn et al. (2003) also included a BPFS process group who wrote about the outcome of their BPFS for seven minutes, followed by seven minutes of writing about what they could be doing in 10 years to realise that future, and a further seven minutes of what they could be doing in 20 years. This leads to a risk of performance bias because the treatment of the participants differs through more than the topic of writing alone, rendering it difficult to attribute post-test differences to the writing topic. In Vaughn et al.'s (2003) study, the effects of the process writing topic relative to the outcome and control writing topics are contaminated by the differences in structure, as the level of structure could impact the influence of writing on outcomes.

The second methodological factor which may introduce risk of performance bias is different groups of participants receiving different 'doses' of writing. In the majority (25) of studies, BPFS participants completed the same number of writing sessions, each lasting for the same amount of time, as controls. The authors of these studies asked participants to complete a specific number of

writing sessions and instructed them to write for a specific amount of time (e.g. five eight-minute sessions; Aborida, 2016), and excluded from analyses any individuals who did not adhere to that exact, specified dose. The authors of the remaining 12 studies did not ensure that there were no between-group differences in dosage by allowing flexibility in terms of how many times participants completed the writing exercise and how long they wrote for. They did not check whether there were significant differences between BPFS and control groups in the average 'dose' of writing completed. The degree of flexibility— and resulting risk of performance bias— was found to vary greatly across studies. In three studies, flexibility was granted only in terms of how long participants spent on the writing exercise (see studies marked as 'some risk' in Table 6.6). Participants in both groups were required to complete the same number of writing sessions to be classed as completers and have their data included in analyses. For example, Liau et al. (2016) required that all participants completed two writing sessions but reported that sessions lasted 'around 20 minutes' each, suggesting that rigid time-limits were not imposed. In nine studies, greater flexibility was allowed. The authors in these studies included participants as completers even if they did not complete all the prescribed writing sessions, and/ or encouraged participants to write as many times as they wished (see studies marked as 'high-risk' in Table 6.6). For example, Shapira and Mongrain (2010) asked participants to complete seven sessions over seven consecutive days and encouraged participants to continue writing over a six-month follow-up period. They also did not report that any guidance was given to participants with regards to how long they should spend writing at each session. They included participants in analyses if they completed at least one session. This gives rise to a large potential between-group difference in dose, and therefore a high risk of performance bias. It is not possible to draw meaningful conclusions with regards to the relative effects of the BPFS task in comparison to the control task when equal dosages may not have been administered, particularly for studies in which there was potential for large differences in dosage. If a BPFS task was found to be significantly more effective than a control task and the BPFS group may have completed more writing sessions than the control group, it would not be possible to conclude that

the control task would not have been equally effective had the control group completed more sessions.

Detection bias

There was a risk of detection bias in the current review because of failure to ensure equal follow-up periods between groups, use of non-validated measures and low power due to small samples.

Unequal periods of time between the intervention and the time of post-test measurements could elevate risk of detection bias, particularly in longitudinal studies. Most (35) authors collected post-test or follow-up data on a specific day (e.g. two weeks post-writing; Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013; four weeks post-writing; Manthey et al., 2016). Winn and Troop (2002) allowed follow-up responses to be completed over eight to 12 weeks but performed analyses to assess whether groups differed on the mean number of days between completing the writing task and follow-up. They found no significant difference between groups (BPFS group mean= 73.7 (SD= 8.1) days; control group mean= 74.2 (SD= 7.6) days). However, the authors of two studies (Murn, 2014 and Vaughn et al., 2003) did not ensure that there were no between-group differences in the number of days between completing the writing phase and the follow-up. Vaughn et al. (2003) and Murn (2014) had follow-ups four-to-seven weeks post-writing and six-to-10 weeks post-writing, respectively. The potential for follow-ups at different time-points across groups makes fair comparisons difficult. It is possible that an effect may be detected four weeks post-writing, but may have dissipated by seven weeks, or that an effect may not become detectable until six weeks post-writing. For example, in a meta-analysis of 146 writing intervention studies (most of which were focussed on writing about traumatic past experiences), the amount of time between intervention and follow-up moderated the effects of the intervention (Frattaroli, 2006). Larger effect-sizes were found in studies with follow-ups which took place less than one month post-writing.

Another source of detection bias across the included studies is the use of measures which have not been empirically-validated. 16 studies only included

published, previously-validated instruments[93]. However, the authors of all other included studies had used at least one measure which had not been validated or was in some way biased (e.g. Austenfeld et al.'s (2007) and Austenfeld & Stanton's (2008) blood pressure measure). Measures were graded as having some ROB if they had not been validated in their current form but were a modification of an empirically-validated instrument. 17 studies included at least one instrument with some ROB. For example, Boselie et al. (2017) used the FES (Hanssen et al., 2013) to measure participants' P-FEX and N-FEX. This scale was not validated by Hanssen et al. (2013) but is an adaptation of MacLeod's (1996) SPT, which is a published instrument that has undergone empirical validation of its psychometric properties. Meevissen et al. (2011) and Manthey et al. (2016) also introduced some ROB with their non-validated Dutch and German translations of validated scales. Translation of a scale does not mean that that scale will have psychometric equivalence with the original, validated scale (Stewart & Napoles-Springer, 2000). When using non-validated translations, there is ROB from a possible lack of semantic equivalence (the items in each version mean the same thing in each respective language), conceptual equivalence (the constructs measured exist and are relevant in both the source and the target versions) and item equivalence (differences between response choices are the same in both languages, and there is not differentially greater bias in items in one language than the other) with the original-language version (Quittner et al., 2000; Stewart & Napoles-Springer, 2000).

Instruments were graded as having a high ROB if they had been generated by the author for that study and had not been based on previously-validated measures. Five studies included at least one high-risk instrument. For example. Peters et al. (2010) used a non-validated two-item scale which they created for their study to measure state optimism. Findings from measures which have not undergone rigorous empirical validation should be treated with caution. Producing scales to accurately measure a subjective characteristic is a challenging, multistage process (Streiner, Norman & Cairney, 2015). A scale which has not been subjected to this process may harvest data which are not

[93] Single-item visual analogue scales (VASs) were categorised as validated measures; evidence suggests that they are valid, reliable and as responsive as validated multi-item instruments (e.g. de Boer et al., 2004; Folstein & Luria, 1973).

reflective of the construct that the scale was intended to measure (Streiner et al., 2015). Even if the scale does successfully measure the intended construct, it may not reproducibly demonstrate the true variability within and between individuals (Streiner et al., 2015). Findings from non-empirically-validated measures can therefore be misleading and should be treated with caution.

The final source of detection bias assessed in the included studies was sample size and statistical power. The majority (23) of the studies were found to have sufficient statistical power for their analyses to reveal a true effect, for all outcomes. In nine studies, analyses were underpowered for some outcomes but not others (e.g. Boselie et al., 2017; Peters et al., 2010). Underpowered analyses may not detect a population effect if one exists (Dancey & Reidy, 2017). If an effect is apparent in the results of underpowered analyses, it is not possible to conclude with confidence that it is not a product of sampling error (Dancey & Reidy, 2017). Some studies were deemed to be at a higher ROB from small samples than others. Some analyses (e.g. Peters et al.'s (2010) state optimism analysis) were underpowered according to desired sample size estimates, but results appeared to be accurate (negligible differences between means, large p values and small effect sizes suggest a null effect may not be a type two error). In five studies, the ROB was deemed to be high for one or more outcomes. For example, in Ph.D. Study Two the lack of an effect of BPFS-W on PA is at high risk of type two error, because the p value was approaching significance and the effect size was moderate. The results of studies which lacked statistical power should be treated with caution, as they may not accurately mirror the effects of BPFS-W.

Attrition bias

Attrition and loss of participants was found to be a recurring issue across included studies. Only eight studies did not include reports of attrition or loss of participants (Frein & Ponsler, 2014; Hanssen et al., 2013; Murn, 2014; Ng, 2016; Peters et al., 2016; Renner et al., 2014; Troop et al., 2013; Vaughn et al., 2003). In a further four studies (Boselie et al., 2016a; Harrist et al., 2007; King, 2001; Peters et al., 2010), there was no attrition overall, but there was some loss of participants on individual variables. In Harrist et al.'s (2007) study all 75

participants originally-recruited into the study completed all measurements for PA and NA, but frequency of MCU could be accessed for 68 of them.

In the remaining studies, participants withdrew from the whole study, rather than only failure to provide data for individual outcomes; rates of attrition varied greatly across studies. The most commonly-reported cause of loss of participants was non-completion of outcome measures post-writing. In Sheldon and Lyubomirsky's (2006) study, 70 participants completed baseline measures. Only three failed to complete the follow-ups, leaving a final sample of 67. Liau et al. (2016) originally recruited 191 individuals. Only 29 withdrew their participation, leaving 162 remaining at the post-test stage. Conversely, Manthey et al. (2016) lost over half of their sample to attrition; 740 participants were recruited, with 666 completing baseline measures. Only 322 remained in the study at the eight-week follow-up (and 3.5% of those were excluded due to failure to complete the writing intervention). Shapira and Mongrain (2010) recruited 1002 individuals but lost 79.7% of them to attrition. In other instances[94], participants were not lost due to withdrawal, but were excluded due to either failure to adhere to the intervention protocol, or due to false inclusions; when those who are not eligible for inclusion in the study are accidentally allocated to a condition. In Lyubomirsky et al.'s (2011) study, 23 participants were removed from analyses because they failed to complete at least four of the eight prescribed writing sessions. In Boselie et al.'s (2014) study, 80 participants were originally randomised to conditions, but six were excluded because they were later found not to meet inclusion criteria. Less common reasons for loss of participants were incomplete responses (e.g. Boehm et al., 2011), extreme scores or outliers on baseline measures (e.g. Boehm et al., 2011), and technical difficulties (e.g. Boselie et al., 2017). Attrition and loss of participants is problematic as it can reduce the internal and external validity of a study (Miller & Wright, 1995; Zhou & Fishbach, 2016). It is widely-accepted that participants who drop out may well be unrepresentative of individuals who

[94] Due to lack of clarity in reporting across studies it is difficult to provide accurate frequencies for these causes of participant loss. However, it appears that after failure to complete outcome measures, the most common cause was poor intervention adherence, followed by similar frequencies of false inclusion, technical issues, and extreme scores. Incomplete responses appeared to be the least common reason for exclusion and loss of participants. The reader should treat this statement as an estimate.

remain in an investigation and may differ from them in some way (Jüni, Altman & Egger, 2001; Siddiqui, Flay & Hu, 1996).

The differences between those who drop out/ are excluded and those who remain may be more marked in clinical trials or in medical research, where patients who fail to adhere to treatment generally differ from those who do adhere in ways related to their disease prognosis, or where they may experience severe side effects of the treatment or an increase in their symptoms (Coronary Drug Project Research Group, 1980; Sackett & Gent, 1979). Although negative effects on participants are unlikely from BPFS-W (and in most studies, samples were comprised of healthy student participants, with none including clinical groups), some authors did report differences between those who dropped out and those who remained. Shapira and Mongrain (2010) reported that the participants who completed their study were older, less needy, and had lower baseline depression than those who did not. In Liau et al.'s (2016) study, completers had higher optimism and lower depression and NA. Manthey et al. (2016) found that females were 1.58 times more likely to complete their study than males. In most studies, authors either reported that there were no significant differences between those who completed their studies and those who did not or did not report that they had performed any analyses to investigate this. Even when differences in baseline measures and/ or demographic variables are not found, it is important to acknowledge that those who dropped out may systematically differ in some way to those who completed, which may threaten the internal and external validity of the study.

For decades, it has been recommended that intention-to-treat analyses are the best course of action to counteract this bias (CRD, 2009; May, DeMets, Friedman & Passamani, 1981; Sackett & Gent, 1979; White, Horton, Carpenter & Pocock, 2011). In an intention-to-treat analysis, all participants originally allocated to conditions are included, even in cases of deviation from intervention protocol, or false inclusion (Armjo-Olivo, Warren & Magee, 2009; Senn, 1997; as cited by Hollis & Campbell, 1999; Sheiner & Rubin, 1995;). Of course, in an ideal intention-to-treat analysis, all participants should have completed all outcome assessments (Hollis & Campbell, 1999). In the case of drop-out, full

application of intention-to-treat is impossible (Pocock & Abdalla, 1998). However, Deeks, Higgins & Altman, 2005 (as cited by Armijo-Olivo et al., 2009) suggest that even those with missing outcome data should remain in analyses. Intention-to-treat analyses in this situation should include all participants who began the investigation, in the groups that they were originally allocated to, using imputation techniques such as 'last observation carried forward' (Altman, 2009; Armjo-Olivo et al., 2009; Moher et al., 2012; Shao & Zhong, 2003; White et al., 2011). There is empirical evidence which demonstrates that analyses with non-completers excluded show inflated (and less-commonly, deflated) effects of treatments in comparison to the results of intention-to-treat analyses (Jüni & Egger, 2005; Tierney & Stewart, 2005), suggesting that it successfully reduces bias occurring from attrition and loss of participants. However, none of the included studies affected by attrition or loss of participants performed true intention-to-treat analyses. This may have been a sensible decision in terms of experimental control (in that an intention-to-treat approach would perhaps have introduced performance bias; including in analyses participants who had not adhered to an intervention may have made it difficult to draw conclusions based on the results of that intervention). However, rejection of the intention-to-treat approach may have increased ROB, and possibly resulted in lower generalisability of findings.

6.5 Narrative synthesis

6.5.1 Physical health outcomes

6.5.1.1 Self-reported physical symptoms

In seven studies, surveys were used to measure self-reported symptoms of physical illness. The majority (5) of these studies found that BPFS-W did not appear to significantly reduce physical symptoms relative to control activities (Austenfeld, 2007; Austenfeld & Stanton, 2008[95]; Austenfeld et al., 2006; Winn & Troop, 2002; Ph.D. Study One[96]; Ph.D. Study Two). Only two studies indicated amelioration of physical symptoms. Maddalena et al. (2014) found that

[95] Austenfeld (2007) and Austenfeld and Stanton (2008) found reduced physical symptoms from one month pre- to one month post-writing in both BPFS and control groups with no significant between-group difference.

[96] There appears to be no significant between-group difference in physical symptoms in Ph.D. Study One, based on descriptive statistics as described in Table 6.3.

BPFS-W participants decreased in self-reported physical symptoms from baseline to one month following three writing sessions, whilst controls increased in physical symptoms. Yogo and Fujihara (2008) found that BPFS-W participants reported decreased physical symptoms from immediately before to immediately after three writing sessions[97]. There are no clear methodological differences between studies which did and did not find significant gains in physical health after BPFS-W. There are, however, areas of ROB which may explain the inconsistencies in findings. Non-validated measures of physical health were used both in studies that did find BPFS-W to be beneficial and those that did not. However, there appears to be greater reason to be cautious with the findings from the two studies which did find benefits to physical health. Maddalena et al. (2015) not only administered a measure of physical symptoms which had not undergone any empirical validation, but they also included in the main analyses only the individual items from that measure which were found in preliminary analyses of the same data to significantly improve. Yogo and Fujihara's (2008) finding that there was a significant decrease in physical symptoms following BPFS-W is likely to be biased not only from their use of a non-validated modification of the PILL, but also from their measurement of physical symptoms only immediately pre- and post-writing. There is unlikely to be any true change in physical symptoms in this short amount of time, therefore the effect found may be spurious. Both Maddalena et al.'s (2014) and Yogo and Fujihara's (2008) analyses were possibly underpowered, which further suggests that the significant effects found may be spurious (Button et al., 2013). Overall, it appears likely that BPFS-W is not beneficial for physical symptoms. There is a higher quantity of studies which do not report benefits than those that do, and the studies reporting benefits have a higher ROB.

6.5.1.2 Medical care utilisation (MCU)

MCU (for illness, not injury) was measured in four studies as an indirect measure of physical illness. In two studies (Austenfeld, 2007; Austenfeld & Stanton, 2008; Austenfeld et al., 2006), there was no significant effect of BPFS-W on MCU. However, King (2001) reported that there was significantly lower

[97] Yogo and Fujihara (2008) did not report the effect of the control task, nor did they report whether there was a significant main effect of group. Their findings should be treated with caution.

MCU in the five months following the intervention in BPFS participants in comparison to controls (when pre-writing levels were controlled for), as well as a significant decrease in MCU from pre- to post-writing in the BPFS group (but no change in controls). In Harrist et al.'s (2007) study, descriptive statistics suggested greater change from three months pre- to three months post-writing in the BPFS-W group in comparison to the control writing group[98]. Given that all four of these studies were of fair and similar quality (and none lacked statistical power), the discrepancy in findings may be explained by differences in intervention procedures. First, in the studies by Austenfeld (2007), Austenfeld and Stanton (2008), and Austenfeld et al. (2006), participants were asked to write about how they would overcome an obstacle as part of the BPFS-W intervention, whereas King's (2001) and Harrist et al.'s (2007) participants were not. Second, in the studies by Austenfeld (2007), Austenfeld and Stanton (2008) and Austenfeld et al. (2006), three writing sessions were spaced around one week apart, whereas King (2001) and Harrist et al. (2007) required that their participants completed four writing sessions over four consecutive days. Therefore, it appears that BPFS-W may result in a reduced need for medical care, but perhaps only when writing instructions are open rather than structured, when sessions are spaced closely together, or when at least four sessions are completed. Further research should be undertaken to determine the intervention procedure requirements which must be satisfied for a reduction in MCU to occur.

6.5.2 Physiological outcomes

6.5.2.1 Salivary-cortisol

Salivary-cortisol was measured in only one of the included studies (Nazarian & Smyth, 2013). There was no significant difference in change in salivary-cortisol (from immediately pre- to ten minutes post-writing) between BPFS and control participants. Despite the evidence being from one study alone, findings appear robust. First, the study was found to be of generally high quality. Although Nazarian and Smyth's (2013) analysis of the effects of BPFS-W on cortisol was underpowered according to desired sample size estimates, the p value was

[98] Results from these descriptive statistics mirrored inferential findings from which it was not possible to isolate the pure effects of BPFS-W in comparison to the control task. Please see Table 6.3.

large and the effect size was small, suggesting that the null finding is unlikely to be a type two error. Second, it appears theoretically unlikely that BPFS-W would cause a cortisol spike. Cortisol is a biomarker of psychological stress (Hellhammer, Wüst & Kudielka, 2009; Lee, Hwang, Cheon & Jung, 2012). Research has demonstrated that cortisol levels peak twenty-to-thirty minutes following the onset of a stress-inducing stimulus (Kirschbaum & Hellhammer, 1989). This is in line with the second measurement of salivary-cortisol in Nazarian and Smyth's (2013) study (30 minutes after beginning the 20-minute writing task). Given that the image of a positive future in which life goals have been achieved is unlikely to be a threatening stimulus, it appears implausible that BPFS-W would result in significantly greater increases in cortisol in comparison to writing about a neutral control topic. Therefore, it is likely that the intervention does not influence cortisol levels; at least not immediately post-writing.

6.5.2.2 Blood pressure

The effect of BPFS-W on blood pressure was also explored in a single study (Austenfeld 2007; Austenfeld & Stanton, 2008). Unlike the measure of salivary-cortisol in Nazarian and Smyth's (2013) study, blood pressure was assessed not as a measure of acute physiological reactivity to the intervention, but as a long-term outcome. There was no significant difference between BPFS and control groups in blood pressure four weeks following the third and final writing session (when baseline blood pressure was controlled for), suggesting that BPFS-W does not impact blood pressure. It should, however, be acknowledged that the blood pressure measurement taken may not be an accurate representation of the participants' average blood pressure, outside of the laboratory environment and the testing session. First, there is ROB from the average blood pressure value being generated from only two readings; such a small number of readings tends to produce inaccurate estimates of average blood pressure due to high beat-to-beat variability (Pickering et al., 2005). Second, blood pressure has been found to rise (and less commonly, fall) in the presence of a clinician or in a medical setting (Dillon, Seacat, Saucier & Doyle-Campbell, 2015; Kumpusalo, Teho, Laitila & Takala, 2002). Research has shown that blood pressure measurements taken in a clinician's office do not

correlate with measurements taken elsewhere (such as in an individual's home), and those taken by the individuals themselves (Pickering et al., 2005). Although the researchers were academics rather than clinicians, it is possible that the blood pressure reading taken was reflective of acute cardiovascular reactivity in response to the research setting, which may have masked any potential changes in average blood pressure from pre- to four weeks post-writing. Before firm conclusions with regards to the effects of BPFS-W on blood pressure can be drawn, further research with more sensitive and accurate measures is warranted. Future work could use ambulatory blood pressure monitoring (ABPM), allowing participants to measure their blood pressure themselves outside of a laboratory using a validated device (Perry, 2013; Pickering et al., 2005). ABPM is advantageous because it enables a greater number of readings to be taken (therefore the average reading is a more accurate representation of average blood pressure) and eliminates the potential for measurements to be reflections of cardiovascular reactivity to the experimental setting or presence of a researcher (Coats, 1996; Pickering et al., 1988). ABPM would also give rise to exploration of the temporal profile of effects of BPFS-W on blood pressure (Coats, 1996); particularly when effects become apparent and for how long effects are sustained.

6.5.3 Psychological health outcomes

6.5.3.1 Positive affect (PA)

PA was the most frequently-assessed outcome, measured in 26 of the 37 included studies. The short-term, immediate effects of BPFS-W on PA were measured in 25 studies. Longer-term effects were measured in five studies. Immediate and longer-term effects are discussed separately for ease of comparison across studies.

Immediate benefits to PA were found in 20 studies[99] (e.g. Boselie et al., 2017; Peters et al., 2016; Ph.D. Study Two)[100], whereas null findings arose in only five studies (Aborida, 2016[101]; King, 2001; Liau et al., 2016[102]; Nazarian & Smyth, 2013[103]; Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013). There are no clear differences in procedures or ROB between studies which did find a significant positive effect of BPFS-W on immediate PA and those which did not. Although Aborida's (2016) and Odou and Vella-Brorick's (2013) and Seear and Vella-Brodrick's (2013) analyses were underpowered in terms of estimates of desired sample sizes, descriptive statistics suggested that their null effects on PA immediately post-writing were not type two errors. It is therefore unclear as to why BPFS-W did not increase immediate PA in all cases. However, the evidence overwhelmingly suggests that BPFS-W does usually elicit an immediate increase in PA. Furthermore, the studies in which acute gains in PA occurred varied considerably in terms of whether supplementary mental imagery exercises were used, the setting in which BPFS-W was completed, how many writing sessions were completed, how long participants wrote for in each session, and how far apart sessions were spaced. Therefore, it appears that BPFS-W is immediately beneficial regardless of the administration procedure used.

The evidence surrounding the sustained effects of BPFS-W on PA is less conclusive. Of the five studies which investigated long-term effects on PA, only two (Manthey et al., 2016; Meevissen et al., 2011) suggested that BPFS-W may be beneficial. Manthey et al.'s (2016) descriptive statistics[104] suggest that there

[99] In Harrist et al.'s (2007) and Manthey et al.'s (2016) studies and in Ph.D. Study One, inferential statistics for the effect of BPFS versus control writing were not reported. Findings were inferred from descriptive statistics. See Table 6.4 for details.

[100] In Ph.D. Study Two and Boselie et al.'s (2014) study, there were time * group interactions which, when explored with post-hocs, demonstrated significant reductions in PA in controls, but not in the BPFS group. This is treated as a benefit; perhaps BPFS-W buffered against drops in PA. However, it is equally possible that BPFS-W did not impact PA; as suggested by Troop et al. (2013), control tasks may have decreased PA due to participants becoming bored.

[101] Aborida (2016) measured PA and job affective well-being. There was no significant effect on either.

[102] Liau et al. (2016) found significant decreases in PA from pre- to post-writing in BPFS and control groups but no time * group interaction.

[103] Nazarian and Smyth (2013) found a marginally significantly (p= .05) greater increase in PA relative to controls (from change scores), but no significant time (pre-post) * group (BPFS versus control) interaction.

[104] Patterns from Manthey et al.'s (2016) descriptive statistics mirror findings from inferential statistics, in which BPFS and gratitude intervention participants' data were merged. See Table 6.4.

was a greater increase in PA from baseline to four weeks following eight writing sessions (no timing reported) over eight weeks, in BPFS participants than controls. However, it should be noted that the measure of PA used may have introduced some bias. Manthey et al. (2016) used a German translation of the SPANE. Although the SPANE is a published measure which has undergone empirical validation, the translation used has not been validated thus findings from this instrument should be treated with caution. Nevertheless, Manthey et al.'s (2016) study is otherwise generally of fair quality. It is therefore likely that their findings are an accurate representation of the effects of their manipulation on sustained PA.

Meevissen et al.'s (2011) findings are somewhat more contentious than those of Manthey et al. (2016). Meevissen et al. (2011) asked participants to complete a single 20-minute writing session and found that BPFS-W participants reported significantly greater PA one and two weeks post-writing than controls. Meevissen et al. (2011) also conducted within-group analyses; in BPFS-W participants, there was no significant difference in PA between baseline and the one- and two-week follow-ups. However, there was a significant decrease in PA from baseline to the one- and two-week follow-ups in controls. It is possible that the BPFS-W task buffered against a drop in PA, and the control task did not. Nevertheless, there is some ROB in this study which questions the extent to which findings can be taken as evidence. First, as was the case in Manthey et al.'s (2016) study, there is some ROB in Meevissen et al.'s (2011) findings from the instrument used to measure PA; a Dutch translation of the shortened PANAS, which has not been subjected to cultural and linguistic validation. As discussed earlier, it may be that this measure of PA used lacks validity from possible semantic and conceptual errors, thus findings should be treated with caution. Second, following the single writing session, Meevissen et al. (2011) required that participants completed five minutes of imagery of what they had written about, daily for two weeks. The follow-ups took place one and two weeks post-writing, thus overlapped with the imagery. Troop et al. (2013) have suggested that participants find the topic of daily activities to be boring, therefore the drop in PA in the control group may have arisen due to daily completion of a boring imagery exercise. If this is the case, it is possible that

BPFS-W did not protect against a reduction in PA; it merely did not deplete it. It is difficult to draw conclusions from Meevissen et al.'s (2011) study due to this possible contaminating effect of imagery.

The results of the remaining three studies in which sustained PA was measured suggest that there is no sustained benefit to PA (Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013; Sheldon & Lyubomirsky, 2006[105]; Troop et al., 2013[106]). Analyses in Odou and Vella-Brodrick's (2013) and Seear and Vella-Brodrick's (2013) study were underpowered thus it is possible that their null finding is a type two error. However, Sheldon and Lyubomirsky's (2006) and Troop et al.'s (2013) analyses were sufficiently powered to have detected an effect had one occurred. Unlike in Meevissen et al.'s (2011) and Manthey et al.'s (2016) studies, completely validated measures were used in these studies, meaning evidence that the intervention is not beneficial may be more robust. It is worth noting that Manthey et al.'s (2016) findings are the highest quality evidence in favour of the benefits of the intervention for sustained PA, and there is a difference between Manthey et al.'s (2016) procedure and those of the other four studies which measured sustained PA. This difference may offer explanation as to why inconsistency in findings occurred. In studies which demonstrated no effect (or inconclusive findings, in the case of Meevissen et al., 2011), the writing intervention consisted (at least in terms of formally-prescribed sessions rather than encouragement of participants to continue if they wished) of either a single writing session (Sheldon & Lyubomirsky, 2006; Meevissen et al., 2011) or multiple sessions over one day (Troop et al., 2013), or over seven consecutive days (Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013). In contrast, Manthey et al. (2016) asked participants to complete eight writing sessions over a period of eight weeks. It is possible that this longer intervention-span allowed participants to spend more time thinking about their BPFS than shorter intervention-spans, and therefore led to a greater increase in PA.

[105] Sheldon and Lyubomirsky (2006) did not conduct inferential analyses of the effects of BPFS-W on PA at follow-up (two and four weeks post-intervention). The finding reported here is based on descriptive statistics. See Table 6.4.

[106] Troop et al. (2013) measured three types of PA; activating, relaxed, and safe/ content. Relaxed affect alone increased in BPFS and control groups from baseline to two-week follow-up. However, there was no between-group difference.

Overall, the evidence suggests that BPFS-W elicits immediate gains in PA, regardless of how many writing sessions are used, how long participants write for, or how far apart sessions are spaced. Nevertheless, it appears that this immediate increase in PA is not indicative of sustained therapeutic change[107], as the available evidence suggests that BPFS-W is not usually beneficial in terms of increasing PA long-term. Indeed, Peters et al. (2016) found an immediate benefit to PA following BPFS-W, however their participants' PA returned to baseline in as short a time as 20 minutes post-writing (after an eye-tracking task). Nevertheless, it is possible that sustained benefits occur when longer intervention-spans are used, e.g. when participants are asked to complete writing sessions over a series of weeks, rather than days. Certainly, further evidence must be generated from higher quality studies before conclusions with regards to the longer-term effects of BPFS-W can be drawn. Further research should investigate the impact of total intervention-span on PA, to begin to establish whether it is possible to increase sustained PA using BPFS-W, and if so, what the boundary conditions for this effect are.

6.5.3.2 Negative affect (NA)

Following PA, NA was the second most commonly-measured outcome of the BPFS-W intervention. It was measured in the majority (23) of included studies. All 23 studies measured the immediate, short-term effects of the intervention on NA. Only four investigated longer-term effects. As with the synthesis of evidence conducted to explore the effects of BPFS-W on PA detailed above, immediate and longer-term effects on NA will be discussed separately, for ease of comparison across studies.

[107] Pleasurable activities can induce PA without being therapeutic. Drinking tea is not a therapeutic activity, yet Einöther, Baas, Rowson and Giesbrecht (2015) found that drinking tea elicited significantly greater PA than drinking water after 10 minutes (non-attributable to the effects of caffeine and theanine as these substances do not reach threshold levels in blood plasma and the brain until 30 to 40 minutes post-consumption; Magkos & Kavouras, 2005; Van der Pijl, Chen & Mulder, 2010). Perhaps BPFS-W is enjoyable and makes participants feel positive but is not therapeutically-active and does not lead to longer-term changes in psychological well-being.

The majority (18) of the 23 studies in which the immediate effects of BPFS-W on NA were explored suggest that it does not impact NA (e.g. Aborida, 2016 [108]; Boselie et al., 2014[109]; Frein & Ponsler, 2014; Peters et al., 2010; Renner et al., 2014[110]; Titova et al., 2017; Winn & Troop, 2002). The results of only five studies suggest that it does impact NA (Harrist et al., 2007; Liau et al., 2016[111]; Manthey et al., 2016; Odou & Vella-Brodrick; 2013; Seear & Vella-Brodrick, 2013; Yogo & Fujihara, 2008[112]). There were no clear distinguishing procedural factors or areas of ROB between these five studies and those which did not find an effect. It is therefore unclear as to why a small amount of evidence showed the intervention to be beneficial in terms of immediate dampening of NA, whilst the majority did not. However, the evidence overwhelmingly suggests that BPFS-W does not impact NA immediately post-writing. Studies varied in terms of whether imagery was used, the length, spacing and frequency of writing sessions and the writing instructions administered. It therefore appears that the null effects are generalisable across variations in administration procedures.

The longer-term effects of BPFS-W on NA were measured in only four studies (Manthey et al., 2016; Meevissen et al., 2011; Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013; Sheldon & Lyubomirsky, 2006). Manthey et al.'s (2016) findings suggest a greater decrease in NA from baseline to four weeks following the final of eight writing sessions (completed over eight consecutive weeks) in those who wrote about a BPFS in comparison to controls.

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[108] In Harrist et al.'s (2007) and Manthey et al.'s (2016) studies and Ph.D. Study One, inferential statistics for effects of BPFS versus control writing on NA were unavailable. NA findings have thus been inferred from descriptive statistics. See Table 6.4.

[109] In Boselie et al.'s (2014), Peters et al.'s (2010), Sheldon & Lyubomirsky's (2006) and Winn & Troop's (2002) studies, there was a significant decrease from pre- to post-writing in BPFS and control groups. However, there was no between-group difference in NA. In Ph.D. Study Two, there was significantly greater NA on the second of four consecutive writing days, in comparison to the first and fourth days, but no between-group difference.

[110] There was no between-group difference in change in NA from pre- to post-writing as measured using the PANAS. However, Renner et al. (2014) also used a VAS with 'positive' at one pole, and 'negative' at the other, and found a greater decrease in NA in the BPFS group than controls. Therefore, the effects of Renner et al.'s (2014) BPFS manipulation on NA are unclear.

[111] Liau et al. (2016) found a greater drop in NA in BPFS participants than controls from before to after the first writing session, but no significant between-group difference in change in NA after the second.

[112] Yogo and Fujihara (2008) used the depressed/ anxious affect subscale from the MMS. This was included in the NA rather than depression and anxiety syntheses as it is impossible to separate anxious and depressed affect scores using this instrument. A measure of depressed/ anxious mood together is likely reflective of NA. They reported a decrease in NA in the BPFS group from pre-to post-writing but did not report whether this differed from change in controls.

Conversely, findings from the other three studies[113], demonstrate no between-group difference. Odou and Vella-Brodrick's (2013) and Seear and Vella-Brodrick's (2013) analyses were underpowered. However, risk of type two error appears to be low; descriptive statistics suggest that the effect which occurred immediately post-writing had dissipated at follow-up. Analyses in all other studies were sufficiently powered, thus null findings are unlikely to be type two errors. There are, however, areas of ROB in these studies. Both Manthey et al. (2016) and Meevissen et al. (2011) used affect measures which had been translated without linguistic validation, as previously discussed in the narrative surrounding PA. This reduces the likelihood that the results reflect the true effects of BPFS-W on NA, thus findings should be treated with caution. It is possible that Manthey et al.'s (2016) use of a non-linguistically-validated measure offers explanation as to why an effect was yielded in their study but not in the other three studies; perhaps Manthey et al. (2016) did not measure NA at all. Nevertheless, it is equally possible that the discrepancy in findings is attributable to Manthey et al.'s (2016) intervention-span being longer than the intervention-spans in the other studies. This may have allowed participants to spend more time thinking about their BPFS than the procedures of other studies allowed, resulting in a greater long-term reduction in NA.

Overall, the evidence suggests that BPFS-W is unlikely to impact NA immediately post-writing. In terms of sustained effects, it appears that the intervention is not usually beneficial, but that it may be beneficial when longer intervention-spans are used. Further research, using completely validated measures, should be undertaken to explore this possibility.

6.5.3.3 Optimism, positive and negative future-expectancies, and optimistic explanatory-style

The effect of BPFS-W self on optimism— and P-FEX, N-FEX and optimistic explanatory-style— was measured in 11 studies. The majority (8) explored the immediate effects of BPFS-W on P-FEX and N-FEX (Boselie et al., 2014; 2016a; 2016b; 2017; Hanssen et al., 2013; Meevissen et al., 2011; Peters et al.,

[113] Meevissen et al. (2011) reported a significant decrease in NA in both BPFS and control groups. However, from the analyses reported, there does not appear to be a significant between-group difference in change in NA.

2010; 2016). Findings were consistent; all eight studies demonstrated that individuals who wrote about a BPFS reported significantly greater P-FEX and significantly lower N-FEX immediately post-writing. The consistency in findings, along with the fair quality of the evidence, suggests that findings are robust. It is possible to conclude with some confidence that BPFS-W is beneficial for increasing P-FEX and decreasing N-FEX immediately post-writing.

Liau et al. (2016) and Peters et al. (2010) measured optimism immediately post-intervention and found that BPFS-W did not appear to increase optimism. These findings are unexpected given the consistency in findings that the intervention is beneficial in terms of increasing positive, and decreasing negative, future-expectancies. There are, however, some characteristics of Liau et al.'s (2016) and Peters et al.'s (2010) studies which may explain this apparent inconsistency.

There are several areas of ROB in Peters et al.'s (2010) study which may explain the null results found with regards to the effect of BPFS-W on optimism. First, Peters et al.'s (2010) analyses were underpowered. This reduces the confidence with which it is possible to interpret the null findings as evidence that BPFS-W does not impact optimism. It may be that the intervention did lead to an increase in optimism but analyses failed to detect it (although this does appear unlikely because descriptive statistics suggest negligible between-group differences). Second, Peters et al.'s (2010) optimism measure had been created for their study and had not undergone empirical validation. It consisted of two items, asking participants to indicate, using 10-point Likert scales, how positive their expectations were about the coming week, and how optimistic they felt about their future. The use of this non-validated scale introduced high ROB into Peters et al.'s (2010) findings, as participants' scores on this measure may not be reflective of their true optimism levels (Streiner et al., 2015). Even if the construct measured is optimism, the between-group variation demonstrated by it may not reflect the true variability in optimism in the sample (Streiner et al., 2015). Finally, Peters et al. (2010) did not measure optimism at baseline. Failure to control for possible baseline between-group differences in analyses means that it is not possible to conclude that participants in both groups were

influenced equally. Peters et al.'s (2010) findings are therefore inconclusive and should not be treated as persuasive evidence that BPFS-W does not increase optimism.

Although Peters et al.'s (2010) null findings with regards to the effects of BPFS-W on optimism may have arisen from bias, Liau et al.'s (2016) analyses were not underpowered, and their measure of optimism had undergone empirical validation. Therefore, Liau et al.'s (2016) null finding is more likely to be an accurate reflection of the effects of BPFS-W on optimism than that of Peters et al. (2010). However, the difference in findings between Liau et al.'s (2016) study and those which found BPFS-W beneficial in terms of modifying future-expectancies may still be attributable to the measures used. Liau et al. (2016) used the LOT-R to measure optimism, whereas the authors of the other studies used either the SPT or an adaptation of the SPT (the FES) to measure future-expectancies. There are two reasons why these measures may yield contrasting results. The first is that the possible range of scores on the LOT-R is much smaller than the possible ranges on the SPT and the FES. The LOT-R contains six items (plus four 'fillers'), and each item is scored on a five-point Likert scale (possible score range= 0-24). The SPT consists of 20 items for the N-FEX subscale, and 10 items for the P-FEX subscale. The FES contains 10 items for the N-FEX subscale, and 10 items for the P-FEX subscale. Each item in both the SPT and the FES is scored on a seven-point Likert scale. The SPT has a range of 10 to 70 and 20 to 140 for P-FEX and N-FEX respectively, and the FES has a range of 10 to 70 for both P-FEX and N-FEX. Scales with larger quantities of items and more alternative responses on Likert scales (up to seven items; Lozano, García-Cueto & Muñiz, 2008; Preston & Colman, 2000) possess greater discriminatory power, precision, and sensitivity to variation (Green & Roa, 1970; Lozano et al., 2008; McDowell, 2006; Organisation for Economic Co-operation and Development, 2013; Preston & Colman, 2000). It is therefore possible that the SPT and FES have greater discriminatory power than the LOT-R and may have detected changes in optimistic thoughts which the LOT-R was not sensitive enough to detect.

The structure of the LOT-R is not the only property of the scale which may make it less sensitive than the SPT and FES; it is also a more general measure of an individual's expectations for the future. The LOT-R contains only broad items such as 'If something can go wrong for me, it will' and 'I'm always optimistic about my future'. Many of the items in the SPT and the FES are specific, such as 'You will have health problems' and 'You will make good and lasting friendships'. Perhaps studies which employed the SPT or the FES found effects which Liau et al. (2016) did not find using the LOT-R because the changes in optimistic thoughts which occurred immediately following BPFS-W were specific rather than broad, global changes in optimism overall. This is to say that perhaps changes in several specific life domains would be sufficient for change in future-expectancies on the SPT and the FES to be detected, whereas the LOT-R would only be sensitive to generalised, non-domain-specific changes in future-oriented thought. Of course, if this is the case then it is not true to state that changes in scores on the SPT or FES are reflective of changes in optimism; the term 'optimism' pertains to an individual's feelings about and perceptions of their whole future life, rather than specific domains of it (Carver, 2014; Scheier & Carver, 1985). The current state of the literature appears to suggest, therefore, that BPFS-W can encourage individuals to perceive their future more positively immediately post-writing, but perhaps this does not translate into immediate gains in the broader construct of optimism.

The evidence surrounding the sustained effects of BPFS-W on optimism, P-FEX and N-FEX, and optimistic explanatory-style is both sparser and more inconsistent than the evidence surrounding immediate effects. The longer-term effects were measured in only three studies (Meevissen et al., 2011; Peters et al., 2013; Ph.D. Study Two), and findings both within and across these studies are conflicting.

Sustained optimism was measured in all three studies; one and two weeks post-writing in Meevissen et al.'s (2011) and Peters et al.'s (2013) studies, and four and eight weeks post-writing in Ph.D. Study Two. Meevissen et al. (2011) and Peters et al. (2013) found significantly higher optimism in BPFS-W participants than controls two weeks post-writing; this effect was not apparent

one week post-writing in either study. Conversely, in Ph.D. Study Two, there was no significant difference between BPFS and control groups four or eight weeks post-writing. There are several possible explanations for this difference in findings. It is conceivable that BPFS-W does beneficially impact optimism, but that this effect does not become apparent until two weeks post-intervention and dissipates before four weeks post-intervention. However, it is important to acknowledge that in both Meevissen et al.'s (2011) and Peters et al.'s (2013) studies, the writing intervention was supplemented with mental imagery about BPFSs (or daily activities, in the control group), which participants completed for two weeks and one week post-writing, respectively. In Ph.D. Study Two, no supplementary imagery was administered. It is therefore possible that imagery is necessary for sustained effects of BPFS-W to occur; to the point that it may be that it is the imagery, rather than the writing, which brings about therapeutic change. It should also be noted that, if performing mental imagery about a BPFS does actively impact optimism, the effects found by Meevissen et al. (2011)— and to a lesser extent Peters et al. (2013)— are not truly 'sustained' effects. Meevissen et al.'s (2011) two-week follow-up was completed on the day of the final imagery session, and Peters et al.'s (2013) two-week follow-up was conducted one week following the final imagery session. Perhaps, therefore, their results reflect the acute effects of the imagery on optimism. This may appear unlikely given that Peters et al. (2010) found no change in optimism immediately following their formal writing and imagery session. However, perhaps participants experienced an immediate boost in optimism following later imagery sessions having practiced it and become better able to perform it. It would be useful for further research to empirically compare the effects of BPFS-W both with and without the use of a supplementary imagery exercise on optimism, to assess the impact of imagery. Optimism should be measured at staggered follow-up points, to ascertain when effects emerge and when they dissipate.

As well as assessing optimism, Meevissen et al. (2011) and Peters et al. (2013) measured optimistic explanatory-style. Meevissen et al. (2011)[114] found no significant between-group difference in optimistic explanatory-style at one-week and two-week follow-ups. On the other hand, Peters et al. (2013) found that BPFS-W participants reported increased optimistic explanatory-style relative to controls at one-week follow-up (although this dissipated by the two-week follow-up). It is unclear as to why this difference in findings between Meevissen et al.'s (2011) and Peters et al.'s (2013) studies has occurred; their procedures are strikingly similar. Both required that participants completed a single writing session in a laboratory, with instructions to focus their writing on three specific spheres; personal, professional and relational. Both used supplementary imagery and administered follow-ups after one and two weeks. Both studies were also of similar quality, and there are no clear areas of ROB which could be expected to explain the contrasting findings observed. The only difference between these studies which could potentially have resulted in inconsistency is the structure of the writing sessions administered. In Peters et al.'s (2013) study, participants wrote for 15 minutes, and spent five minutes on each of the three spheres. Meevissen et al.'s (2011) writing task was less structured; participants were given 20 minutes to write down statements about their personal, professional and relational BPFS, starting each statement with 'In the future I will'. They were then asked to write these statements in the form of a story, and no time-limits were given for forming the narrative. It is possible that for gains in optimistic explanatory-style to be yielded, the participant should complete a structured, time-limited writing task, although it is not clear why this might be. Meevissen et al. (2011) did not report how long participants wrote for, or whether there were between-group differences in writing time. Therefore, one possibility is that Meevissen et al.'s (2011) participants did not write in prose for long enough for effects to occur. This suggestion is more conceivable than participants writing for too long, given that Meevissen et al.'s (2011) participants were largely students who received a €25 gift certificate in return for their time in taking part in the study. When a financial reward is offered, a smaller proportion of the sample are likely to take part in the study for reasons such as benefiting

[114] Meevissen et al. (2011) also conducted within-group analyses and found an increase in optimistic explanatory-style from baseline to one-week follow-up in the BPFS group, and no significant change in the control group. However, there was no significant between-group difference at two-week follow-up.

others or advancement of knowledge; the intrinsic motivation of participants to engage with the study is likely to be lower, thus participants may be less conscientious (Callison-Burch, 2009; Downs, Holbrook, Sheng & Cranor, 2010; Fry & Dwyer, 2001; Russell, Moralejo & Burgess, 2000; Zutlevics, 2016). Therefore, perhaps Meevissen et al.'s (2011) participants wrote for a minimal amount of time, and as such did not benefit from the intervention in terms of gains in optimistic explanatory-style. Further research is needed to ascertain whether length and structure of writing tasks impacts effects of BPFS-W on optimistic explanatory-style, and if so, to identify the parameters required for increases in optimistic explanatory-style to occur.

Meevissen et al. (2011) were the only authors to measure the sustained effects of the intervention on P-FEX and N-FEX. Results demonstrated significantly lower N-FEX in the BPFS group in comparison to controls, one and two weeks following a single writing session. Meevissen et al. (2011) found no significant between-group difference in P-FEX, but post-hoc analyses suggested that BPFS participants did significantly increase in P-FEX over time, whilst control participants demonstrated no significant change. It thus appears that BPFS-W may be beneficial in terms of sustained change in future-expectancies. However, it should be acknowledged that, as previously discussed, Meevissen et al.'s (2011) participants completed daily supplementary imagery about their BPFS (or daily activities) throughout the follow-up period. Therefore, if imagery does actively impact future-expectancies, then the effects found in Meevissen et al.'s (2011) study are not truly long-term effects.

Overall, the evidence suggests that BPFS-W is beneficial in terms of encouraging optimistic, positive thoughts. At the very least, it has been consistently found to increase P-FEX and dampen N-FEX immediately post-writing. It does, however, appear that the intervention does not elicit immediate gains in optimism broadly. The evidence surrounding the sustained effects of BPFS-W on optimism and related variables is inconclusive. There are procedural variations across studies which could offer explanation as to why inconsistent findings were obtained, such as differences in lengths of follow-up periods. However, some procedural factors (such as the use of supplementary

imagery throughout follow-ups, and authors failing to report the temporal length of writing sessions; Meevissen et al., 2011; Peters et al., 2013) make it difficult to draw conclusions with any confidence. Further research is therefore needed; first to ascertain whether BPFS-W does lead to sustained gains in optimism and optimistic thoughts, and second to identify the procedural parameters within which improvements are best encouraged.

6.5.3.4 Anxiety

Anxiety was measured as an outcome of BPFS-W by Renner et al. (2014), and in Ph.D. Study One. The results of these studies were consistent. In Ph.D. Study One[115], there appeared to be no significant difference between BPFS-W and control writing participants in anxiety one, four and eight weeks following a single writing session, and no change over time. In Renner et al.'s (2014) study, there was no significant between-group difference in scores on a bipolar VAS with 'anxious' at one pole and 'secure' at the other, administered immediately post-writing. Renner et al.'s (2014) analyses were underpowered according to desired sample size estimates, however it appears that the null effect found is not a type two error because the p value reported is large and the descriptive statistics demonstrate comparable levels of anxiety in each group. Ph.D. Study One and Renner et al.'s (2014) study were otherwise found to be at low ROB, thus it appears that findings are robust. Taken together, the results of these studies suggest that there is no immediate or long-term benefit of BPFS-W in terms of ameliorating anxiety, at least when a single writing session is administered.

6.5.3.5 Stress

Stress was measured in two studies (Ph.D. Study One and Troop et al., 2013). Findings were consistent. Troop et al. (2013) found no significant difference between BPFS participants and controls in stress levels two weeks following three fifteen-minute sessions, and no significant change over time. In Ph.D. Study One, there appeared to be no significant between-group differences in stress one, four and eight weeks following a single writing session. Despite the

[115] Stress, anxiety and depression findings from Ph.D. Study One are based on descriptive statistics, as it was not possible to extract pure effects of BPFS-W versus control writing from inferential analyses. Patterns from descriptive statistics mirrored outcomes of inferential analyses. See Table 6.4.

small number of studies, it is likely that these null results give an accurate reflection of the effects of BPFS-W on stress. Both studies were found to be at a low ROB and possess sufficiently large samples for analyses to have detected a significant effect had one occurred. Furthermore, follow-ups across the two studies provided potential for both short-term and latent changes in stress to be detected. It should be acknowledged that there may be some bias from the self-report measures used in Ph.D. Study One and in Troop et al.'s (2013) study. Measurement of stress is difficult due to human lives being complex and life-stress being multifaceted (Monroe & Roberts, 1990), meaning that results of any self-report measure of stress should be treated with caution. However, the null findings corroborate the results of Austenfeld's (2007) and Austenfeld and Stanton's (2008) study, from which it was possible to tentatively infer that BPFS-W did not appear to have impacted individuals' blood pressure one month post-intervention. Stress has been found to be associated with high blood pressure (McCraty, 2004; Matthews, Cottington, Talbott, Kuller & Siegel, 1987; Sparrenberger et al., 2009; Vrijkotte, van Doornen & de Geus, 2000) and other interventions found to successfully reduce self-reported stress have also been found to reduce blood pressure (e.g. Carlson, Speca, Faris & Patel, 2007; McCraty, 2004). Therefore, if there was a change in stress because of BPFS-W which was undetected by self-report measures, there would likely have been an accompanying change in blood pressure. Collectively, the low ROB in Ph.D. Study One and Troop et al.'s (2013) study, and the physiological evidence from Austenfeld's (2007) and Austenfeld and Stanton's (2008) study, suggest that BPFS-W is unlikely to reduce stress.

6.5.3.6 Depression

Depression was measured in six studies, which yielded mixed findings. All were found to be of fair quality and were adequately powered, thus the conflicting results are likely to be accurate representations of the effects of each manipulation. To allow fair comparison across studies, immediate and long-term effects of the intervention on depression are discussed separately.

The immediate, post-test effects of BPFS-W on depression were measured in three studies. Shapira and Mongrain (2010) and Liau et al. (2016) found no

significant difference in depression levels immediately post-writing between BPFS and control groups. On the other hand, Manthey et al.'s (2016)[116] results suggest that BPFS-W did lower symptoms of depression immediately post-writing. There was a difference in the writing instructions used which may offer explanation as to why Manthey et al.'s (2016) study yielded an immediate reduction in depression and the other two studies did not. Shapira and Mongrain (2010) and Liau et al. (2016) asked participants to write about a general BPFS and suggested areas to include (e.g. family life). Conversely, Manthey et al.'s (2010) participants were required to write about a different sphere of their BPFS (specified by the authors) during each of eight sessions. It is therefore possible that this additional structure is needed for immediate reductions in depression to occur. Furthermore, during the final writing session— after which immediate measures of depression were taken— participants chose their own topic. Perhaps this opportunity to focus on a single area of their future which was of particular importance to them was critical for reductions in depression to occur immediately post-writing.

The longer-term impacts of BPFS-W on depression were measured in five studies. Again, findings were mixed. No significant benefits in terms of reducing depression were found in the study by Austenfeld (2007) and Austenfeld and Stanton (2008), in Austenfeld et al.'s (2006) work, or in Ph.D. Study One. On the other hand, the immediate effect on depression found by Manthey et al. (2016) was found to have been maintained at a four-week follow-up. Shapira and Mongrain (2010), who found no effect on depression immediately post-intervention, reported significantly lower depression in BPFS participants relative to controls at one- and three-months follow-ups (although there was no significant between-group difference at a six-month follow-up). There are several procedural differences between the two studies which did demonstrate a sustained reduction in depression and the three which did not. The studies by Austenfeld (2007) and Austenfeld and Stanton (2008) and Austenfeld et al. (2006) included three sessions, which were 20 and 25 minutes long, respectively. In Ph.D. Study One there was a single, 20-minute session. On the other hand, Manthey et al. (2016) and Shapira and Mongrain (2010) instructed

[116] Based on descriptive statistics. See Table 6.4 for further details.

participants to write for seven and eight sessions, respectively. They did not report time-limits. It is tempting to suggest that more than three writing sessions are required for long-term changes in depression to occur, however Manthey et al. (2016) and Shapira and Mongrain (2010) included participants in analyses if they completed at least one session. Therefore, it is possible that their participants did not complete a greater number of sessions than the participants in the studies which did not yield an effect. An alternative explanation for the discrepancy in findings stems from the setting and timing of writing sessions. In the studies by Austenfeld (2007) and Austenfeld and Stanton (2008), and Austenfeld et al. (2006), and in Ph.D. Study One, participants completed the intervention in a laboratory or semi-private cubicle and were asked to write for a prespecified amount of time. Conversely, Manthey et al.'s (2016) and Shapira and Mongrain's (2010) participants wrote online, and no time-limits for writing sessions were reported. This could be non-reporting, but it is possible that participants were allowed to write for as long as they liked. Perhaps, therefore, it is the flexibility of being able to write when and where participants liked and for as long as they liked which made Manthey et al.'s (2016) and Shapira and Mongrain's (2010) BPFS-W tasks beneficial for long-term reductions in depression, although it is not clear why this might be. Further research is needed to determine the number, length, and setting of writing tasks required for sustained reductions in depression to occur following BPFS-W.

To summarise, it appears that BPFS-W may lead to an immediate decrease in depression but perhaps only when participants are given instructions which allow them time to focus on specific areas of their future life. Sustained reductions in depression are also possible, but perhaps only when more than three writing sessions are used, or when participants are allowed flexibility in terms of when and where they write and for how long.

6.5.3.7 Neuroticism

Neuroticism was measured by Meevissen et al. (2011) only. There was no significant difference between BPFS and control participants in neuroticism at one- and two-week follow-ups (when baseline levels were controlled for). The study was of generally high quality and was adequately powered, suggesting

that the null result is an accurate representation of the effect of BPFS-W on neuroticism, relative to the control task of writing about daily activities. Indeed, it is theoretically conceivable that BPFS-W does not reduce neuroticism. Generally, neuroticism is regarded as a relatively stable personality trait which is moderately heritable and genetically-influenced (Cobb-Clark & Schurer, 2012; Floderus-Myrhed, Pederson & Rasmuson, 1980; Jang, Livesley & Vemon, 1996; Lahey, 2009). Some evidence suggests that neuroticism is malleable when individuals undergo psychological interventions, but that changes in scores on neuroticism scales may reflect changes in symptoms of psychological illness, rather than changes in neuroticism as an underlying personality trait (Armstrong & Rimes, 2016; Farmer et al., 2002; Spinhoven, Huijbers, Ormel & Speckens, 2017). Therefore, given that BPFS-W appears not to elicit reductions in anxiety, stress and (to an extent) depression, it is unsurprising that it was not found to reduce neuroticism. This, coupled with the relatively low ROB in Meevissen et al.'s (2011) study, suggests that it is unlikely that BPFS-W impacts neuroticism— at least not before two weeks post-writing.

6.5.3.8 Burn-out

The impact of BPFS-W on burn-out was investigated by Aborida (2016) only. There was no significant difference between the BPFS and control groups in change in burn-out levels from before to immediately following the final of five writing sessions. Aborida's (2016) study was underpowered according to desired sample size estimates, therefore it is possible that the null effect may represent a type two error. However, it appears that the risk of this is relatively low, because the p values reported are very large and descriptive statistics suggest negligible between-group differences. Aborida's (2016) study is otherwise of fair quality, thus it appears that this finding is robust. It is important to acknowledge, however, the differences between the writing task used by Aborida (2016) and standard writing tasks. Typically, the BPFS-W intervention involves writing for at least twenty minutes (e.g. Frein & Ponsler, 2014; Geschwind et al., 2015; Hanssen et al., 2013), whereas Aborida (2016) asked participants to complete five minutes of BPFS-W followed by three minutes of writing about the processes towards a BPFS. It is possible that this is not enough time for participants to truly engage with the task and adequately

envision a better future. This is especially likely given that Aborida (2016) found no significant difference between the BPFS group and controls on any outcome variable (including PA immediately post-intervention, which as previously discussed has been found to be consistently induced by BPFS-W). It should also be noted that Aborida (2016) measured only the immediate effects of the intervention on burn-out, thus a longer-term benefit remains a possibility. It would be useful for future research to explore the effects of the standard BPFS-W intervention on burn-out, with a longer follow-up and a larger sample, before ruling out the possibility that the intervention is beneficial for this outcome.

6.5.3.9 Life-satisfaction

Life-satisfaction was measured in five studies[117]. Findings were conflicting. In Boehm et al.'s (2011) and Manthey et al.'s (2016) studies, there was a significantly greater increase in life-satisfaction in BPFS participants than controls over time (from baseline to immediately following the sixth writing session and baseline to a one-month follow-up, and from baseline to the eighth and final writing session to a four-week follow-up, respectively). In Peters et al.'s (2013) study, BPFS participants reported a greater increase in life-satisfaction from one day before the single writing session to one week following writing (with daily supplementary imagery every day in between). However, this effect was not maintained two weeks post-writing (one week after the final imagery session). Conversely, in Liau et al.'s (2016) study, there was no significant difference between BPFS participants and controls from immediately before the first writing session to immediately post the second and final writing session, and in Ph.D. Study Two there was no significant between-group difference four and eight weeks post-writing. All five studies are of fair and comparable quality, and there are no clear procedural differences between studies that did and did not find BPFS-W to be beneficial. It is therefore unclear as to why differences in findings occurred. Further research is needed to determine under what conditions the intervention boosts life-satisfaction.

[117] A further two studies measured life-satisfaction as part of psychological well-being composites. Vaughn et al. (2003) found significantly greater well-being in those who wrote about a BPFS outcome than in controls and those who wrote about the process towards their BPFS. Lyubomirsky et al.'s (2011) descriptive statistics suggested no difference between BPFS participants and controls. Results should be treated with caution as it is impossible to isolate effects on life-satisfaction alone due to presence of other measures. See Table 6.4 for details.

6.5.3.10 Mental well-being

Mental well-being[118] was measured in only one study (Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013). There was no significant difference between the BPFS group and controls in mental well-being immediately following the seventh and final writing session or at two-week follow-up, when baseline levels were controlled for. The sample was large enough for analyses to have sufficient statistical power to detect an effect immediately post-writing had one occurred. Analyses for the two-week follow-up were underpowered according to desired sample size estimates, however descriptive statistics suggest comparable between-group differences to those found immediately post-writing. Risk of type two error, therefore, appears low. However, there is a procedural factor which may have induced bias and could account for the apparent lack of benefits of BPFS-W for mental well-being. The authors did not give participants a time-limit as to how long they should write for and did not report what the average length of writing was. It is therefore possible that participants did not write for long enough to engage with the intervention properly. This suggestion is purely speculative; it is equally possible that a level of time pressure is necessary for participants to focus sufficient attention on their writing. These possibilities should be investigated before conclusions with regards to the effects of the intervention on mental well-being are attempted. Further research should include partial replication of Odou and Vella-Brodrick's (2013) and Seear and Vella-Brodrick's (2013) study, with a larger sample and instructions for some participants to write for a sufficient but constrained time-period (e.g. 20 minutes; King, 2001), and others to write for as long as they wish.

6.5.3.11 Happiness

Happiness (and happy and sad affects) were measured in four of the studies included in the current review (Austenfeld et al., 2006; Ng, 2016; Renner et al., 2014; Shapira & Mongrain, 2010). For ease of comparison, short-term and long-term effects of BPFS-W on happiness are discussed separately.

[118] Mental well-being- in the context of the Warwick-Edinburgh Mental Well-Being Scale used in this study- refers to levels of psychological functioning, life-satisfaction, and ability to have positive relationships (Stewart-Brown & Janmohamed, 2008).

The immediate, short-term effects of BPFS-W on happiness were measured by Renner et al. (2014) and Shapira and Mongrain (2010). Renner et al. (2014) administered two VASs, the first with poles labelled 'happy' and 'sad' and the second with poles labelled 'dull' and 'glad'[119] immediately before and immediately after a single writing session. They found that there was a significantly greater decrease in sadness and dullness (and therefore a greater increase in happiness and gladness) in those who wrote about a BPFS in comparison to controls. Shapira and Mongrain (2010) measured happiness before and after each of seven writing sessions and found that BPFS participants reported greater happiness post-writing in comparison to controls. It should be noted that sampling error may have occurred in Renner et al.'s (2014) study. The authors attempted to induce negative mood prior to the writing session, and BPFS participants demonstrated a greater increase in sadness and dullness (and thus a greater decrease in happiness and gladness) than controls. Therefore, it is possible that the greater increase in happiness found in the BPFS group in comparison to controls following writing is attributable to between-group differences in participant characteristics, rather than effects of the BPFS-W task. For example, perhaps Renner et al.'s (2014) BPFS participants were more emotionally reactive than controls. Conversely, Shapira and Mongrain's (2010) work was found to be of high quality, thus it is likely that their findings can be trusted. Therefore, the consistency in Renner et al.'s (2014) and Shapira and Mongrain's (2010) findings suggests that BPFS-W is beneficial in terms of eliciting immediate gains in happiness. Furthermore, Renner et al.'s (2014) and Shapira and Mongrain's (2010) intervention procedures differed considerably. Renner et al. (2014) asked participants to complete a single, 15-minute writing session, followed by five minutes of mental imagery, about their general BPFS. On the other hand, Shapira and Mongrain's (2010) participants were asked to complete seven sessions over seven consecutive days, with no time-limits apparent and no mental imagery. They were asked to write about a different sphere of their BPFS each day. This consistency across studies with different procedures is important, as it suggests

[119] Gladness was included in the discussion of effects of BPFS-W on happiness because semantics and linguistics literature regards 'happy' and 'glad' as semantically-equivalent (Huong & Van Lam, 2008; Van Lam, 2016).

that procedural variations do not affect the immediate effects of the intervention on happiness.

Evidence surrounding the sustained effects of BPFS-W is less consistent. Austenfeld et al. (2006), Ng (2016) and Shapira and Mongrain (2010) measured happiness as a longer-term outcome variable. In Shapira and Mongrain's (2010) study, the immediate benefit to happiness was found to have dissipated by the one-month follow-up, however there was significantly greater happiness in the BPFS group in comparison to controls at three- and six-month follow-ups. This finding suggests that BPFS-W not only provides an immediate happiness boost from taking part in a pleasant activity, but that it leads to long-term, beneficial change. In contrast, Austenfeld et al.'s (2006) and Ng's (2016) findings[120] suggest that there was no significant difference between BPFS-W participants and controls in happiness three months and three weeks post-writing, respectively. It is unclear why the difference in findings between Austenfeld et al.'s (2006), Ng's (2016) and Shapira and Mongrain's (2010) studies occurred. During the ROB assessment, it was found that all three studies were generally at low ROB and analyses were not underpowered. In terms of intervention procedures, all used multiple sessions, and none supplemented BPFS-W with imagery. One possible explanation for the discrepancy is that Shapira and Mongrain (2010) used more writing sessions than Ng (2016) and Austenfeld et al. (2006). Shapira and Mongrain (2010) asked participants to complete seven sessions over seven days and sent e-mail reminders between the main intervention period and the six-month follow-up to encourage participants to continue with the task (although it should be noted that participants were included in analyses if they completed at least one session). On the other hand, Austenfeld et al. (2006) administered only three sessions, and Ng (2016) asked participants to complete one formal laboratory session and then continue at home over the following three weeks[121]. It is possible that BPFS-W induces sustained changes in happiness only when higher 'doses' of writing are used. Nevertheless, although Austenfeld et al. (2006), Ng (2016) and Shapira and Mongrain (2010) were the only authors to include happiness as an outcome per

[120] Findings reported here are based on inferences from descriptive statistics and possible selective recording of effects, thus should be treated with caution. See Table 6.4 for details.
[121] Ng (2016) did not report how many sessions participants completed in total.

se, Lyubomirsky et al. (2011)[122] included the SHS in a composite measure of general well-being, and found that there appeared to be no significant difference between the BPFS group and controls in well-being immediately after the eighth and final writing session and at a six-month follow-up. Although it may appear that this absence of an effect following eight writing sessions negates the suggestion that more sessions are needed to induce long-term changes in happiness, it is possible that an effect on happiness was masked by lack of change in the other variables in Lyubomirsky et al.'s (2011) composite; life-satisfaction and unpleasant and pleasant affect. Therefore, the effects of BPFS-W on sustained, long-term happiness are unclear. It would be beneficial for experimental manipulations of numbers of writing sessions to be conducted to explore the possible moderating influence of this factor on the effects of the intervention.

6.5.3.12 Self-esteem

Self-esteem was measured only by Murn (2014). There was a significantly greater increase in self-esteem from pre- to post-intervention (immediately following the third and final writing session), and from post-intervention to six-to-ten-week follow-up, in BPFS participants in comparison to controls. These results suggest that not only does BPFS-W immediately boost self-esteem, but that it also has a sustained beneficial effect. Murn's (2014) study is generally of high quality, thus it is likely that this finding is robust. However, there is a potential area of bias in Murn's (2014) study which must be acknowledged. Follow-up measures were taken six to ten weeks following the intervention, and the author did not perform analyses to ensure that there was no significant between-group difference in the time span between completion of the intervention and the follow-up. This is problematic because the BPFS and control groups may have completed the follow-up at different times. It is therefore impossible to rule out the potential that both groups improved equally in terms of self-esteem, but the control group completed follow-up measures

[122] Inferential analyses for effects of BPFS-W in comparison to control were not reported; effects of BPFS-W were combined with those of a gratitude task. Therefore, findings discussed here are based on descriptive statistics from the BPFS group and controls, as well as interpretations of the likely inferential outcomes from what has been reported by Lyubomirsky et al. (2011). Findings should be treated with caution. See Table 6.4.

either before the effect was sufficiently large to be detected or after it had dissipated, and the BPFS group did not. Murn's (2014) study should be replicated with equal follow-ups ensured to allow conclusions with regards to the sustained effects of BPFS-W on self-esteem to be drawn with more confidence. Nevertheless, it is important to note that the potential inequality of follow-up time-points cannot explain the immediate, post-test benefits of writing about a BPFS on self-esteem found in Murn's (2014) work. Although the evidence is from a single study, it does appear likely that writing about a BPFS elicits immediate increases in self-esteem, at least when Murn's (2014) procedure of writing using the standard writing instructions for three 20-minute sessions over three consecutive days is employed.

6.5.3.13 Body-esteem and body-comparison

Body-esteem (and body-comparison; a construct found to significantly correlate with body-esteem, in that the more positive feelings an individual has about their body, the less frequently they are likely to compare their body to the bodies of their same-sex peers; Murn, 2014) were measured by Murn (2014) only. There were no significant differences between participants who wrote about a BPFS for 20 minutes a day on three consecutive days and controls in change in body-esteem or body-comparison from baseline to immediately post-intervention and six- to 10-week follow-up. It appears that these findings can be trusted, as Murn's (2014) study was found to be generally of high quality and analyses were not underpowered. It appears likely that writing about a general BPFS would not impact body-esteem and body-comparison; it is possible that Murn's (2014) participants did not include aspects about the functionality or appearance of their bodies in this narrative. If participants did not write about their goals surrounding their body, then it is conceivable that their thoughts and feelings about their body would not change. It would be interesting for future research to assess the impact of writing about a body-specific BPFS on body-esteem and body-comparison, especially given that Murn (2014) did find that writing about a global, general BPFS was beneficial in terms of increasing global self-esteem.

6.5.3.14 Self-compassion, self-reassurance, and self-criticism

Self-compassion is the cognitive treatment people give themselves when they have made a mistake; either through kindness and understanding or through self-criticism (Neff, 2003a). The concept of self-reassurance is similar and has been regarded as an aspect of the broader construct of self-compassion (Hermanto & Zuroff, 2016; Kupeli, Chilcot, Schmidt, Campbell & Troop, 2013). The term refers specifically to the ability to reassure oneself compassionately, kindly and supportively when faced with difficulty, and can be a way of coping with setbacks (Gilbert, 2005, as cited by Kupeli et al., 2013; Gilbert, Baldwin, Irons, Baccus & Palmer, 2006; Gilbert, Clarke, Hempel, Miles, & Irons, 2004). Self-criticism is sometimes (although not always; Kupeli et al., 2013) held as the opposite of self-compassion and self-reassurance (Neff, 2003b). The term refers to negative self-evaluations and self-scrutiny, with fear of disapproval and loss of acceptance, particularly when the individual perceives that they have made an error or failed a goal (Blatt & Homann, 1992; Kupeli et al., 2013).

Self-compassion, and self-criticism and self-reassurance, were measured by Murn (2014) and Troop et al. (2013), respectively. In Murn's (2014) study, there was no significant difference between BPFS and control group participants in change in self-compassion from baseline to immediately post-intervention and follow-up (six to ten weeks post-writing). On the other hand, Troop et al.'s (2013) BPFS-W participants reported a significant decrease in self-criticism, and controls showed no change. For self-reassurance, there was no significant difference in the BPFS group between baseline measurements and two-week follow-up, but in the control group a significant decrease was reported. Both Murn's (2014) and Troop et al.'s (2013) studies were found to be of high quality, suggesting findings are accurate reflections of the effects of BPFS-W on self-compassion, self-reassurance, and self-criticism. From these findings, it is possible that the intervention is not effective in terms of bolstering self-compassion broadly but dampens self-criticism and buffers against reductions in self-reassurance specifically. Troop et al. (2013) noted that their investigation was conducted at a time in the academic year when their student participants were approaching examinations and suggested that BPFS-W may have protected students against the effects that this threatening situation may have

had on self-reassurance levels. It is also entirely possible that Murn (2014) found no significant effect on self-compassion where Troop et al. (2013) did find effects on self-criticism and self-reassurance due to differences in follow-up lengths. Troop et al.'s (2013) follow-up was conducted two weeks following the intervention, whereas Murn's (2014) follow-up occurred after six to ten weeks. It may be that a detectable effect had occurred in Murn's (2014) participants after two weeks, but that this had dissipated after six to ten weeks. It would be useful for further research to be conducted using staggered follow-ups to explore the potential that BPFS-W does impact the wider construct of self-compassion but perhaps with short-lived effects, as well as to establish when effects emerge and dissipate.

6.5.3.15 Dependency

Dependency (a need to lean on others and for social approval; de Graaf et al., 2009) was measured by Renner et al. (2014) only. In this study, there was a significantly[123] larger change in dependency in the control group than in the BPFS group from before to after a single 15-minute writing session. The control group decreased in dependency, whereas the BPFS group increased in dependency. This finding is surprising, as the control task was intended to be neutral and non-therapeutic. These findings could be explained by an area of ROB. Renner et al. (2014) administered a negative mood induction (NMI), in between the baseline assessment and the writing task. Dependency was measured only at baseline and immediately post-writing, thus the effects of the NMI on dependency are unknown. It is therefore possible that the BPFS group increased in dependency because of the NMI and then decreased because of BPFS-W to a level that was above the baseline level (so that an overall increase from baseline to post-writing was yielded). The control group may have increased in dependency because of the NMI to a lesser extent than the BPFS group, and then decreased during the control task to a level below that of the group mean at baseline. This perhaps could have occurred due to dampening of the effects of the NMI over time, or because the neutral writing task served to divert attention from negative feelings. It is conceivable that the NMI

[123] The effect was marginally significant (p= .05) and analyses were underpowered according to desired sample size estimates. Therefore, inferences from this effect should be treated with caution.

differentially impacted BPFS and control participants, as Renner et al. (2014) found that BPFS participants reported a significantly greater increase in NA following the induction in comparison to controls[124], thus may have been more reactive or sensitive in some way.

Although these suggestions do not fully explain why the control task was found to be beneficial[125], they do offer a possible explanation as to why the change in the control group may have been greater than the change in the BPFS group. The important point is that Renner et al.'s (2014) findings surrounding the effects of BPFS-W on dependency relative to a control task may have been confounded by influence of the NMI, and as such should be treated with caution. Renner et al. (2014) presented a different explanation for the unexpected findings. They suggested that past-directed thinking was likely to be involved in the control task of writing about a typical day, and that reflection of past events induces a greater sense of independence. They also suggested that thinking and writing about a BPFS may increase a sense of dependency on and need for others due to the lack of certainty in the future. Renner et al. (2014) asserted that this suggestion is theoretical and encouraged the investment of future research into exploration of this potential effect. However, it would be sensible to first replicate Renner et al.'s (2014) study with the NMI removed, to more accurately establish the immediate effects of writing about a BPFS (and a typical day) on dependency. It would also be useful to include a longer-term measure of dependency, to establish whether a potential negative effect of BPFS-W is likely to be transient or sustained.

6.5.3.16 Perfectionism

Perfectionism was also measured only by Renner et al. (2014). There was no significant difference between BPFS-W participants and controls in change in perfectionist attitudes from pre- to post-writing. It is difficult to draw firm conclusions from this study because of two areas of ROB. First, analyses were

[124] There was a significantly greater increase in NA in BPFS participants on the positive-negative, dull-glad and happy-sad VASs, but no significant between-group difference in PANAS subscales or anxious-secure VAS change-scores.

[125] The effects within each group are based on the direction of change only; analyses to establish whether there were significant within-group changes were not conducted.

underpowered according to desired sample size estimates, thus it is possible that the null effect is a type two error (although the risk of this does not appear to be high, because descriptive statistics suggest between-group differences are negligible). Second, Renner et al. (2014) did not conduct analyses to ascertain whether the NMI impacted BPFS and control group perfectionism levels differentially, as discussed in relation to dependency, above. The study is otherwise at low ROB. Therefore, it is possible that the null effect is an accurate representation of the effects of BPFS-W on perfectionism, at least immediately post-intervention when a single writing session is used. However, some caution should be exercised when drawing conclusions from these findings. Replication of Renner et al.'s (2014) study, with a larger sample and the NMI removed, is warranted before conclusions with regards to the effects of BPFS-W on perfectionism can be drawn.

6.5.3.17 Hostility (and hostile affect)

Hostility (and hostile affect) was measured in three studies (Austenfeld, 2007; Austenfeld & Stanton, 2008; Austenfeld et al., 2006; Yogo & Fujihara, 2008). Findings were consistent. Yogo and Fujihara (2008) found no significant change in hostile affect from before to after a single writing session in the BPFS group[126]. Austenfeld (2007) and Austenfeld and Stanton (2008), and Austenfeld et al. (2006), investigated the longer-term effects of BPFS-W on hostility and found no significant differences between the BPFS group and controls in hostility at one- and three-month follow-ups, respectively (when baseline levels were partialled out). Yogo and Fujihara's (2008) analyses were underpowered, thus it is possible that their null effect is a type two error, however all three studies were otherwise at low ROB. This, coupled with the consistency across the studies, suggests that these findings are an accurate reflection of the effects of BPFS-W on hostility. Procedural differences across studies demonstrate that the intervention is ineffective at reducing hostility whether standard writing instructions are used (Yogo & Fujihara, 2008) or participants write about overcoming an obstacle as part of their BPFS narrative (Austenfeld, 2007; Austenfeld & Stanton, 2008; Austenfeld et al., 2006) or about a specific element

[126] Assumed non-significant as this effect was unreported; it appears that Yogo and Fujihara (2008) selectively reported only significant effects. This finding should be treated with caution.

of their BPFS (Austenfeld et al., 2006). Spacing of sessions also does not appear to impact effects on hostility; null findings occur when sessions are completed over one week (Austenfeld, 2007; Austenfeld & Stanton, 2008; Austenfeld et al., 2006) or two weeks (Yogo & Fujihara, 2008). The range of follow-up time-points also suggest that it is unlikely that an effect occurred and dissipated between measurements. It would be justifiable to replicate Yogo and Fujihara's (2008) investigation of immediate effects using a larger sample size. However, it is possible to conclude with some confidence that BPFS-W does not impact hostility either immediately post-writing or longer-term, and that null findings are generalisable across intervention procedures— or at least across the procedures used in the studies discussed here.

6.5.3.18 Fearful and guilty affects

The impacts of BPFS-W on fearful and guilty affects were measured in Austenfeld et al.'s (2006) study only. There were no significant differences between those who completed three 25-minute BPFS-W sessions over eight weeks and controls in fearful and guilty affect three months following the final writing session (when baseline levels were controlled for). Austenfeld et al.'s (2006) study was found to be at low ROB and their sample was sufficiently large for analyses to detect an effect had one occurred, thus it appears that these findings can be trusted. However, participants were told to complete the fearful and guilty affect measures (fear and guilt subscales of the PANAS-X) in relation to how they had been feeling over the past few weeks, thus it is possible that effects of BPFS-W had occurred and dissipated prior to the three-month follow-up. Further research to investigate this possibility should be undertaken before conclusions with regards to the effects of the intervention on fearful and guilty affects can be drawn.

6.5.3.19 Need-satisfaction

Need-satisfaction was measured by Layous et al. (2013) alone. The effects of BPFS-W on three dimensions of need-satisfaction (autonomy, relatedness and competence) were explored, as well as effects on need-satisfaction overall. There were no significant differences between participants who wrote about a BPFS for four weekly, fifteen-minute sessions and controls in change in any

dimension of need-satisfaction (and need-satisfaction generally) from pre- to post-intervention. Although the evidence surrounding the effects of BPFS-W on need-satisfaction comprises only one study, it is robust. During the ROB assessment, Layous et al.'s (2013) work was found to be of high quality. Therefore, it is likely that their findings are accurate, and that BPFS-W is not beneficial in terms of increasing need-satisfaction, at least immediately post-writing when Layous et al.'s (2013) procedure of four weekly sessions is employed.

6.5.4 Cognitive-process outcomes

6.5.4.1 Self-efficacy

Self-efficacy was measured as an outcome in two of the 12 studies through which the impact of BPFS-W on cognitive-process outcomes was investigated. In Ph.D. Study One, generalised self-efficacy was assessed, and in McGovern's (2004) study, self-efficacy for self-regulated learning was measured specifically. In both studies, there appeared to be no significant effect[127] of BPFS-W on self-efficacy relative to a neutral control task, at one-, four- and eight-week follow-ups and a two-week follow-up, respectively. McGovern's (2004) study and Ph.D. Study One were of fair and good quality respectively and analyses were adequately powered. Therefore, although it has been accumulated from only two studies, it appears that the strength of the evidence is high, and findings are robust. It is also important to note that these studies differed markedly in their procedures in administration of BPFS-W. McGovern (2004) asked participants to write for four sessions (although they included participants who completed only three), yet in Ph.D. Study One participants completed a single session. McGovern's (2004) writing sessions were online, yet in Ph.D. Study One participants wrote in a laboratory. McGovern's (2004) writing instructions were specific, relating to an academic future only, yet the writing instructions in Ph.D. Study One regarded a general BPFS. Such contrasting procedures are important, as they to an extent negate a possibility that either study failed to find an effect due to the specific intervention procedure used. It therefore appears possible to conclude that BPFS-W is not beneficial in terms of increasing self-efficacy, regardless of the administration procedure used.

[127] Based on descriptive statistics. Please see Table 6.5.

6.5.4.2 Mindfulness

The effect of BPFS-W on mindfulness was measured by Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013) only. Although this study was found to be generally of fair quality in terms of methodology and design, results of analyses are unclear. Mindfulness was measured at baseline, immediately following the final of seven writing sessions, and at a two-week follow-up. The authors reported a significant main effect of condition; there was significantly greater mindfulness in BPFS participants in comparison to controls over time; there was no significant between-group difference at baseline. They also reported a significant main effect of time; they stated that there was significantly greater mindfulness at three-week follow-up in comparison to baseline and post-test. However, the authors reported that there was no significant condition * time interaction. It is therefore unclear whether the results of their study support the possibility of an effect of BPFS-W on mindfulness, especially given that no descriptive statistics were reported to assist with verification and clarification. Odou and Vella-Brodrick's (2013) and Seear and Vella-Brodrick's (2013) finding should therefore be treated with caution. Further research is needed to establish the effects of BPFS-W on mindfulness.

6.5.4.3 Flow

Two studies investigated of the impact of BPFS-W on flow. Aborida (2016) measured work-related flow specifically, and Layous et al. (2013) measured the experience of flow generally. The term 'flow' denotes an experiential state in which an individual becomes involved and immersed in an activity to the extent that their focus of attention is narrowed to only that activity (Csikszentmihalyi, 1990; 1997; as cited by Calvo-Porral, Faíña-Medín & Nieto-Mengotti, 2017). When an individual experiences flow, task-irrelevant thoughts become absent; the individual loses self-consciousness, responds to clear goals, and feels a sense of control over their environment (Csikszentmihalyi, 1997). The findings from Aborida's (2016) and Layous et al.'s (2013) studies with regards to flow were contrasting. Aborida (2016) found that there was no significant difference between BPFS participants and controls in change in work-related flow from immediately before the first writing session to immediately following the fifth and final writing session. On the other hand, Layous et al. (2013) found significantly

greater increases in flow from immediately before the first writing session to immediately following the fourth and final writing session in the BPFS group in comparison to controls. Although Aborida's (2016) and Layous et al.'s (2013) studies were generally of fair quality, there are two areas of ROB which must be acknowledged. First, Layous et al.'s (2013) measure of flow does not appear to have been empirically-validated. This means that the measure may not reflect participants' levels of flow, or at least may not accurately demonstrate the true variability in flow within and between participants (Streiner et al., 2015). Second, Aborida's (2016) analyses were underpowered according to desired sample size estimates. The null effect found in Aborida's (2016) study may, therefore, represent a type two error, although the risk of this does not appear to be high because descriptive statistics suggest negligible between-group differences and the p value is large. Additionally, there are two procedural differences between Aborida's (2016) and Layous et al.'s (2013) studies which may have given rise to the contradictory findings. First, the length of time between writing sessions differed. Aborida's (2016) writing sessions were spaced daily across five consecutive days, whereas Layous et al.'s (2013) sessions were spaced weekly across four consecutive weeks. Layous et al. (2013) suggested that BPFS-W may be beneficial in terms of increasing flow because it may remind individuals of activities that they have enjoyed and have become immersed in, and thus motivate them to engage in those activities. They also suggested that the intervention would prompt individuals to begin to work towards their BPFS in the intervention period, and that this could increase their flow experience. It may therefore be that the five-day intervention-span in Aborida's (2016) study was not enough time for participants to have begun working on the goals that they set for themselves in the process of writing about a BPFS, which may offer explanation as to why Aborida (2016) did not find increases in flow in the BPFS group relative to controls. Second, Aborida (2016) measured flow only in relation to a person's work, whereas Layous et al. (2013) used a more holistic measure. It is therefore entirely possible that Aborida's (2016) participants did experience increased flow in activities outside of their work lives, but that this change was undetected by the work-specific measures used. This is likely, given that Aborida (2016) did not ask participants to focus only on their work in their writing. Overall, it is possible that BPFS-W does increase flow experience,

but perhaps only over longer intervention-spans which enable participants to begin working on their goals before the end of the intervention. This possibility should be investigated, using holistic and empirically-validated measures and a sufficient sample, to allow conclusions with regards to the effects of BPFS-W on flow experience to be drawn with greater confidence.

6.5.4.4 Future-orientation

The effect of BPFS-W on future-orientation was measured in Ph.D. Study Two only. There was no significant difference between BPFS participants and controls in future-orientation four and eight weeks post-writing. It is somewhat surprising that an intervention requiring participants to write about the future would not increase their focus on higher-order, long-term goals. It should be acknowledged that the measure of future-orientation used in this study had not undergone empirical validation or testing of psychometric properties, thus there is a risk of detection bias. Study participants had a high level of future-orientation according to this scale at baseline; scores on the scale range from four to 20, and the BPFS and control groups in Ph.D. Study Two had baseline mean scores of 17.06 and 16.21, respectively. It may therefore be that a ceiling effect occurred; the scale may measure a restricted spectrum of future-orientation tendencies, in that the range of the scale may be smaller than the range of future-orientation truly present in the study sample (van der Putten, Hobart, Freedman & Thompson, 1999). A ceiling effect would result in the scale lacking sensitivity to detect change over time or differences between groups (van der Putten et al., 1999). Future research using validated measures of future-orientation should be conducted before conclusions are attempted.

6.5.4.5 Working-memory

The effect of BPFS-W on working-memory capacity was explored by Boselie et al. (2014; 2016a; 2016b)[128] and Yogo and Fujihara (2008). In the first three studies mentioned here, participants completed a single, 15-minute writing session. Working-memory was measured immediately post-writing in Boselie et al.'s (2014) and Boselie et al.'s (2016b) studies, and both pre- and post-writing

[128] In Boselie et al.'s (2014; 2016a) investigations, inferential statistics for the main effect of writing group were not reported. Therefore, the results above are based on inferences from descriptive statistics and should be treated with caution. Please see Table 6.5 for details.

in Boselie et al.'s (2016a) investigation. In all three studies, there appeared to be no significant difference between BPFS and control participants in working-memory immediately post-writing. In Yogo and Fujihara's (2008) study, working-memory was assessed one week before three 20-minute writing tasks (spaced over a two-week period), and at follow-ups conducted one and five weeks following the third session. Results demonstrated that there was no significant long-term difference between those who wrote about a BPFS and controls. These consistent null findings suggest that the intervention is not beneficial regardless of variations of dosage, at least up to three sessions. Nevertheless, there are some areas of ROB in these studies. First, Boselie et al. (2016a) and Boselie et al. (2016b) did not perform analyses to ascertain whether there were between-group differences in working-memory at baseline and did not control for this potential in main analyses. This is problematic, as although there were no significant between-group differences in working-memory following writing, failure to explore baseline differences means that it remains a possibility that there was a significant between-group difference in change in working-memory. Second, it should be noted that in Boselie et al.'s (2016b) investigation, analyses surrounding working-memory were underpowered according to desired sample size estimates[129]. This means that it is possible that the null finding from this study represents a type two error; there may have been an effect of BPFS-W on working-memory which was not detected in analyses. However, high ROB from this appears unlikely given that the p value was large and descriptive statistics suggest negligible between-group differences. Collectively, the consistent evidence (including findings from Yogo and Fujihara's (2008) study which was found to be at low ROB) suggests that BPFS-W is not beneficial for working-memory capacity either immediately following writing or longer-term. Further research with more rigorous methodology and analyses would enable more confidence in this conclusion.

6.5.4.6 Set-shifting

Set-shifting (the ability to switch attention between different tasks and mental sets; Monsell, 2003) was measured by Boselie et al. (2017) only. There was no

[129] Boselie et al.'s (2014) study was also underpowered, but findings reported here are based on descriptive statistics given that the inferential analyses surrounding the main effect of group on working-memory were not reported.

significant between-group difference in set-shifting immediately following a single, 15-minute writing session, suggesting that BPFS-W is not beneficial for set-shifting. However, it should be noted that there are areas of ROB in Boselie et al.'s (2017) study. First, analyses surrounding effects of BPFS-W on set-shifting were underpowered, thus the null finding may be a type two error. Second, Boselie et al. (2017) did not measure set-shifting at baseline, thus it is unknown whether pre-writing between-group differences in set-shifting existed. This means that although there was no significant between-group difference in set-shifting immediately post-writing, it is possible that one group improved more than the other to reach that post-writing level. Although these areas of ROB render it difficult to draw conclusions from these findings in isolation, in the context of wider literature it does appear unlikely that BPFS-W would impact set-shifting ability. As discussed above, it is unlikely that the intervention is beneficial for working-memory capacity. Given that set-shifting is dependent in-part upon working-memory (Pantelis et al., 2009), it is conceivable that BPFS-W would not be beneficial for set-shifting. It thus appears that BPFS-W does not impact set-shifting ability immediately post-writing. Replication of Boselie et al.'s (2017) study with baseline differences assessed and controlled for, as well as with sufficiently-powered analyses, would allow conclusions to be drawn with more confidence. It would also be useful to include longer-term measurement of set-shifting to allow assessment of possible sustained effects of BPFS-W.

6.5.4.7 Attentional-bias

The effects of BPFS-W on attentional-bias (attentional preference to positive and negative faces) was measured by Peters et al. (2016) only. In this study, there was found to be no significant main effect of condition, that is whether participants wrote about a BPFS or daily activities in a single 15-minute writing session, on attentional-bias. There was also no significant interaction between condition and time (immediately pre-writing in comparison to immediately post-writing). These results are likely to be an accurate reflection of the immediate effects of BPFS-W on attentional-bias, for two reasons. First, Peters et al.'s (2016) study was found during the ROB assessment to be of a high quality, thus conclusions from this study can be made with confidence. Second, the null results are unsurprising when placed in the wider context of other findings from

the current systematic review. As discussed earlier in this chapter, BPFS-W does not appear to be beneficial in terms of reducing self-reported anxiety, either immediately post-writing or longer-term. It is widely accepted that attentional-bias to threat cues (such as the faces displaying anger used by Peters et al., 2016) is heavily implicated in vulnerability to anxiety (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg & Van Ijzendoorn, 2007; Fox, Russo & Dutton, 2002; MacLeod, Mathews & Tata, 1986; Yiend & Mathews, 2001), to the extent that attentional-bias modification has been suggested as a potential treatment for anxiety symptoms (Liu, Taber-Thomas, Fu & Pérez-Edgar, 2018; MacLeod & Clarke, 2015; Mogg, Waters & Bradley, 2017; Naim, Kivity, Bar-Haim & Huppert, 2018). Therefore, given that BPFS-W does not appear to be beneficial in terms of ameliorating anxiety symptoms, it is conceivable that it would not impact attentional-bias, either. It should, however, be noted that in Peters et al.'s (2016) study, as well as in the studies in which self-reported anxiety was included as an outcome (Renner et al. 2014; Ph.D. Study One), a single writing session was used. Therefore, the potential that a single session is too low a dose for changes in anxiety and related cognitive processes to occur, and that BPFS-W may be beneficial for anxiety when more writing sessions are completed, remains open for empirical investigation.

6.5.4.8 Self-regulation

Self-regulation was measured in two studies. Findings are conflicting. In Ph.D. Study One, there was significantly higher self-regulation in those who wrote about a BPFS in comparison to those who wrote about the details of their previous day, eight weeks post-writing (but not one or four weeks post-writing). On the other hand, in Ph.D. Study Two, there was no significant difference between the BPFS group and controls in self-regulation four and eight weeks post-writing. Both studies were found during the ROB assessment to be of generally high quality, thus it appears findings can be trusted. It is likely that results are accurate representations of the effects of both procedures on self-regulation. There is an important procedural difference between Ph.D. Study One and Ph.D. Study Two which may offer explanation as to why these studies yielded contrasting findings. In Ph.D. Study One, participants completed a single 20-minute writing session in a laboratory, whereas in Ph.D. Study Two

four 20-minute sessions were administered online. As discussed in Chapter Five, Section 5.5 of the current thesis, it is possible that a laboratory setting is critical for the possible self-regulatory benefits of BPFS-W to emerge. Online administration leaves participants open to the influence of distractions (Sheese, Brown & Graziano, 2004), thus it is possible that participants in Study Two did not engage with the intervention sufficiently for gains in self-regulation to occur. Further research is needed to determine the generalisability of the self-regulation benefits found in Study One to settings other than a laboratory.

6.5.4.9 Emotion-regulation

The effects of BPFS-W on emotion-regulation were measured in Ph.D. Study One only. Descriptive statistics[130] indicate no difference between groups or over time (one, four and eight weeks post-writing) in emotion-regulation. It appears that although the evidence is from one study alone, this finding is robust because Ph.D. Study One was found to be of high quality. However, it should be noted that this finding is somewhat surprising given that BPFS-W for a single 20-minute session was found to be beneficial for general, behavioural self-regulation in the same study. Some researchers suggest that all self-regulatory processes (such as regulating emotions, maintaining attention and eating healthily) are governed by one global (and limited-capacity) resource (Baumeister & Heatherton, 1996; Muraven, Tice & Baumeister, 1998; Oaten & Cheng, 2006; Vohs & Heatherton, 2000, although this continues to be debated; Baumeister, Tice & Vohs, 2018; Etherton et al., 2018; Job, Dweck & Walton, 2010). This resource can be strengthened with practice; exercising self-regulation in one sphere (e.g. regular physical exercise) has been found to lead to increased self-regulatory strength in other spheres (e.g. resisting smoking; Oaten & Cheng, 2006). This said, given that there was an increase in general self-regulation in Ph.D. Study One as a product of BPFS-W, it is surprising that no change in ability to regulate emotions was observed.

Of course, it is possible that a different procedure to that used in Ph.D. Study One would be beneficial. Perhaps improvement in emotion-regulation

[130] Pure effects of BPFS tasks relative to the control task could not be isolated. Patterns of descriptive statistics mirrored inferential findings. See Table 6.5 for details.

specifically requires different procedural and methodological parameters than improvements in the wider construct of self-regulation. Therefore, it would be useful for further research to be conducted to examine whether BPFS-W may be beneficial when a different intervention procedure is used, for example a higher 'dosage' of writing than the single session completed in Ph.D. Study One, through administration of a greater number of writing sessions. It is also a possibility— given that wider, global self-regulation improvements do not become apparent until eight weeks post-intervention— that the change eight weeks post-writing is not yet great enough for measures of individual, specific spheres of self-regulation to detect changes. Perhaps, with continued building of a global self-regulatory resource which may occur past the eight-week follow-up, a longer follow-up would allow more specific changes in emotion-regulation to become apparent. This is an empirical question for further research to address.

6.5.5 Effects of procedural variations on intervention outcomes

Throughout the above synthesis of evidence surrounding the effects of BPFS-W on physical, physiological, psychological and cognitive-process variables, it has frequently been suggested that inconsistencies in findings may have occurred because of procedural variations in the administration of the intervention between studies. In a small number of studies in the current systematic review, the effects of some procedural and methodological variations have been empirically investigated.

6.5.5.1 Temporal spacing of writing sessions

Maddalena et al. (2014) explored the impact of temporal spacing of writing sessions; some participants completed three 20-minute writing sessions over three weeks, whilst others were required to complete all three sessions in a single day. Results demonstrated that the spacing of writing sessions had no significant effect on the impact of BPFS-W on symptoms of physical illness. It could therefore be suggested that spacing of writing sessions does not moderate the effects of the intervention on physical symptoms. However, there are areas of ROB and poor experimental control in Maddalena et al.'s (2014) study which make it impossible to draw this conclusion with any confidence.

First, analyses were underpowered, as discussed previously in this chapter. Second, although participants were asked to complete three writing sessions, Maddalena et al. (2014) included participants in analyses if they completed at least two. No analyses were performed to ascertain whether there were any between-group differences in the number of sessions completed. This opens the possibility that, theoretically, one spacing condition was more beneficial than the other, but that this benefit was veiled by a possible benefit of a greater number of writing sessions in the other spacing condition. Second, all participants who completed the three writing sessions in one day wrote in a classroom, whereas those who completed their writing sessions weekly wrote either in a classroom or at home. This difference in settings between the two groups is problematic as possible impacts of setting on intervention efficacy may have masked an effect of spacing. These areas of ROB render it impossible to conclude from Maddalena et al.'s (2014) findings that spacing of writing sessions does not impact the effects of BPFS-W. Further research, using more stringent experimental control, is necessary to enable understanding of the effects of spacing on intervention outcomes.

6.5.5.2 Setting of writing sessions

The impact of the writing session setting on intervention efficacy was investigated by Layous et al. (2013). In this study, participants completed four, 15-minute writing sessions over four consecutive weeks. Some participants completed sessions online, whilst others completed them in-person, in small groups of four to 10 participants. Results showed that there was no significant effect of setting on the efficacy of the intervention for PA, need-satisfaction, and flow. Layous et al. (2013) inferred that the respective positive characteristics of each setting may have offset each other. They posit that the in-person setting may have fostered greater motivation and focus; participants were prompted by an experimenter and could not have been distracted by activities such as Facebook, unlike in the online setting. They suggest that the online setting may have been less stressful and more convenient, as participants could decide where and when to complete their writing tasks. Although these suggestions are conceivable, caution should be exercised in interpretation of Layous et al.'s (2013) null finding; it should not be taken as robust evidence that writing online

and in-person are equivalent, due to an area of poor experimental control which may have introduced a confounding variable. The administration of the in-person condition writing tasks in small groups may have contaminated results, given that at least a proportion of the online participants will probably have chosen a time and place where they were alone. Awareness of other people in the room may have impacted participants in the in-person condition. For example, external distractions from noise generated from other participants could have negatively influenced participants' ability to enter a flow state. To illustrate, pen clicking is a common external distraction in a classroom setting which has been found to be associated with decreased performance across all members of the class (Tesch, Coelho & Drozdenko, 2011). It is possible that this contamination masked an effect, especially considering a finding from Sin and Lyubomirsky's (2009) meta-analysis which suggested that positive psychology-type activities are most effective face-to-face on an individual basis, followed by face-to-face in groups, with self-administered activities (which an online BPFS task essentially is) being the least effective. Therefore, further research is needed before conclusions with regards to comparative effects of online versus in-person settings can be attempted. An online condition contrasted with an individual in-person condition would be a fairer, more robust comparison.

6.5.5.3 Number of writing sessions

The effect of the number of writing sessions completed on intervention outcomes was investigated by Odou and Vella-Brodrick (2013) and Seear and Vella-Brodrick (2013)[131]. In this study, participants were asked to complete seven writing sessions over seven consecutive days and were encouraged to continue writing for a further two weeks. The authors investigated the effect of continuation of the intervention on mental well-being, PA and NA using ANCOVAs, with baseline scores for each outcome entered as covariates. It should be noted that the group IV levels consisted of the BPFS group and a

[131] Ng (2016) and Sheldon and Lyubomirsky (2006) also investigated the impact of continuation (as a categorical variable). Manthey et al. (2016) measured the impact of adherence (number of sessions completed out of eight prescribed). Findings from these studies are not reported in this review, as it was not possible to extract pure effects of dosage on BPFS-W efficacy from inferential or descriptive statistics.

'three good things' intervention group only, as the control group in this study were assigned no activity. Results demonstrated that there was no significant main effect of intervention type or continuation on any outcome, and no significant interaction. It could be inferred from these findings that the number of writing sessions completed does not impact the effectiveness of the intervention. However, as with the other studies of the effects of procedural variations already discussed, there are fragilities in Odou and Vella-Brodrick's (2013) and Seear and Vella-Brodrick's (2013) experimental control which mean that conclusions cannot be drawn with any confidence.

In this study, participants who did not fully adhere to the intervention and complete all seven sessions remained in analyses, and the authors did not perform analyses to ascertain whether there were any between-group differences in the number of writing sessions completed. This is problematic, as it is possible that participants who continued with the intervention after the initial seven days did not complete more writing sessions than those who did not continue, as they may have completed a smaller proportion of the initial seven sessions. This possible area of bias is compounded by inclusion of continuation as a categorical (i.e. yes versus no) variable, so a participant was classed as having continued with the intervention over the two-week period following the initial seven days whether they had written for one additional session or every day. This categorisation may have introduced a lack of sensitivity in analyses; they do not shed light on what optimal dosages could be, as the categories used are too broad. As has been suggested throughout the current synthesis surrounding the effects of procedural variations, further, more tightly-controlled investigation is required before conclusions can be drawn. It would be useful to examine dosage as a continuous, rather than categorical, variable. This would allow greater sensitivity in analyses surrounding whether optimal dosages of writing exist.

6.5.5.4 Process versus outcome focus

Most of the variations on the BPFS-W intervention are procedural, for example the number of writing sessions administered, the temporal spacing between sessions, the length of writing sessions and the timings of follow-up

measurements. However, in McGovern's (2004) and Vaughn et al.'s (2003) studies and in Ph.D. Study One[132], the effect of a variation in the content of the writing task itself was investigated.

In Ph.D. Study One and in Vaughn et al.'s (2003) study, participants completed a single, 20-minute writing session, and in McGovern's (2004) study participants were asked to complete four writing sessions, each lasting a minimum of 20 minutes. In Ph.D. Study One and in Vaughn et al.'s (2003) study, some participants completed the standard BPFS-W intervention about the outcome of a general positive future during which their life goals have been realised, whilst others wrote about the lower-order, process goals that they would need to achieve to reach their BPFS. McGovern et al.'s (2004) instructions were similar, but participants focussed on a specific academic future self (when they get their desired grade at the end of the University semester). In Vaughn et al.'s (2003) study, psychological well-being (measured using a scale comprised of the SWLS and the PGIS) was assessed four and seven weeks post-intervention. In Ph.D. Study One, PA and NA were measured immediately pre- and post-writing, and other psychological well-being variables (depression, anxiety and stress) as well as self-efficacy, emotion-regulation and self-regulation were measured at baseline and at one-, four-, and eight-week follow-ups. Symptoms of physical illness were measured at the four- and eight-week follow-ups only. McGovern's (2004) study included a single outcome; self-efficacy for self-regulated learning, measured at baseline and two-weeks post-writing.

In Vaughn et al.'s (2003) study the outcome condition was found to be superior to the control condition; there was found to be significantly greater psychological well-being in the standard, outcome BPFS group in comparison to the process and control groups, and no significant difference between the process group and the control group. On the other hand, in Ph.D. Study One there were no significant differences between outcome, process and control groups on any

[132] As previously stated, these inferences are based on patterns from descriptive statistics. In Ph.D. Study One, it was not possible to extract the pure comparative effects of the writing process, writing outcome and writing control groups on most outcomes, due to there being no significant modality (writing versus simulation) * task (outcome versus process versus control) interaction. Therefore, results should be treated with caution. See Tables 6.3, 6.4 and 6.5 for further details.

outcome variable other than PA and self-regulation; for these two outcomes there was significantly higher levels in both intervention groups than controls, but no significant difference between process and outcome groups. In McGovern's (2004) study, there was no significant difference between outcome, process and control groups on self-efficacy for self-regulated learning.

It is difficult to compare findings across these studies, because they differ in the outcomes assessed. The only outcome to have been measured in more than one study was self-efficacy, and findings were consistent. In both McGovern's (2004) study and in Ph.D. Study One, BPFS-W was not found to be beneficial for increasing self-efficacy relative to a control task, regardless of whether the writing instructions were outcome- or process-focussed. This is to say that BPFS-W does not appear to increase self-efficacy, and that this finding is generalisable across differences in writing instructions, as well as across differences in administration procedures such as the number of writing sessions (as evidenced by procedural differences between McGovern et al.'s (2004) investigation and that of Ph.D. Study One). There were no other outcomes which were measured in more than one study. McGovern (2004) measured only self-efficacy, therefore the discrepancy in findings between Vaughn et al.'s (2003) study and Ph.D. Study One only are discussed in the following paragraphs.

There are several possible reasons for the discrepancy in findings between Vaughn et al.'s (2003) study and Ph.D. Study One. It important to acknowledge that different outcomes were measured in these studies. It is entirely possible that outcome-focussed rather than process-focussed instructions are required to yield gains in general psychological well-being, whereas self-regulation gains may be achieved using either instruction type. However, there are other differences between these two studies which may explain why Vaughn et al. (2003) found process-focussed writing instructions to be less beneficial than outcome-focussed instructions, yet in Ph.D. Study One the effects of the instructions were found to be equal.

First, the process writing instructions used by Vaughn et al. (2003) differed from those used in Ph.D. Study One. In Vaughn et al.'s (2003) study, the instructions were more structured; participants wrote for around seven minutes about the outcome of their BPFS, followed by what they could be doing in 10 and 20 years' time to achieve this outcome, for seven minutes each. In Ph.D. Study One, participants were only told to write about the little steps that they needed to take to reach their BPFS, and that they should write for 20 minutes. No sub-topics or future distances were suggested. It is possible that the additional structure in Vaughn et al.'s (2003) process writing task reduced its effectiveness in comparison to their outcome task (for which participants were told merely to write about their BPFS for 20 minutes, like the outcome participants in Ph.D. Study One). This is to say that perhaps process-focussed writing is beneficial for general psychological well-being, but seven minutes was not long enough for participants to properly engage with each sub-topic. The difference in structure between Vaughn et al.'s (2003) outcome and process conditions means that a fair comparison of the effects of these instructions is not possible. Second, Vaughn et al.'s (2003) analyses were underpowered. It is therefore possible that the significant differences between the outcome group and the process and control groups were spurious (Button et al., 2013). Third, Vaughn et al. (2003) did not control for possible pre-manipulation between-group differences, thus it is possible that the significant between-group difference in well-being was naturally-occurring and attributable to selection bias. Vaughn et al.'s (2003) study should be replicated using a larger sample, analyses which control for possible baseline differences in outcomes, and open, unstructured BPFS process instructions to allow fair conclusions to be drawn with regards to the effects of process- in comparison to outcome-focussed BPFS-W on general psychological well-being. Overall, however, given that Ph.D. Study One was at low ROB and demonstrated no differences between the effects of process- and outcome-focussed writing instructions on multiple variables, it is likely that the effects of these tasks are comparable.

6.6 Summary of evidence, evaluation and conclusions

The current systematic review was conducted to address two aims. The first aim was to establish whether the existing literature suggested that BPFS-W is beneficial for physical health (including physiological variables) and psychological well-being, as well as cognitive processes which may be related to or impact well-being. The second aim was to explore whether variations in intervention administration procedures between studies may impact its effectiveness. A review and summary of findings relating to each of these aims is discussed in this section.

6.6.1 Summary of evidence

6.6.1.1 Physical health outcomes

The literature surrounding the effects of BPFS-W on physical health outcomes is relatively small. Physical health was measured by both questionnaires and MCU records. In five of the seven studies in which physical symptoms were measured using surveys, BPFS-W participants did not report reduced symptoms relative to controls, and the two studies which did report benefits were at greater ROB than the other five studies. For MCU, two studies reported reductions and two reported no change. These studies were of fair and similar quality; therefore, results are likely accurate. There were some procedural differences that may explain the inconsistent findings; it was suggested that at least four sessions spaced closely together may be needed for MCU visits to decrease, as well as open task instructions.

It should be noted that variation in MCU visits is not necessarily a direct and proportionate reflection of variation in physical symptoms; some individuals have a higher symptom threshold for service utilisation than others (van Loenen, van den Berg, Faber & Westert, 2015). MCU is predicted by a myriad of variables including health anxiety, general neuroticism, frequency of utilisation by family members, perceived social support and loneliness (Byrne et al., 2003; Cardol et al., 2005; Conroy, Smyth, Siriwardena & Fernandes, 1999; Ellaway, Wood & Macintyre, 1999; Jerram & Coleman, 1999). Therefore, it may be accurate to infer that BPFS-W reduces MCU (under certain conditions) but this does not necessarily translate into a reduction in symptoms of physical

illness, and instead may reflect change in another variable such as loneliness (Ellaway et al., 1999). All things considered, it appears unlikely that BPFS-W leads to physical health improvements. Most of the evidence, as well as the evidence with the lowest ROB, has demonstrated no significant differences between BPFS-W participants and controls in change in physical symptoms following writing.

6.6.1.2 Psychological health outcomes

The body of evidence surrounding the effects of BPFS-W on psychological well-being was found to be larger, more intricate and more complex than that surrounding physical health. Where physical health has been measured in the included studies as a single, global outcome, multiple specific aspects of psychological well-being have been measured separately. There was one aspect of psychological well-being that BPFS-W was found to be detrimental for; Renner et al. (2014) found that BPFS participants increased in dependency, whereas controls decreased, immediately post-writing. It is not clear why this may have happened, but it may be that Renner et al.'s (2014) results are not be an accurate representation of intervention effects due to possible contamination from an NMI as well as possible sampling error. This study was the only piece of evidence to suggest a negative effect of BPFS-W on well-being.

All studies in which anxiety, stress, neuroticism, burn out, mental well-being, body-esteem, body-comparison, self-compassion, perfectionism, need-satisfaction, hostility and fearful and guilty affects were measured demonstrated no significant improvement from BPFS-W on these outcomes. It may well be that the intervention does not influence these aspects of psychological well-being. However, these outcomes were investigated in very small evidence bodies; hostility was measured in only three studies, anxiety and stress were measured in two, and the other outcomes were each measured in single studies. It is therefore only possible at this stage to suggest that BPFS-W does not affect these outcomes when the administration characteristics used in those studies are employed. For example, both studies which included anxiety as an outcome required that participants completed a single writing session. It is entirely possible that higher doses of writing could elicit reductions in anxiety.

Burn-out was measured in only one study, immediately following five eight-minute writing sessions. Perhaps reductions in burn-out would occur if longer sessions were administered. It should also be noted that perfectionism, dependency, need-satisfaction and burn-out were measured only immediately post-intervention, thus it remains possible to speculate that long-term benefits to these aspects of well-being may occur.

The evidence surrounding self-esteem, self-criticism and self-reassurance suggested that BPFS-W bolsters these aspects of psychological well-being long-term, based on single studies. These studies were found to be at generally low ROB, thus it is likely that their findings were accurate representations of the effects of their intervention procedures. However, it is only possible to attempt to draw conclusions with regards to the effects of BPFS-W on self-esteem, self-criticism and self-reassurance when the administration procedures adopted in those studies are used. Further research is needed to establish whether these findings are generalisable regardless of intervention administration characteristics, or whether variations in procedures such as the number and length of writing sessions impacts the effects of BPFS-W on each respective outcome.

The bodies of evidence surrounding some of the other psychological well-being outcomes were found to be larger, and as such allowed greater understanding of the effects of BPFS-W. Interestingly, the findings of the current review suggest that the effects of the intervention on some outcomes may be different immediately post-writing to longer-term. Perhaps the clearest finding of the review is that BPFS-W consistently increases PA immediately post-writing. Immediate benefits to PA were found in 20 out of 25 studies. It was not clear why it was not beneficial for PA in 100% of the studies; there were no clear differences between studies which did and did not find an effect in either administration procedures or ROB. However, from the high level of consistency across the large number of studies, it appears that BPFS-W does elicit an immediate increase in PA. Furthermore, the studies which demonstrated immediate increases in PA varied considerably in their administration procedures. This suggests findings are generalisable across procedural

variations. Nevertheless, the literature surrounding the sustained effects of BPFS-W on PA was found to be smaller and more contentious. Two out of five studies suggested that BPFS-W increases PA long-term. However, both studies had several areas of ROB, such as translated outcome measures that had not been subjected to linguistic validation procedures. The three studies which did not find long-term gains in PA were found to be at lower ROB, thus results from these studies are likely more accurate representations of intervention effects than the results of studies which did demonstrate sustained benefits. Therefore, it appears that BPFS-W immediately boosts PA, but that this is short-lived and dissipates over time.

The review findings surrounding happiness, NA, depression, and optimism (as well as future-expectancies and optimistic explanatory-style) also demonstrated different immediate in comparison to long-term effects. Happiness was found to increase immediately post-writing and this effect appeared to be generalisable across procedural variations, but sustained happiness is perhaps only possible when higher doses of writing are used. P-FEX, too, were found to consistently increase immediately post-writing, and N-FEX were found to decrease (although there was no immediate impact on optimism as a broader construct). Findings surrounding long-term gains in optimistic thoughts, on the other hand, were far more conflicting and inconclusive. First, a single study demonstrated a sustained reduction in N-FEX and a possible sustained increase in P-FEX, however it is possible that this was an acute effect of imagery which took place across the follow-up period, rather than a true sustained effect of BPFS-W. Second, in terms of optimism more broadly, effects may not occur until two weeks following writing, but may dissipate before four weeks post-writing—although this inference was tentative, again due to possible contamination of effects by use of imagery across the follow-up period. Finally, it was suggested that sustained effects on optimistic explanatory-style are possible, but perhaps only when structured, time-limited writing tasks are administered. NA was generally not found to change immediately post-writing, regardless of the intervention procedure used. Longer-term findings were inconclusive. The studies with the lowest levels of ROB yielded null effects, but it was suggested that sustained reductions in NA may be possible when longer intervention-

spans are used. Depression may reduce immediately post-intervention, but perhaps only when participants have opportunity to focus on specific areas of their BPFS, rather than needing to write about a positive future more generally. Sustained changes in depression, too, were found to be possible, but perhaps only when more than three writing sessions were completed or when participants decide when and where they write and for how long. As discussed in Section 6.5, further research is needed to determine the sustained effects of BPFS-W on happiness, NA, depression and optimism, as well as how procedural characteristics may impact outcomes. This is particularly true for optimism (and related variables), depression and NA due to areas of ROB.

Collectively, the results of this systematic review suggest that BPFS-W elicits immediate benefits to some areas of psychological well-being, such as increased positive affect and happiness, regardless of the intervention procedure used. Generally, BPFS-W does not appear to have sustained well-being benefits; some benefits may be possible but perhaps only within certain procedural perameters. Further research is needed to determine the effects of the intervention on some psychological well-being outcomes (as well as the generalisability of effects across intervention procedures) due to small numbers of studies as well as areas of ROB.

6.6.1.3 Cognitive-process outcomes

The review findings with regards to the evidence surrounding the effects of BPFS-W on nine cognitive-process variables are interesting. For three of these outcomes (mindfulness, flow and self-regulation), findings were unclear. Mindfulness was measured in a single study (Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013), and the results of this study had not been reported clearly. The authors reported that there was significantly greater mindfulness three weeks post-writing in comparison to baseline and immediately post-writing, and in BPFS participants in comparison to controls across time. However, they stated that there was no significant interaction between time and group. It is not possible to draw conclusions from this study without further research, or at least re-analysis of the data from it. For flow, findings were conflicting, and it is possible that the intervention is beneficial only

when longer intervention-spans are used. However, this inference was based on only two studies, one of which did not use an empirically-validated measure and the other was underpowered. Conflicting findings were also yielded for self-regulation; it may be that BPFS-W is beneficial only when it is administered in a laboratory. Again, this inference was based on the findings of only two studies. For the remaining six cognitive outcomes the available evidence suggested that the intervention is not beneficial. This was based on only one (for future-orientation, set-shifting, attentional-bias and emotion-regulation), two (for self-efficacy) or four (for working-memory) studies, thus it is a possibility that results are not generalisable to procedural variations other than those used in those studies. Further research is needed to fully establish whether certain procedural parameters allow gains in flow and self-regulation to occur, and whether the null findings surrounding the other cognitive outcomes are generalisable across variations in procedures. Nevertheless, the consistency in null findings across cognitive outcomes suggests that the intervention does not— based on the current, limited state of the evidence— affect cognitive processes associated with physical health and psychological well-being. This is interesting, as it may mean that the intervention does not induce change broadly at the cognitive level; this may be important when considering King's (2001) self-regulation theory of the possible effects of BPFS-W.

Null findings surrounding working-memory and set-shifting warrant further discussion. When considered in the context of the association between self-regulation and executive functions (EFs), these findings may shed some light on the proposed mechanisms through which BPFS-W may elicit health benefits. King (2001) suggested that the activity elicits well-being benefits through increasing self-regulation. Although its effects on self-regulation have been measured in two studies alone (Ph.D. Study One and Ph.D. Study Two), a possible mediating role of self-regulation has been accepted by multiple authors (e.g. Frattaroli, 2006; Frein & Ponsler, 2014; Layous et al., 2013; Liau et al., 2016; King, 2002; Sheldon & Lyubomirsly, 2006). Self-regulation is proposed to be at least in-part dependent upon three major EFs (Hofmann, Schmeicel & Baddeley, 2012). The first is the 'updating' operation of working-memory, which denotes the ability to retain information in a state which is active, retrievable,

and protected against distractions (Baddeley, 2007; Kane, Bleckley, Conway & Engle, 2001; Smith & Jonides, 1997). Individuals with greater working-memory capacity are better able to resist attending to distractors at early processing stages, leading to lower disruption of attention to goal-directed functions (Hofmann et al., 2012; Kane et al., 2001; Unsworth, Schrock & Engle, 2004). The second is mental set-shifting; the ability to shift between several mental sets and tasks (Monsell, 2003). High set-switching abilities may enable individuals to abandon efforts and means which are costly relative to productivity towards goal-achievement, and instead pursue more cost-effective means of attaining the same ideal standard (Hofmann et al., 2012). The third is inhibition; the ability to consciously inhibit automatic, prepotent behavioural responses which would be damaging when an individual is attempting to reduce the discrepancy between current and ideal standards (Hofmann et al., 2012; Miyake et al., 2000). Considering these links between self-regulation and EFs, it is possible to tentatively suggest from the review findings that self-regulation may not be the mechanism through which BPFS-W elicits the limited well-being benefits sometimes observed.

Findings surrounding the effects of BPFS-W on working-memory suggest that the intervention does not bolster self-regulation. The intervention was found not to be beneficial in increasing working-memory capacity (neither immediately post-intervention, nor longer-term). It appeared to be ineffective regardless of the 'dosage' administered, at least up to a dosage of three, 20-minute writing sessions. Although there were areas of ROB in the evidence base surrounding effects on working-memory, one of the studies was found to be of high quality and the findings were consistent across all four studies. It therefore is likely that the intervention does not affect working-memory capacity. Findings surrounding intervention effects on set-shifting also provide evidence against the proposition that BPFS-W aids self-regulation. Boselie et al. (2017) found that the intervention was not beneficial in increasing set-shifting ability immediately post-writing (although this is a less robust finding than that for working-memory and further, high-quality, research into the effects of BPFS-W on set-shifting— particularly long-term effects— is warranted, as discussed in Section 6.5.4.6). The findings of the current review therefore suggest that BPFS-W may not

result in an increase in two of the three EFs which subserve self-regulation. If there are no gains in these processes, then it is likely that there would be no gains in self-regulation, either. This inference does to an extent contradict the review findings surrounding self-regulation, as measured by retrospective self-report questionnaires. The evidence base surrounding self-regulation comprised of two studies, from which findings were conflicting. The studies were both at low ROB, thus it is likely that findings were accurate representations of intervention effects on self-reported self-regulation, and it was suggested that the discrepancy may have arisen due to differences in administration procedures between the two studies. Nevertheless, the evidence suggested that, under certain conditions, BPFS-W likely does increase participants' reports of their self-regulatory capacity. Considering the above discussion, however, it may be that it does not truly increase self-regulation, and instead changes participants' perceptions of their self-regulatory function. Of course, these suggestions are theoretical and further research is needed to establish whether self-regulation has a role in any observed well-being benefits of BPFS-W.

Further, high quality, examination of the effects of the intervention on working-memory capacity and set-shifting is needed. It would also be useful to investigate the effects of BPFS-W on the third fundamental EF subtending self-regulation; inhibition. Additionally, it is possible that the self-report measure used in Ph.D. Studies One and Two did not provide a reliable representation of participants' self-regulatory function, for two reasons. First, self-report measures of self-regulation are subjective, and do not directly measure self-regulation as a cognitive and behavioural construct. As with all self-report measures, they at best measure the participant's perception of their self-regulation, and at worst are vulnerable to social desirability bias and recall error (Brener, Billy & O'Grady, 2003; King & Bruner, 2000; Tourangeau, 2000; Van de Mortel, 2008; Verplanken & Orbell, 2003). Behaviours associated with poor self-regulation are frequently underestimated by respondents (Boyd, Windsor, Perkins & Lowe, 1998; Popham & Schmidt, 1981; Rose et al., 2008; Wagenknecht, Burke, Perkins, Haley & Friedman, 1992). Second, it is possible that self-regulatory function and goal-directed action increase following BPFS-W in only the spheres written about by participants (although some evidence suggests that

self-regulation relies on a global resource, as discussed in Section 6.5.4.9). In all studies in which working-memory, set-shifting and self-reported self-regulation were measured, participants were free to write about any aspect of a positive future self. Self-reported self-regulation was measured using the SSRQ. The SSRQ measures broad goal-directed action, thus it may not have been sensitive enough to accurately reflect changes in specific self-regulatory behaviours. Considering these factors, it may be useful to investigate the effects of writing about a specific sphere of a BPFS (such as health goals) on self-regulatory behaviours related to the achievement of goals in that sphere. A health-related BPFS would be a sensible starting point, because the objective approach to measuring self-regulation lends itself to factors associated with health behaviour change; they are in-part dependent upon self-regulatory processes (de Ridder & de Wit, 2006), and can be assessed using biomarkers. For example, plasma- and saliva-cotinine levels and breath-carbon monoxide levels are reflective of nicotine intake (Jarvis, Primatesta, Erens, Feyerabend & Bryant, 2003; Marrone et al., 2011), and body composition and metabolic indices (e.g. fat mass and blood-cholesterol) are reflective of dietary intake (Johnston, Tjonn & Swan, 2004). However, health behaviour change is complex and multifaceted (e.g. de Ridder & de Wit, 2006; Kuitunen-Paul et al., 2019; Schwarzer, 2008). Therefore, BPFS-W would need to be implemented as an adjunct to existing interventions (such as cognitive-behavioural skills training and self-monitoring; Burke, Wang & Sevick, 2011; Killen, Maccoby & Taylor, 1984), so that its benefits to self-regulation over and above those of the existing interventions are measured. These endeavours would together generate a more holistic and robust representation of the effects of the intervention on self-regulation. This is important, because if self-regulation is not affected by BPFS-W, then possible well-being benefits of the intervention cannot occur through this mechanism.

6.6.1.4 Physiological outcomes

The findings of the current review suggest that BPFS-W elicits very little cognitive change, if any at all. It also appears that the intervention may not result in any physiological reactivity or change. Physiological outcomes were measured in two studies; the first using salivary-cortisol as a measure of acute

physiological reactivity and the second using blood pressure as a long-term indicator of physiological change. BPFS-W appeared not to affect either outcome. The lack of change in salivary-cortisol is unsurprising, given that cortisol is a biomarker of psychological stress (Hellhammer et al., 2009; Lee et al., 2012). Although the review findings suggest that BPFS-W may not be particularly beneficial for most aspects of psychological well-being, they certainly do not suggest that it is stressful or damaging thus a cortisol spike would not be expected. The findings with regards to the effects of the activity on blood pressure long-term are somewhat more difficult to interpret. Although a null effect was reported (Austenfeld, 2007; Austenfeld & Stanton, 2008), there was some ROB from the measure of blood pressure used, which means that further research should be conducted using a more reliable measurement strategy before firm conclusions should be attempted. This is especially true given that the evidence was based on a single study. Nevertheless, this study was otherwise at low ROB thus it is conceivable that BPFS-W does not impact blood pressure. Blood pressure has long been accepted as a predictor of physical health (Räikkönen, Matthews, Flory, Owens & Gump, 1999); particularly of cardiovascular function but also of risk of stroke and kidney disease (Chobanian et al., 2003; Devereux et al., 1983; Perloff, Sokolow & Cowan, 1983; Sokolow, Werdegar, Kain & Hinman, 1966). Higher blood pressure has also been found to be associated with lower psychological well-being; it has been found to be positively-associated with anxiety, stress and pessimism (Markovitz, Matthews, Wing, Kuller & Meilahn, 1991; Räikkönen et al., 1999; Räikkönen, Matthews, Flory & Owens, 1999; Rau, 2006). Blood pressure can therefore be considered as an objective health and well-being indicator. This is important, as the other measures of physical and psychological well-being used in the literature surrounding the effects of BPFS-W have been self-report instruments, which measure respondents' subjective perceptions of their health and well-being. If further research using more robust measurement strategies yield null effects, then this— coupled with the apparent lack of intervention effects at either the cognitive level or the self-reported symptomatic level— would be strong accumulative evidence that BPFS-W is not truly therapeutic in terms of beneficial changes to physical or psychological health.

6.6.1.5 Effects of procedural variations

Throughout the review, it was found to be possible to tentatively attribute some differences in findings to variations in procedural factors. For example, higher doses of writing were suggested to be necessary for sustained effects on depression, happiness and MCU to occur, and longer intervention-spans (e.g. due to greater time between writing sessions) appeared to strengthen effects on PA, NA and flow. These suggestions were based on procedural differences between studies which did and did not find the intervention beneficial for a given outcome. It is entirely possible that these inconsistencies in findings were not in any way related to differences in procedures. The review findings surrounding the effects of BPFS-W on life-satisfaction exemplify this possibility. Life-satisfaction was measured in five of the included studies, with conflicting findings. There were no clear procedural differences between these studies, and no clear differences in ROB. It is therefore unclear why the inconsistent findings occurred, but it is unlikely that they arose as a product of procedural variations. Furthermore, for some outcomes (e.g. immediate PA, hostility, working-memory) the evidence was comprised of studies which differed in intervention procedures yet yielded consistent findings, suggesting that the effects of BPFS-W on at least some outcomes are not moderated by procedural variables.

There was found to be very little existing empirical evidence surrounding the effects of intervention administration characteristics on outcomes. The only procedural variations to have been experimentally-examined were found in the current review to be temporal spacing of writing sessions sessions (three in one day versus three weekly over three weeks), setting of writing sessions (online versus in-person), and number of writing sessions. The current, limited, literature suggests that these procedural variables do not impact the efficacy of BPFS-W. However, it is not possible to draw firm conclusions from these results, for two reasons. First, there were areas of potential bias in the studies which explored the effects of procedural variations. Second, the effects of each procedural variation were explored in single studies. This means that the results may not be generalisable to intervention administration procedures other than those adopted in those studies and may not be generalisable to outcomes other

than those measured. For example, Layous et al. (2013) measured the effects of setting on benefits of the intervention to PA, need-satisfaction and flow only. It is therefore possible that the same manipulation could have elicited differential effects on depression, for example. It is also possible that setting does not impact intervention outcomes when four writing sessions are used as was the case in Layous et al.'s (2013) study. Perhaps setting is more important when a smaller dose is used, and participants have less time to engage with the intervention.

From the limited literature available, it is possible to tentatively suggest that procedural variations may not influence the efficacy of BPFS-W. It also appears that manipulations to writing instructions largely do not impact intervention outcomes. The current review revealed that there are several broad types of writing instruction used; writing about specific spheres of a BPFS, writing about the process that the individual must follow to reach a BPFS, incorporating how to overcome an obstacle into the writing, and the standard instructions to write broadly about a BPFS when life dreams have been realised. The only instruction types to be experimentally-manipulated are process- and outcome-focussed writing instructions. The comparative effects of these instructions were investigated in three studies (McGovern, 2004; Vaughn et al., 2003; Ph.D. Study One), and findings were conflicting. In Ph.D. Study One, the process and outcome conditions were found to be equally effective. Both were more effective than the control task for some outcomes (e.g. self-reported self-regulation, PA), and both were no more effective than the control task for other outcomes (e.g. self-efficacy, anxiety, stress). Process-focussed and outcome-focussed instructions were also found to be equivalent in McGovern's (2004) study; neither the outcome nor the process task was found to be more beneficial than the control task in increasing self-efficacy. However, in Vaughn et al.'s (2003) study the outcome condition was found to be more effective for general psychological well-being than the process and control conditions, and the process condition was no more effective than the control condition.

The only outcome assessed in more than one study was self-efficacy, which was measured in McGovern's (2004) study and in Ph.D. Study One. Findings

across these studies were consistent; BPFS-W appears not to be beneficial for self-efficacy regardless of whether writing instructions are outcome- or process-focussed. This is only conclusion that can be drawn with regards to the effects of outcome-focussed in comparison to process-focussed writing instructions with any confidence. It is difficult to draw comparisons across Ph.D. Study One and Vaughn et al.'s (2003) studies because they differed in the outcomes assessed. Of course, the differences in the outcomes measured may explain why Vaughn et al. (2003) found outcome-focussed writing instructions to be more effective than process-focussed instructions, whereas in Ph.D. Study One the instruction types were found to be comparable. Perhaps process-focussed writing is less beneficial than outcome-focussed writing for general psychological well-being, whereas for other outcomes the effects of these instruction types are equal. However, conflicting findings may have also have arisen due to areas of bias in Vaughn et al.'s (2003) study. It was suggested in Section 6.5.5.4 that further research is needed to confirm the effects of process- in comparison to outcome-focussed instructions on intervention efficacy.

6.6.2 Evaluation

This systematic review is the most comprehensive and contemporary record of the literature surrounding the effects of BPFS-W on physical and psychological health and related cognitive processes. This was achieved primarily through a rigorous literature search. A multi-stage search strategy allowed a large quantity of eligible studies to be retrieved and included. In doing a systematic review, the author should aim to identify all the studies relevant to the review's research questions and subsequently evaluate the validity of those studies and synthesise the evidence from them (O'Hagan, Matalon & Riesenberg, 2018). Indeed, it appears that saturation was reached, because during the reference and citation searches very few previously-unidentified records pertaining to BPFS-W were found. Of course, it is not possible to be certain that all the studies eligible to be included in the current review were retrieved. It is difficult to ensure that all— or at least a representative sample— of the eligible grey literature has been identified and retrieved, given that grey literature is not usually systematically disseminated, organised or bibliographically-controlled (Benzies, Premji, Hayden & Serrett, 2006; Debachere, 1995; Hopewell, Clarke

& Mallett, 2005; Lawrence, 2012; Smith, 2009). Indeed, some eligible studies may not have been published in any format at all, including manuscripts of academic research and student projects which were never disseminated outside of the research team who created them (Dickersin, 1997). It is important to be mindful of this when drawing conclusions from the current review. This is because studies with null effects are less likely to be published in peer-reviewed journals and as such are more difficult to access (Hopewell et al., 2005). Therefore, the current review findings may be biased and may present an inflated representation of the effects of BPFS-W (Dickersin, 1997; Hopewell et al., 2005). This remains the case despite the apparent saturation achieved in the reference and citation searches, as non-significant findings are less likely to be cited than significant findings (Gøtzsche, 1987; Hopewell et al., 2005). Nevertheless, it is likely that at least a large proportion of the existing evidence was identified and included in syntheses.

It is likely that the included records are representative of the evidence surrounding the effects of BPFS-W, thus increasing the accuracy of the review findings. The quality assessment process also contributed to the utility of the current systematic review. This was detailed, with some areas of assessment performed at the outcome level. It has allowed areas of possible bias in the included primary studies to be considered in syntheses of results from these studies, which has in turn reduced bias in the systematic review and increased confidence in its findings and the future directions that they encourage (Whiting, Rutjes, Reitsma, Bossuyt & Kleijnen, 2003). This ROB assessment is the only attempt to date to include areas of bias in the synthesis of the BPFS-W intervention evidence, and it is of critical importance to future research in this area. It has demonstrated areas of bias and lack of clarity in the existing literature and has revealed differences in findings across studies which could potentially be explained by areas of bias within studies (Whiting et al., 2003). It has therefore allowed the development of suggestions for further research, informed by hindsight, to replicate previous studies with areas of bias removed and weaknesses of design modified. This should eventually result in a more robust evidence base surrounding the effects of BPFS-W. However, there are

weaknesses in the current review which should be considered when findings and recommendations from it are used.

There is an important limitation of the current review methodology; only studies published in English were eligible for inclusion. This was decided because it would have been too costly to fund another individual to conduct searches in multiple languages and then translate the records retrieved into English. The exclusion of studies which were not published in English may have introduced an element of bias in the current review. Although language bias is a continued source of debate and agreement with regards to its impact and direction of effect has not yet been reached (Jüni, Holenstein, Sterne, Bartlett & Egger, 2002), some evidence suggests that results demonstrating significant, positive change are more likely to be published in English-language journals than journals which use other languages (Dickersin, 2005; Egger et al., 1997). Therefore, the exclusion of records published in a language other than English may have resulted in inclusion of a higher proportion of the records showing positive and/ or significant findings than of records showing negative and/ or null findings. This is to say that the included studies may not be a truly representative sample of all the studies conducted to explore the effects of BPFS-W. Therefore, the inferences made in the synthesis of this review, based on the accumulative evidence from the included studies, may lack accuracy and estimates of the effects of the intervention may be inflated.

The limitations of the included evidence should also be acknowledged. First, it is broad yet sparse; the effects of BPFS-W on over 30 physical, psychological and process variables have been explored. However, the effects of the intervention on many variables are evidenced by small numbers of studies; some outcomes have been measured in single studies (e.g. self-esteem, set-shifting, attentional-bias). This reduces confidence in the replicability and generalisability of the review findings. Second, it should be noted that there were also found to be problems with how results in included studies had been reported. For some studies, estimates had to be made based on descriptive statistics, because the pure effects of BPFS-W in comparison to a control task had not been reported. Usually this occurred due to the presence of another IV

in an author's design (e.g. the modality (writing versus simulation) variable in Ph.D. Study One). Although this is not problematic at the study level and does not indicate a ROB, it does mean that review findings based on the results of these studies are less robust. In other studies, reporting of results was unclear. For example, Yogo and Fujihara (2008) stated that they had measured hostile affect but did not report the effects of BPFS-W on this outcome. It was assumed that this may have arisen from selective reporting of significant effects, as all the effects that had been reported were significant ones (e.g. effects on depressed/ anxious affect). Inferences were made from these studies to enable as much of the existing evidence as possible to be included in the current systematic review. However, the inclusion of these studies reduces the confidence with which conclusions can be drawn with regards to affected outcomes. Correspondence with authors for details and attempts at re-analysis of data from these studies were impractical within the time-constraints of a Ph.D. programme. It may be useful for further research to build on the current review by performing these analyses, as this would enable conclusions to be drawn with greater confidence. The third limitation of the included evidence is ROB. There were found to be numerous areas of possible bias which may have impacted the results of individual studies, as discussed throughout the narrative synthesis. There were also areas of bias which have affected the overall quality of the literature, and thus reduced the confidence in the review findings. These aspects of bias were explored at length in Section 6.4.5, and as such will not be discussed in detail here. However, the reader should be mindful of these areas of possible bias so that the findings of the review are not considered separately from them. This is to say that the review findings are based on the current available evidence, and that evidence does not appear to be entirely robust.

6.6.3 Conclusions

Overall, findings of the current systematic review suggest that BPFS-W does elicit immediate gains in some aspects of psychological well-being, such as PA and happiness. It also appears that some immediate benefits are generalisable across variations in intervention administration procedures. Nevertheless, the activity does not appear to be as beneficial and robust as has been previously assumed. Findings from this review suggest that BPFS-W is not beneficial for

physical health, and it does not appear to encourage cognitive or physiological change. The intervention does not appear to be beneficial long-term for most aspects of psychological well-being, either. Long-term benefits may be possible, but perhaps only within certain procedures, or only for a limited amount of time. Loveday et al.'s (2016) systematic review highlighted that the intervention has been found to be beneficial across multiple procedural variations, although the authors asserted that the effects of procedural variations had not been empirically explored. The current review has demonstrated that null findings are just as generalisable across procedural variations as positive outcomes are, if not more so. Loveday et al. (2016) stated that the intervention has been found to be effective when administered both online and in-person and suggested that this demonstrates that it is robust because its effects are maintained across a range of delivery methods. Whilst it is true that BPFS-W has been found to benefit well-being when both online and in-person settings were used (e.g. Boehm et al., 2011; Layous et al., 2013), it is equally true that null effects have been yielded across both settings, too (e.g. Aborida, 2016; Austenfeld et al., 2006). It remains unclear which procedural differences influence the effects of BPFS-W, if any do at all, due to the limited amount of available literature surrounding empirical investigation of the effects of procedural variables. It would perhaps be useful for further research to explore what the necessary boundary conditions are for sustained benefits to be yielded.

It may be possible to create an enhanced version of the BPFS-W intervention through empirical investigation of the most effective administration characteristics such as the optimal dose and spacing of sessions. Perhaps this enhanced version would possess the therapeutic power to induce long-term change. However, from the current state of the literature, it appears likely that the most accurate summary of the effects of BPFS-W is that it immediately boosts positive emotions regardless of intervention characteristics, but that longer-term changes are less reliable. Studies with a range of procedural and intervention administration characteristics have yielded null findings with regards to sustained benefits, so perhaps an optimal recipe for these changes does not exist. It was briefly mentioned earlier in this chapter that activities can be pleasurable without being therapeutic, and the example of increases in PA

following consumption of tea was discussed. It appears that this is a useful analogy for the effects of BPFS-W. Consumption of tea immediately and temporarily increases positive feelings yet is not truly therapeutic in terms of sustained well-being gains (Einöther et al., 2015). The results of the current systematic review suggest that BPFS-W is an enjoyable activity which makes participants feel positive, but it is largely not therapeutically-active, and does not reliably lead to sustained symptomatic, cognitive or physiological change.

Chapter Seven

General Discussion

7.1 Summary of findings

One of the initial aims of this thesis was to examine the comparative effects of process and outcome best possible future self (BPFS) writing and mental simulation tasks on physical and psychological well-being, as well as on self-regulation, emotion-regulation and self-efficacy. The other was to examine whether increases in self-regulation may be the mechanism through which BPFS writing (BPFS-W) elicits the well-being benefits reported in previous literature (e.g. King, 2001; Shapira & Mongrain, 2010). These aims were first addressed in Study One (Chapter Four). Although all BPFS conditions demonstrated significantly higher positive affect immediately post-writing in comparison to controls, results showed no significant between-group differences in physical and psychological well-being, emotion-regulation and self-efficacy at follow-ups. There was, however, higher self-regulation eight weeks post-intervention in BPFS-W participants relative to writing controls. This was irrespective of whether participants wrote about the outcome of achieving their BPFS or the process they would have to go through to reach it. On the other hand, there was no significant difference between simulation outcome participants and simulation controls. Simulating the process towards a BPFS was found to be detrimental to self-regulation.

Given that the effects of BPFS simulation and writing on self-regulation differed, it was decided that they are likely not the comparable interventions that King (2001) suggested they are. Therefore, it was decided that the rest of the thesis should focus only on BPFS-W. The increase in self-regulation found in Study One was promising and suggested that BPFS-W may foster self-regulatory processes as suggested by King (2001; 2002). However, no benefits of BPFS-

W were found to physical and psychological well-being. It was suggested that this may have been due to some of the procedural characteristics of the study, such as a single writing session, which may have lowered the therapeutic power of BPFS-W. It was therefore decided to replicate the procedure used by King (2001) in Study Two of the current research programme, to ascertain whether the self-regulation benefits found in Study One would be fostered by a BPFS procedure which has previously been found to benefit well-being. If this had been the case it would have demonstrated that self-regulation and well-being benefits could occur within the same procedural parameters, and that the self-regulation benefits of BPFS-W may be flexible in terms of the conditions required for them to be yielded. This was not the case; BPFS-W across four 20-minute sessions elicited neither well-being nor self-regulation benefits. These findings were unexpected, given the similarity of the procedure used to that of King (2001). However, although King's (2001) study was replicated as closely as possible, there were some procedural differences between Study Two and King's (2001) study which were necessary due to the time-constraints of a Ph.D. programme, such as a shorter follow-up period and online administration. It was suggested that the null findings in Study Two may have been attributable to these factors.

Across Studies One and Two, it was acknowledged that other studies (e.g. Maddalena, Saxey-Reese and Barnes, 2014; Peters, Meevissen & Hanssen, 2013) have used procedures which differed from King's (2001). It was also acknowledged that well-being benefits have been yielded from studies with some of the characteristics of Studies One and Two. Therefore, it was clear that it is not necessary to follow King's (2001) procedure to harvest effects. What was less clear was what procedural characteristics— or combinations of characteristics— are necessary for well-being benefits to occur. It was difficult to compare findings across BPFS-W studies because they differ markedly in procedural factors such as the number, spacing and length of writing sessions, the specific writing instructions used, and the length of follow-up periods. Accurate interpretations of differences in findings between Studies One and Two and other investigations were, therefore, near impossible.

It was thus decided that a systematic review would be a useful contribution to the literature surrounding the effects of BPFS-W. Systematic reviews condense complex evidence bases and provide holistic interpretative platforms from which inconsistencies and patterns can be identified far more easily than they could be from comparisons of individual studies (Haase, 2011). Therefore, a systematic review of the effects of BPFS-W was conducted and is presented in Chapter Six. There were two main aims of the review. The first was to establish whether the available evidence suggested that BPFS-W benefits physical and psychological well-being relative to no-activity and placebo controls. The second aim was to establish whether procedural differences between studies impact the efficacy of BPFS-W. The systematic review is the most comprehensive and contemporary record of the evidence surrounding the effects of BPFS-W in existence. Generally, the review findings demonstrated that BPFS-W consistently elicits immediate boosts in some aspects of psychological well-being (such as positive affect), irrespective of intervention administration procedures. However, it appears that BPFS-W does not reliably impact physical, physiological or cognitive outcomes, and largely does not elicit sustained improvements in psychological well-being. It was suggested that long-term well-being benefits may be possible, but perhaps only when certain procedures are used or only for a limited time-period. However, it remained unclear which procedural differences, if any, may strengthen the therapeutic power of the intervention.

7.2 Contributions to knowledge and implications

7.2.1 Effects of mental simulation of a BPFS

Future-oriented mental simulation has previously been found to be beneficial for self-regulatory processes such as planning, as well as for reductions in anxiety and increases in self-efficacy, particularly when participants simulate the process towards a goal rather than the outcome of achieving it (e.g. Armitage & Reidy, 2008; Pham & Taylor, 1997, as cited by Taylor & Pham, 1996; Pham & Taylor, 1999). Interestingly, in Study One of the current thesis mental simulation of a BPFS was not found to be effective for self-regulation, emotion-regulation, self-efficacy or well-being. These findings are important, because they may point towards the limits of the use of mental simulation as an intervention.

It is possible that the null effects of mental simulation on well-being, emotion-regulation and self-efficacy— and the reduction in self-regulation following process simulation— are attributable to the temporal proximity and specificity of a BPFS. Typically, future-oriented mental simulation tasks direct participants to generate imagery about an imminent, specific event, such as a dental appointment or an examination (Armitage & Reidy, 2012; Pham & Taylor, 1999). A BPFS is broad and is several years from the present, and construal-level theory suggests that imagined events become more abstract and decontextualized as their temporal distance increases (e.g. Liberman & Trope, 2008). Perhaps, therefore, imagery surrounding a BPFS does not possess the likeness to reality that is thought to be critical for translating thought into goal-directed action through mental simulation (Taylor et al., 1998; see Chapter Two, Section 2.3.2). Investigation of the effects of goal proximity and specificity on the efficacy of mental simulation would be a fruitful avenue for future research. This would develop understanding of the flexibility and applicability of mental simulation to different goals and situations.

The null effect of BPFS outcome simulation— and the damaging effect of process simulation— on self-regulation found in Study One contrast with the benefits to self-regulation following BPFS-W. It is possible to tentatively suggest from this that King's (2001) implication that BPFS-W and simulation are comparable processes, or that BPFS-W involves simulation, are incorrect. The current thesis has negated, to an extent, several assumptions surrounding the effects and mechanics of BPFS-W, as discussed in Sections 7.2.2 and 7.2.3.

7.2.2 Effects of BPFS-W on physical and psychological well-being

The academic narrative surrounding the effects of BPFS-W on physical and psychological well-being evokes an image of success. For example, Boselie, Vancleef and Peters (2017) state that its efficacy has been 'proven'. The promotion of the intervention through the academic narrative has resulted in its transition into public use. BPFS-W has been recommended across multiple well-being websites and self-help sources, targeted at both general society and clinical groups (e.g. Greater Good in Action, n.d.; Soaringwords, n.d.). It has

also been recommended for clinical practice and psychotherapy (e.g. O'Hanlon & Bertolino, 2011). However, the findings from the two experimental studies and the systematic review in the current thesis suggest that this confidence in BPFS-W is, to an extent, unfounded.

In Study One, there was significantly higher positive affect (PA) immediately following BPFS-W in comparison to control writing, regardless of whether participants wrote about the process towards their BPFS or the outcome of achieving it. This finding was not replicated in Study Two. It was suggested in Chapter Five that this null finding may have occurred for one of two reasons. First, analyses may have been underpowered. Second participants may not have engaged with the intervention sufficiently for gains in PA to occur due to internet-mediated administration. The findings of the systematic review demonstrated that BPFS consistently elicits immediate gains in some aspects of psychological well-being, such as happiness and PA, and this effect appears generalisable across laboratory and online settings. It is most likely, therefore, that the lack of change in PA from pre- to post-BPFS-W found in Study Two is attributable to low power, especially given that the p value was approaching significance and the effect size was moderate.

The findings of the current thesis suggest that BPFS-W typically elicits an immediate increase in some aspects of psychological well-being, such as PA. Findings surrounding the longer-term effects, however, are less promising. In Studies One and Two, there were found to be no sustained effects of BPFS-W on any well-being outcomes. It was suggested in Chapters Four and Five that these null findings may have arisen from the intervention procedures used. This may be true; the results of the systematic review demonstrated that there has been very little research effort invested into exploration of the effects of procedural variations on intervention outcomes, thus their effects remain unknown. The current evidence, comprising only three studies, suggests that temporal spacing of writing sessions (three in one day versus weekly over three weeks), number of writing sessions, and setting (online versus in person) do not

impact intervention outcomes[133]. To attempt to draw conclusions from this evidence base alone would not be sensible. Each procedural variable was explored in only a single study, and the quality assessment conducted as part of the review (see Chapter Six, Section 6.4.5) revealed areas of risk of bias in these studies. Furthermore, even if these studies were to be replicated with areas of possible bias controlled for, they would reveal little about the effects of procedural variations as they test a small proportion of the existing differences across studies. For example, no significant difference between spacing of writing instructions across a single day and across three weeks would not conclusively mean that spacing does not impact outcomes, because numerous other administration patterns have been used, such as daily across three consecutive days (see Section 6.4.4). A large amount of further research would be necessary for meaningful conclusions with regards to the effects of procedural characteristics on BPFS-W outcomes to be attempted.

The results of the systematic review demonstrated that, based on the available evidence, the effects of differences in intervention administration characteristics on outcomes of BPFS-W are largely unknown. It was suggested that it may be possible to create an enhanced version of the BPFS-W intervention through extensive empirical investigation of the most effective procedural characteristics. However, if the intervention must be performed only within specific procedural parameters, then the costs of researching what these conditions are may not be worth the possible benefits. Many individuals are found to show poor adherence and compliance across psychological treatments (Arch & Craske, 2009; Brown et al., 2011; Cavanagh, 2010; Ogrodniczuk, Piper & Joyce, 2006), so would likely not adhere to a set of specific, inflexible procedural instructions. This would undermine the main attraction of writing interventions including BPFS-W: that they are accessible and cost-effective (Pennebaker, 2004). It is also possible that an optimal, robust recipe for well-being improvements following BPFS-W does not exist. An important finding from the systematic review is that null findings were highly generalisable across

[133] Although not a procedural variable per se, the systematic review also demonstrated that outcomes do not appear to differ as results of whether participants are asked to write about the process towards their BPFS or the outcome. Again, caution is advised due to the small number of studies which have investigated this possibility as well as potential bias in those studies.

procedural variations. Regardless of whether single or multiple writing sessions were used, whether the intervention was administered in a laboratory or online, whether supplementary imagery was used and how far apart sessions were completed, sustained well-being benefits were frequently found not to occur.

Overall, the findings of the current thesis suggest that BPFS-W is not as beneficial or robust as has been previously assumed. Short-term, it does boost positive feelings. However, long-term changes in physical and psychological well-being are unreliable, perhaps irrespective of the administration procedure used.

7.2.3 Effects of BPFS-W on self-regulation

A role of self-regulation in the mechanisms through which BPFS-W may benefit physical and psychological well-being was first suggested by King (2001; 2002). This suggestion has since been repeated and endorsed multiple times (e.g. Frattaroli, 2006; Sheldon & Lyubomirsky, 2006). However, the first study to include investigation of the effects of BPFS-W on self-regulation was Study One of this research programme. Furthermore, the only existing synthesis of the evidence surrounding possible effects on self-regulation and related variables is presented in Chapter Six.

The significantly higher self-regulation in individuals who wrote about a BPFS in comparison to writing controls found eight weeks following a single laboratory-based 20-minute writing session in Study One was promising and suggested that BPFS-W may foster self-regulatory processes, as suggested by King (2001; 2002). However, this effect was not replicated in Study Two, following four 20-minute writing sessions which were administered online. It is possible that a laboratory setting in which participants are free from distractions is required or gains in self-regulation to emerge. Together, Studies One and Two suggest that self-regulatory processes may be encouraged by BPFS-W, but perhaps only in a controlled laboratory environment.

The results of the systematic review (Chapter Six) painted a somewhat more complex picture. Synthesis of the available evidence suggested that BPFS-W is

not beneficial for working-memory or set-shifting. Successful self-regulation is at least in-part dependent on the efficiency of these processes (Hofmann, Schmeicel & Baddeley, 2012; Kane, Bleckley, Conway & Engle, 2001; Unsworth, Schrock & Engle, 2004). Therefore, it is possible to tentatively suggest that if there are no benefits of BPFS-W to working-memory capacity and set-shifting, then the intervention would not be expected to benefit self-regulation. This inference contradicts the findings of Study One which suggested that self-regulation may be bolstered by BPFS-W, at least under certain conditions such as a laboratory intervention setting. The evidence surrounding effects on working-memory and set-shifting synthesised in the systematic review was based on cognitive tasks, whereas the findings in Studies One and Two in the current research programme were yielded from self-report measures. Therefore, it was suggested in Chapter Six that the intervention is unlikely to benefit self-regulation but may increase individuals' perceptions of their recent self-regulatory function. Overall, the findings of this thesis suggest it is unlikely that BPFS-W increases self-regulation. This is an important contribution to knowledge, because if BPFS-W does not impact self-regulation, then it cannot be the mechanism through which BPFS-W may elicit well-being benefits.

7.3 Limitations and directions for future research

The null effects of BPFS-W and mental simulation found in Studies One and Two may have arisen due to the specific procedures used (as well as the characteristics of a BPFS being too broad to be simulated effectively). They may also, as suggested by the systematic review findings, be accurate reflections of the limited effects of BPFS interventions. However, a limitation of the current thesis which could account for the null effects yielded in these studies is the method used to recruit participants. In both studies, individuals were given incentives to participate. First year undergraduate Psychology students were offered course credit and other participants were offered £5 vouchers. Incentives can, of course, motivate individuals to take part in research studies (Boutis & Willison, 2008). This means that the data collection process is faster and more efficient, which was important in the current programme of research given that it had to be completed within the time-

constraints of a Ph.D. However, there is evidence that some individuals who participate in studies with incentives do so only for the incentive and have no other motivation to take part (Aby, Pheley & Steinberg, 1996; Zullino, Conus, Borgeat & Bonsak, 2003). Unsurprisingly, therefore, offering incentives has been found to reduce the quality of information provided by participants (Bentley & Thacker, 2004; Lemmens & Elliott, 2001; McKeganey, 2001). Importantly, Sheldon and Lyubomirsky (2006) investigated the effects of self-concordant motivation (i.e. motivated to participate by valuing the activity, rather than for a reward or to avoid feeling anxious or guilty for not participating; Sheldon & Elliot, 1999) on the effects of BPFS-W, and found that participants with high self-concordant motivation benefitted more from the intervention. Perhaps the individuals who participated in Studies One and Two of the current thesis did so only for the reward of a gift voucher or course credit, which could mean that they did not invest effort in their participation. This is a hypothetical suggestion, because participants' reasons for completing the studies were not sought. It should also be noted that multiple other BPFS studies have offered similar incentives (e.g. Murn, 2013; Odou & Vella-Brodrick, 2013; Seear & Vella-Brodrick, 2013). Findings from these studies are mixed and the effect of offering incentives on BPFS intervention outcomes has not been investigated. However, it is possible that the lack of significant effects of BPFS-W and simulation on outcomes in the studies in this thesis may be at least in-part attributable to a low level of participant effort. Further research could investigate the effects of incentives on outcomes of BPFS interventions.

An important contribution of the current thesis is that BPFS-W may not yield gains in self-regulation, providing evidence against King's (2001; 2002) hypothesis that increased self-regulation is the mechanism through which possible physical and psychological well-being benefits of the intervention occur. However, the evidence is to an extent inconclusive. There are two clear directions for further research which would allow conclusions with regards to the effects of BPFS-W on self-regulation to be drawn with greater confidence. First, the findings of the systematic review demonstrated that BPFS-W likely does not affect working-memory or set-shifting (upon which self-regulation is thought to depend in-part) but may bolster perceptions of self-regulation (under certain

conditions). Working-memory and set-shifting are two of three fundamental executive functions known to subtend self-regulatory processes, the third being inhibition, which has not been measured an as outcome of BPFS-W (Hofmann et al., 2012; Miyake et al., 2000; Monsell, 2003). Therefore, a sensible avenue for further research would be to explore the effects of the intervention on inhibition, for example using the Stroop Test (Stroop, 1935). Second, it is possible that BPFS-W only impacts self-regulation in the spheres written about by participants, as discussed in Chapter Six (Section 6.6.1.3). It would be useful to investigate this by assigning participants a writing task about a specific sphere of their BPFS, and then measuring self-regulation of action towards goals in that sphere. As suggested in Section 6.6.1.3, a sensible starting point would be to ask participants to write about health goals, because health behaviours lend themselves to objective measurements of self-regulation by way of biomarkers. This would have the additional benefit of eliminating need for self-report measures of self-regulation, which are known to be subjective and at risk of social desirability bias and recall error (Brener, Billy & O'Grady, 2003; King & Bruner, 2000; Tourangeau, 2000; Van de Mortel, 2008; Verplanken & Orbell, 2003). These endeavours would together generate a more robust representation of the effects of BPFS-W on self-regulation. Investigation of the effects of the intervention on inhibition would provide a more complete picture of its impact on the cognitive processes upon which self-regulation depends. If the intervention does not elicit change in working-memory, set-shifting or inhibition, then it is unlikely that it would impact self-regulation. Study of biomarkers of goal-directed action would be particularly important, because this would indicate whether any changes in self-regulatory processes are enough to spur goal-directed action.

The findings of the current thesis suggest that BPFS-W is not a reliable means of boosting well-being long-term, and that it may not elicit changes in cognitive variables including self-regulation, either. The findings also suggest that procedural variations do not influence intervention efficacy, and that there is unlikely to be an optimal recipe for success. It should, however, be noted that the current thesis has not included investigation of the participant characteristics or individual differences which may influence intervention effects. The

systematic review revealed that most studies of the effects of BPFS-W have employed young, healthy students as participants (see Chapter Six, Section 6.4.3). Perhaps BPFS-W does have potential to be a useful activity for well-being or self-regulation, but not in the samples within which it has been investigated so far. There is some evidence surrounding the effects of individual differences on outcomes of BPFS-W, and findings are mixed. For example, Meevissen, Peters and Alberts (2011) and Sheldon and Lyubomirsky (2006) found that trait optimism and gender did not moderate the effects of BPFS-W on well-being. On the other hand, Ng (2016) and Austenfeld (2007) found that the intervention was more beneficial for individuals high in neuroticism and low in emotional processing. It therefore appears that some individual differences may moderate the effects of BPFS-W. It would be useful for future research to be conducted to investigate the effects of the intervention in a wider variety of samples, and to explore the effects of demographic and personality characteristics on intervention outcomes. This is an important suggestion; BPFS-W is cost-effective and accessible (Pennebaker, 2004), thus it is important to ascertain whether there may be individuals who could reliably benefit from it.

The specific suggestions for future research above- and those discussed throughout this thesis- may expand understanding of the possible effects of BPFS-W. However, before these possibilities are explored it may be sensible for research to be undertaken to confirm and strengthen the existing evidence. In recent years there has been growing concern that Psychology is facing a 'replicability crisis' due to frequent failed efforts to replicate results (Hengartner, 2018; Maxwell, Lau & Howard, 2015; Rodgers & Shrout, 2018; Witte & Zenker, 2017). Often, an initial study designed to test a hypothesis will yield a statistically-significant effect, yet replications will not (Maxwell et al., 2015). This pattern has given rise to questions regarding the authenticity of research results in Psychology. None of the BPFS-W studies were true replications; as discussed at length throughout this thesis they differed in intervention procedures and assessed outcomes. This was perhaps due to greater value placed on novelty and innovation compared to confirmation by journal editors and reviewers (Neuliep & Crandall, 1990; 1993; Open Science Collaboration,

2012). It has been acknowledged that for true effects of a manipulation to be identified, multiple replications are needed, and that studies should include large samples accumulated from multiple laboratories (Maxwell et al., 2015; McShane, Tackett, Bockenhölt & Gelman, 2018). Therefore, given that evidence for most outcomes is comprised of the results of a small number of studies (as demonstrated in Chapter Six), it appears that the true effects of BPFS-W remain unknown. Considering this, it would be justifiable to conduct multi-lab replications of BPFS-W studies using large samples to increase confidence in findings.

7.4 Summary and Conclusion

The overarching aim of this thesis was to investigate King's (2001; 2002) suggestion that BPFS-W benefits physical and psychological well-being, and that it does so through increasing self-regulation. In addressing this aim, the effects of BPFS-W were empirically compared with the effects of future-oriented mental simulation, which has previously been found to bolster self-regulatory processes such as planning, and to improve goal performance (e.g. Pham & Taylor, 1999; Taylor & Pham, 1999). Mental simulation is more effective when the processes towards a goal are imagined in comparison to the outcome of achieving a goal. Therefore, the effects of process- and outcome-focussed BPFS-W tasks were also compared to investigate whether this manipulation could also strengthen the therapeutic power of BPFS-W.

A single 20-minute BPFS-W task was found to boost participants' reports of their self-regulatory function eight weeks post-writing in Study One of the thesis, regardless of whether the writing was process- or outcome-focussed. However, outcome-focussed mental simulation did not benefit self-regulation relative to control simulation, and process-focussed simulation was found to be detrimental. Physical and psychological symptoms were not reduced by process- or outcome-focussed writing or simulation. As discussed in Chapter Four, it is likely that a BPFS is too broad and distal to be truly mentally-simulated, which may explain the unexpected results of the mental simulation manipulations. The null effects of BPFS-W on well-being were more difficult to interpret. They may be attributable to the administration procedure used in

Study One, including use of a single writing session. This potential was explored in Study Two by replicating King's (2001) original BPFS-W procedure of four 20-minute sessions across four consecutive days. Again, there were no benefits to physical and psychological well-being, and the benefits to self-regulation found in Study One were not replicated. It was suggested in Chapter Five that these null findings may be attributable to some remaining procedural differences between Study Two and King's (2001) investigation, including online administration.

In Chapters Four and Five it was acknowledged that it is difficult to interpret differences in findings across BPFS-W studies due to marked variations in procedural factors such as the writing instructions used, the number, spacing and length of writing sessions, and the timing of follow-ups. The systematic review presented in Chapter Six was conducted to identify patterns and inconsistencies which could be overlooked when drawing comparisons across individual studies. The review demonstrated that BPFS-W consistently elicits an immediate increase in some aspects of psychological well-being, but does not appear to have reliable physical, physiological or cognitive benefits. It also does not appear to benefit most aspects of psychological well-being long-term. The review also demonstrated that, although there is little empirical evidence surrounding the effects of procedural variations on outcomes, null findings are generalisable. It was suggested in the systematic review and earlier in the current chapter that it may be possible to create an enhanced version of the BPFS-W intervention, or that perhaps individual differences research could identify a subset of the population for whom the intervention is beneficial. However, the results of the thesis generally suggest that BPFS-W is not a reliable means of boosting sustained well-being or alleviating symptoms. The results also suggest that it is not beneficial for increasing self-regulatory function (although further research is needed before firm conclusions regarding effects on self-regulation are drawn). Nevertheless, it is important to acknowledge that BPFS-W does not require financial resources, and reliably elicits temporary positive feelings.

To conclude, the current thesis has made an original contribution to knowledge by directly investigating the effects of BPFS-W on self-regulation, which has previously been proposed as a mechanism of effect. The thesis has also contributed through the production of a systematic review which is, to the knowledge of the researcher, the most comprehensive synthesis of the evidence surrounding the effects of BPFS-W in existence. This thesis has demonstrated that the therapeutic effects of BPFS-W on physical and psychological well-being— as well as on related processes including self-regulation— appear to be limited, at least in healthy student populations. However, use of BPFS-W need not be cautioned; it is free and accessible, and reliably elicits immediate, temporary increases in positive feelings, regardless of the administration procedure used.

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Appendices

A.1 Conference presentation

Bean, M., Cutts, K., & Reidy, J. (2017). Writing about and mentally-simulating the best possible future: Impacts on self-regulation. *Proceedings of the British Psychological Society Annual Conference, Brighton, UK,* 98*.*

A.2 Study One advertisement for research participation site

A.3 Study One participant information sheets[134]

A.3.1 Information sheet before recruitment modifications

You are invited to participate in a study looking at the effects of imagining life activities on wellbeing. I am a post-graduate Psychology student at Sheffield Hallam University, and this research is being carried out as part of my PhD. Participation will include completion of a series of tasks and questionnaires. The initial phase of the study will take place in a Psychology lab at Sheffield Hallam University, and will take around 30 minutes to complete. There will be 3 further follow-up studies which will take place 1 week, 1 month and 2 months following the intervention phase. These will last around 10 minutes each.

You are to remain anonymous; your responses to questions and activities will be matched using a unique participant identification code. You should generate this yourself using the last 3 digits of your mobile telephone number and the first 3 letters of a pet's or best friend's name. All data provided by you will be stored securely on an encrypted memory stick and password-protected computer only. Data will remain confidential and will be seen only by the researcher and supervisors named on this form, apart from in the exceptional circumstance that you disclose information that yourself or others may be at risk of harm. Once the project is completed, it is possible that your data may be included in scholarly articles. If this were the case, all raw data (e.g. questionnaires) would be stored in the University Research Archive for up to 5 years- however be assured that your data would remain confidential and your personal details would not be accessible. Otherwise, data will be destroyed as confidential waste. Raw data will not be included in the write-up of this investigation; only average scores and ranges of scores will be presented. Data will not be passed on to other institutions or agencies.

Your e-mail address will be required by the researcher in order for you to be eligible to participate. This is because the researcher will need to send you invitations to complete the follow-up phases. However, e-mail addresses will be stored separately to the data; therefore it will not be in any way possible to connect your personal e-mail address to the data.

You do not have to take part in this study- participation is completely voluntary. You have the right to withdraw from this study at any time throughout your participation, and the right to refuse responses to any questions which you are not comfortable in answering, without there being any consequences. You have

[134] There are two versions of the information sheet, research ethics proforma and approval letter for Study One. Initially, only first year Psychology students from Sheffield Hallam University were invited to participate. They were offered only course credits for their time. However, it was decided to widen the participant pool to facilitate achievement of a larger sample size. The study was then open to anyone. Participants who were not first year Sheffield Hallam Psychology students were offered a £5 high street voucher for their time.

the right to withdraw any data that you provide up to 7 days after the initial phase and each follow-up phase; after this point it will not be possible due to anonymity making data impossible to identify. You have the right to ask the researcher any questions that you have prior to participating, and you may also contact them via e-mail at any time throughout your participation if you so wish.

You may also contact the researcher if you would like to be sent a copy of the report after it has been completed and submitted.

First year Psychology students will receive 60 minutes of psycreds on completion of the study.

Researcher: Megan Bean a9022330@my.shu.ac.uk
Supervisors: Dr. Katie Cutts (Director of Studies) k.cutts@shu.ac.uk
Dr. John Reidy (Second Supervisor) ssljgr@exchange.shu.ac.uk

A.3.2 Information sheet after recruitment modifications

You are invited to participate in a study looking at the effects of imagining life activities on wellbeing. I am a post-graduate Psychology student at Sheffield Hallam University, and this research is being carried out as part of my PhD. Participation will include completion of a series of tasks and questionnaires. The initial phase of the study will take place in a Psychology lab at Sheffield Hallam University, and will take around 30 minutes to complete. There will be 3 further follow-up studies which will take place 1 week, 1 month and 2 months following the intervention phase. These will last around 10 minutes each.

You are to remain anonymous; your responses to questions and activities will be matched using a unique participant identification code. You should generate this yourself using the last 3 digits of your mobile telephone number and the first 3 letters of a pet's or best friend's name. All data provided by you will be stored securely on an encrypted memory stick and password-protected computer only. Data will remain confidential and will be seen only by the researcher and supervisors named on this form, apart from in the exceptional circumstance that you disclose information that yourself or others may be at risk of harm. Once the project is completed, it is possible that your data may be included in scholarly articles. If this were the case, all raw data (e.g. questionnaires) would be stored in the University Research Archive for up to 5 years- however be assured that your data would remain confidential and your personal details would not be accessible. Otherwise, data will be destroyed as confidential waste. Raw data will not be included in the write-up of this investigation; only average scores and ranges of scores will be presented. Data will not be passed on to other institutions or agencies.

Your e-mail address will be required by the researcher in order for you to be eligible to participate. This is because the researcher will need to send you invitations to complete the follow-up phases. However, e-mail addresses will be stored separately to the data; therefore it will not be in any way possible to connect your personal e-mail address to the data.

You do not have to take part in this study- participation is completely voluntary. You have the right to withdraw from this study at any time throughout your participation, and the right to refuse responses to any questions which you are not comfortable in answering, without there being any consequences. You have the right to withdraw any data that you provide up to 7 days after the initial phase and each follow-up phase; after this point it will not be possible due to anonymity making data impossible to identify. You have the right to ask the researcher any questions that you have prior to participating, and you may also contact them via e-mail at any time throughout your participation if you so wish.

You may also contact the researcher if you would like to be sent a copy of the report after it has been completed and submitted.

First year Psychology students will receive 60 minutes of psycreds on completion of the study. Other participants will receive a £5 high street shopping voucher on completion of the study.

Researcher: Megan Bean a9022330@my.shu.ac.uk
Supervisors: Dr. Katie Cutts (Director of Studies) k.cutts@shu.ac.uk
Dr. John Reidy (Second Supervisor) ssljgr@exchange.shu.ac.uk

A.4 Study One participant consent form

- Who has spoken to you about the current study? …….

Please write ‘yes’ or ‘no’ on the dotted lines to provide your responses to the following questions:

- Have you read and understood the information sheet explaining this research? ……
- Have you been provided with sufficient opportunity to ask questions? ……
- Have you received satisfactory answers to any questions that you asked? ……
- Has the researcher provided you with sufficient information explaining the current study? ……
- Do you understand that you have the right to withdraw, without consequence, from this study:
    1. At any time throughout your participation? ……
    2. Without giving any reason for withdrawing? ……
- Do you understand that you have the right to refuse to give answers to any questions that you do not feel comfortable in answering, without giving reason, and without consequences? ……
- Do you understand that you have the right to withdraw any data that you provide up to 7 days after the intervention phase of the study and up to 7 days after each follow-up phase? ……

By providing your signature on this consent form, you are confirming that the current study has been explained to you sufficiently by the researcher, and that you have received satisfactory answers to any questions that you may have had. You are also confirming that you are aware of your right to withdraw from the study at any time, and of your right to refuse responses to any questions that you are not at ease in answering, without there being any consequences whatsoever.

- Do you agree to voluntarily participate in this research? ……

By signing this consent form, you are giving your informed consent of your voluntary participation in this investigation, and the anonymous, confidential inclusion of your data in the analyses and write-up of the investigation.
Participant signature: ……. Date: ……
Participant name: ……
Researcher signature: …… Date: …….
Please file your copies of the information sheet and consent form together in a safe place.
Megan Bean (Researcher): a9022330@my.shu.ac.uk
Katie Cutts (Director of Studies): k.cutts@shu.ac.uk

A.5 Study One debrief sheet

Thank you for your participation in this investigation. Writing or mentally simulating (imagining as though you are in the moment) about a best possible future self has been found to be associated with a range of benefits including; improved health and psychological wellbeing, higher University examination grades and improved studying techniques. Some research has found that simulating about actions to be taken in order to reach a desirable future event is more effective than simulating about the positive event itself. The present investigation aimed to explore whether this pattern remained when individuals wrote about positive futures or activities to help them to reach a positive future. It also aimed to investigate whether beneficial effects of writing or simulating are brought about by increases in individuals' levels of self-regulation.

Some participants were asked to write and others were asked to mentally simulate about 1 of 3 topics; a best possible future self, the process that they would have to undergo or the tasks that they would have to complete in order to reach their best possible future self, or an emotionally neutral control topic (describing what you did yesterday). It is hoped that individuals who have been participants in the current study will experience some benefits from taking part, which has been the case in previous studies conducted in the same area.

Please be assured that any information which was disclosed by you during the course of your participation will remain anonymous and confidential.

If you have any worries with regards to your physical or psychological health, you might be interested in contacting some of the University's health service providers. Contact details for a selection of these services are provided below:
Medical centre: 0114 225 2134
Counselling/ wellbeing service: 0114 2252136
student.wellbeing@shu.ac.uk

If you have any further questions about the study or your participation, wish to contact the researcher or would like to receive a copy of the write-up of this investigation once it has been completed, you may do so at any time via e-mail:

Megan Bean (Researcher): a9022330@my.shu.ac.uk
Director of studies: k.cutts@shu.ac.uk
Second supervisor: ssljgr@exchange.shu.ac.uk

A.6 Study One ethics proformas and approval letters

A.6.1 Ethics proformas

A.6.1.1 Ethics proforma before recruitment modifications

Research Ethics Checklist (SHUREC1)

This form is designed to help staff and students to complete an ethical scrutiny of proposed research. The SHU Research Ethics Policy should be consulted before completing the form.
Answering the questions below will help you decide whether your proposed research requires ethical review by a Faculty Research Ethics Committee (FREC). In cases of uncertainty, members of the FREC can be approached for advice.
Please note: staff based in University central departments should submit to the University Ethics Committee (SHUREC) for review and advice.
The final responsibility for ensuring that ethical research practices are followed rests with the supervisor for student research and with the principal investigator for staff research projects.
Note that students and staff are responsible for making suitable arrangements for keeping data secure and, if relevant, for keeping the identity of participants anonymous. They are also responsible for following SHU guidelines about data encryption.
The form also enables the University and Faculty to keep a record confirming that research conducted has been subjected to ethical scrutiny.

- For student projects, the form may be completed by the student and the supervisor and/or module leader (as applicable). In all cases, it should be counter-signed by the supervisor and/or module leader, and kept as a record showing that ethical scrutiny has occurred. Students should retain a copy for inclusion in their research projects, and staff should keep a copy in the student file.
- For staff research, the form should be completed and kept by the principal investigator.

Please note if it may be necessary to conduct a health and safety risk assessment for the proposed research. Further information can be obtained from the Faculty Safety Co-ordinator.

General Details

(Table cells will expand as you type)

Name of principal investigator or student	Megan Bean
SHU email address	a9022330@my.shu.ac.uk
Course or qualification (student)	Doctor of Philosophy
Name of supervisor (if applicable)	Dr Katie Cutts (Director of Studies)
email address	k.cutts@shu.ac.uk
Title of proposed research	Effects of writing and mentally simulating about a best possible future on health: What are the 'active ingredients'?
Proposed start date	February 2015
Proposed end date	June 2015

Brief outline of research to include, rationale & aims (250-500 words). In addition for research with human, participants, include recruitment method, participant details & proposed methodology (250-500)	King (2001) found that students who wrote about a best possible future self (BPFS) or a trauma demonstrated improved health compared to peers who wrote about daily plans. The prominent theoretical explanation for this effect is that writing facilitates self-regulation. To clarify, possible selves are personalised representations of goals (Markus & Nurius, 1986), and goals that individuals set for themselves reflect self-regulatory processes (Austin & Vancouver, 1996). Furthermore, King (2001) asserts that control participants did not show health benefits, and their assigned task may be considered writing about lower-order goals. Outcome goals (e.g. BPFS, hold a higher order in the motivational hierarchy than immediate/ short-term goals such as plans for the day, hence are less likely to be regularly considered (King, 2001). Therefore, encouraging individuals to consider higher-order goals through writing about them may enable them to explore aspects of their motivational lives that are mostly unexamined (King, 2001). Examination of literature outside of BPFS writing supports the suggestion of self-regulation as a mechanism through which the intervention elicits health benefits; however it is not entirely consistent with King's (2001) postulation. Pham and Taylor (1999) assigned students to mental simulation conditions 5 to 7 days prior to a course examination. In their investigation, some students simulated the outcome of actually achieving a commendable grade, whilst others simulated the process required in order to achieve this outcome. In contrast to King's (2001) suggestion that considering higher-order goals may be more beneficial in terms of self-regulatory processes than lower-order goals, Pham and Taylor (1999) found that process simulation (lower-order goals) improved studying techniques and was associated with augmented grades, and that the latter effect was mediated by diminished anxiety levels and improved planning abilities. In line with this, Taylor and

	Schneider (1989) postulate that mental simulations facilitate self-directed action, and Pham and Taylor (1999) theorise that simulation is beneficial as it facilitates generation of a clear image of a desirable future and enables the individual to construct a plan as to how they will reach it. Although Pham and Taylor's (1999) findings suggest process simulations are more beneficial for self-regulation than outcome simulations, the question of whether this is maintained in writing interventions has not been explored. Hence, the present investigation aims to compare effects of writing/ simulating about a BPFS with effects of writing/ simulating about the process which must be successfully followed to attain it. Furthermore, although frequently suggested (e.g. King, 2001; Taylor & Schneider, 1989), a mediating role of self-regulation in producing health benefits following simulation or writing about goals has not been investigated directly. This research aims to explore affective and behavioural self-regulation as outcomes of both intervention tasks, as well as mediating and moderating effects of any changes in self-regulation on changes in physical and psychological health. Participants The sample will consist of Psychology students at Sheffield Hallam University. Participants will be recruited using an advertisement placed on the University's internal research participation site. Method A mixed measures experimental design will be implemented. There will be two between-participants independent variables. The first is task type; outcome, process, and control. The second is intervention mode; writing, and mental simulation. There will be one within-participants independent variable, which is time (pre- and post-intervention, and 1 week, 1 month and 2 month follow-ups). Dependent variables will be physical health, psychological health, positive affect, negative

	affect, and behavioural and affective self-regulation. Mediating and moderating effects of any change in self-regulation on other outcome variables will be explored. Procedure and Materials The participant will meet with the researcher and complete the following: • Physical health: Physical Symptoms Inventory (PSI; Spector & Jex, 1998) • Psychological health: Depression, Anxiety and Stress Scale- 21 (DASS-21; Lovibond & Lovibond, 1995) • Positive and negative affect: Positive and Negative Affectivity Scale (PANAS; Watson, Clark & Tellegan, 1988) • Behavioural self-regulation: Self-Regulation Questionnaire (SRQ; Brown, Miller & Lawendowski, 1999) • Affective Self-regulation: Affective-Style Questionnaire (ASQ; Hofmann & Kashdan, 2010) They will have 1 minute to identify their BPFS in 10 years, followed by 20 minutes of mental simulation or writing about their BPFS (outcome) or the process of attaining it, or a neutral control topic. They will then repeat the PANAS before non-emotively listing the contents of their simulations/writing. Follow-up studies using the measures listed above will also occur at 1 week, 1 month and 2 months following the intervention, to explore for how long any effects remain, and to capture effects which may have a latent onset. References attached

Will the research be conducted with partners & subcontractors?	**Yes/No** No (If **YES**, outline how you will ensure that their ethical policies are consistent with university policy.)

1. Health Related Research Involving the NHS or Social Care / Community Care or the Criminal Justice Service or with Research participants unable to provide informed consent

Question		**Yes/No**
1.	Does the research involve? • Patients recruited because of their past or present use of the NHS or SC • Relatives/carers of patients recruited because of their past or present use of the NHS or SC • Access to data, organs or other bodily material of past or present NHS patients • Foetal material and IVF involving NHS patients • The recently dead in NHS premises • Prisoners or others within the criminal justice system recruited for health-related research* • Police, courts, prisoners or others within the criminal justice system* • Participants who are unable to provide informed consent due to their incapacity even if the project is not health related	No
2.	Is this a research project as opposed to service evaluation or audit? *For NHS definitions please see the following website* http://www.nres.nhs.uk/applications/is-your-project-research/	No

If you have answered **YES** to questions **1 & 2** then you **must** seek the appropriate external approvals from the NHS, Social Care, or Criminal Justice System under their Research Governance schemes. Further information is provided below.
NHS https://www.myresearchproject.org.uk/Signin.aspx
* All prison projects also need National Offender Management Service (NOMS) Approval and Governor's Approval and may need Ministry of Justice approval. Further guidance at: http://www.ohrn.nhs.uk/toolkit/Toolkit4thEdition.pdf

NB FRECs provide Independent Scientific Review for NHS or SC research and initial scrutiny for ethics applications as required for university sponsorship of the research. Applicants can use the NHS proforma and submit this initially to the FREC.

2. Research with Human Participants

Question		Yes/No
1.	Does the research involve human participants? This includes surveys, questionnaires, observing behaviour etc.	Yes
Note	*If YES, then please answer questions 2 to 10* *If NO, please go to Section 3*	
2.	Will any of the participants be vulnerable?	No
Note	*'Vulnerable' people include young people under 18, people with learning disabilities, people who may be limited by age or sickness or disability from understanding the research, etc.*	
3	Are drugs, placebos or other substances (e.g. food substances, vitamins) to be administered to the study participants or will the study involve invasive, intrusive or potentially harmful procedures of any kind?	No
4	Will tissue samples (including blood) be obtained from participants?	No
5	Is pain or more than mild discomfort likely to result from the study?	No
6	Will the study involve prolonged or repetitive testing?	No
7	Is there any reasonable and foreseeable risk of physical or emotional harm to any of the participants?	No
Note	*Harm may be caused by distressing or intrusive interview questions, uncomfortable procedures involving the participant, invasion of privacy, topics relating to highly personal information, topics relating to illegal activity, etc.*	
8	Will anyone be taking part without giving their informed consent?	No
9	Is it covert research?	No
Note	*'Covert research' refers to research that is conducted without the knowledge of participants.*	
10	Will the research output allow identification of any individual who has not given their express consent to be identified?	No

If you answered **YES only** to question **1,** you **must** submit the signed form to the FREC for registration and scrutiny. If you have answered **YES** to any of the other questions you are **required** to submit a SHUREC2A (or 2B) to the FREC. If you answered **YES** to question **8** and participants cannot provide informed consent due to their incapacity you must obtain the appropriate approvals from the NHS research governance system.

3. Research in Organisations

Question		Yes/No
1	Will the research involve working with/within an organisation (e.g. school, business, charity, museum, government department, international agency, etc)?	No
2	If you answered YES to question 1, do you have granted access to conduct the research? *If YES, students please show evidence to your supervisor. PI should retain safely.*	
3	If you answered NO to question 2, is it because: A. you have not yet asked B. you have asked and not yet received an answer C. you have asked and been refused access.	
Note	*You will only be able to start the research when you have been granted access.*	

4. Research with Products and Artefacts

Question		Yes/No
1.	Will the research involve working with copyrighted documents, films, broadcasts, photographs, artworks, designs, products, programmes, databases, networks, processes or secure data?	No
2.	If you answered YES to question 1, are the materials you intend to use in the public domain?	
Notes	*'In the public domain' does not mean the same thing as 'publicly accessible'.* − *Information which is 'in the public domain' is no longer protected by copyright (i.e. copyright has either expired or been waived) and can be used without permission.* − *Information which is 'publicly accessible' (e.g. TV broadcasts, websites, artworks, newspapers) is available for anyone to consult/view. It is still protected by copyright even if there is no copyright notice. In UK law, copyright protection is automatic and does not require a copyright statement, although it is always good practice to provide one. It is necessary to check the terms and conditions of use to find out exactly how the material may be reused etc.* *If you answered YES to question 1, be aware that you may need to consider other ethics codes. For example, when conducting Internet research, consult the code of the Association of Internet Researchers; for educational research, consult the Code of Ethics of the British Educational Research Association.*	
3.	If you answered NO to question 2, do you have explicit permission to use these materials as data? *If YES, please show evidence to your supervisor. PI should*	

Question		Yes/No
	retain permission.	
4.	If you answered NO to question 3, is it because: A. you have not yet asked permission B. you have asked and not yet received and answer C. you have asked and been refused access.	**A/B/C**
Note	*You will only be able to start the research when you have been granted permission to use the specified material.*	

Adherence to SHU policy and procedures

Personal statement	
I can confirm that: − I have read the Sheffield Hallam University Research Ethics Policy and Procedures − I agree to abide by its principles.	
Student / Researcher/ Principal Investigator (as applicable)	
Name: Megan Bean	Date: 16.12.2014
Signature:	
Supervisor or other person giving ethical sign-off	
I can confirm that completion of this form has not identified the need for ethical approval by the FREC or an NHS, Social Care or other external REC. The research will not commence until any approvals required under Sections 3 & 4 have been received.	
Name: Katie Cutts	Date: 16.12.2014
Signature:	
Other signing box	
Name:	Date:
Signature:	

Please ensure the following are included with this form if applicable, tick box to indicate:

	Yes	**No**	**N/A**
Research proposal if prepared previously	☐	☐	☒
Any recruitment materials (e.g. posters, letters, etc.)	☒	☐	☐
Participant information sheet	☒	☐	☐
Participant consent form	☒	☐	☐
Details of any measures to be used (e.g. questionnaires, etc.)	☒	☐	☐
Details of any support materials provided to participants	☐	☐	☒
Debriefing materials	☒	☐	☐

References

Austin, J.T., & Vancouver, J.B. (1996). Goal constructs in psychology: Structure, process and content. *Psychological Bulletin, 120*, 338-375.

Brown, J.M., Miller, W.R., & Lawendowski, L.A. (1999). The Self-Regulation Questionnaire. In L. VandeCreek & T.L. Jackson (Eds.), *Innovations in Clinical Practice: A source book.* (Vol. 17, pp. 281-289). Sarasota, FL: Professional Resource Press.

Hofmann, S.G., & Kashdan, T.B. (2010). The Affective Style Questionnaire: Development and psychometric properties. *Journal of Psychopathology and Behavioural Assessment, 32*(2), 255-263.

King, L.A. (2001). The health benefits of writing about life goals. *Personality and Social Psychology Bulletin, 27*(7), 798-807.

Lovibond, S.H., & Lovibond, P.F. (1995). *Manual for the Depression, Anxiety, Stress Scales*. Sydney: Psychology Foundation.

Markus, H., & Nurius, P. (1986). Possible Selves. *American Psychologist, 41,* 954-969.

Pham, L.B., & Taylor, S.E. (1999). From thought to action: Effects of process-versus outcome-based mental simulations on performance. *Personality and Social Psychology Bulletin, 25,* 250-260.

Spector, P.E., & Jex, S.M. (1998). Development of 4 self-report measures of job stressors and strain: Interpersonal Conflict at Work Scale, Organizational Constraints Scale, Quantitative Workload Inventory, and Physical Symptoms Inventory. *Journal of Occupational Health Psychology, 3*, 356-367.

Taylor, S.E., & Schneider, S.K. (1989). Coping and the simulation of events. *Social Cognition, 7*(2), 174-194.

Watson, D., Clark, L.A., & Tellegan, A. (1988). Development and validation of a brief measure of positive and negative affect- the PANAS scale. *Journal of Personality and Social Psychology, 54,* 1063-1070.

Application for Research Ethics Approval (SHUREC2A)

SECTION A

Important Note - If you have already written a research proposal (e.g. for a funder) that answers the methodology questions in this section please include a copy of the proposal and leave those questions blank. You **MUST** however complete **ALL** of Section B and C (risk assessment).

1. Name of principal investigator: Megan Bean

Faculty: Development and Society

Email address: a9022330@my.shu.ac.uk

2. Title of research: Effects of writing and mentally simulating about a best possible future on health: What are the 'active ingredients'?

3. Supervisor (if applicable)**:** Dr. Katie Cutts

Email address: k.cutts@shu.ac.uk

4. Proposal Tracking number (applicable for externally funded research):

5. Other investigators (within or outside SHU)

Title	Name	Post	Division	Organisation

6. Proposed duration of project

Start date: February 2015 **End Date:** June 2015

7. Location of research if outside SHU: N/A

8. Main purpose of research:

- ☒ Educational qualification
- ☐ Publicly funded research
- ☐ Staff research project
- ☐ Other (Please supply details)

9. Background to the study and scientific rationale (500 words approx.)

Pennebaker and Beall (1986) found that students who wrote about a personally traumatic experience had reduced symptoms of physical ill-health than peers who wrote about a non-emotive topic. These findings have been replicated a number of times (e.g. Francis & Pennebaker, 1992). Furthermore, psychological benefits have

been reported; including elevated mood (Páez, Velasco & González, 1999) and amelioration of psychopathological symptoms (e.g. Sloan, Marx & Epstein, 2005). Similarly, King (2001) found that students who wrote about a best possible future self (BPFS) or a trauma demonstrated improved health compared to peers who wrote about daily plans.

The prominent theoretical explanation for this effect is that writing facilitates self-regulation. To clarify, possible selves are personalised representations of goals (Markus & Nurius, 1986), and goals that individuals set for themselves reflect self-regulatory processes (Austin & Vancouver, 1996). Furthermore, King (2001) asserts that control participants did not show health benefits, and their task involves writing about lower-order goals. Outcome goals (e.g. BPFS, hold a higher order in the motivational hierarchy than immediate/ short-term goals such as plans for the day, hence are less likely to be regularly considered (King, 2001). Therefore, encouraging individuals to consider higher-order goals through writing about them may enable them to explore aspects of their motivational lives that are mostly unexamined (King, 2001).

Mental simulation literature supports suggestions of self-regulation as a mechanism through which writing about a BPFS elicits health benefits; however it is not entirely consistent with King's (2001) postulation. Pham and Taylor (1999) assigned students to mental simulation conditions some days prior to an examination. Some simulated the outcome of achieving a commendable grade, whilst others simulated the process required in order to achieve this outcome. In contrast to King's (2001) suggestion that considering higher-order goals may be more beneficial in terms of self-regulatory processes than lower-order goals, Pham and Taylor (1999) found that process simulation (lower-order goals) improved studying and was associated with augmented grades, and that the latter effect was mediated by diminished anxiety levels and improved planning abilities. In line with this, Taylor and Schneider (1989) postulate that simulations facilitate self-directed action, and Pham and Taylor (1999) theorise that simulation is beneficial as it facilitates generation of a clear image of a desirable future and enables the individual to construct a plan as to how to reach it.

Although Pham and Taylor's (1999) findings suggest process simulations are more beneficial for self-regulation than outcome simulations, the question of whether this is maintained in writing interventions remains unexplored. Hence, this study aims to compare effects of writing/ simulating BPFS with effects of writing/ simulating about the process which must be successfully followed to attain it. Furthermore, although frequently suggested (e.g. King, 2001; Taylor & Schneider, 1989), a mediating role of self-regulation in producing health benefits following simulation or writing about goals has not been investigated directly. This research aims to explore affective and behavioural self-regulation as outcomes of intervention tasks, as well as mediating and moderating effects of changes in self-regulation on changes in physical and psychological health.

References attached

10. Has the scientific / scholarly basis of this research been approved? (For example by Research Degrees Subcommittee or an external funding body)

☐ Yes
☒ No - to be submitted
☐ Currently undergoing an approval process
☐ Irrelevant (e.g. there is no relevant committee governing this work)

11. Main research questions

The following aims will be addressed in the current research:
1) Is there a difference between writing about and mental simulations of future goals in terms of effects on physical and psychological health?
2)Does writing/ simulating about higher-order (outcome) goals exert different effects on physical and psychological health in comparison to writing/ mentally-simulating about lower-order (process) goals?
3) Is there a change in behavioural and affective self-regulation abilities following mental simulation/ writing interventions, and if so, does this mediate any changes in physical and psychological health?

12. Summary of methods including proposed data analyses

The proposed analysis predominantly will constitute ANOVA. The procedure of the proposed initial study of my PhD will last for a duration of 2 months. In the first phase of the initial study, the participant will meet with the researcher and complete the following measures:

- Physical health: Physical Symptoms Inventory (PSI; Spector & Jex, 1998)
- Psychological health: Depression, Anxiety and Stress Scale- 21 (DASS-21; Lovibond & Lovibond, 1995)
- Positive and negative affect: Positive and Negative Affectivity Scale (PANAS; Watson, Clark & Tellegan, 1988)
- Behavioural self-regulation: Self-Regulation Questionnaire (SRQ; Brown, Miller & Lawendowski, 1999)
- Affective Self-regulation: Affective-Style Questionnaire (ASQ; Hofmann & Kashdan, 2010)

The participant will then be asked to think for 1 minute about what their BPFS in 10 years is, then will be required to engage in 20 minutes of mental simulation or writing about their BPFS (outcome) or the process of attaining it, or a neutral control topic. They will then repeat the PANAS. Finally, they will be asked to non-emotively list contents of their simulations/writing.
Follow-up studies will also occur at 1 week, 1 month and 2 months following the initial study, to explore for how long any effects are maintained before they dissipate, and to capture any effects which may have a latent onset.
Questionnaires will be scored based on published criteria.
References attached

SECTION B

1. Describe the arrangements for selecting/sampling and briefing potential participants. This should include copies of any advertisements for volunteers or letters to individuals/organisations inviting participation. The sample sizes with power calculations if appropriate should be included.

Power analyses conducted using G*Power for a 3*2 ANOVA suggests that, based on a medium effect size (f) of 0.2350 and a design including 6 between participant groups, a sample of 178 participants will be required in order to obtain a power of 0.8.

Students at Sheffield Hallam University will be recruited by the researcher placing an online advertisement (attached) for participants on the institution's internal research participation site. Furthermore, an information sheet will be given to participants prior to their agreeing to partke in the investigation. This will explain the participant's right to withdraw, and will outline what their participation would entail.

2. What is the potential for participants to benefit from participation in the research?

Writing and mental-simulations of outcome goals and goal processes have been found to be related to both physical and psychological health improvements, and to better performance in terms of goal attainment. Psycreds will be awarded to first year Undergraduate Psychology students.

3. Describe any possible negative consequences of participation in the research along with the ways in which these consequences will be limited.

N/A

4. Describe the arrangements for obtaining participants' consent. This should include copies of the information that they will receive & written consent forms where appropriate. If children or young people are to be participants in the study details of the arrangements for obtaining consent from parents or those acting in *loco parentis* or as advocates should be provided.

Upon arrival to a mutually-convenient meeting with the researcher on the first day of the study, the potential participant will be presented with an information sheet and a consent form. In order to be eligible to partake in the investigation, participants will be required to print their name and sign the consent form to confirm that they have read the information sheet in full, have been provided opportunity to ask questions (and have received satisfactory answers), have been given sufficient information about the study, and have been made aware of their right to withdraw from it or to refuse to answer questions or disclose information without there being any consequences. The researcher must obtain an individual's written informed consent of their voluntary participation in the study through the participant signing the consent form to allow the individual to partake in the investigation. Information sheet and consent form attached.

5. Describe how participants will be made aware of their right to withdraw from the research. This should also include information about participants' right to withhold information and a reasonable time span for withdrawal should be specified.

Potential participants will be provided with an information sheet. This will inform them of their right to withdraw from the study at any point during their participation, or to refuse

to provide answers to any questions that they do not feel at ease in answering. Participants will also be informed through the information sheet that they have the right to refuse to disclose any information or to withdraw their data up to 7 days following completion of the initial intervention phase of the study and each follow-up investigation, but that it would not be possible after this point due to anonymity making it impossible to identify their data.

6. If your project requires that you work with vulnerable participants describe how you will implement safeguarding procedures during data collection.

N/A

7. If Disclosure and Barring Service (DBS) checks are required, please supply details

N/A

8. Describe the arrangements for debriefing the participants. This should include copies of the information that participants will receive where appropriate.

Upon completion of the intervention stage and the first two follow-up stages, participants will be verbally briefed about what is expected of them in the next stage of the study. Upon completion of the final follow-up study, participants will be issued a debrief sheet. This will thank them for their time, and will provide them with further information about the investigation, including what the full aim of the research is. The debrief sheet will also assure participants that they have the right to e-mail the researcher with any questions that they may have about the project, or to request a full copy of the report once it has been completed. Furthermore, participants will be made aware of health and wellbeing services available at the University, and how they are able to access them if they have any concerns or queries. Debrief sheet attached.

9. Describe the arrangements for ensuring participant confidentiality. This should include details of:

- how data will be stored to ensure compliance with data protection legislation
- how results will be presented
- exceptional circumstances where confidentiality may not be preserved
- how and when confidential data will be disposed of

Throughout their involvement in the investigation, participants are to remain anonymous. Each individual will be allocated a unique participant identification code in order that their data from all measures and tasks can be matched. Participant identification codes will consist of the last three digits of an individual's mobile telephone number and the first three letters of a pet's or best friend's name. Consent forms are the only document which will require that participants provide their name and signature, and consent forms will be stored separately from all other data so that names cannot be matched with participant data. Participants' e-mail addresses will also be requested so that the researcher is able to send a reminder to participants to attend follow-up studies. However, the researcher will store e-mail addresses separately from all other data, to ensure that addresses are not in any way possible to match to data. Responses made by the participant will be seen only by the researcher and the

supervisors named on the consent form. Only in exceptional circumstances in which the participant discloses information which reveals that they or another individual are at risk of harm will participant confidentiality not be preserved. This will be ensured by storing data securely on an encrypted memory stick and password-protected computer only. Only averages of scores and ranges of scores will be included in the write-up of results from the investigation. No raw data will be presented whatsoever. After submission of the report write-up, either data will be stored for up to 5 years in the University research archive so that they are available if any work related to the study were to be published in scholary articles, or it will be destroyed as confidential waste.

10. Are there any conflicts of interest in you undertaking this research? (E.g. are you undertaking research on work colleagues or in an organisation where you are a consultant?) Please supply details of how this will be addressed.
No

11. What are the expected outcomes, impacts and benefits of the research?
It is expected that the current study will uncover mechanisms through which writing about a best possible future self improves physical and psychological health. Knowledge of what these mechanisms are will broaden theory and will provide a basis from which interventions can be developed further. This will enable production of more powerful writing interventions with the hope that they can be tested further in clinical populations.

12. Please give details of any plans for dissemination of the results of the research
Results will be dissemminated via:
. PhD thesis . Conferences
. Publications/ peer-reviewed scientific journals . Presentations

SECTION C

RISK ASSESSMENT FOR THE RESEARCHER

1. Will the proposed data collection take place on campus?

☒ Yes (Please answer questions 4, 6 and 7)
☐ No (Please complete all questions)

2. Where will the data collection take place?
(Tick as many as apply if data collection will take place in multiple venues)

	Location	**Please specify**
☐	Researcher's Residence	
☐	Participant's Residence	
☐	Education Establishment	
☐	Other e.g. business/voluntary organisation, public venue	
☐	Outside UK	

3. **How will you travel to and from the data collection venue?**

☐ On foot ☐ By car ☐ Public Transport
☐ Other (Please specify)

Please outline how you will ensure your personal safety when travelling to and from the data collection venue

4. **How will you ensure your own personal safety whilst at the research venue?**

I will ensure my safety by informing someone of when I am going to be data collecting and when I expect to return from the testing venue. I will also carry with me a charged mobile telephone, and provide my supervisor with my contact number.

5. **If you are carrying out research off-campus, you must ensure that each time you go out to collect data you ensure that someone you trust knows where you are going (without breaching the confidentiality of your participants), how you are getting there (preferably including your travel route), when you expect to get back, and what to do should you not return at the specified time.** (See Lone Working Guidelines). Please outline here the procedure you propose using to do this.

6. **Are there any potential risks to your health and wellbeing associated with either (a) the venue where the research will take place and/or (b) the research topic itself?**

☒ None that I am aware of
☐ Yes (Please outline below)

7. **Does this research project require a health and safety risk analysis for the procedures to be used?**

☐ Yes
☒ No

(If YES the completed Health and Safety Project Safety Plan for Procedures should be attached)

Adherence to SHU policy and procedures

Personal statement	
I confirm that: • this research will conform to the principles outlined in the Sheffield Hallam University Research Ethics policy • this application is accurate to the best of my knowledge	
Principal Investigator	
Signature	
Date	Megan Bean 16.12.2014
Supervisor (if applicable)	
Signature	
Date	Katie Cutts 16.12.2014
Other signature	
Signature	
Date	

Please ensure the following are included with this form if applicable, tick box to indicate:

	Yes	**No**	**N/A**
Research proposal if prepared previously	☐	☐	☒
Any recruitment materials (e.g. posters, letters, etc.)	☒	☐	☐
Participant information sheet	☒	☐	☐
Participant consent form	☒	☐	☐
Details of measures to be used (e.g. questionnaires, etc.)	☒	☐	☐
Outline interview schedule / focus group schedule	☐	☐	☒
Debriefing materials	☒	☐	☐
Health and Safety Project Safety Plan for Procedures	☐	☐	☒

References

Austin, J.T., & Vancouver, J.B. (1996). Goal constructs in psychology: Structure, process and content. *Psychological Bulletin, 120*, 338-375.

Brown, J.M., Miller, W.R., & Lawendowski, L.A. (1999). The Self-Regulation Questionnaire. In L. VandeCreek & T.L. Jackson (Eds.), *Innovations in Clinical Practice: A source book.* (Vol. 17, pp. 281-289). Sarasota, FL: Professional Resource Press.

Francis, M., & Pennebaker, J.W. (1992). Putting stress into words: The impact of writing on physiological, absentee, and self-reported emotional well-being measures. *American Journal of Health Promotion, 6*(4), 280-287.

Hofmann, S.G., & Kashdan, T.B. (2010). The Affective Style Questionnaire: Development and psychometric properties. *Journal of Psychopathology and Behavioural Assessment, 32*(2), 255-263.

King, L.A. (2001). The health benefits of writing about life goals. *Personality and Social Psychology Bulletin, 27*(7), 798-807.

Lovibond, S.H., & Lovibond, P.F. (1995). *Manual for the Depression, Anxiety, Stress Scales*. Sydney: Psychology Foundation.

Markus, H., & Nurius, P. (1986). Possible Selves. *American Psychologist, 41,* 954-969.

Páez, D., Velasco, C. & González, J.L. (1999). Expressive writing and the role of alexithymia as a dispositional deficit in self-disclosure and psychological health. *Journal of Personality and Social Psychology, 77,* 630-641.

Pennebaker, J.W., & Beall, S.K. (1986). Confronting a traumatic event: Toward an understanding of inhibition and disease. *Journal of Abnormal Psychology, 95*(3), 274-281.

Pham, L.B., & Taylor, S.E. (1999). From thought to action: Effects of process-versus outcome-based mental simulations on performance. *Personality and Social Psychology Bulletin, 25,* 250-260.

Sloan, D.M., Marx, B.P., & Epstein, E.M. (2005). Further examination of the exposure model underlying the efficacy of written emotional disclosure. *Journal of Consulting and Clinical Psychology, 73*(3), 549-554.

Spector, P.E., & Jex, S.M. (1998). Development of 4 self-report measures of job stressors and strain: Interpersonal Conflict at Work Scale, Organizational Constraints Scale, Quantitative Workload Inventory, and Physical Symptoms Inventory. *Journal of Occupational Health Psychology, 3*, 356-367.

Taylor, S.E., & Schneider, S.K. (1989). Coping and the simulation of events. *Social Cognition, 7*(2), 174-194.

Watson, D., Clark, L.A., & Tellegan, A. (1988). Development and validation of a brief measure of positive and negative affect- the PANAS scale. *Journal of Personality and Social Psychology, 54,* 1063-107

A.6.1.2 Ethics application for recruitment modifications

Application for Research Ethics Approval (SHUREC2A)

SECTION A
Important Note - If you have already written a research proposal (e.g. for a funder) that answers the methodology questions in this section please include a copy of the proposal and leave those questions blank. You **MUST** however complete **ALL** of Section B and C (risk assessment).

1. Name of principal investigator: Megan Bean
Faculty: Development and Society
Email address: a9022330@my.shu.ac.uk

2. Title of research: Effects of writing and mentally simulating about a best possible future on health: What are the 'active ingredients'?

3. Supervisor (if applicable)**:** Dr. Katie Cutts
Email address: k.cutts@shu.ac.uk

4. Proposal Tracking number (applicable for externally funded research):

5. Other investigators (within or outside SHU)

Title	Name	Post	Division	Organisation

6. Proposed duration of project
Start date: February 2015 **End Date:** June 2015

7. Location of research if outside SHU: N/A

8. Main purpose of research:

- ☒ Educational qualification
- ☐ Publicly funded research
- ☐ Staff research project
- ☐ Other (Please supply details)

9. Background to the study and scientific rationale (500 words approx.)
Pennebaker and Beall (1986) found that students who wrote about a personally traumatic experience had reduced sy,ptoms of physical ill-health than peers who wrote about a non-emotive topic. These findings have been replicated a number of times (e.g. Francis & Pennebaker, 1992). Furthermore, psychological benefits have been reported; including elevated mood (Páez, Velasco & González, 1999) and amelioration of psychopathological symptoms (e.g. Sloan, Marx & Epstein, 2005). Similarly, King

(2001) found that students who wrote about a best possible future self (BPFS) or a trauma demonstrated improved health compared to peers who wrote about daily plans. The prominent theoretical explanation for this effect is that writing facilitates self-regulation. To clarify, possible selves are personalised representations of goals (Markus & Nurius, 1986), and goals that individuals set for themselves reflect self-regulatory processes (Austin & Vancouver, 1996). Furthermore, King (2001) asserts that control participants did not show health benefits, and their task involves writing about lower-order goals. Outcome goals (e.g. BPFS, hold a higher order in the motivational hierarchy than immediate/ short-term goals such as plans for the day, hence are less likely to be regularly considered (King, 2001). Therefore, encouraging individuals to consider higher-order goals through writing about them may enable them to explore aspects of their motivational lives that are mostly unexamined (King, 2001).
Mental simulation literature supports suggestions of self-regulation as a mechanism through which writing about a BPFS elicits health benefits; however it is not entirely consistent with King's (2001) postulation. Pham and Taylor (1999) assigned students to mental simulation conditions some days prior to an examination. Some simulated the outcome of achieving a commendable grade, whilst others simulated the process required in order to achieve this outcome. In contrast to King's (2001) suggestion that considering higher-order goals may be more beneficial in terms of self-regulatory processes than lower-order goals, Pham and Taylor (1999) found that process simulation (lower-order goals) improved studying and was associated with augmented grades, and that the latter effect was mediated by diminished anxiety levels and improved planning abilities. In line with this, Taylor and Schneider (1989) postulate that simulations facilitate self-directed action, and Pham and Taylor (1999) theorise that simulation is beneficial as it facilitates generation of a clear image of a desirable future and enables the individual to construct a plan as to how to reach it.
Although Pham and Taylor's (1999) findings suggest process simulations are more beneficial for self-regulation than outcome simulations, the question of whether this is maintained in writing interventions remains unexplored. Hence, this study aims to compare effects of writing/ simulating BPFS with effects of writing/ simulating about the process which must be successfully followed to attain it.
Furthermore, although frequently suggested (e.g. King, 2001; Taylor & Schneider, 1989), a mediating role of self-regulation in producing health benefits following simulation or writing about goals has not been investigated directly. This research aims to explore affective and behavioural self-regulation as outcomes of intervention tasks, as well as mediating and moderating effects of changes in self-regulation on changes in physical and psychological health.
References attached

10. Has the scientific / scholarly basis of this research been approved? (For example by Research Degrees Subcommittee or an external funding body)

- ☐ Yes
- ☒ No - to be submitted
- ☐ Currently undergoing an approval process
- ☐ Irrelevant (e.g. there is no relevant committee governing this work)

11. Main research questions

The following aims will be addressed in the current research:

1) Is there a difference between writing about and mental simulations of future goals in terms of effects on physical and psychological health?

2)Does writing/ simulating about higher-order (outcome) goals exert different effects on physical and psychological health in comparison to writing/ mentally-simulating about lower-order (process) goals?

3) Is there a change in behavioural and affective self-regulation abilities following mental simulation/ writing interventions, and if so, does this mediate any changes in physical and psychological health?

12. Summary of methods including proposed data analyses

The proposed analysis predominantly will constitute ANOVA. The procedure of the proposed initial study of my PhD will last for a duration of 2 months. In the first phase of the initial study, the participant will meet with the researcher and complete the following measures:

- Physical health: Physical Symptoms Inventory (PSI; Spector & Jex, 1998)
- Psychological health: Depression, Anxiety and Stress Scale- 21 (DASS-21; Lovibond & Lovibond, 1995)
- Positive and negative affect: Positive and Negative Affectivity Scale (PANAS; Watson, Clark & Tellegan, 1988)
- Behavioural self-regulation: Self-Regulation Questionnaire (SRQ; Brown, Miller & Lawendowski, 1999)
- Affective Self-regulation: Affective-Style Questionnaire (ASQ; Hofmann & Kashdan, 2010)

The participant will then be asked to think for 1 minute about what their BPFS in 10 years is, then will be required to engage in 20 minutes of mental simulation or writing about their BPFS (outcome) or the process of attaining it, or a neutral control topic. They will then repeat the PANAS. Finally, they will be asked to non-emotively list contents of their simulations/writing.

Follow-up studies will also occur at 1 week, 1 month and 2 months following the initial study, to explore for how long any effects are maintained before they dissipate, and to capture any effects which may have a latent onset.

Questionnaires will be scored based on published criteria.

References attached

SECTION B

1. Describe the arrangements for selecting/sampling and briefing potential participants. This should include copies of any advertisements for volunteers or letters to individuals/organisations inviting participation. The sample sizes with power calculations if appropriate should be included.

Power analyses conducted using G*Power for a 3*2 ANOVA suggests that, based on a medium effect size (f) of 0.2350 and a design including 6 between participant groups, a sample of 178 participants will be required in order to obtain a power of 0.8.

Participants will be recruited through opportunity sampling, by the researcher placing an online advertisement (attached) for participants on the University's internal research participation site, as well as physically passing a printed copy of the advertisement to

individuals. Furthermore, an information sheet will be given to participants prior to their agreeing to partke in the investigation. This will explain the participant's right to withdraw, and will outline what their participation would entail.

2. What is the potential for participants to benefit from participation in the research?

Writing and mental-simulations of outcome goals and goal processes have been found to be related to both physical and psychological health improvements, and to better performance in terms of goal attainment. Psycreds will be awarded to first year Undergraduate Psychology students. All other participants will be issued a £5 voucher upon completion of their participation.

3. Describe any possible negative consequences of participation in the research along with the ways in which these consequences will be limited.

N/A

4. Describe the arrangements for obtaining participants' consent. This should include copies of the information that they will receive & written consent forms where appropriate. If children or young people are to be participants in the study details of the arrangements for obtaining consent from parents or those acting in *loco parentis* or as advocates should be provided.

Upon arrival to a mutually-convenient meeting with the researcher on the first day of the study, the potential participant will be presented with an information sheet and a consent form. In order to be eligible to partake in the investigation, participants will be required to print their name and sign the consent form to confirm that they have read the information sheet in full, have been provided opportunity to ask questions (and have received satisfactory answers), have been given sufficient information about the study, and have been made aware of their right to withdraw from it or to refuse to answer questions or disclose information without there being any consequences. The researcher must obtain an individual's written informed consent of their voluntary participation in the study through the participant signing the consent form to allow the individual to partake in the investigation. Information sheet and consent form attached.

5. Describe how participants will be made aware of their right to withdraw from the research. This should also include information about participants' right to withhold information and a reasonable time span for withdrawal should be specified.

Potential participants will be provided with an information sheet. This will inform them of their right to withdraw from the study at any point during their participation, or to refuse to provide answers to any questions that they do not feel at ease in answering. Participants will also be informed through the information sheet that they have the right to refuse to disclose any information or to withdraw their data up to 7 days following completion of the initial intervention phase of the study and each follow-up investigation, but that it would not be possible after this point due to anonymity making it impossible to identify their data.

6. If your project requires that you work with vulnerable participants describe how you will implement safeguarding procedures during data collection. N/A

7. If Disclosure and Barring Service (DBS) checks are required, please supply details N/A

8. Describe the arrangements for debriefing the participants. This should include copies of the information that participants will receive where appropriate. Upon completion of the intervention stage and the first two follow-up stages, participants will be verbally briefed about what is expected of them in the next stage of the study. Upon completion of the final follow-up study, participants will be issued a debrief sheet. This will thank them for their time, and will provide them with further information about the investigation, including what the full aim of the research is. The debrief sheet will also assure participants that they have the right to e-mail the researcher with any questions that they may have about the project, or to request a full copy of the report once it has been completed. Furthermore, participants will be made aware of health and wellbeing services available at the University, and how they are able to access them if they have any concerns or queries. Debrief sheet attached.

9. Describe the arrangements for ensuring participant confidentiality. This should include details of:

- how data will be stored to ensure compliance with data protection legislation
- how results will be presented
- exceptional circumstances where confidentiality may not be preserved
- how and when confidential data will be disposed of

Throughout their involvement in the investigation, participants are to remain anonymous. Each individual will be allocated a unique participant identification code in order that their data from all measures and tasks can be matched. Participant identification codes will consist of the last three digits of an individual's mobile telephone number and the first three letters of a pet's or best friend's name. Consent forms are the only document which will require that participants provide their name and signature, and consent forms will be stored separately from all other data so that names cannot be matched with participant data. Participants' e-mail addresses will also be requested so that the researcher is able to send a reminder to participants to attend follow-up studies. However, the researcher will store e-mail addresses separately from all other data, to ensure that addresses are not in any way possible to match to data. Responses made by the participant will be seen only by the researcher and the supervisors named on the consent form. Only in exceptional circumstances in which the participant discloses information which reveals that they or another individual are at risk of harm will participant confidentiality not be preserved. This will be ensured by storing data securely on an encrypted memory stick and password-protected computer only. Only averages of scores and ranges of scores will be included in the write-up of results from the investigation. No raw data will be presented whatsoever. After submission of the report write-up, either data will be stored for up to 5 years in the University research archive so that they are available if any work related to the study were to be published in scholary articles, or it will be destroyed as confidential waste.

10. Are there any conflicts of interest in you undertaking this research? (E.g. are you undertaking research on work colleagues or in an organisation where you are a consultant?) Please supply details of how this will be addressed. No

11. What are the expected outcomes, impacts and benefits of the research?
It is expected that the current study will uncover mechanisms through which writing about a best possible future self improves physical and psychological health. Knowledge of what these mechanisms are will broaden theory and will provide a basis from which interventions can be developed further. This will enable production of more powerful writing interventions with the hope that they can be tested further in clinical populations.

12. Please give details of any plans for dissemination of the results of the research
Results will be dissemminated via:
. PhD thesis
. Conferences
. Publications/ peer-reviewed scientific journals
. Presentations

SECTION C

RISK ASSESSMENT FOR THE RESEARCHER

7. Will the proposed data collection take place on campus?

☒ Yes (Please answer questions 4, 6 and 7)
☐ No (Please complete all questions)

8. Where will the data collection take place?
(Tick as many as apply if data collection will take place in multiple venues)

	Location	**Please specify**
☐	Researcher's Residence	
☐	Participant's Residence	
☐	Education Establishment	
☐	Other e.g. business/voluntary organisation, public venue	
☐	Outside UK	

9. How will you travel to and from the data collection venue?

☐ On foot ☐ By car ☒ Public Transport
☐ Other (Please specify)

Please outline how you will ensure your personal safety when travelling to and from the data collection venue

10. **How will you ensure your own personal safety whilst at the research venue?**

I will ensure my safety by informing someone of when I am going to be data collecting and when I expect to return from the testing venue. I will also carry with me a charged mobile telephone, and provide my supervisor with my contact number.

11. **If you are carrying out research off-campus, you must ensure that each time you go out to collect data you ensure that someone you trust knows where you are going (without breaching the confidentiality of your participants), how you are getting there (preferably including your travel route), when you expect to get back, and what to do should you not return at the specified time.** (See Lone Working Guidelines). Please outline here the procedure you propose using to do this.

    Although all data collection will take place on campus, I may leave campus in order to hand out advertisements and recruit participants. When doing this, I will inform my colleagues of the area that I am travelling to and send a text message to a colleague when I am leaving campus as well as when I arrive at my destination. I will also message when leaving my destination. I will provide them with the time that I expect to be back, and will message if I am going to be late. I will use public transport to travel and will conduct recruitment off-campus only during daylight hours. I will ask my colleague to try to call me if they have not heard from me by the time I expected to leave my destination, and ask them to alert authorities if I do not respond to them. I will carry with me a fully charged and credit-loaded mobile phone.

12. **Are there any potential risks to your health and wellbeing associated with either (a) the venue where the research will take place and/or (b) the research topic itself?**

    ☒ None that I am aware of
    ☐ Yes (Please outline below)

7. **Does this research project require a health and safety risk analysis for the procedures to be used?**

☐ Yes
☒ No

(If YES the completed Health and Safety Project Safety Plan for Procedures should be attached)

Adherence to SHU policy and procedures

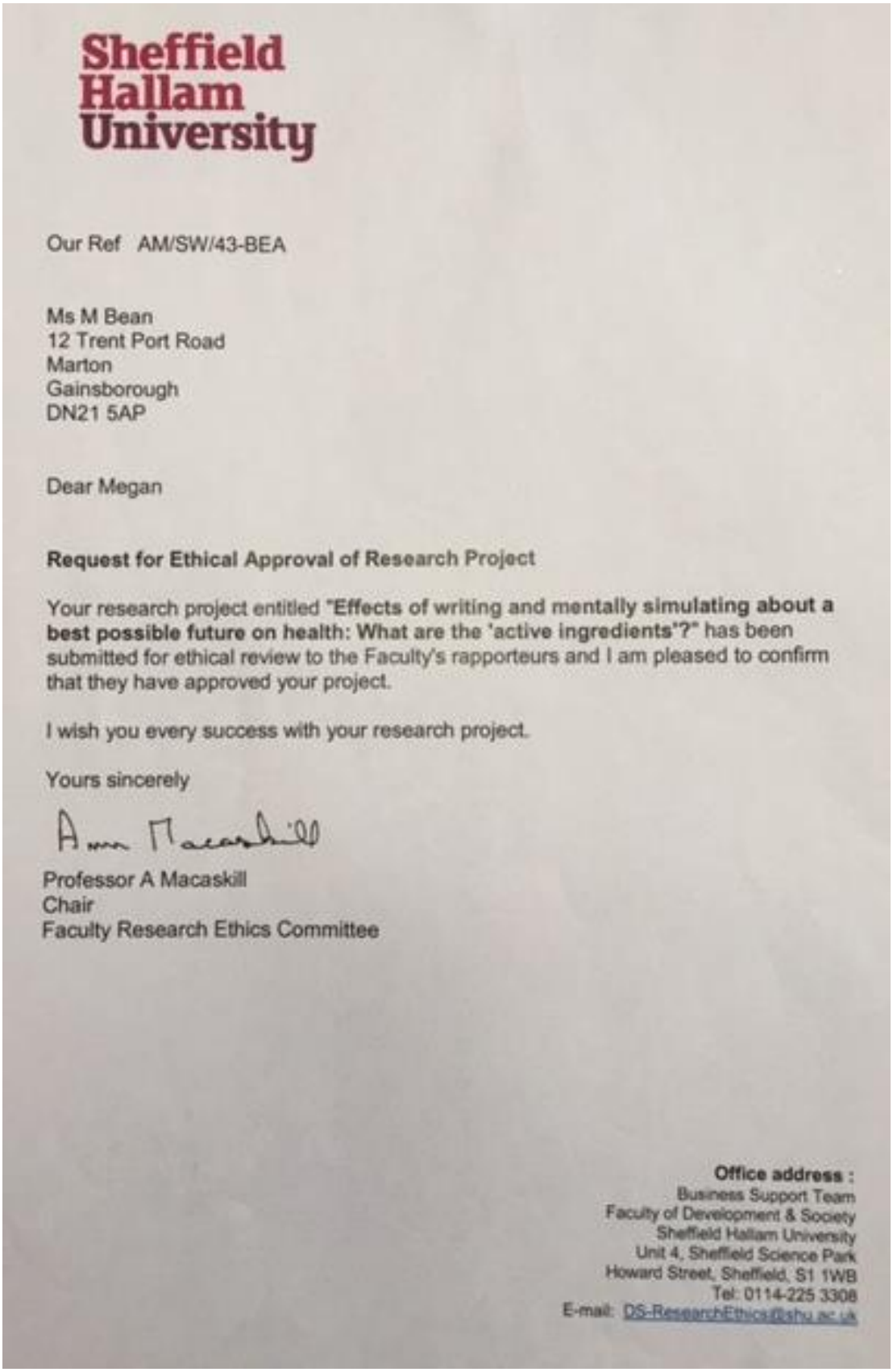
Personal statement	
I confirm that: • this research will conform to the principles outlined in the Sheffield Hallam University Research Ethics policy • this application is accurate to the best of my knowledge	
Principal Investigator	
Signature	
Date	Megan Bean 16.12.2014
Supervisor (if applicable)	
Signature	
Date	Katie Cutts 16.12.2014
Other signature	
Signature	
Date	

Please ensure the following are included with this form if applicable, tick box to indicate:

	Yes	**No**	**N/A**
Research proposal if prepared previously	☐	☐	☒
Any recruitment materials (e.g. posters, letters, etc.)	☒	☐	☐
Participant information sheet	☒	☐	☐
Participant consent form	☒	☐	☐
Details of measures to be used (e.g. questionnaires, etc.)	☒	☐	☐
Outline interview schedule / focus group schedule	☐	☐	☒
Debriefing materials	☒	☐	☐
Health and Safety Project Safety Plan for Procedures	☐	☐	☒

A.6.2 Approval letters

A.6.2.1 Original approval letter for the study



Sheffield Hallam University

Our Ref AM/SW/43-BEA

Ms M Bean
12 Trent Port Road
Marton
Gainsborough
DN21 5AP

Dear Megan

Request for Ethical Approval of Research Project

Your research project entitled **"Effects of writing and mentally simulating about a best possible future on health: What are the 'active ingredients'?"** has been submitted for ethical review to the Faculty's rapporteurs and I am pleased to confirm that they have approved your project.

I wish you every success with your research project.

Yours sincerely

Professor A Macaskill
Chair
Faculty Research Ethics Committee

Office address :
Business Support Team
Faculty of Development & Society
Sheffield Hallam University
Unit 4, Sheffield Science Park
Howard Street, Sheffield, S1 1WB
Tel: 0114-225 3308
E-mail: DS-ResearchEthics@shu.ac.uk

A.6.2.2 Approval of recruitment modifications

Sheffield Hallam University

Our Ref AM/SW/43-BEA(a)
Ms M Bean
12 Trent Port Road
Marton
Gainsborough
DN21 5AP

10th July 2015

Dear Megan

Request for Modification to Ethical Approval of Research Project

Your research project entitled "**Effects of writing and mentally simulating about a best possible future on health: What are the 'active ingredients'?**" has been submitted for the following minor modification:

- use vouchers as incentives (£5)
- recruit participants from outside of Sheffield Hallam University's student population

I am pleased to confirm that this modification to your application has been approved.

I wish you every success with your research project.

Yours sincerely

Professor A Macaskill Chair Faculty Research Ethics Committee

Office address :
Business Support Team
Faculty of Development & Society
Sheffield Hallam University
Unit 4, Sheffield Science Park
Howard Street, Sheffield, S1 1WB
Tel: 0114-225 3308 E-mail: DS-ResearchEthics@shu.ac.uk

A.7 Study One SPSS outputs from main analyses

A.7.1 Immediate effects

A.7.1.2 Positive affect

ANOVA

Between-Subjects Factors

		Value Label	N
modality	1.00	Writing	57
	2.00	Simulation	61
Task	1.00	Outcome	41
	2.00	Process	39
	3.00	Control	38

Tests of Between-Subjects Effects

Dependent Variable: PANAS_positive_post

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	3848.258[a]	6	641.376	16.968	.000	.478
Intercept	502.715	1	502.715	13.300	.000	.107
PANAS_positive_pre	2712.406	1	2712.406	71.758	.000	.393
modality	59.769	1	59.769	1.581	.211	.014
Task	541.798	2	270.899	7.167	.001	.114
modality * task	74.462	2	37.231	.985	.377	.017
Error	4195.717	111	37.799			
Total	119742.132	118				
Corrected Total	8043.974	117				

a. R Squared = .478 (Adjusted R Squared = .450)

Post-hoc analyses of significant main effect of task: Pairwise comparisons

1. Grand Mean

Dependent Variable: PANAS_positive_post

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
30.728[a]	.567	29.605	31.851

a. Covariates appearing in the model are evaluated at the following values: PANAS_positive_pre = 29.3312.

Estimates

Dependent Variable: PANAS_positive_post

Task	Mean	Std. Error	95% Confidence Interval	
			Lower Bound	Upper Bound
outcome	33.210[a]	.963	31.302	35.119
process	31.028[a]	.985	29.076	32.979
control	27.945[a]	1.002	25.959	29.931

a. Covariates appearing in the model are evaluated at the following values: PANAS_positive_pre = 29.3312.

Pairwise Comparisons

Dependent Variable: PANAS_positive_post

(I) task	(J) task	Mean Difference (I-J)	Std. Error	Sig.[b]	95% Confidence Interval for Difference[b]	
					Lower Bound	Upper Bound
outcome	process	2.182	1.377	.116	-.546	4.911
	control	5.265*	1.395	.000	2.502	8.029
process	outcome	-2.182	1.377	.116	-4.911	.546
	control	3.083*	1.406	.030	.296	5.869
control	outcome	-5.265*	1.395	.000	-8.029	-2.502
	process	-3.083*	1.406	.030	-5.869	-.296

Based on estimated marginal means

*. The mean difference is significant at the .05 level.

b. Adjustment for multiple comparisons: Least Significant Difference (equivalent to no adjustments).

Univariate Tests

Dependent Variable: PANAS_positive_post

	Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Contrast	541.798	2	270.899	7.167	.001	.114
Error	4195.717	111	37.799			

The F tests the effect of task. This test is based on the linearly independent pairwise comparisons among the estimated marginal means.

A.7.1.3 Negative affect

ANCOVA

Between-Subjects Factors

		Value Label	N
modality	1.00	writing	57
	2.00	simulation	61
task	1.00	outcome	41
	2.00	process	39
	3.00	control	38

Tests of Between-Subjects Effects

Dependent Variable: PANAS_negative_post_Rec

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	.009[a]	6	.002	6.886	.000	.271
Intercept	.007	1	.007	29.118	.000	.208
PANAS_negative_pre_Rec	.008	1	.008	34.453	.000	.237
modality	1.841E-7	1	1.841E-7	.001	.977	.000
task	.001	2	.000	1.473	.234	.026
modality * task	.001	2	.000	1.649	.197	.029
Error	.025	111	.000			
Total	.904	118				
Corrected Total	.035	117				

a. R Squared = .271 (Adjusted R Squared = .232)

A.7.2 Long-term effects

A.7.2.1 Depression

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	Depression_1_ week_tot_sqrt
2	Depression_4_ week_tot_sqrt
3	Depression_8_ week_tot_sqrt

Between-Subjects Factors

		Value Label	N
modality	1.00	Writing	36
	2.00	simulation	45
task	1.00	Outcome	28
	2.00	Process	29
	3.00	Control	24

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.037	1.421[b]	2.000	73.000	.248	.037
	Wilks' Lambda	.963	1.421[b]	2.000	73.000	.248	.037
	Hotelling's Trace	.039	1.421[b]	2.000	73.000	.248	.037
	Roy's Largest Root	.039	1.421[b]	2.000	73.000	.248	.037
time_point * Depression_baseline _mult2_sqrt	Pillai's Trace	.023	.873[b]	2.000	73.000	.422	.023
	Wilks' Lambda	.977	.873[b]	2.000	73.000	.422	.023
	Hotelling's Trace	.024	.873[b]	2.000	73.000	.422	.023
	Roy's Largest Root	.024	.873[b]	2.000	73.000	.422	.023
time_point * modality	Pillai's Trace	.028	1.036[b]	2.000	73.000	.360	.028
	Wilks' Lambda	.972	1.036[b]	2.000	73.000	.360	.028
	Hotelling's Trace	.028	1.036[b]	2.000	73.000	.360	.028
	Roy's Largest Root	.028	1.036[b]	2.000	73.000	.360	.028
time_point * task	Pillai's Trace	.020	.365	4.000	148.000	.834	.010
	Wilks' Lambda	.981	.360[b]	4.000	146.000	.836	.010
	Hotelling's Trace	.020	.356	4.000	144.000	.839	.010
	Roy's Largest Root	.017	.612[c]	2.000	74.000	.545	.016
time_point * modality * task	Pillai's Trace	.013	.238	4.000	148.000	.916	.006
	Wilks' Lambda	.987	.235[b]	4.000	146.000	.918	.006
	Hotelling's Trace	.013	.232	4.000	144.000	.920	.006
	Roy's Largest Root	.007	.242[c]	2.000	74.000	.786	.006

a. Design: Intercept + Depression_baseline_mult2_sqrt + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic c. The statistic is an upper bound on F that yields a lower bound on the significance level.

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	Df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
time_point	.955	3.338	2	.188	.957	1.000	.500

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + Depression_baseline_mult2_sqrt + modality + task + modality * task
Within Subjects Design: time_point

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	2.242	2	1.121	1.450	.238	.019
	Greenhouse-Geisser	2.242	1.914	1.171	1.450	.238	.019
	Huynh-Feldt	2.242	2.000	1.121	1.450	.238	.019
	Lower-bound	2.242	1.000	2.242	1.450	.232	.019
time_point * Depression_baseline_mult2_sqrt	Sphericity Assumed	1.418	2	.709	.917	.402	.012
	Greenhouse-Geisser	1.418	1.914	.741	.917	.398	.012
	Huynh-Feldt	1.418	2.000	.709	.917	.402	.012
	Lower-bound	1.418	1.000	1.418	.917	.341	.012
time_point * modality	Sphericity Assumed	1.470	2	.735	.951	.389	.013
	Greenhouse-Geisser	1.470	1.914	.768	.951	.386	.013
	Huynh-Feldt	1.470	2.000	.735	.951	.389	.013
	Lower-bound	1.470	1.000	1.470	.951	.333	.013
time_point * task	Sphericity Assumed	1.286	4	.322	.416	.797	.011
	Greenhouse-Geisser	1.286	3.829	.336	.416	.789	.011
	Huynh-Feldt	1.286	4.000	.322	.416	.797	.011
	Lower-bound	1.286	2.000	.643	.416	.661	.011
time_point * modality * task	Sphericity Assumed	.737	4	.184	.238	.916	.006
	Greenhouse-Geisser	.737	3.829	.193	.238	.910	.006
	Huynh-Feldt	.737	4.000	.184	.238	.916	.006
	Lower-bound	.737	2.000	.369	.238	.788	.006
Error(time_point)	Sphericity Assumed	114.401	148	.773			
	Greenhouse-Geisser	114.401	141.668	.808			
	Huynh-Feldt	114.401	148.000	.773			
	Lower-bound	114.401	74.000	1.546			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	1.448	1	1.448	1.546	.218	.020
	Quadratic	.795	1	.795	1.303	.257	.017
time_point * Depression_baseline_mult2_sqrt	Linear	1.005	1	1.005	1.073	.304	.014
	Quadratic	.413	1	.413	.678	.413	.009
time_point * modality	Linear	.510	1	.510	.544	.463	.007
	Quadratic	.960	1	.960	1.575	.213	.021
time_point * task	Linear	1.132	2	.566	.604	.549	.016
	Quadratic	.154	2	.077	.127	.881	.003
time_point * modality * task	Linear	.449	2	.225	.240	.787	.006
	Quadratic	.288	2	.144	.236	.790	.006
Error(time_point)	Linear	69.279	74	.936			
	Quadratic	45.122	74	.610			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	52.006	1	52.006	24.064	.000	.245
Depression_baseline_mult2_sqrt	170.322	1	170.322	78.808	.000	.516
modality	2.326	1	2.326	1.076	.303	.014
task	8.888	2	4.444	2.056	.135	.053
modality * task	.123	2	.061	.028	.972	.001
Error	159.930	74	2.161			

A.7.2.2 Anxiety

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	Anxiety_1_week_tot_sqrt
2	Anxiety_4_week_tot_sqrt
3	Anxiety_8_week_tot_sqrt

Between-Subjects Factors

		Value Label	N
modality	1.00	writing	36
	2.00	simulation	45
Task	1.00	outcome	28
	2.00	process	29
	3.00	control	24

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.013	.484[b]	2.000	73.000	.618	.013
	Wilks' Lambda	.987	.484[b]	2.000	73.000	.618	.013
	Hotelling's Trace	.013	.484[b]	2.000	73.000	.618	.013
	Roy's Largest Root	.013	.484[b]	2.000	73.000	.618	.013
time_point * Anxiety_baseline_mult2_sqrt	Pillai's Trace	.005	.166[b]	2.000	73.000	.847	.005
	Wilks' Lambda	.995	.166[b]	2.000	73.000	.847	.005
	Hotelling's Trace	.005	.166[b]	2.000	73.000	.847	.005
	Roy's Largest Root	.005	.166[b]	2.000	73.000	.847	.005
time_point * modality	Pillai's Trace	.004	.152[b]	2.000	73.000	.859	.004
	Wilks' Lambda	.996	.152[b]	2.000	73.000	.859	.004
	Hotelling's Trace	.004	.152[b]	2.000	73.000	.859	.004
	Roy's Largest Root	.004	.152[b]	2.000	73.000	.859	.004
time_point * task	Pillai's Trace	.027	.508	4.000	148.000	.730	.014
	Wilks' Lambda	.973	.504[b]	4.000	146.000	.733	.014
	Hotelling's Trace	.028	.501	4.000	144.000	.735	.014
	Roy's Largest Root	.028	1.019[c]	2.000	74.000	.366	.027
time_point * modality * task	Pillai's Trace	.074	1.429	4.000	148.000	.227	.037
	Wilks' Lambda	.926	1.438[b]	4.000	146.000	.224	.038
	Hotelling's Trace	.080	1.447	4.000	144.000	.222	.039
	Roy's Largest Root	.080	2.974[c]	2.000	74.000	.057	.074

a. Design: Intercept + Anxiety_baseline_mult2_sqrt + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic

c. The statistic is an upper bound on F that yields a lower bound on the significance level.

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	Df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
time_point	.928	5.425	2	.066	.933	1.000	.500

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + Anxiety_baseline_mult2_sqrt + modality + task + modality * task

Within Subjects Design: time_point

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	.771	2	.386	.445	.642	.006
	Greenhouse-Geisser	.771	1.866	.413	.445	.628	.006
	Huynh-Feldt	.771	2.000	.386	.445	.642	.006
	Lower-bound	.771	1.000	.771	.445	.507	.006
time_point * Anxiety_baseline_mult2_sqrt	Sphericity Assumed	.316	2	.158	.182	.834	.002
	Greenhouse-Geisser	.316	1.866	.169	.182	.819	.002
	Huynh-Feldt	.316	2.000	.158	.182	.834	.002
	Lower-bound	.316	1.000	.316	.182	.671	.002
time_point * modality	Sphericity Assumed	.199	2	.100	.115	.892	.002
	Greenhouse-Geisser	.199	1.866	.107	.115	.879	.002
	Huynh-Feldt	.199	2.000	.100	.115	.892	.002
	Lower-bound	.199	1.000	.199	.115	.736	.002
time_point * task	Sphericity Assumed	1.409	4	.352	.407	.804	.011
	Greenhouse-Geisser	1.409	3.733	.377	.407	.791	.011
	Huynh-Feldt	1.409	4.000	.352	.407	.804	.011
	Lower-bound	1.409	2.000	.704	.407	.667	.011
time_point * modality * task	Sphericity Assumed	5.465	4	1.366	1.577	.183	.041
	Greenhouse-Geisser	5.465	3.733	1.464	1.577	.188	.041
	Huynh-Feldt	5.465	4.000	1.366	1.577	.183	.041
	Lower-bound	5.465	2.000	2.732	1.577	.213	.041
Error(time_point)	Sphericity Assumed	128.230	148	.866			
	Greenhouse-Geisser	128.230	138.108	.928			
	Huynh-Feldt	128.230	148.000	.866			
	Lower-bound	128.230	74.000	1.733			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.239	1	.239	.220	.641	.003
	Quadratic	.532	1	.532	.825	.367	.011
time_point * Anxiety_baseline_mult2_sqrt	Linear	.199	1	.199	.183	.670	.002
	Quadratic	.117	1	.117	.182	.671	.002
time_point * modality	Linear	.023	1	.023	.021	.886	.000
	Quadratic	.176	1	.176	.273	.603	.004
time_point * task	Linear	.115	2	.057	.053	.949	.001
	Quadratic	1.294	2	.647	1.003	.372	.026
time_point * modality * task	Linear	3.234	2	1.617	1.486	.233	.039
	Quadratic	2.231	2	1.116	1.730	.184	.045
Error(time_point)	Linear	80.509	74	1.088			
	Quadratic	47.721	74	.645			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	1.246	1	1.246	.521	.473	.007
Anxiety_baseline_mult2_sqrt	147.884	1	147.884	61.800	.000	.455
Modality	1.577	1	1.577	.659	.419	.009
Task	12.694	2	6.347	2.652	.077	.067
modality * task	3.581	2	1.790	.748	.477	.020
Error	177.079	74	2.393			

A.7.2.3 Stress

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	Stress_1_week_tot_sqrt
2	Stress_4_week_tot_sqrt
3	Stress_8_week_tot_sqrt

Between-Subjects Factors

		Value Label	N
modality	1.00	writing	36
	2.00	simulation	45
task	1.00	outcome	28
	2.00	process	29
	3.00	control	24

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.021	.771[b]	2.000	73.000	.466	.021
	Wilks' Lambda	.979	.771[b]	2.000	73.000	.466	.021
	Hotelling's Trace	.021	.771[b]	2.000	73.000	.466	.021
	Roy's Largest Root	.021	.771[b]	2.000	73.000	.466	.021
time_point * Stress_baseline_mult2_sqrt	Pillai's Trace	.002	.065[b]	2.000	73.000	.937	.002
	Wilks' Lambda	.998	.065[b]	2.000	73.000	.937	.002
	Hotelling's Trace	.002	.065[b]	2.000	73.000	.937	.002
	Roy's Largest Root	.002	.065[b]	2.000	73.000	.937	.002
time_point * modality	Pillai's Trace	.066	2.596[b]	2.000	73.000	.081	.066
	Wilks' Lambda	.934	2.596[b]	2.000	73.000	.081	.066
	Hotelling's Trace	.071	2.596[b]	2.000	73.000	.081	.066
	Roy's Largest Root	.071	2.596[b]	2.000	73.000	.081	.066
time_point * task	Pillai's Trace	.036	.681	4.000	148.000	.606	.018
	Wilks' Lambda	.964	.677[b]	4.000	146.000	.609	.018
	Hotelling's Trace	.037	.673	4.000	144.000	.612	.018
	Roy's Largest Root	.036	1.328[c]	2.000	74.000	.271	.035
time_point * modality * task	Pillai's Trace	.041	.773	4.000	148.000	.544	.020
	Wilks' Lambda	.959	.766[b]	4.000	146.000	.549	.021
	Hotelling's Trace	.042	.759	4.000	144.000	.553	.021
	Roy's Largest Root	.035	1.299[c]	2.000	74.000	.279	.034

a. Design: Intercept + Stress_baseline_mult2_sqrt + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic

c. The statistic is an upper bound on F that yields a lower bound on the significance level.

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
time_point	.955	3.355	2	.187	.957	1.000	.500

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + Stress_baseline_mult2_sqrt + modality + task + modality * task

Within Subjects Design: time_point

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	1.036	2	.518	.824	.441	.011
	Greenhouse-Geisser	1.036	1.914	.541	.824	.436	.011
	Huynh-Feldt	1.036	2.000	.518	.824	.441	.011
	Lower-bound	1.036	1.000	1.036	.824	.367	.011
time_point * Stress_baseline_mult2_sqrt	Sphericity Assumed	.097	2	.048	.077	.926	.001
	Greenhouse-Geisser	.097	1.914	.051	.077	.919	.001
	Huynh-Feldt	.097	2.000	.048	.077	.926	.001
	Lower-bound	.097	1.000	.097	.077	.782	.001
time_point * modality	Sphericity Assumed	2.699	2	1.349	2.147	.120	.028
	Greenhouse-Geisser	2.699	1.914	1.410	2.147	.123	.028
	Huynh-Feldt	2.699	2.000	1.349	2.147	.120	.028
	Lower-bound	2.699	1.000	2.699	2.147	.147	.028
time_point * task	Sphericity Assumed	1.560	4	.390	.621	.649	.016
	Greenhouse-Geisser	1.560	3.828	.407	.621	.642	.016
	Huynh-Feldt	1.560	4.000	.390	.621	.649	.016
	Lower-bound	1.560	2.000	.780	.621	.540	.016
time_point * modality * task	Sphericity Assumed	1.751	4	.438	.697	.595	.018
	Greenhouse-Geisser	1.751	3.828	.458	.697	.589	.018
	Huynh-Feldt	1.751	4.000	.438	.697	.595	.018
	Lower-bound	1.751	2.000	.876	.697	.501	.018
Error(time_point)	Sphericity Assumed	92.991	148	.628			
	Greenhouse-Geisser	92.991	141.638	.657			
	Huynh-Feldt	92.991	148.000	.628			
	Lower-bound	92.991	74.000	1.257			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.384	1	.384	.527	.470	.007
	Quadratic	.652	1	.652	1.234	.270	.016
time_point * Stress_baseline_mult2_sqrt	Linear	.096	1	.096	.132	.717	.002
	Quadratic	.001	1	.001	.001	.974	.000
time_point * modality	Linear	1.072	1	1.072	1.472	.229	.020
	Quadratic	1.627	1	1.627	3.077	.084	.040
time_point * task	Linear	1.043	2	.521	.716	.492	.019
	Quadratic	.517	2	.258	.489	.615	.013
time_point * modality * task	Linear	.936	2	.468	.643	.529	.017
	Quadratic	.816	2	.408	.772	.466	.020
Error(time_point)	Linear	53.866	74	.728			
	Quadratic	39.125	74	.529			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	16.787	1	16.787	8.614	.004	.104
Stress_baseline_mult2_sqrt	177.747	1	177.747	91.211	.000	.552
Modality	4.793	1	4.793	2.460	.121	.032
Task	4.404	2	2.202	1.130	.329	.030
modality * task	1.052	2	.526	.270	.764	.007
Error	144.208	74	1.949			

A.7.2.4 Physical symptoms

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	PSI_4_weeks_log10
2	PSI_8_week_log10

Between-Subjects Factors

		Value Label	N
modality	1.00	Writing	36
	2.00	Simulation	47
task	1.00	Outcome	29
	2.00	Process	29
	3.00	Control	25

Multivariate tests

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.015	1.146[b]	1.000	76.000	.288	.015
	Wilks' Lambda	.985	1.146[b]	1.000	76.000	.288	.015
	Hotelling's Trace	.015	1.146[b]	1.000	76.000	.288	.015
	Roy's Largest Root	.015	1.146[b]	1.000	76.000	.288	.015
time_point * PSi_baseline_sqrt	Pillai's Trace	.013	.985[b]	1.000	76.000	.324	.013
	Wilks' Lambda	.987	.985[b]	1.000	76.000	.324	.013
	Hotelling's Trace	.013	.985[b]	1.000	76.000	.324	.013
	Roy's Largest Root	.013	.985[b]	1.000	76.000	.324	.013
time_point * modality	Pillai's Trace	.001	.048[b]	1.000	76.000	.828	.001
	Wilks' Lambda	.999	.048[b]	1.000	76.000	.828	.001
	Hotelling's Trace	.001	.048[b]	1.000	76.000	.828	.001
	Roy's Largest Root	.001	.048[b]	1.000	76.000	.828	.001
time_point * task	Pillai's Trace	.001	.022[b]	2.000	76.000	.978	.001
	Wilks' Lambda	.999	.022[b]	2.000	76.000	.978	.001
	Hotelling's Trace	.001	.022[b]	2.000	76.000	.978	.001
	Roy's Largest Root	.001	.022[b]	2.000	76.000	.978	.001
time_point * modality * task	Pillai's Trace	.002	.057[b]	2.000	76.000	.944	.002
	Wilks' Lambda	.998	.057[b]	2.000	76.000	.944	.002
	Hotelling's Trace	.002	.057[b]	2.000	76.000	.944	.002
	Roy's Largest Root	.002	.057[b]	2.000	76.000	.944	.002

a. Design: Intercept + PSi_baseline_sqrt + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	.004	1	.004	1.146	.288	.015
	Greenhouse-Geisser	.004	1.000	.004	1.146	.288	.015
	Huynh-Feldt	.004	1.000	.004	1.146	.288	.015
	Lower-bound	.004	1.000	.004	1.146	.288	.015
time_point * PSi_baseline_sqrt	Sphericity Assumed	.004	1	.004	.985	.324	.013
	Greenhouse-Geisser	.004	1.000	.004	.985	.324	.013
	Huynh-Feldt	.004	1.000	.004	.985	.324	.013
	Lower-bound	.004	1.000	.004	.985	.324	.013
time_point * modality	Sphericity Assumed	.000	1	.000	.048	.828	.001
	Greenhouse-Geisser	.000	1.000	.000	.048	.828	.001
	Huynh-Feldt	.000	1.000	.000	.048	.828	.001
	Lower-bound	.000	1.000	.000	.048	.828	.001
time_point * task	Sphericity Assumed	.000	2	8.172E-5	.022	.978	.001
	Greenhouse-Geisser	.000	2.000	8.172E-5	.022	.978	.001
	Huynh-Feldt	.000	2.000	8.172E-5	.022	.978	.001
	Lower-bound	.000	2.000	8.172E-5	.022	.978	.001
time_point * modality * task	Sphericity Assumed	.000	2	.000	.057	.944	.002
	Greenhouse-Geisser	.000	2.000	.000	.057	.944	.002
	Huynh-Feldt	.000	2.000	.000	.057	.944	.002
	Lower-bound	.000	2.000	.000	.057	.944	.002
Error(time_point)	Sphericity Assumed	.280	76	.004			
	Greenhouse-Geisser	.280	76.000	.004			
	Huynh-Feldt	.280	76.000	.004			
	Lower-bound	.280	76.000	.004			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.004	1	.004	1.146	.288	.015
time_point * PSi_baseline_sqrt	Linear	.004	1	.004	.985	.324	.013
time_point * modality	Linear	.000	1	.000	.048	.828	.001
time_point * task	Linear	.000	2	8.172E-5	.022	.978	.001
time_point * modality * task	Linear	.000	2	.000	.057	.944	.002
Error(time_point)	Linear	.280	76	.004			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	1.095	1	1.095	120.616	.000	.613
PSi_baseline_sqrt	.927	1	.927	102.177	.000	.573
modality	.000	1	.000	.052	.820	.001
task	.003	2	.002	.167	.847	.004
modality * task	.032	2	.016	1.775	.176	.045
Error	.690	76	.009			

A.7.2.5 Generalised self-efficacy

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	GSE_1_week
2	GSE_4_weeks
3	GSE_8_weeks

Between-Subjects Factors

		Value Label	N
modality	1.00	Writing	36
	2.00	Simulation	46
task	1.00	Outcome	28
	2.00	Process	29
	3.00	Control	25

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.023	.878[b]	2.000	74.000	.420	.023
	Wilks' Lambda	.977	.878[b]	2.000	74.000	.420	.023
	Hotelling's Trace	.024	.878[b]	2.000	74.000	.420	.023
	Roy's Largest Root	.024	.878[b]	2.000	74.000	.420	.023
time_point * GSE_baseline	Pillai's Trace	.021	.806[b]	2.000	74.000	.451	.021
	Wilks' Lambda	.979	.806[b]	2.000	74.000	.451	.021
	Hotelling's Trace	.022	.806[b]	2.000	74.000	.451	.021
	Roy's Largest Root	.022	.806[b]	2.000	74.000	.451	.021
time_point * modality	Pillai's Trace	.005	.170[b]	2.000	74.000	.844	.005
	Wilks' Lambda	.995	.170[b]	2.000	74.000	.844	.005
	Hotelling's Trace	.005	.170[b]	2.000	74.000	.844	.005
	Roy's Largest Root	.005	.170[b]	2.000	74.000	.844	.005
time_point * task	Pillai's Trace	.058	1.121	4.000	150.000	.349	.029
	Wilks' Lambda	.943	1.107[b]	4.000	148.000	.355	.029
	Hotelling's Trace	.060	1.094	4.000	146.000	.362	.029
	Roy's Largest Root	.039	1.479[c]	2.000	75.000	.234	.038
time_point * modality * task	Pillai's Trace	.005	.088	4.000	150.000	.986	.002
	Wilks' Lambda	.995	.087[b]	4.000	148.000	.986	.002
	Hotelling's Trace	.005	.085	4.000	146.000	.987	.002
	Roy's Largest Root	.003	.128[c]	2.000	75.000	.880	.003

a. Design: Intercept + GSE_baseline + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic c. The statistic is an upper bound on F that yields a lower bound on the significance level.

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
time_point	.966	2.553	2	.279	.967	1.000	.500

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + GSE_baseline + modality + task + modality * task

Within Subjects Design: time_point

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	7.158	2	3.579	.771	.465	.010
	Greenhouse-Geisser	7.158	1.934	3.700	.771	.461	.010
	Huynh-Feldt	7.158	2.000	3.579	.771	.465	.010
	Lower-bound	7.158	1.000	7.158	.771	.383	.010
time_point * GSE_baseline	Sphericity Assumed	6.818	2	3.409	.734	.482	.010
	Greenhouse-Geisser	6.818	1.934	3.525	.734	.477	.010
	Huynh-Feldt	6.818	2.000	3.409	.734	.482	.010
	Lower-bound	6.818	1.000	6.818	.734	.394	.010
time_point * modality	Sphericity Assumed	1.815	2	.907	.195	.823	.003
	Greenhouse-Geisser	1.815	1.934	.938	.195	.816	.003
	Huynh-Feldt	1.815	2.000	.907	.195	.823	.003
	Lower-bound	1.815	1.000	1.815	.195	.660	.003
time_point * task	Sphericity Assumed	20.249	4	5.062	1.090	.364	.028
	Greenhouse-Geisser	20.249	3.869	5.234	1.090	.363	.028
	Huynh-Feldt	20.249	4.000	5.062	1.090	.364	.028
	Lower-bound	20.249	2.000	10.125	1.090	.341	.028
time_point * modality * task	Sphericity Assumed	1.622	4	.406	.087	.986	.002
	Greenhouse-Geisser	1.622	3.869	.419	.087	.985	.002
	Huynh-Feldt	1.622	4.000	.406	.087	.986	.002
	Lower-bound	1.622	2.000	.811	.087	.916	.002
Error(time_point)	Sphericity Assumed	696.586	150	4.644			
	Greenhouse-Geisser	696.586	145.080	4.801			
	Huynh-Feldt	696.586	150.000	4.644			
	Lower-bound	696.586	75.000	9.288			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.574	1	.574	.121	.729	.002
	Quadratic	6.585	1	6.585	1.446	.233	.019
time_point * GSE_baseline	Linear	.165	1	.165	.035	.852	.000
	Quadratic	6.654	1	6.654	1.461	.231	.019
time_point * modality	Linear	1.506	1	1.506	.318	.574	.004
	Quadratic	.309	1	.309	.068	.795	.001
time_point * task	Linear	7.386	2	3.693	.780	.462	.020
	Quadratic	12.863	2	6.432	1.413	.250	.036
time_point * modality * task	Linear	.460	2	.230	.049	.953	.001
	Quadratic	1.162	2	.581	.128	.880	.003
Error(time_point)	Linear	355.093	75	4.735			
	Quadratic	341.493	75	4.553			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	181.288	1	181.288	7.681	.007	.093
GSE_baseline	2844.750	1	2844.750	120.531	.000	.616
modality	21.500	1	21.500	.911	.343	.012
Task	15.553	2	7.777	.329	.720	.009
modality * task	55.928	2	27.964	1.185	.311	.031
Error	1770.135	75	23.602			

A.7.2.6 Difficulties in emotion-regulation

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	DERS_1_week
2	DERS_4_weeks
3	DERS_8_weeks

Between-Subjects Factors

		Value Label	N
modality	1.00	Writing	36
	2.00	Simulation	46
task	1.00	Outcome	28
	2.00	Process	29
	3.00	Control	25

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.030	1.153[b]	2.000	74.000	.321	.030
	Wilks' Lambda	.970	1.153[b]	2.000	74.000	.321	.030
	Hotelling's Trace	.031	1.153[b]	2.000	74.000	.321	.030
	Roy's Largest Root	.031	1.153[b]	2.000	74.000	.321	.030
time_point * DERS_baseline	Pillai's Trace	.037	1.435[b]	2.000	74.000	.245	.037
	Wilks' Lambda	.963	1.435[b]	2.000	74.000	.245	.037
	Hotelling's Trace	.039	1.435[b]	2.000	74.000	.245	.037
	Roy's Largest Root	.039	1.435[b]	2.000	74.000	.245	.037
time_point * modality	Pillai's Trace	.012	.452[b]	2.000	74.000	.638	.012
	Wilks' Lambda	.988	.452[b]	2.000	74.000	.638	.012
	Hotelling's Trace	.012	.452[b]	2.000	74.000	.638	.012
	Roy's Largest Root	.012	.452[b]	2.000	74.000	.638	.012
time_point * task	Pillai's Trace	.029	.552	4.000	150.000	.698	.014
	Wilks' Lambda	.971	.547[b]	4.000	148.000	.701	.015
	Hotelling's Trace	.030	.543	4.000	146.000	.705	.015
	Roy's Largest Root	.027	1.023[c]	2.000	75.000	.365	.027
time_point * modality * task	Pillai's Trace	.080	1.571	4.000	150.000	.185	.040
	Wilks' Lambda	.921	1.563[b]	4.000	148.000	.187	.041
	Hotelling's Trace	.085	1.555	4.000	146.000	.189	.041
	Roy's Largest Root	.070	2.623[c]	2.000	75.000	.079	.065

a. Design: Intercept + DERS_baseline + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic

c. The statistic is an upper bound on F that yields a lower bound on the significance level.

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
time_point	.776	18.743	2	.000	.817	.900	.500

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + DERS_baseline + modality + task + modality * task

Within Subjects Design: time_point

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	133.724	2	66.862	.783	.459	.010
	Greenhouse-Geisser	133.724	1.634	81.823	.783	.436	.010
	Huynh-Feldt	133.724	1.799	74.316	.783	.447	.010
	Lower-bound	133.724	1.000	133.724	.783	.379	.010
time_point * DERS_baseline	Sphericity Assumed	180.047	2	90.023	1.054	.351	.014
	Greenhouse-Geisser	180.047	1.634	110.167	1.054	.340	.014
	Huynh-Feldt	180.047	1.799	100.060	1.054	.346	.014
	Lower-bound	180.047	1.000	180.047	1.054	.308	.014
time_point * modality	Sphericity Assumed	66.816	2	33.408	.391	.677	.005
	Greenhouse-Geisser	66.816	1.634	40.883	.391	.635	.005
	Huynh-Feldt	66.816	1.799	37.133	.391	.655	.005
	Lower-bound	66.816	1.000	66.816	.391	.534	.005
time_point * task	Sphericity Assumed	264.011	4	66.003	.773	.545	.020
	Greenhouse-Geisser	264.011	3.269	80.772	.773	.522	.020
	Huynh-Feldt	264.011	3.599	73.361	.773	.533	.020
	Lower-bound	264.011	2.000	132.006	.773	.465	.020
time_point * modality * task	Sphericity Assumed	426.621	4	106.655	1.248	.293	.032
	Greenhouse-Geisser	426.621	3.269	130.520	1.248	.295	.032
	Huynh-Feldt	426.621	3.599	118.546	1.248	.294	.032
	Lower-bound	426.621	2.000	213.311	1.248	.293	.032
Error(time_point)	Sphericity Assumed	12815.833	150	85.439			
	Greenhouse-Geisser	12815.833	122.573	104.556			
	Huynh-Feldt	12815.833	134.954	94.964			
	Lower-bound	12815.833	75.000	170.878			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	67.771	1	67.771	.550	.461	.007
	Quadratic	65.953	1	65.953	1.386	.243	.018
time_point * DERS_baseline	Linear	108.307	1	108.307	.878	.352	.012
	Quadratic	71.740	1	71.740	1.508	.223	.020
time_point * modality	Linear	50.863	1	50.863	.412	.523	.005
	Quadratic	15.954	1	15.954	.335	.564	.004
time_point * task	Linear	252.226	2	126.113	1.023	.365	.027
	Quadratic	11.785	2	5.893	.124	.884	.003
time_point * modality * task	Linear	178.674	2	89.337	.725	.488	.019
	Quadratic	247.947	2	123.974	2.606	.080	.065
Error(time_point)	Linear	9248.107	75	123.308			
	Quadratic	3567.725	75	47.570			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	681.378	1	681.378	2.140	.148	.028
DERS_baseline	68395.461	1	68395.461	214.800	.000	.741
modality	1071.713	1	1071.713	3.366	.071	.043
task	64.502	2	32.251	.101	.904	.003
modality * task	262.353	2	131.176	.412	.664	.011
Error	23881.082	75	318.414			

A.7.2.7 Behavioural self-regulation

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	SSRQ_1_week_t_f
2	SSRQ_4_weeks
3	SSRQ_8_weeks

Between-Subjects Factors

		Value Label	N
modality	1.00	writing	36
	2.00	simulation	46
Task	1.00	outcome	28
	2.00	process	29
	3.00	control	25

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.048	1.871[b]	2.000	74.000	.161	.048
	Wilks' Lambda	.952	1.871[b]	2.000	74.000	.161	.048
	Hotelling's Trace	.051	1.871[b]	2.000	74.000	.161	.048
	Roy's Largest Root	.051	1.871[b]	2.000	74.000	.161	.048
time_point * SSRQ_baseline	Pillai's Trace	.050	1.938[b]	2.000	74.000	.151	.050
	Wilks' Lambda	.950	1.938[b]	2.000	74.000	.151	.050
	Hotelling's Trace	.052	1.938[b]	2.000	74.000	.151	.050
	Roy's Largest Root	.052	1.938[b]	2.000	74.000	.151	.050
time_point * modality	Pillai's Trace	.033	1.255[b]	2.000	74.000	.291	.033
	Wilks' Lambda	.967	1.255[b]	2.000	74.000	.291	.033
	Hotelling's Trace	.034	1.255[b]	2.000	74.000	.291	.033
	Roy's Largest Root	.034	1.255[b]	2.000	74.000	.291	.033
time_point * task	Pillai's Trace	.072	1.409	4.000	150.000	.234	.036
	Wilks' Lambda	.928	1.407[b]	4.000	148.000	.235	.037
	Hotelling's Trace	.077	1.404	4.000	146.000	.236	.037
	Roy's Largest Root	.069	2.581[c]	2.000	75.000	.082	.064
time_point * modality * task	Pillai's Trace	.109	2.168	4.000	150.000	.075	.055
	Wilks' Lambda	.891	2.189[b]	4.000	148.000	.073	.056
	Hotelling's Trace	.121	2.209	4.000	146.000	.071	.057
	Roy's Largest Root	.114	4.279[c]	2.000	75.000	.017	.102

a. Design: Intercept + SSRQ_baseline + modality + task + modality * task

Within Subjects Design: time_point

b. Exact statistic

c. The statistic is an upper bound on F that yields a lower bound on the significance level.

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
time_point	.962	2.834	2	.242	.964	1.000	.500

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + SSRQ_baseline + modality + task + modality * task

Within Subjects Design: time_point

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	132.985	2	66.493	1.872	.157	.024
	Greenhouse-Geisser	132.985	1.928	68.990	1.872	.159	.024
	Huynh-Feldt	132.985	2.000	66.493	1.872	.157	.024
	Lower-bound	132.985	1.000	132.985	1.872	.175	.024
time_point * SSRQ_baseline	Sphericity Assumed	136.455	2	68.227	1.921	.150	.025
	Greenhouse-Geisser	136.455	1.928	70.791	1.921	.152	.025
	Huynh-Feldt	136.455	2.000	68.227	1.921	.150	.025
	Lower-bound	136.455	1.000	136.455	1.921	.170	.025
time_point * modality	Sphericity Assumed	101.305	2	50.653	1.426	.244	.019
	Greenhouse-Geisser	101.305	1.928	52.556	1.426	.244	.019
	Huynh-Feldt	101.305	2.000	50.653	1.426	.244	.019
	Lower-bound	101.305	1.000	101.305	1.426	.236	.019
time_point * task	Sphericity Assumed	220.087	4	55.022	1.549	.191	.040
	Greenhouse-Geisser	220.087	3.855	57.089	1.549	.193	.040
	Huynh-Feldt	220.087	4.000	55.022	1.549	.191	.040
	Lower-bound	220.087	2.000	110.043	1.549	.219	.040
time_point * modality * task	Sphericity Assumed	377.830	4	94.458	2.659	.035	.066
	Greenhouse-Geisser	377.830	3.855	98.006	2.659	.037	.066
	Huynh-Feldt	377.830	4.000	94.458	2.659	.035	.066
	Lower-bound	377.830	2.000	188.915	2.659	.077	.066
Error(time_point)	Sphericity Assumed	5328.754	150	35.525			
	Greenhouse-Geisser	5328.754	144.569	36.860			
	Huynh-Feldt	5328.754	150.000	35.525			
	Lower-bound	5328.754	75.000	71.050			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	89.078	1	89.078	2.108	.151	.027
	Quadratic	43.907	1	43.907	1.524	.221	.020
time_point * SSRQ_baseline	Linear	88.293	1	88.293	2.090	.152	.027
	Quadratic	48.161	1	48.161	1.672	.200	.022
time_point * modality	Linear	79.330	1	79.330	1.878	.175	.024
	Quadratic	21.975	1	21.975	.763	.385	.010
time_point * task	Linear	182.549	2	91.275	2.160	.122	.054
	Quadratic	37.538	2	18.769	.652	.524	.017
time_point * modality * task	Linear	359.248	2	179.624	4.252	.018	.102
	Quadratic	18.582	2	9.291	.323	.725	.009
Error(time_point)	Linear	3168.587	75	42.248			
	Quadratic	2160.166	75	28.802			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	975.620	1	975.620	5.097	.027	.064
SSRQ_baseline	45770.608	1	45770.608	239.141	.000	.761
modality	8.382	1	8.382	.044	.835	.001
task	44.148	2	22.074	.115	.891	.003
modality * task	1184.393	2	592.196	3.094	.051	.076
Error	14354.723	75	191.396			

Post-hoc analyses of significant time-point*modality*task interaction: Differences in self-regulation as a function of modality and task, at each follow-up time-point separately

ANCOVA for one-week follow-up

Between-Subjects Factors

		Value Label	N
modality	1.00	writing	51
	2.00	simulation	59
Task	1.00	outcome	37
	2.00	process	38
	3.00	control	35

Tests of Between-Subjects Effects

Dependent Variable: SSRQ_1_week_t_f

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	21637.363[a]	6	3606.227	49.154	.000	.741
Intercept	460.717	1	460.717	6.280	.014	.057
SSRQ_baseline	20163.837	1	20163.837	274.841	.000	.727
modality	149.975	1	149.975	2.044	.156	.019
Task	8.104	2	4.052	.055	.946	.001
modality * task	150.609	2	75.305	1.026	.362	.020
Error	7556.631	103	73.365			
Total	1439961.828	110				
Corrected Total	29193.994	109				

a. R Squared = .741 (Adjusted R Squared = .726)

ANCOVA for four-week follow-up

Between-Subjects Factors

		Value Label	N
modality	1.00	writing	46
	2.00	simulation	49
task	1.00	outcome	32
	2.00	process	33
	3.00	control	30

Tests of Between-Subjects Effects

Dependent Variable: SSRQ_4_weeks

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	21570.305[a]	6	3595.051	41.907	.000	.741
Intercept	88.847	1	88.847	1.036	.312	.012
SSRQ_baseline	20023.180	1	20023.180	233.406	.000	.726
modality	128.190	1	128.190	1.494	.225	.017
task	73.756	2	36.878	.430	.652	.010
modality * task	350.162	2	175.081	2.041	.136	.044
Error	7549.232	88	85.787			
Total	1251912.000	95				
Corrected Total	29119.537	94				

a. R Squared = .741 (Adjusted R Squared = .723)

ANCOVA for eight-week follow-up

Between-Subjects Factors

		Value Label	N
modality	1.00	Writing	36
	2.00	Simulation	53
task	1.00	Outcome	31
	2.00	Process	31
	3.00	Control	27

Tests of Between-Subjects Effects

Dependent Variable: SSRQ_8_weeks

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	21460.529[a]	6	3576.755	30.422	.000	.690
Intercept	122.780	1	122.780	1.044	.310	.013
SSRQ_baseline	19083.863	1	19083.863	162.315	.000	.664
modality	72.103	1	72.103	.613	.436	.007
task	45.026	2	22.513	.191	.826	.005
modality * task	1118.516	2	559.258	4.757	.011	.104
Error	9640.999	82	117.573			
Total	1193224.000	89				
Corrected Total	31101.528	88				

a. R Squared = .690 (Adjusted R Squared = .667)

Post-hoc analyses of significant modality*task interaction at eight-week follow-up:

Main effect of task in writing and simulation modalities separately

Writing

Between-Subjects Factors[a]

		Value Label	N
task	1.00	outcome	13
	2.00	process	12
	3.00	control	11

a. modality = writing

Tests of Between-Subjects Effects[a]

Dependent Variable: SSRQ_8_weeks

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	12945.677[b]	3	4315.226	54.471	.000	.836
Intercept	108.253	1	108.253	1.366	.251	.041
SSRQ_baseline	12056.249	1	12056.249	152.185	.000	.826
task	527.928	2	263.964	3.332	.048	.172
Error	2535.073	32	79.221			
Total	486763.000	36				
Corrected Total	15480.750	35				

a. modality = writing

b. R Squared = .836 (Adjusted R Squared = .821)

1. Grand Mean[a]

Dependent Variable: SSRQ_8_weeks

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
114.190[b]	1.487	111.161	117.219

a. modality = writing

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 110.2635.

Estimates[a]

Dependent Variable: SSRQ_8_weeks

task	Mean	Std. Error	95% Confidence Interval	
			Lower Bound	Upper Bound
outcome	116.747[b]	2.541	111.571	121.924
process	117.234[b]	2.588	111.962	122.506
control	108.589[b]	2.709	103.070	114.108

a. modality = writing

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 110.2635.

Pairwise Comparisons[a]

Dependent Variable: SSRQ_8_weeks

(I) task	(J) task	Mean Difference (I-J)	Std. Error	Sig.[c]	95% Confidence Interval for Difference[c]	
					Lower Bound	Upper Bound
outcome	process	-.486	3.679	.896	-7.980	7.007
	control	8.159*	3.775	.038	.470	15.847
process	outcome	.486	3.679	.896	-7.007	7.980
	control	8.645*	3.716	.026	1.076	16.214
control	outcome	-8.159*	3.775	.038	-15.847	-.470
	process	-8.645*	3.716	.026	-16.214	-1.076

Based on estimated marginal means

*. The mean difference is significant at the .05 level.

a. modality = writing

c. Adjustment for multiple comparisons: Least Significant Difference (equivalent to no adjustments).

Univariate Tests[a]

Dependent Variable: SSRQ_8_weeks

	Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Contrast	527.928	2	263.964	3.332	.048	.172
Error	2535.073	32	79.221			

The F tests the effect of task. This test is based on the linearly independent pairwise comparisons among the estimated marginal means.[a]

a. modality = writing

Simulation

Between-Subjects Factors[a]

		Value Label	N
task	1.00	Outcome	18
	2.00	Process	19
	3.00	Control	16

a. modality = simulation

Tests of Between-Subjects Effects[a]

Dependent Variable: SSRQ_8_weeks

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	9135.115[b]	3	3045.038	23.010	.000	.585
Intercept	560.305	1	560.305	4.234	.045	.080
SSRQ_baseline	7649.182	1	7649.182	57.802	.000	.541
task	878.191	2	439.095	3.318	.045	.119
Error	6484.357	49	132.334			
Total	706461.000	53				
Corrected Total	15619.472	52				

a. modality = simulation

b. R Squared = .585 (Adjusted R Squared = .559)

1. Grand Mean[a]

Dependent Variable: SSRQ_8_weeks

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
114.437[b]	1.584	111.253	117.620

a. modality = simulation

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 112.3960.

Estimates[a]

Dependent Variable: SSRQ_8_weeks

task	Mean	Std. Error	95% Confidence Interval	
			Lower Bound	Upper Bound
outcome	115.540[b]	2.726	110.062	121.018
process	108.987[b]	2.651	103.660	114.314
control	118.783[b]	2.876	113.004	124.563

a. modality = simulation

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 112.3960.

Pairwise Comparisons[a]

Dependent Variable: SSRQ_8_weeks

(I) task	(J) task	Mean Difference (I-J)	Std. Error	Sig.[c]	95% Confidence Interval for Difference[c]	
					Lower Bound	Upper Bound
outcome	process	6.553	3.820	.093	-1.124	14.230
	control	-3.244	3.964	.417	-11.210	4.722
process	outcome	-6.553	3.820	.093	-14.230	1.124
	control	-9.797*	3.910	.016	-17.654	-1.939
control	outcome	3.244	3.964	.417	-4.722	11.210
	process	9.797*	3.910	.016	1.939	17.654

Based on estimated marginal means

*. The mean difference is significant at the .05 level.

a. modality = simulation

c. Adjustment for multiple comparisons: Least Significant Difference (equivalent to no adjustments).

Univariate Tests[a]

Dependent Variable: SSRQ_8_weeks

	Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Contrast	878.191	2	439.095	3.318	.045	.119
Error	6484.357	49	132.334			

The F tests the effect of task. This test is based on the linearly independent pairwise comparisons among the estimated marginal means.[a]

a. modality = simulation

Main effect of modality in outcome, process and control tasks separately

Outcome

Between-Subjects Factors[a]

		Value Label	N
modality	1.00	writing	13
	2.00	simulation	18

a. task = outcome

Tests of Between-Subjects Effects[a]

Dependent Variable: SSRQ_8_weeks

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	6138.394[b]	2	3069.197	33.757	.000	.707
Intercept	70.038	1	70.038	.770	.388	.027
SSRQ_baseline	5610.970	1	5610.970	61.712	.000	.688
modality	30.704	1	30.704	.338	.566	.012
Error	2545.799	28	90.921			
Total	412701.000	31				
Corrected Total	8684.194	30				

a. task = outcome

b. R Squared = .707 (Adjusted R Squared = .686)

Grand Mean[a]

Dependent Variable: SSRQ_8_weeks

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
114.336[b]	1.739	110.774	117.897

a. task = outcome

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 110.3226.

Process

Between-Subjects Factors[a]

		Value Label	N
modality	1.00	writing	12
	2.00	simulation	19

a. task = process

Tests of Between-Subjects Effects[a]

Dependent Variable: SSRQ_8_weeks

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	5621.275[b]	2	2810.638	17.518	.000	.556
Intercept	392.558	1	392.558	2.447	.129	.080
SSRQ_baseline	4184.239	1	4184.239	26.079	.000	.482
modality	908.125	1	908.125	5.660	.024	.168
Error	4492.467	28	160.445			
Total	402570.000	31				
Corrected Total	10113.742	30				

a. task = process

b. R Squared = .556 (Adjusted R Squared = .524)

Grand Mean[a]

Dependent Variable: SSRQ_8_weeks

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
113.779[b]	2.336	108.994	118.565

a. task = process

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 111.4292.

Estimates[a]

Dependent Variable: SSRQ_8_weeks

modality	Mean	Std. Error	95% Confidence Interval	
			Lower Bound	Upper Bound
writing	119.373[b]	3.672	111.852	126.895
simulation	108.185[b]	2.914	102.217	114.154

a. task = process

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 111.4292.

Pairwise Comparisons[a]

Dependent Variable: SSRQ_8_weeks

(I) modality	(J) modality	Mean Difference (I-J)	Std. Error	Sig.[c]	95% Confidence Interval for Difference[c]	
					Lower Bound	Upper Bound
writing	simulation	11.188*	4.702	.024	1.555	20.820
simulation	writing	-11.188*	4.702	.024	-20.820	-1.555

Based on estimated marginal means

*. The mean difference is significant at the .05 level.

a. task = process

c. Adjustment for multiple comparisons: Least Significant Difference (equivalent to no adjustments).

Univariate Tests[a]

Dependent Variable: SSRQ_8_weeks

	Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Contrast	908.125	1	908.125	5.660	.024	.168
Error	4492.467	28	160.445			

The F tests the effect of modality. This test is based on the linearly independent pairwise comparisons among the estimated marginal means.[a]

a. task = process

Control

Between-Subjects Factors[a]

		Value Label	N
modality	1.00	writing	11
	2.00	simulation	16

a. task = control

Tests of Between-Subjects Effects[a]

Dependent Variable: SSRQ_8_weeks

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	9898.138[b]	2	4949.069	54.326	.000	.819
Intercept	64.364	1	64.364	.707	.409	.029
SSRQ_baseline	9705.006	1	9705.006	106.532	.000	.816
Modality	390.879	1	390.879	4.291	.049	.152
Error	2186.380	24	91.099			
Total	377953.000	27				
Corrected Total	12084.519	26				

a. task = control

b. R Squared = .819 (Adjusted R Squared = .804)

Grand Mean[a]

Dependent Variable: SSRQ_8_weeks

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
115.689[b]	1.869	111.831	119.547

a. task = control

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 113.0432.

Estimates[a]

Dependent Variable: SSRQ_8_weeks

modality	Mean	Std. Error	95% Confidence Interval	
			Lower Bound	Upper Bound
writing	111.810[b]	2.881	105.865	117.756
simulation	119.568[b]	2.388	114.640	124.496

a. task = control

b. Covariates appearing in the model are evaluated at the following values: SSRQ_baseline = 113.0432.

Pairwise Comparisons[a]

Dependent Variable: SSRQ_8_weeks

(I) modality	(J) modality	Mean Difference (I-J)	Std. Error	Sig.[c]	95% Confidence Interval for Difference[c]	
					Lower Bound	Upper Bound
writing	simulation	-7.758*	3.745	.049	-15.487	-.028
simulation	writing	7.758*	3.745	.049	.028	15.487

Based on estimated marginal means

*. The mean difference is significant at the .05 level.

a. task = control

c. Adjustment for multiple comparisons: Least Significant Difference (equivalent to no adjustments).

Univariate Tests[a]

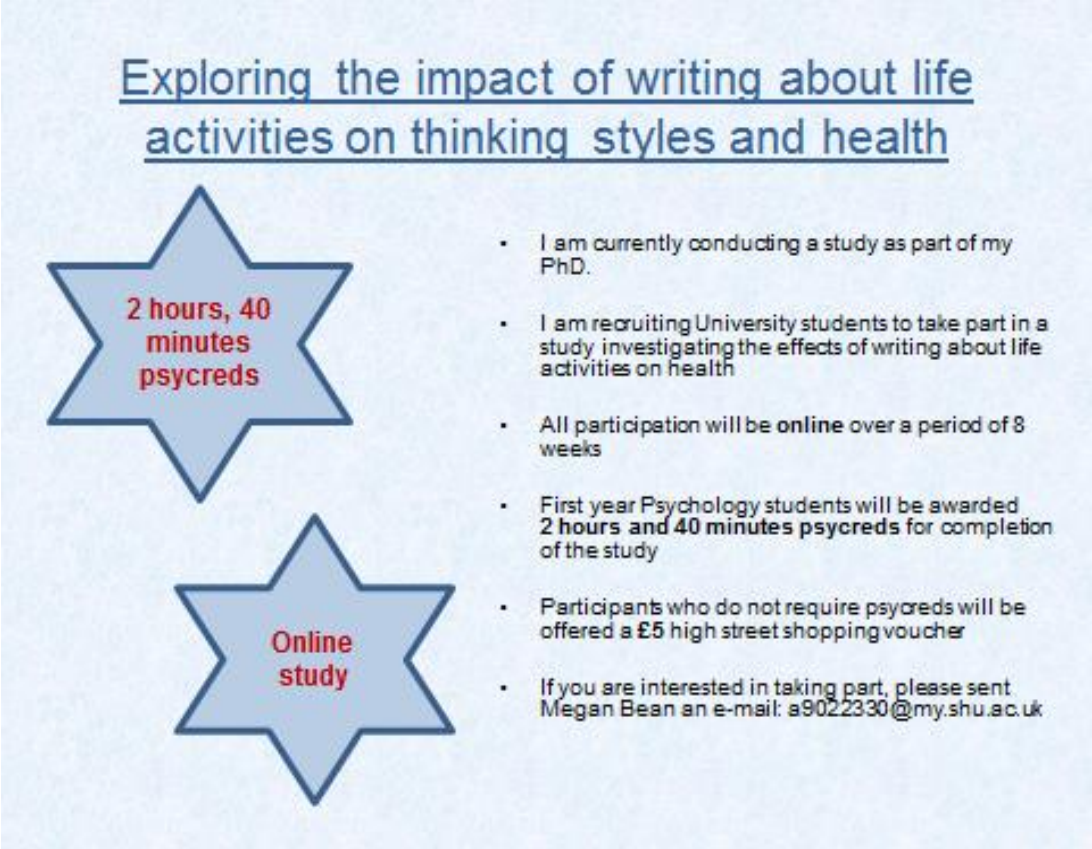
Dependent Variable: SSRQ_8_weeks

	Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Contrast	390.879	1	390.879	4.291	.049	.152
Error	2186.380	24	91.099			

The F tests the effect of modality. This test is based on the linearly independent pairwise comparisons among the estimated marginal means.[a]

a. task = control

A.8 Study Two advertisement for participants



A.9 Study Two participant information sheet

You are invited to participate in a study looking at the effects of writing about life activities on thinking styles and health. I am a post-graduate Psychology student at Sheffield Hallam University, and this research is being carried out as part of my PhD. All participation will occur online, and will include completion of some questionnaires as well as writing about some life activities.

Stage One requires participation on 4 consecutive days. On the first day, participation will take around 40 minutes. On the remaining 3 days of Stage One, it will take around 25 minutes per day. Stage Two will constitute follow-ups which will take place 4 and 8 weeks from the date of the last writing session in Stage One. These will take around 20 minutes each. We ask that you complete each stage in a quiet room, on your own, where there are no distractions and you can concentrate.

Your data will be collected anonymously; your responses to questions and activities will be matched using a unique participant identification code. You will be instructed how to generate this should you choose to proceed with the study. All data provided by you will be stored securely on an encrypted memory stick and password-protected computer. A master copy of the data file containing all data from this study will be placed on the University's research store; this will be accessible only to the researcher and supervisors named on this information sheet. Data will remain confidential, apart from in the exceptional circumstance that you disclose information that yourself or others may be at risk of harm. Once the project is completed, it is possible that your data may be included in scholarly articles. Furthermore, upon completion of the PhD, data will be stored in the University Research Data Archive where it will be accessible to other legitimate researchers. This will be for a period of 10 years following the final request to access the data- however be assured that your data would remain confidential and your personal details would not be accessible. Raw data will not be included in the write-up of this investigation; only average scores and ranges of scores will be presented.

Your e-mail address will be retained by the researcher in order for you to take part. This is because the researcher needs to send you links to the study. However, e-mail addresses will be stored separately to the data; therefore it will not be in any way possible to connect your personal e-mail address to the data.

You do not have to take part in this study- participation is voluntary. You have the right to withdraw at any time throughout your participation, and the right to refuse responses to any questions, without there being any consequences. You have the right to withdraw any data that you provide up to 7 days after providing it; after this point it will not be possible due to anonymity making data impossible to identify. You have the right to ask the researcher any questions that you have prior to participating using the e-mail address below. You may also contact them at any time throughout your participation, including to request a copy of the report upon completion if you so wish.

First year Psychology students will receive **2 hours, 40 minutes of psycreds** on completion of the study. Participants who do not require psycreds will be offered a £5 high street shopping voucher.

Researcher: **Megan Bean** a9022330@my.shu.ac.uk
Supervisors: **Dr. Katie Cutts** (Director of Studies) k.cutts@shu.ac.uk
Dr. John Reidy (Second Supervisor) ssljgr@exchange.shu.ac.uk

A.10 Study Two participant consent form

Please place an X in the box at the bottom of the page if you would like to proceed with your participation, and if you are content with the terms of participation outlined below.

• I have read and understood the information sheet explaining this research
• I have been provided with sufficient opportunity to ask questions, and have received satisfactory answers to any that I asked
• I have had sufficient information about the current study
• I understand that I have the right to withdraw, without consequence, from this study at any time throughout my participation and without giving reason
• I understand that I have the right to refuse to give answers to any questions that I do not want to answer, without giving reason, and without consequence
• I understand that I have the right to withdraw any data that I provide for up to 7 days after I provide it

By placing an X in the box below, you are giving your informed consent of your voluntary participation in this investigation, and the anonymous, confidential inclusion of your data in the analysis and write up of the investigation. You are also consenting to your data being placed on the University's research data archive upon completion of the PhD, for use by other legitimate researchers. If you would not like to take part, please close the browser. No information about you has been recorded.

By placing an 'X' in the box below you indicate that you have been properly informed about the aims of the study and you provide your consent for participation.

☐

Megan Bean (Researcher): a9022330@my.shu.ac.uk
Katie Cutts (Director of Studies): k.cutts@shu.ac.uk
John Reidy (Second supervisor): ssljgr@exchange.shu.ac.uk

A.11 Study Two debrief sheet

Thank you for your participation in this investigation. Your time is very much appreciated and the information that you have provided will be very helpful.

Writing about a best possible future self has been found to be associated with a range of benefits including improved health and psychological wellbeing. The aim of the study that you have taken part in was to investigate whether beneficial effects of writing are brought about by increases in individuals' levels of self-regulation.

The topic that you wrote about was 1 of 2 topics; a best possible future self or an emotionally neutral control topic (describing plans for the day). It is hoped that you will experience some benefits from taking part, which has been the case in previous studies conducted in the same area.

Please be assured that any information which was disclosed by you during the course of your participation will remain anonymous and confidential.

If you have any worries with regards to your physical or psychological health, you might be interested in contacting some health service providers. Contact details for a selection of these services are provided below.
Services within Sheffield Hallam University:
Medical centre: 0114 225 2134
Counselling/ wellbeing service: 0114 225 2136 / student.wellbeing@shu.ac.uk
Services outside of the University:
Sheffield Mind: 0114 258 4489
Rethink Mental Illness (Sheffield): 0114 267 7660
Samaritans: (freephone) 116 123 / jo@samaritans.org

If you have any further questions about the study or your participation, wish to contact the researcher or would like to receive a copy of the write-up of this investigation once it has been completed, you may do so at any time via e-mail:
Megan Bean (Researcher): a9022330@my.shu.ac.uk
Dr. Katie Cutts (Director of studies): k.cutts@shu.ac.uk
Dr. John Reidy (Second supervisor): ssljgr@exchange.shu.ac.uk

A.12 Study Two ethics proforma, data management plan, and approval letter

A.12.1 Ethics proforma

Sheffield Hallam University

RESEARCH ETHICS CHECKLIST (SHUREC1)

This form is designed to help staff and postgraduate research students to complete an ethical scrutiny of proposed research. The SHU Research Ethics Policy should be consulted before completing the form.

Answering the questions below will help you decide whether your proposed research requires ethical review by a Faculty Research Ethics Committee (FREC). In cases of uncertainty, members of the FREC can be approached for advice.

Please note: staff based in University central departments should submit to the University Ethics Committee (SHUREC) for review and advice.

The final responsibility for ensuring that ethical research practices are followed rests with the supervisor for student research and with the principal investigator for staff research projects.

Note that students and staff are responsible for making suitable arrangements for keeping data secure and, if relevant, for keeping the identity of participants anonymous. They are also responsible for following SHU guidelines about data encryption and research data management.

The form also enables the University and Faculty to keep a record confirming that research conducted has been subjected to ethical scrutiny.

- For postgraduate research student projects, the form should be completed by the student and counter-signed by the supervisor, and kept as a record showing that ethical scrutiny has occurred. Students should retain a copy for inclusion in their thesis, and staff should keep a copy in the student file.
- For staff research, the form should be completed and kept by the principal investigator.

Please note if it may be necessary to conduct a health and safety risk assessment for the proposed research. Further information can be obtained from the Faculty Safety Co-ordinator.

General Details

Name of principal investigator or	Megan Bean
SHU email	a9022330@my.shu.ac.uk
Name of	Dr. Katie Cutts (Director of Studies)
email address	k.cutts@shu.ac.uk
Title of proposed research	Writing about a best possible future self for 20 minutes on 4 consecutive days: The role of self-regulation in the production of health benefits.
Proposed start	July 2016
Proposed end date	December 2016
Brief outline of research to include, rationale & aims (500 -750 words).	King (2001) found that students who wrote about a best possible future self (BPFS) for 20 minutes on 4 consecutive days demonstrated improved health compared to peers who wrote about their plans for the day. This demonstration of the benefits of writing about a BPFS is well-evidenced and robust, and has been replicated numerous times (e.g. Sheldon & Lyubomirsky, 2006; Renner, Schwarz, Peters & Huibers, 2014; Boehm, Lyubomirsky, & Sheldon, 2011). The prominent theoretical explanation for the benefits of writing about a BPFS is that the activity facilitates self-regulation. Positive attainable future selves are personalised representations of an individual's goals, which are reflective of self-regulatory processes (Markus & Nurius, 1986; Austin & Vancouver, 1996). It is conceivable then that writing about a BPFS promotes awareness of future goals (King, 2002), and as such facilitates self-regulation. Framing this in a self-regulation theory perspective, outcome goals (e.g. BPFS) hold a higher order in an individual's motivational hierarchy than short-term goals such as plans for the day ahead, and as such are less likely to be regularly considered (King, 2001). Therefore, bringing an individual’s attention to their higher-order goals by instructing them to write them down may enable them to explore aspects of their motivational lives which may previously have been mostly unexamined or unconsidered (King, 2001). Through imagination of what will bring future fulfilment, BPFS writing encourages the individual to assess and identify their priorities and consider what they truly require in order to create a positive future life (King, 2002). This explanation for the effects of writing about a BPFS is conceivable, however, to the knowledge of the

	researcher the effects of BPFS writing on self-regulation had not been explored until recently. In the first study of this programme of research, individuals who wrote about a BPFS demonstrated greater self-regulation than those who wrote about the details of their previous day at a follow-up which occurred 8 weeks following the writing activity. Surprisingly, however, no gains in physical or psychological health following the BPFS writing task were found. It is possible that this was a product of deviations from King's (2001) original paradigm; for example King (2001) included 4, 20 minute writing sessions, whereas in the first study in this programme only one writing session was used. Potentially, this was not a sufficient dosage for health benefits to occur. A major difficulty in the interpretation of differences in findings between BPFS writing studies is methodological variation (see Frattaroli, 2006; Sloan & Marx, 2004); inconsistency clouds the visibility of the sources of differences in findings between studies and renders accurate interpretation difficult. With regards to the current research, it is unknown whether the traditional protocol of 4 writing sessions would foster self-regulation gains, and whether health improvements and self-regulation gains can occur under the same conditions. Therefore, the aim of the current study is to explore whether self-regulation gains and health benefits occur when the traditional protocol is used, and whether there is a mediating role of gains in self-regulation in the production of physical and psychological health outcomes following writing about a BPFS. Please see outline of methods, including the proposed methodology, attached.
Where data is collected from human participants, outline the nature of the data, details of anonymisation, storage and disposal procedures if these are required (300 - 750 words).	Data will be quantitative. This will constitute participants' responses to online questionnaires. The surveys will be displayed and responses will be recorded using a password-protected Qualtrics account. Data will be downloaded from Qualtrics into an SPSS data file, and will then be deleted from the Qualtrics software. All data collected will be anonymous; participants will be asked to generate a unique code (using the last three digits of their mobile number and the first three letters of a pet's or best friend's name), and will be required to use this at every stage of their participation. They will not at any point be asked to provide their name. Participants' e-mail addresses will be required in order to send them links to study materials, however these will be stored in a separate password-protected file to the data, therefore it

	will not be possible to match e-mail addresses with data. Each e-mail address will be deleted after the final online link has been sent, or at the point that the owner of the e-mail address expresses a desire to withdraw from the study or cease to participate any further. Data will be stored securely on an encrypted memory stick and password-protected computer. A master copy of the SPSS data file will be stored on the University's research data store (Q:\Research drive). Access to the SPSS file will be restricted to the researcher and her PhD supervisors alone. Only averages and ranges of scores will be included in the write-up of the results of the investigation; no raw data whatsoever will be presented. Upon submission of the PhD thesis data will be registered and stored in the University research data archive (SHURDA), and will be made accessible to legitimate researchers. The data will be stored in SHURDA for a period of 10 years following the final request for access by a third party.
Will the research be conducted with partners & subcontractors?	**Yes/No** **NO** (If **YES**, outline how you will ensure that their ethical policies are consistent with university policy.)

1. Health Related Research involving the NHS or Social Care / Community Care or the
Criminal Justice System or with research participants unable to provide informed consent

Question		Yes/No
1.	Does the research involve? • Patients recruited because of their past or present use of the NHS or Social Care • Relatives/carers of patients recruited because of their past or present use of the NHS or Social Care • Access to data, organs or other bodily material of past or present NHS patients • Foetal material and IVF involving NHS patients • The recently dead in NHS premises • Prisoners or others within the criminal justice system recruited for health- related research* • Police, court officials, prisoners or others within the criminal justice system* • Participants who are unable to provide informed consent due to their incapacity even if the project is not health related	No
2.	Is this a research project as opposed to service evaluation or audit? *For NHS definitions please see the following website* http://www.nres.nhs.uk/applications/is-your-project-research/	

If you have answered **YES** to questions **1 & 2** then you **must** seek the appropriate external approvals from the NHS, Social Care or the National Offender Management Service (NOMS) under their independent Research Governance schemes. Further information is provided below.

NHS https://www.myresearchproject.org.uk/Signin.aspx

* Prison projects may also need National Offender Management Service (NOMS) Approval and Governor's Approval and may need Ministry of Justice approval. Further guidance at:
http://www.hra.nhs.uk/research-community/applying-for-approvals/national-offender-management-service-noms/

NB FRECs provide Independent Scientific Review for NHS or SC research and initial scrutiny for ethics applications as required for university sponsorship of the research. Applicants can use the NHS proforma and submit this initially to their FREC.

2. Research with Human Participants

Question		Yes/ No
1.	Does the research involve human participants? This includes surveys, questionnaires, observing behaviour etc.	YES
Note	*If YES, then please answer questions 2 to 10*	
If NO, please go to Section 3		
2.	Will any of the participants be vulnerable?	NO
Note	*'Vulnerable' people include children and young people, people with learning disabilities, people who may be limited by age or sickness or disability, etc. See definition*	
3	Are drugs, placebos or other substances (e.g. food substances, vitamins) to be administered to the study participants or will the study involve invasive, intrusive or potentially harmful procedures of any kind?	NO
4	Will tissue samples (including blood) be obtained from participants?	NO
5	Is pain or more than mild discomfort likely to result from the study?	NO
6	Will the study involve prolonged or repetitive testing?	NO
7	Is there any reasonable and foreseeable risk of physical or emotional harm to any of the participants?	NO
Note	*Harm may be caused by distressing or intrusive interview questions, uncomfortable procedures involving the participant, invasion of privacy, topics relating to highly personal information, topics relating to illegal activity, etc.*	
8	Will anyone be taking part without giving their informed consent?	NO
9	Is it covert research?	NO
Note	*'Covert research' refers to research that is conducted without the knowledge of participants.*	
10	Will the research output allow identification of any individual who has not given their express consent to be identified?	NO

If you answered **YES only** to question **1,** you must complete the box below and submit the signed form to the FREC for registration and scrutiny.

Data Handling

Where data is collected from human participants, outline the nature of the data, details of anonymisation, storage and disposal procedures if these are required (300 -750 words).

Data will be quantitative. This will constitute participants' responses to online questionnaires. The surveys will be displayed and responses will be recorded using a password-protected Qualtrics account. Data will be downloaded from Qualtrics into an SPSS data file, and will then be deleted from the Qualtrics software. All data collected will be anonymous; participants will be asked to generate a unique code (using the last three digits of their mobile number and the first three letters of a pet's or best friend's name), and will be required to use this at every stage of their

participation. They will not at any point be asked to provide their name. Participants' e-mail addresses will be required in order to send them links to study materials, however these will be stored in a separate password-protected file to the data, therefore it will not be possible to match e-mail addresses with data. Each e-mail address will be deleted after the final online link has been sent, or at the point that the owner of the e-mail address expresses a desire to withdraw from the study or cease to participate any further. Data will be stored securely on an encrypted memory stick and password-protected computer. A master copy of the SPSS data file will be stored on the University's research data store (Q:\Research drive). Access to the SPSS file will be restricted to the researcher and her PhD supervisors alone. Only averages and ranges of scores will be included in the write-up of the results of the investigation; no raw data whatsoever will be presented. Upon submission of the PhD thesis data will be registered and stored in the University research data archive (SHURDA) and will be made accessible to legitimate researchers. The data will be stored in SHURDA for a period of 10 years following the final request for access by a third party.

If you have answered **YES** to any of the other questions you are **required** to submit a SHUREC2A (or 2B) to the FREC. If you answered **YES** to question **8** and participants cannot provide informed consent due to their incapacity you must obtain the appropriate approvals from the NHS research governance system.

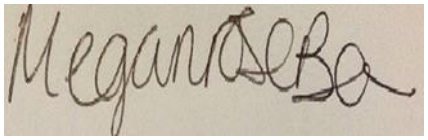
3. Research in Organisations

Question		**Yes/No**
1	Will the research involve working with/within an organisation (e.g. school, business, charity, museum, government department, international agency, etc.)?	No
2	If you answered YES to question 1, do you have granted access to conduct the research? *If YES, students please show evidence to your supervisor. PI should retain safely.*	
3	If you answered NO to question 2, is it because: A. you have not yet asked B. you have asked and not yet received an answer C. you have asked and been refused access. *Note You will only be able to start the research when you have been granted access.*	

4. Research with Products and Artefacts

Question		Yes/ No
1.	Will the research involve working with copyrighted documents, films, broadcasts, photographs, artworks, designs, products, programmes, databases, networks, processes, existing datasets or secure data?	No
2.	If you answered YES to question 1, are the materials you intend to use in the public domain? *Notes 'In the public domain' does not mean the same thing as 'publicly accessible'.* − *Information which is 'in the public domain' is no longer protected by copyright (i.e. copyright has either expired or been waived) and can be used without permission.* − *Information which is 'publicly accessible' (e.g. TV broadcasts, websites, artworks, newspapers) is available for anyone to consult/view. It is still protected by copyright even if there is no copyright notice. In UK law, copyright protection is automatic and does not require a copyright statement, although it is always good practice to provide one. It is necessary to check the terms and conditions of use to find out exactly how the material may be reused etc.* *If you answered YES to question 1, be aware that you may need to consider other ethics codes. For example, when conducting Internet research, consult the code of the Association of Internet Researchers; for educational research, consult the Code of Ethics of the British Educational Research Association.*	
3.	If you answered NO to question 2, do you have explicit permission to use these materials as data? *If YES, please show evidence to your supervisor. PI should retain permission.*	
4.	If you answered NO to question 3, is it because: A. you have not yet asked permission B. you have asked and not yet received and answer C. you have asked and been refused access. *Note You will only be able to start the research when you have been granted permission to use the specified material.*	

Adherence to SHU policy and procedures

Personal statement	
I can confirm that: − I have read the Sheffield Hallam University Research Ethics Policy and Procedures − I agree to abide by its principles.	
Student / Researcher/ Principal Investigator (as applicable)	
Name: Megan Bean	Date: 17/06/16
Signature: 	
Supervisor or other person giving ethical sign-off	
I can confirm that completion of this form has not identified the need for ethical approval by the FREC or an NHS, Social Care or other external REC. The research will not commence until any approvals required under Sections 3 & 4 have been received.	
Name: Dr Katie Cutts	Date: 21st June 2016
Signature: 	
Additional Signature if required:	
Name: Dr John Reidy	Date:28th June 2016
Signature: 	

Please ensure the following are included with this form if applicable, tick box to indicate:

	Yes	No	N/A
Research proposal if prepared previously	☐	☐	☒
Any recruitment materials (e.g. posters, letters, etc.)	☒	☐	☐
Participant information sheet	☒	☐	☐
Participant consent form	☒	☐	☐
Details of measures to be used (e.g. questionnaires, etc.)	☒	☐	☐
Outline interview schedule / focus group schedule	☐	☐	☒
Debriefing materials	☒	☐	☐
Health and Safety Project Safety Plan for Procedures	☐	☐	☒
Data Management Plan*	☒	☐	

If you have not already done so, please send a copy of your Data management Plan to rdm@shu.ac.uk
It will be used to tailor support and make sure enough data storage will be available for your data.
Completed form to be sent to Relevant FREC. Contact details on the website.

Proposed Methodology

The current study will be a partial replication of the work of King (2001). It will use an online methodology. The study will be advertised using the advertisement attached, and will be placed online (on University research participation sites), and if individuals are interested in taking part they should e-mail the researcher using the e-mail displayed on the advertisement. Participants will contact the researcher if they wish to take part in the study. They will then be sent a series of 6 links to their e-mail address. These links will direct them to study materials, presented on Qualtrics. The first link will be sent to participants on Day 1 of the study, and will contain an information sheet and consent form (attached), as well as the following baseline measures:

- Physical health: Physical Symptoms Inventory (PSI; Spector & Jex, 1998).
- Psychological wellbeing: Satisfaction with Life Scale (SWLS; Diener, Emmons, Larsen & Griffin, 1985) and Life Orientation Test (LOT; Scheier & Carver,1985).
- Behavioural self-regulation: Short Self Regulation Questionnaire (SSRQ; Carey, Neal & Collins, 2004).
- Future orientation: Future Orientation Scale (FOS; Crespo, Jose, Kielikowski & Pryor, 2013).
- Affect: Positive and Negative Affectivity Scale (PANAS; Watson, Clark & Tellegen, 1988).

Upon completion of these measures, participants will be asked to type for 20 minutes about their best possible future self or a neutral control topic. Immediately following this they will complete the PANAS again.

The second, third and fourth links will contain an identical typing task to the one presented in the first link. Participants will also be asked to complete the PANAS both immediately before and immediately after the writing task on each day. The first, second, third and fourth links will be sent to participants over 4 consecutive days.

Participants will then be asked to complete 2 follow-ups, which will contain the measures used at baseline prior to the first 20-minute typing task. Follow-up links will be sent to participants 4 and 8 weeks following the final writing session in order to explore for how long effects are maintained, and to capture any effects which may have a latent onset. At the end of the first 5 links, participants will be briefed about what is expected of them in the next link. They will be reminded at the start of each link that their participation is voluntary and that they do not have to answer any questions, and can withdraw from the study if they wish. At the end of the final link, a debrief sheet will be presented (attached).

Questionnaires will be scored based on published criteria.

Analyses for the proposed study will predominantly constitute ANOVA- type and mediation analyses.

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A.12.2 Data Management Plan

MY PLAN (SHU TEMPLATE FOR DOCTORAL STUDENTS)

DMP TITLE

ADMIN DETAILS

Project Name: Writing about a best possible future self for 20 minutes on 4 consecutive days: The role of self-regulation in the production of health benefits.

Principal Investigator / Researcher: Megan Bean

Project Data Contact: a9022330@my.shu.ac.uk

Description: Data being collected for a study which will form part of my PhD programme of research.

Institution: Sheffield Hallam University

DATA COLLECTION

What data will be produced?

Data will be quantitative. This will be responses to online surveys. The surveys will be displayed and responses will be recorded using a password-protected Qualtrics account. Data will be downloaded from Qualtrics into an SPSS data file. Once data have been downloaded from Qualtrics into an SPSS file, it will be deleted from the Qualtrics software.

DATA DOCUMENTATION

How will your data be documented and described?

All quantitative data from the online surveys along with quantitative data from the text analysis will be saved in a single SPSS file. This file will be named: MB_writing_futureself.selfregulation.date.

Clear labels will be assigned to variables in SPSS. An accompanying word document will also be provided, which will describe data processing such as questionnaire scoring procedures, missing data analyses and data transformations.

ETHICAL AND COPYRIGHT ISSUES

How will you deal with any ethical and copyright issues?

Ethical issues

Prior to being able to access any of the online study materials, participants will be presented with an information sheet. This will provide them with information regarding confidentiality, anonymity, rights to withdraw during the study, and rights to withdraw any data up to 7 days after they provide it. The information sheet will also detail the storage and disposal of data, including informing

participant that data may be used by a third party following completion of the PhD, and that data will be stored in the University's research data archive for 10 years following the last request for access by a third party. The researcher's e-mail address will be provided and participants will be assured that they are able to use this to ask any questions that they have prior to commencing the study or at any point during or following their participation. Participants will then be asked to check a box to provide their informed consent to voluntarily participate in the study, and to state that they have read and understood the information sheet, and have received satisfactory answers to any questions that they may have had. Until the box has been checked, participants will not be able to access the study materials or provide any data.

Copyright issues

- Sheffield Hallam University will have ownership all of the primary data.
- The researcher, Megan Bean, will own the PhD thesis.
- Copyright for any published research from this data authored by the researcher and her PhD supervisors will be owned by the researcher and her PhD supervisors; Dr. John Reidy and Dr. Katie Cutts.

DATA STORAGE

How will your data be structured, stored and backed up?

Backup copies of the SPSS data file will be created and stored safely on an encrypted UBS memory stick. A master copy of the data file will also be stored on Sheffield Hallam University's research store (Q:\Research drive). This is backed up daily and is fully recoverable. Access to the data file in the research store will be restricted to the researcher and her PhD supervisors only.

The researcher will be responsible for ensuring back up and recovery of data.

DATA PRESERVATION

What are the plans for the long-term preservation of data supporting your research?

At the end of my PhD, all of the data will be registered and placed in the University's research data archive (SHURDA) along with associated documentation. It will be stored in SHURDA for 10 years following any requests by a third party to access the data. The data stored in SHURDA will be linked to any publications that arise from it during my studies, which will be stored in SHURA.

DATA SHARING

What are your plans for data sharing after submission of your thesis?

At the end of the PhD, data will be deposited in SHURDA. It will then be accessible to legitimate researchers.

The Creative Commons Attribution license will be attached to the data; legitimate researchers will be able to use the data but must acknowledge Megan Bean, Dr. Katie Cutts, and Dr. John Reidy for their work in producing the data.

A.12.3 Approval letter

Sheffield Hallam University

Our Ref AM/SW/273-BEA

Ms M Bean
12 Trent Port Road
Marton
Gainsborough
DN21 5AP

19th July 2016

Dear Megan

Request for Ethical Approval of Research Project

Your research ethics checklist (SHUREC1) entitled "**Writing about a best possible future self for 20 minutes on 4 consecutive days: The role of self-regulation in the production of health benefits**" has been submitted for ethical review to the Faculty's rapporteurs and I am pleased to confirm that they have approved your project.

I wish you every success with your research project.

Yours sincerely

Professor A Macaskill
Chair
Faculty Research Ethics Committee

Office address :
Business Support Team
Faculty of Development & Society
Sheffield Hallam University
Unit 4, Sheffield Science Park
Howard Street, Sheffield, S1 1WB
Tel: 0114-225 3308 E-mail: DS-ResearchEthics@shu.ac.uk

A.13 Study Two SPSS output from main analyses

A.13.1 Immediate effects

A.13.1.1 Positive affect

ANOVA

Within-Subjects Factors

Measure: MEASURE_1

Day	Pre_post	Dependent Variable
1	1	T1_positive_affect_pre
	2	T1_positive_affect_post
2	1	D2_positive_affect_pre
	2	D2_positive_affect_post
3	1	D3_positive_affect_pre
	2	D3_positive_affect_post
4	1	D4_positive_affect_pre
	2	D4_positive_affect_post

Between-Subjects Factors

		Value Label	N
Group	1.00	experimental	18
	2.00	control	19

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
Day	Pillai's Trace	.135	1.714[b]	3.000	33.000	.183	.135
	Wilks' Lambda	.865	1.714[b]	3.000	33.000	.183	.135
	Hotelling's Trace	.156	1.714[b]	3.000	33.000	.183	.135
	Roy's Largest Root	.156	1.714[b]	3.000	33.000	.183	.135
Day * Group	Pillai's Trace	.082	.981[b]	3.000	33.000	.413	.082
	Wilks' Lambda	.918	.981[b]	3.000	33.000	.413	.082
	Hotelling's Trace	.089	.981[b]	3.000	33.000	.413	.082
	Roy's Largest Root	.089	.981[b]	3.000	33.000	.413	.082
Pre_post	Pillai's Trace	.038	1.378[b]	1.000	35.000	.248	.038
	Wilks' Lambda	.962	1.378[b]	1.000	35.000	.248	.038
	Hotelling's Trace	.039	1.378[b]	1.000	35.000	.248	.038
	Roy's Largest Root	.039	1.378[b]	1.000	35.000	.248	.038
Pre_post * Group	Pillai's Trace	.155	6.399[b]	1.000	35.000	.016	.155
	Wilks' Lambda	.845	6.399[b]	1.000	35.000	.016	.155
	Hotelling's Trace	.183	6.399[b]	1.000	35.000	.016	.155
	Roy's Largest Root	.183	6.399[b]	1.000	35.000	.016	.155
Day * Pre_post	Pillai's Trace	.189	2.568[b]	3.000	33.000	.071	.189
	Wilks' Lambda	.811	2.568[b]	3.000	33.000	.071	.189
	Hotelling's Trace	.233	2.568[b]	3.000	33.000	.071	.189
	Roy's Largest Root	.233	2.568[b]	3.000	33.000	.071	.189
Day * Pre_post * Group	Pillai's Trace	.112	1.391[b]	3.000	33.000	.263	.112
	Wilks' Lambda	.888	1.391[b]	3.000	33.000	.263	.112
	Hotelling's Trace	.126	1.391[b]	3.000	33.000	.263	.112
	Roy's Largest Root	.126	1.391[b]	3.000	33.000	.263	.112

a. Design: Intercept + Group

Within Subjects Design: Day + Pre_post + Day * Pre_post

b. Exact statistic

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
Day	.897	3.650	5	.601	.929	1.000	.333
Pre_post	1.000	.000	0	.	1.000	1.000	1.000
Day * Pre_post	.572	18.840	5	.002	.718	.788	.333

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + Group

Within Subjects Design: Day + Pre_post + Day * Pre_post b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Day	Sphericity Assumed	222.503	3	74.168	1.855	.142	.050
	Greenhouse-Geisser	222.503	2.786	79.854	1.855	.146	.050
	Huynh-Feldt	222.503	3.000	74.168	1.855	.142	.050
	Lower-bound	222.503	1.000	222.503	1.855	.182	.050
Day * Group	Sphericity Assumed	109.449	3	36.483	.913	.438	.025
	Greenhouse-Geisser	109.449	2.786	39.280	.913	.432	.025
	Huynh-Feldt	109.449	3.000	36.483	.913	.438	.025
	Lower-bound	109.449	1.000	109.449	.913	.346	.025
Error(Day)	Sphericity Assumed	4197.572	105	39.977			
	Greenhouse-Geisser	4197.572	97.523	43.042			
	Huynh-Feldt	4197.572	105.000	39.977			
	Lower-bound	4197.572	35.000	119.931			
Pre_post	Sphericity Assumed	69.370	1	69.370	1.378	.248	.038
	Greenhouse-Geisser	69.370	1.000	69.370	1.378	.248	.038
	Huynh-Feldt	69.370	1.000	69.370	1.378	.248	.038
	Lower-bound	69.370	1.000	69.370	1.378	.248	.038
Pre_post * Group	Sphericity Assumed	322.073	1	322.073	6.399	.016	.155
	Greenhouse-Geisser	322.073	1.000	322.073	6.399	.016	.155
	Huynh-Feldt	322.073	1.000	322.073	6.399	.016	.155
	Lower-bound	322.073	1.000	322.073	6.399	.016	.155
Error(Pre_post)	Sphericity Assumed	1761.731	35	50.335			
	Greenhouse-Geisser	1761.731	35.000	50.335			
	Huynh-Feldt	1761.731	35.000	50.335			
	Lower-bound	1761.731	35.000	50.335			
Day * Pre_post	Sphericity Assumed	43.879	3	14.626	2.039	.113	.055
	Greenhouse-Geisser	43.879	2.154	20.372	2.039	.134	.055
	Huynh-Feldt	43.879	2.365	18.551	2.039	.128	.055
	Lower-bound	43.879	1.000	43.879	2.039	.162	.055
Day * Pre_post * Group	Sphericity Assumed	22.744	3	7.581	1.057	.371	.029
	Greenhouse-Geisser	22.744	2.154	10.559	1.057	.357	.029
	Huynh-Feldt	22.744	2.365	9.615	1.057	.361	.029
	Lower-bound	22.744	1.000	22.744	1.057	.311	.029
Error(Day*Pre_post)	Sphericity Assumed	753.155	105	7.173			
	Greenhouse-Geisser	753.155	75.385	9.991			
	Huynh-Feldt	753.155	82.788	9.097			
	Lower-bound	753.155	35.000	21.519			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	Day	Pre_post	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Day	Linear		216.739	1	216.739	4.771	.036	.120
	Quadratic		4.676	1	4.676	.130	.721	.004
	Cubic		1.088	1	1.088	.028	.868	.001
Day * Group	Linear		.166	1	.166	.004	.952	.000
	Quadratic		.216	1	.216	.006	.939	.000
	Cubic		109.066	1	109.066	2.828	.102	.075
Error(Day)	Linear		1589.887	35	45.425			
	Quadratic		1258.034	35	35.944			
	Cubic		1349.651	35	38.561			
Pre_post		Linear	69.370	1	69.370	1.378	.248	.038
Pre_post * Group		Linear	322.073	1	322.073	6.399	.016	.155
Error(Pre_post)		Linear	1761.731	35	50.335			
Day * Pre_post	Linear	Linear	26.639	1	26.639	2.081	.158	.056
	Quadratic	Linear	15.833	1	15.833	3.095	.087	.081
	Cubic	Linear	1.407	1	1.407	.391	.536	.011
Day * Pre_post * Group	Linear	Linear	12.190	1	12.190	.952	.336	.026
	Quadratic	Linear	10.157	1	10.157	1.985	.168	.054
	Cubic	Linear	.397	1	.397	.110	.742	.003
Error(Day*Pre_post)	Linear	Linear	448.092	35	12.803			
	Quadratic	Linear	179.053	35	5.116			
	Cubic	Linear	126.010	35	3.600			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	153125.948	1	153125.948	303.240	.000	.897
Group	16.084	1	16.084	.032	.859	.001
Error	17673.788	35	504.965			

Follow-up of significant pre- or post-writing * group interaction

Differences in positive affect between pre- and post- writing, in BPFS and control groups, separately: Paired samples t-tests

BPFS group

Paired Samples Statistics[a]

		Mean	N	Std. Deviation	Std. Error Mean
Pair 1	Positive_affect_pre_average	21.4583	18	5.14657	1.21306
	Positive_affect_post_average	24.5139	18	8.41413	1.98323

a. Group = experimental

Paired Samples Correlations[a]

		N	Correlation	Sig.
Pair 1	Positive_affect_pre_average & Positive_affect_post_average	18	.580	.012

a. Group = experimental

Paired Samples Test[a]

		Paired Differences					t	df	Sig. (2-tailed)
		Mean	Std. Deviation	Std. Error Mean	95% Confidence Interval of the Difference Lower	Upper			
Pair 1	Positive_affect_pre_average - Positive_affect_post_average	-3.05556	6.85702	1.61622	-6.46547	.35436	-1.891	17	.076

a. Group = experimental

Control group

Paired Samples Statistics[a]

		Mean	N	Std. Deviation	Std. Error Mean
Pair 1	Positive_affect_pre_average	23.0789	19	9.37863	2.15161
	Positive_affect_post_average	21.9605	19	9.49205	2.17763

a. Group = control

Paired Samples Correlations[a]

		N	Correlation	Sig.
Pair 1	Positive_affect_pre_average & Positive_affect_post_average	19	.975	.000

a. Group = control

Paired Samples Test[a]

		Paired Differences					t	df	Sig. (2-tailed)
		Mean	Std. Deviation	Std. Error Mean	95% Confidence Interval of the Difference Lower	Upper			
Pair 1	Positive_affect_pre_average - Positive_affect_post_average	1.11842	2.12846	.48830	.09254	2.14431	2.290	18	.034

a. Group = control

Differences in positive affect between BPFS and control groups, at pre- and post- writing time points, separately: Independent samples t-tests

Pre-writing

Group Statistics

	Group	N	Mean	Std. Deviation	Std. Error Mean
Positive_affect_pre_average	Experimental	18	21.4583	5.14657	1.21306
	Control	19	23.0789	9.37863	2.15161

Independent Samples Test

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	T	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
Positive_affect_pre_average	Equal variances assumed	9.682	.004	-.646	35	.522	-1.62061	2.50715	-6.71039	3.46917
	Equal variances not assumed			-.656	28.240	.517	-1.62061	2.47000	-6.67824	3.43702

Post-writing

Group Statistics

	Group	N	Mean	Std. Deviation	Std. Error Mean
Positive_affect_post_average	Experimental	18	24.5139	8.41413	1.98323
	Control	19	21.9605	9.49205	2.17763

Independent Samples Test

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	T	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
Positive_affect_post_average	Equal variances assumed	.263	.611	.864	35	.393	2.55336	2.95522	-3.44605	8.55277
	Equal variances not assumed			.867	34.854	.392	2.55336	2.94538	-3.42697	8.53369

A.13.1.2 Negative affect

ANOVA

Within-Subjects Factors

Measure: MEASURE_1

Day	Pre_post	Dependent Variable
1	1	T1_negative_affect_pre_REC
	2	T1_negative_affect_post_REC
2	1	D2_negative_affect_pre_REC
	2	D2_negative_affect_post_REC
3	1	D3_negative_affect_pre_REC
	2	D3_negative_affect_post_REC
4	1	D4_negative_affect_pre_REC
	2	D4_negative_affect_post_REC2

Between-Subjects Factors

		Value Label	N
Group	1.00	experimental	18
	2.00	control	19

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
Day	Pillai's Trace	.287	4.422[b]	3.000	33.000	.010	.287
	Wilks' Lambda	.713	4.422[b]	3.000	33.000	.010	.287
	Hotelling's Trace	.402	4.422[b]	3.000	33.000	.010	.287
	Roy's Largest Root	.402	4.422[b]	3.000	33.000	.010	.287
Day * Group	Pillai's Trace	.065	.762[b]	3.000	33.000	.524	.065
	Wilks' Lambda	.935	.762[b]	3.000	33.000	.524	.065
	Hotelling's Trace	.069	.762[b]	3.000	33.000	.524	.065
	Roy's Largest Root	.069	.762[b]	3.000	33.000	.524	.065
Pre_post	Pillai's Trace	.023	.806[b]	1.000	35.000	.375	.023
	Wilks' Lambda	.977	.806[b]	1.000	35.000	.375	.023
	Hotelling's Trace	.023	.806[b]	1.000	35.000	.375	.023
	Roy's Largest Root	.023	.806[b]	1.000	35.000	.375	.023
Pre_post * Group	Pillai's Trace	.024	.845[b]	1.000	35.000	.364	.024
	Wilks' Lambda	.976	.845[b]	1.000	35.000	.364	.024
	Hotelling's Trace	.024	.845[b]	1.000	35.000	.364	.024
	Roy's Largest Root	.024	.845[b]	1.000	35.000	.364	.024
Day * Pre_post	Pillai's Trace	.020	.224[b]	3.000	33.000	.879	.020
	Wilks' Lambda	.980	.224[b]	3.000	33.000	.879	.020
	Hotelling's Trace	.020	.224[b]	3.000	33.000	.879	.020
	Roy's Largest Root	.020	.224[b]	3.000	33.000	.879	.020
Day * Pre_post * Group	Pillai's Trace	.067	.795[b]	3.000	33.000	.505	.067
	Wilks' Lambda	.933	.795[b]	3.000	33.000	.505	.067
	Hotelling's Trace	.072	.795[b]	3.000	33.000	.505	.067
	Roy's Largest Root	.072	.795[b]	3.000	33.000	.505	.067

a. Design: Intercept + Group

Within Subjects Design: Day + Pre_post + Day * Pre_post

b. Exact statistic

Mauchly's Test of Sphericity[a]

Measure: MEASURE_1

Within Subjects Effect	Mauchly's W	Approx. Chi-Square	Df	Sig.	Epsilon[b]		
					Greenhouse-Geisser	Huynh-Feldt	Lower-bound
Day	.847	5.597	5	.348	.893	1.000	.333
Pre_post	1.000	.000	0	.	1.000	1.000	1.000
Day * Pre_post	.730	10.614	5	.060	.824	.916	.333

Tests the null hypothesis that the error covariance matrix of the orthonormalized transformed dependent variables is proportional to an identity matrix.

a. Design: Intercept + Group

Within Subjects Design: Day + Pre_post + Day * Pre_post

b. May be used to adjust the degrees of freedom for the averaged tests of significance. Corrected tests are displayed in the Tests of Within-Subjects Effects table.

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Day	Sphericity Assumed	.003	3	.001	3.696	.014	.096
	Greenhouse-Geisser	.003	2.678	.001	3.696	.018	.096
	Huynh-Feldt	.003	3.000	.001	3.696	.014	.096
	Lower-bound	.003	1.000	.003	3.696	.063	.096
Day * Group	Sphericity Assumed	.001	3	.000	1.121	.344	.031
	Greenhouse-Geisser	.001	2.678	.000	1.121	.341	.031
	Huynh-Feldt	.001	3.000	.000	1.121	.344	.031
	Lower-bound	.001	1.000	.001	1.121	.297	.031
Error(Day)	Sphericity Assumed	.024	105	.000			
	Greenhouse-Geisser	.024	93.718	.000			
	Huynh-Feldt	.024	105.000	.000			
	Lower-bound	.024	35.000	.001			
Pre_post	Sphericity Assumed	.000	1	.000	.806	.375	.023
	Greenhouse-Geisser	.000	1.000	.000	.806	.375	.023
	Huynh-Feldt	.000	1.000	.000	.806	.375	.023
	Lower-bound	.000	1.000	.000	.806	.375	.023
Pre_post * Group	Sphericity Assumed	.000	1	.000	.845	.364	.024
	Greenhouse-Geisser	.000	1.000	.000	.845	.364	.024
	Huynh-Feldt	.000	1.000	.000	.845	.364	.024
	Lower-bound	.000	1.000	.000	.845	.364	.024
Error(Pre_post)	Sphericity Assumed	.006	35	.000			
	Greenhouse-Geisser	.006	35.000	.000			
	Huynh-Feldt	.006	35.000	.000			
	Lower-bound	.006	35.000	.000			
Day * Pre_post	Sphericity Assumed	2.961E-5	3	9.870E-6	.174	.914	.005
	Greenhouse-Geisser	2.961E-5	2.471	1.198E-5	.174	.882	.005
	Huynh-Feldt	2.961E-5	2.749	1.077E-5	.174	.900	.005
	Lower-bound	2.961E-5	1.000	2.961E-5	.174	.679	.005
Day * Pre_post * Group	Sphericity Assumed	.000	3	3.838E-5	.676	.569	.019
	Greenhouse-Geisser	.000	2.471	4.660E-5	.676	.541	.019
	Huynh-Feldt	.000	2.749	4.188E-5	.676	.556	.019
	Lower-bound	.000	1.000	.000	.676	.417	.019
Error(Day*Pre_post)	Sphericity Assumed	.006	105	5.677E-5			
	Greenhouse-Geisser	.006	86.477	6.893E-5			
	Huynh-Feldt	.006	96.210	6.196E-5			
	Lower-bound	.006	35.000	.000			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	Day	Pre_post	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Day	Linear		7.521E-6	1	7.521E-6	.026	.872	.001
	Quadratic		.002	1	.002	8.373	.007	.193
	Cubic		.001	1	.001	4.569	.040	.115
Day * Group	Linear		.000	1	.000	1.373	.249	.038
	Quadratic		9.157E-6	1	9.157E-6	.050	.824	.001
	Cubic		.000	1	.000	1.695	.201	.046
Error(Day)	Linear		.010	35	.000			
	Quadratic		.006	35	.000			
	Cubic		.008	35	.000			
Pre_post		Linear	.000	1	.000	.806	.375	.023
Pre_post * Group		Linear	.000	1	.000	.845	.364	.024
Error(Pre_post)		Linear	.006	35	.000			
Day * Pre_post	Linear	Linear	2.936E-5	1	2.936E-5	.575	.453	.016
	Quadratic	Linear	2.386E-7	1	2.386E-7	.003	.954	.000
	Cubic	Linear	9.559E-9	1	9.559E-9	.000	.989	.000
Day * Pre_post * Group	Linear	Linear	8.973E-5	1	8.973E-5	1.757	.194	.048
	Quadratic	Linear	8.511E-7	1	8.511E-7	.012	.913	.000
	Cubic	Linear	2.454E-5	1	2.454E-5	.506	.482	.014
Error(Day*Pre_post)	Linear	Linear	.002	35	5.107E-5			
	Quadratic	Linear	.002	35	7.075E-5			
	Cubic	Linear	.002	35	4.850E-5			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	1.819	1	1.819	948.878	.000	.964
Group	.001	1	.001	.319	.576	.009
Error	.067	35	.002			

Post-hoc analysis of significant main effect of day: Pairwise comparisons

1. Grand Mean

Measure: MEASURE_1

Mean	Std. Error	95% Confidence Interval	
		Lower Bound	Upper Bound
.078	.003	.073	.084

Estimates

Measure: MEASURE_1

Day	Mean	Std. Error	95% Confidence Interval	
			Lower Bound	Upper Bound
1	.076	.003	.069	.082
2	.083	.003	.078	.089
3	.078	.003	.072	.085
4	.077	.003	.071	.083

Pairwise Comparisons

Measure: MEASURE_1

(I) Day	(J) Day	Mean Difference (I-J)	Std. Error	Sig.[b]	95% Confidence Interval for Difference[b]	
					Lower Bound	Upper Bound
1	2	-.008*	.002	.001	-.012	-.003
	3	-.003	.003	.338	-.008	.003
	4	-.001	.002	.632	-.006	.004
2	1	.008*	.002	.001	.003	.012
	3	.005	.003	.071	.000	.011
	4	.006*	.003	.017	.001	.012
3	1	.003	.003	.338	-.003	.008
	2	-.005	.003	.071	-.011	.000
	4	.001	.002	.525	-.003	.006
4	1	.001	.002	.632	-.004	.006
	2	-.006*	.003	.017	-.012	-.001
	3	-.001	.002	.525	-.006	.003

Based on estimated marginal means

*. The mean difference is significant at the .05 level.

b. Adjustment for multiple comparisons: Least Significant Difference (equivalent to no adjustments).

Multivariate Tests

	Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
Pillai's trace	.287	4.422[a]	3.000	33.000	.010	.287
Wilks' lambda	.713	4.422[a]	3.000	33.000	.010	.287
Hotelling's trace	.402	4.422[a]	3.000	33.000	.010	.287
Roy's largest root	.402	4.422[a]	3.000	33.000	.010	.287

Each F tests the multivariate effect of Day. These tests are based on the linearly independent pairwise comparisons among the estimated marginal means.

a. Exact statistic

A.13.2 Long-term effects

A.13.2.1 Psychological well-being

A.13.2.1.1 Psychological well-being composite

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	T2_PSYCH_WELL
2	T3_PSYCH_WELL

Between-Subjects Factors

		Value Label	N
Group	1.00	experimental	17
	2.00	control	14

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.003	.078[b]	1.000	28.000	.782	.003
	Wilks' Lambda	.998	.078[b]	1.000	28.000	.782	.003
	Hotelling's Trace	.003	.078[b]	1.000	28.000	.782	.003
	Roy's Largest Root	.003	.078[b]	1.000	28.000	.782	.003
time_point * T1_PSYCH_WELL	Pillai's Trace	.005	.133[b]	1.000	28.000	.718	.005
	Wilks' Lambda	.995	.133[b]	1.000	28.000	.718	.005
	Hotelling's Trace	.005	.133[b]	1.000	28.000	.718	.005
	Roy's Largest Root	.005	.133[b]	1.000	28.000	.718	.005
time_point * Group	Pillai's Trace	.002	.066[b]	1.000	28.000	.799	.002
	Wilks' Lambda	.998	.066[b]	1.000	28.000	.799	.002
	Hotelling's Trace	.002	.066[b]	1.000	28.000	.799	.002
	Roy's Largest Root	.002	.066[b]	1.000	28.000	.799	.002

a. Design: Intercept + T1_PSYCH_WELL + Group

Within Subjects Design: time_point

b. Exact statistic

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	.013	1	.013	.078	.782	.003
	Greenhouse-Geisser	.013	1.000	.013	.078	.782	.003
	Huynh-Feldt	.013	1.000	.013	.078	.782	.003
	Lower-bound	.013	1.000	.013	.078	.782	.003
time_point * T1_PSYCH_WELL	Sphericity Assumed	.023	1	.023	.133	.718	.005
	Greenhouse-Geisser	.023	1.000	.023	.133	.718	.005
	Huynh-Feldt	.023	1.000	.023	.133	.718	.005
	Lower-bound	.023	1.000	.023	.133	.718	.005
time_point * Group	Sphericity Assumed	.011	1	.011	.066	.799	.002
	Greenhouse-Geisser	.011	1.000	.011	.066	.799	.002
	Huynh-Feldt	.011	1.000	.011	.066	.799	.002
	Lower-bound	.011	1.000	.011	.066	.799	.002
Error(time_point)	Sphericity Assumed	4.819	28	.172			
	Greenhouse-Geisser	4.819	28.000	.172			
	Huynh-Feldt	4.819	28.000	.172			
	Lower-bound	4.819	28.000	.172			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.013	1	.013	.078	.782	.003
time_point * T1_PSYCH_WELL	Linear	.023	1	.023	.133	.718	.005
time_point * Group	Linear	.011	1	.011	.066	.799	.002
Error(time_point)	Linear	4.819	28	.172			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	.050	1	.050	.088	.769	.003
T1_PSYCH_WELL	22.452	1	22.452	39.106	.000	.583
Group	.007	1	.007	.012	.912	.000
Error	16.076	28	.574			

A.13.2.1.2 Optimism

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	T2_LOT
2	T3_LOT

Between-Subjects Factors

		Value Label	N
Group	1.00	Experimental	17
	2.00	Control	14

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time	Pillai's Trace	.008	.235[b]	1.000	28.000	.632	.008
	Wilks' Lambda	.992	.235[b]	1.000	28.000	.632	.008
	Hotelling's Trace	.008	.235[b]	1.000	28.000	.632	.008
	Roy's Largest Root	.008	.235[b]	1.000	28.000	.632	.008
time * T1_LOT	Pillai's Trace	.005	.132[b]	1.000	28.000	.719	.005
	Wilks' Lambda	.995	.132[b]	1.000	28.000	.719	.005
	Hotelling's Trace	.005	.132[b]	1.000	28.000	.719	.005
	Roy's Largest Root	.005	.132[b]	1.000	28.000	.719	.005
time * Group	Pillai's Trace	.028	.818[b]	1.000	28.000	.373	.028
	Wilks' Lambda	.972	.818[b]	1.000	28.000	.373	.028
	Hotelling's Trace	.028	.818[b]	1.000	28.000	.373	.028
	Roy's Largest Root	.028	.818[b]	1.000	28.000	.373	.028

a. Design: Intercept + T1_LOT + Group

Within Subjects Design: time

b. Exact statistic

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time	Sphericity Assumed	.779	1	.779	.235	.632	.008
	Greenhouse-Geisser	.779	1.000	.779	.235	.632	.008
	Huynh-Feldt	.799	1.000	.779	.235	.632	.008
	Lower-bound	.799	1.000	.779	.235	.632	.008
time * T1_LOT	Sphericity Assumed	.439	1	.439	.132	.719	.005
	Greenhouse-Geisser	.439	1.000	.439	.132	.719	.005
	Huynh-Feldt	.439	1.000	.439	.132	.719	.005
	Lower-bound	.439	1.000	.439	.132	.719	.005
time * Group	Sphericity Assumed	2.714	1	2.714	.818	.373	.028
	Greenhouse-Geisser	2.714	1.000	2.714	.818	.373	.028
	Huynh-Feldt	2.714	1.000	2.714	.818	.373	.028
	Lower-bound	2.714	1.000	2.714	.818	.373	.028
Error(time)	Sphericity Assumed	92.872	28	3.317			
	Greenhouse-Geisser	92.872	28.000	3.317			
	Huynh-Feldt	92.872	28.000	3.317			
	Lower-bound	92.872	28.000	3.317			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time	Linear	.779	1	.779	.235	.632	.008
time * T1_LOT	Linear	.439	1	.439	.132	.719	.005
time * Group	Linear	2.714	1	2.714	.818	.373	.028
Error(time)	Linear	92.872	28	3.317			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	81.741	1	81.741	8.868	.006	.241
T1_LOT	329.286	1	329.286	35.725	.000	.561
Group	.394	1	.394	.043	.838	.002
Error	258.084	28	9.217			

A.13.2.1.3 Satisfaction with life

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time	Dependent Variable
1	T2_SWL
2	T3_SWL

Between-Subjects Factors

		Value Label	N
Group	1.00	experimental	17
	2.00	control	14

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time	Pillai's Trace	.036	1.043[b]	1.000	28.000	.316	.036
	Wilks' Lambda	.964	1.043[b]	1.000	28.000	.316	.036
	Hotelling's Trace	.037	1.043[b]	1.000	28.000	.316	.036
	Roy's Largest Root	.037	1.043[b]	1.000	28.000	.316	.036
time * T1_SWL	Pillai's Trace	.007	.202[b]	1.000	28.000	.656	.007
	Wilks' Lambda	.993	.202[b]	1.000	28.000	.656	.007
	Hotelling's Trace	.007	.202[b]	1.000	28.000	.656	.007
	Roy's Largest Root	.007	.202[b]	1.000	28.000	.656	.007
time * Group	Pillai's Trace	.010	.277[b]	1.000	28.000	.603	.010
	Wilks' Lambda	.990	.277[b]	1.000	28.000	.603	.010
	Hotelling's Trace	.010	.277[b]	1.000	28.000	.603	.010
	Roy's Largest Root	.010	.277[b]	1.000	28.000	.603	.010

a. Design: Intercept + T1_SWL + Group

Within Subjects Design: time

b. Exact statistic

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time	Sphericity Assumed	7.978	1	7.978	1.043	.316	.036
	Greenhouse-Geisser	7.978	1.000	7.978	1.043	.316	.036
	Huynh-Feldt	7.978	1.000	7.978	1.043	.316	.036
	Lower-bound	7.978	1.000	7.978	1.043	.316	.036
time * T1_SWL	Sphericity Assumed	1.546	1	1.546	.202	.656	.007
	Greenhouse-Geisser	1.546	1.000	1.546	.202	.656	.007
	Huynh-Feldt	1.546	1.000	1.546	.202	.656	.007
	Lower-bound	1.546	1.000	1.546	.202	.656	.007
time * Group	Sphericity Assumed	2.121	1	2.121	.277	.603	.010
	Greenhouse-Geisser	2.121	1.000	2.121	.277	.603	.010
	Huynh-Feldt	2.121	1.000	2.121	.277	.603	.010
	Lower-bound	2.121	1.000	2.121	.277	.603	.010
Error(time)	Sphericity Assumed	214.118	28	7.647			
	Greenhouse-Geisser	214.118	28.000	7.647			
	Huynh-Feldt	214.118	28.000	7.647			
	Lower-bound	214.118	28.000	7.647			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time	Linear	7.978	1	7.978	1.043	.316	.036
time * T1_SWL	Linear	1.546	1	1.546	.202	.656	.007
time * Group	Linear	2.121	1	2.121	.277	.603	.010
Error(time)	Linear	214.118	28	7.647			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	115.523	1	115.523	2.851	.102	.092
T1_SWL	901.026	1	901.026	22.234	.000	.443
Group	.233	1	.233	.006	.940	.000
Error	1134.688	28	40.525			

A.13.2.2 Physical symptoms

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	T2_PSI
2	T3_PSI

Between-Subjects Factors

		Value Label	N
Group	1.00	experimental	17
	2.00	control	14

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.000	.006[b]	1.000	28.000	.939	.000
	Wilks' Lambda	1.000	.006[b]	1.000	28.000	.939	.000
	Hotelling's Trace	.000	.006[b]	1.000	28.000	.939	.000
	Roy's Largest Root	.000	.006[b]	1.000	28.000	.939	.000
time_point * T1_PSI	Pillai's Trace	.004	.102[b]	1.000	28.000	.752	.004
	Wilks' Lambda	.996	.102[b]	1.000	28.000	.752	.004
	Hotelling's Trace	.004	.102[b]	1.000	28.000	.752	.004
	Roy's Largest Root	.004	.102[b]	1.000	28.000	.752	.004
time_point * Group	Pillai's Trace	.002	.065[b]	1.000	28.000	.801	.002
	Wilks' Lambda	.998	.065[b]	1.000	28.000	.801	.002
	Hotelling's Trace	.002	.065[b]	1.000	28.000	.801	.002
	Roy's Largest Root	.002	.065[b]	1.000	28.000	.801	.002

a. Design: Intercept + T1_PSI + Group

Within Subjects Design: time_point

b. Exact statistic

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	.065	1	.065	.006	.939	.000
	Greenhouse-Geisser	.065	1.000	.065	.006	.939	.000
	Huynh-Feldt	.065	1.000	.065	.006	.939	.000
	Lower-bound	.065	1.000	.065	.006	.939	.000
time_point * T1_PSI	Sphericity Assumed	1.121	1	1.121	.102	.752	.004
	Greenhouse-Geisser	1.121	1.000	1.121	.102	.752	.004
	Huynh-Feldt	1.121	1.000	1.121	.102	.752	.004
	Lower-bound	1.121	1.000	1.121	.102	.752	.004
time_point * Group	Sphericity Assumed	.710	1	.710	.065	.801	.002
	Greenhouse-Geisser	.710	1.000	.710	.065	.801	.002
	Huynh-Feldt	.710	1.000	.710	.065	.801	.002
	Lower-bound	.710	1.000	.710	.065	.801	.002
Error(time_point)	Sphericity Assumed	307.307	28	10.975			
	Greenhouse-Geisser	307.307	28.000	10.975			
	Huynh-Feldt	307.307	28.000	10.975			
	Lower-bound	307.307	28.000	10.975			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.065	1	.065	.006	.939	.000
time_point * T1_PSI	Linear	1.121	1	1.121	.102	.752	.004
time_point * Group	Linear	.710	1	.710	.065	.801	.002
Error(time_point)	Linear	307.307	28	10.975			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	65.721	1	65.721	3.046	.092	.098
T1_PSI	1372.255	1	1372.255	63.609	.000	.694
Group	12.597	1	12.597	.584	.451	.020
Error	604.056	28	21.573			

A.13.2.3 Future orientation

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	T2_fut_orient
2	T3_fut_orient

Between-Subjects Factors

		Value Label	N
Group	1.00	experimental	17
	2.00	control	14

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	.019	1	.019	.006	.941	.000
	Greenhouse-Geisser	.019	1.000	.019	.006	.941	.000
	Huynh-Feldt	.019	1.000	.019	.006	.941	.000
	Lower-bound	.019	1.000	.019	.006	.941	.000
time_point * T1_fut_orient	Sphericity Assumed	.087	1	.087	.026	.874	.001
	Greenhouse-Geisser	.087	1.000	.087	.026	.874	.001
	Huynh-Feldt	.087	1.000	.087	.026	.874	.001
	Lower-bound	.087	1.000	.087	.026	.874	.001
time_point * Group	Sphericity Assumed	.543	1	.543	.160	.692	.006
	Greenhouse-Geisser	.543	1.000	.543	.160	.692	.006
	Huynh-Feldt	.543	1.000	.543	.160	.692	.006
	Lower-bound	.543	1.000	.543	.160	.692	.006
Error(time_point)	Sphericity Assumed	95.049	28	3.395			
	Greenhouse-Geisser	95.049	28.000	3.395			
	Huynh-Feldt	95.049	28.000	3.395			
	Lower-bound	95.049	28.000	3.395			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	.019	1	.019	.006	.941	.000
time_point * T1_fut_orient	Linear	.087	1	.087	.026	.874	.001
time_point * Group	Linear	.543	1	.543	.160	.692	.006
Error(time_point)	Linear	95.049	28	3.395			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	15.501	1	15.501	2.027	.166	.067
T1_fut_orient	415.368	1	415.368	54.308	.000	.660
Group	6.078	1	6.078	.795	.380	.028
Error	214.155	28	7.648			

A.13.2.4 Self-regulation

ANCOVA

Within-Subjects Factors

Measure: MEASURE_1

time_point	Dependent Variable
1	T2_SSRQ
2	T3_SSRQ

Between-Subjects Factors

		Value Label	N
Group	1.00	Experimental	17
	2.00	Control	14

Multivariate Tests[a]

Effect		Value	F	Hypothesis df	Error df	Sig.	Partial Eta Squared
time_point	Pillai's Trace	.013	.373[b]	1.000	28.000	.546	.013
	Wilks' Lambda	.987	.373[b]	1.000	28.000	.546	.013
	Hotelling's Trace	.013	.373[b]	1.000	28.000	.546	.013
	Roy's Largest Root	.013	.373[b]	1.000	28.000	.546	.013
time_point * T1_SSRQ	Pillai's Trace	.008	.233[b]	1.000	28.000	.633	.008
	Wilks' Lambda	.992	.233[b]	1.000	28.000	.633	.008
	Hotelling's Trace	.008	.233[b]	1.000	28.000	.633	.008
	Roy's Largest Root	.008	.233[b]	1.000	28.000	.633	.008
time_point * Group	Pillai's Trace	.000	.010[b]	1.000	28.000	.920	.000
	Wilks' Lambda	1.000	.010[b]	1.000	28.000	.920	.000
	Hotelling's Trace	.000	.010[b]	1.000	28.000	.920	.000
	Roy's Largest Root	.000	.010[b]	1.000	28.000	.920	.000

a. Design: Intercept + T1_SSRQ + Group

Within Subjects Design: time_point

b. Exact statistic

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Sphericity Assumed	15.440	1	15.440	.373	.546	.013
	Greenhouse-Geisser	15.440	1.000	15.440	.373	.546	.013
	Huynh-Feldt	15.440	1.000	15.440	.373	.546	.013
	Lower-bound	15.440	1.000	15.440	.373	.546	.013
time_point * T1_SSRQ	Sphericity Assumed	9.631	1	9.631	.233	.633	.008
	Greenhouse-Geisser	9.631	1.000	9.631	.233	.633	.008
	Huynh-Feldt	9.631	1.000	9.631	.233	.633	.008
	Lower-bound	9.631	1.000	9.631	.233	.633	.008
time_point * Group	Sphericity Assumed	.421	1	.421	.010	.920	.000
	Greenhouse-Geisser	.421	1.000	.421	.010	.920	.000
	Huynh-Feldt	.421	1.000	.421	.010	.920	.000
	Lower-bound	.421	1.000	.421	.010	.920	.000
Error(time_point)	Sphericity Assumed	1159.774	28	41.421			
	Greenhouse-Geisser	1159.774	28.000	41.421			
	Huynh-Feldt	1159.774	28.000	41.421			
	Lower-bound	1159.774	28.000	41.421			

Tests of Within-Subjects Contrasts

Measure: MEASURE_1

Source	time_point	Type III Sum of Squares	Df	Mean Square	F	Sig.	Partial Eta Squared
time_point	Linear	15.440	1	15.440	.373	.546	.013
time_point * T1_SSRQ	Linear	9.631	1	9.631	.233	.633	.008
time_point * Group	Linear	.421	1	.421	.010	.920	.000
Error(time_point)	Linear	1159.774	28	41.421			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	67.660	1	67.660	.521	.476	.018
T1_SSRQ	11493.734	1	11493.734	88.551	.000	.760
Group	11.117	1	11.117	.086	.772	.003
Error	3634.327	28	129.797			

A.14 Systematic review PRISMA checklist

Section/topic	#	Checklist item	Reported in section #
TITLE			
Title	1	Identify the report as a systematic review, meta-analysis, or both.	Title
ABSTRACT			
Structured summary	2	Provide a structured summary including, as applicable: background; objectives; data sources; study eligibility criteria, participants, and interventions; study appraisal and synthesis methods; results; limitations; conclusions and implications of key findings; systematic review registration number.	N/A
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of what is already known.	6.1
Objectives	4	Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).	6.2
METHODS			
Protocol and registration	5	Indicate if a review protocol exists, if and where it can be accessed (e.g., Web address), and, if available, provide registration information including registration number.	6.3
Eligibility criteria	6	Specify study characteristics (e.g., PICOS, length of follow-up) and report characteristics (e.g., years considered, language, publication status) used as criteria for eligibility, giving rationale.	6.3.1
Information sources	7	Describe all information sources (e.g., databases with dates of coverage, contact with study authors to identify additional studies) in the search and date last searched.	6.3.2
Search	8	Present full electronic search strategy for at least one database, including any limits used, such that it could be repeated.	A.16
Study selection	9	State the process for selecting studies (i.e., screening, eligibility, included in systematic review, and, if applicable, included in the meta-analysis).	6.3
Data collection process	10	Describe method of data extraction from reports (e.g., piloted forms, independently, in duplicate) and any processes for obtaining and confirming data from investigators.	6.3.3
Data items	11	List and define all variables for which data were sought (e.g., PICOS, funding sources) and any assumptions and simplifications made.	6.3.3
Risk of bias in individual studies	12	Describe methods used for assessing risk of bias of individual studies (including specification of whether this was done at the study or outcome level), and how this information is to be used in any data synthesis.	6.3.4
Summary measures	13	State the principal summary measures (e.g., risk ratio, difference in means).	N/A
Synthesis of results	14	Describe the methods of handling data and combining results of studies, if done, including measures of consistency (e.g., I^2) for each meta-analysis.	6.3.5

Section/topic	#	Checklist item	Reported in section #
Risk of bias across studies	15	Specify any assessment of risk of bias that may affect the cumulative evidence (e.g., publication bias, selective reporting within studies).	N/A
Additional analyses	16	Describe methods of additional analyses (e.g., sensitivity or subgroup analyses, meta-regression), if done, indicating which were pre-specified.	N/A
RESULTS			
Study selection	17	Give numbers of studies screened, assessed for eligibility, and included in the review, with reasons for exclusions at each stage, ideally with a flow diagram.	6.4.1
Study characteristics	18	For each study, present characteristics for which data were extracted (e.g., study size, PICOS, follow-up period) and provide the citations.	6.4.2; 6.4.3
Risk of bias within studies	19	Present data on risk of bias of each study and, if available, any outcome level assessment (see item 12).	6.4.5
Results of individual studies	20	For all outcomes considered (benefits or harms), present, for each study: (a) simple summary data for each intervention group (b) effect estimates and confidence intervals, ideally with a forest plot[135].	6.4.4
Synthesis of results	21	Present the main results of the review. If meta-analyses are done, include for each, confidence intervals and measures of consistency.	6.5
Risk of bias across studies	22	Present results of any assessment of risk of bias across studies (see Item 15).	N/A
Additional analysis	23	Give results of additional analyses, if done (e.g., sensitivity or subgroup analyses, meta-regression [see Item 16]).	N/A
DISCUSSION			
Summary of evidence	24	Summarize the main findings including the strength of evidence for each main outcome; consider their relevance to key groups (e.g., healthcare providers, users, and policy makers).	6.6.1
Limitations	25	Discuss limitations at study and outcome level (e.g., risk of bias), and at review-level (e.g., incomplete retrieval of identified research, reporting bias).	6.6.2
Conclusions	26	Provide a general interpretation of the results in the context of other evidence, and implications for future research.	6.6.3
FUNDING			
Funding	27	Describe sources of funding for the systematic review and other support (e.g., supply of data); role of funders for the systematic review.	N/A

Moher, D., Liberati, A., Tetzlaff, J., & Altman, D. G. (2009). Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement. *Annals of Internal Medicine*, *151*(4), 264-269.

[135] It was impractical, within the time constraints of a Ph.D. programme, to calculate effect estimates and confidence intervals.

A.15 Systematic review Prospero protocol

PROSPERO
International prospective register of systematic reviews

NHS National Institute for Health Research

Is writing about a best possible future self-beneficial for physical and psychological well-being? A systematic review and meta-analysis

Megan Bean, Katie Cutts, John Reidy

This version was published on 22 January 2017 and is not the current version.

Citation

Megan Bean, Katie Cutts, John Reidy. Is writing about a best possible future self-beneficial for physical and psychological well-being? A systematic review and meta-analysis. PROSPERO 2017 CRD42017055651 Available from:
http://www.crd.york.ac.uk/PROSPERO/display_record.php?ID=CRD42017055651

Review question

Is writing about a best possible future self-beneficial for the physical health of adults?
Is writing about a best possible future self-beneficial for the psychological well-being of adults?
What methodological and procedural factors impact the effectiveness of writing about a best possible future self for physical and psychological well-being?

Searches

The following electronic databases will be searched:
PsycINFO, Scopus, MEDLINE, and Web of Science.
Google Scholar will also be searched.
The search strategy will only include terms which relate to or describe the intervention being reviewed.
The grey literature will also be searched using ProQuest Dissertation Abstracts International and abstracts of relevant conferences (e.g. Division of Health Psychology and European Health Psychology Society proceedings).
Searches will be restricted to the English language.

Types of study to be included

All studies included must include a control group, and must examine the effects of writing about a best possible future self on physical and psychological health. All studies identified for potential inclusion will be evaluated in terms of quality and relevance to the review question by two independent raters. All studies must have full text versions available in English.

Condition or domain being studied

Physical and psychological well-being. This will include all measures of physical and psychological health, e.g. results from self-report as well as records of health centre visit frequency.

Participants/population

Adults (over the age of 18 years).

Intervention(s), exposure(s)

Writing in prose about a best possible future self. This may be as a stand-alone intervention or may be used alongside mental imagery treatments. It may be physical writing, or typing online.

Comparator(s)/control

No writing control or a control group given a 'placebo' writing task (about a topic not expected to be therapeutic).

Context

Primary outcome(s)

Any physical or psychological well-being outcome which could indicate change or no effect as a result of

writing about a best possible future self.

Timing and effect measures
Timings will be measurements pre-treatment (baseline) if available, and post-treatment. This will include immediately post-treatment (e.g. affect) and at follow-up periods.

Secondary outcome(s)
None.

Data extraction (selection and coding)
The first and second authors of this review will independently assess the titles and abstracts of studies identified using the search strategy and from other sources, in order to determine those which may meet the criteria for inclusion in the systematic review. The full text versions of these studies will then be obtained, and screened for eligibility.
The first and second authors will independently extract the following from each paper: title, abstract, authors, intervention details (e.g. length and number of writing sessions, whether writing treatment was stand- alone or used alongside mental imagery), participant details, physical and psychological health outcome measures, and results.
If any disagreements between the first and second author arise with regards to a study's eligibility, they will discuss it with the third author of the review.
If any studies are missing information, the investigators of those studies will be contacted in order to request the data.
The authors will use a PRISMA flow diagram to represent the records obtained, and included (and excluded) throughout the review. It will also document the reasons for inclusion and exclusion.

Risk of bias (quality) assessment
Risk of bias will be assessed using Cochrane's tool for assessing risk of bias.
This process will be completed independently by both the first and second authors. Disagreements which occur between the first and second authors will be discussed with the third author.
Grey literature and published papers will be assessed using the same criteria to further minimise bias risk.

Strategy for data synthesis
A narrative synthesis of findings with regards to intervention details (e.g. number of writing sessions) and health outcomes (i.e. physical health and psychological well-being) will be generated. Meta-analysis will be performed where possible using SPSS.

Analysis of subgroups or subsets
In order to investigate some of the causes of heterogeneity of findings between studies, subgroup analyses are planned. The impact of the number of writing sessions, whether the writing instructions are structured or open, the timing between the writing sessions, whether the writing lessons are laboratory-based or online, and whether the writing treatment was stand-alone or used alongside mental imagery, on the estimation of effect will be investigated.

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03 January 2017

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01 August 2017

Funding sources/sponsors
Sheffield Hallam University

Conflicts of interest
None known

Language
English

Country
England

Stage of review
Review_Ongoing

Subject index terms status
Subject indexing assigned by CRD

Subject index terms
Health Status; Humans; Imagination; Mental Health; Psychological Techniques; Psychotherapeutic Processes; Psychotherapy; Quality of Life; Treatment Outcome; Writing

Date of registration in PROSPERO
22 January 2017

Date of publication of this version
22 January 2017

Details of any existing review of the same topic by the same authors

Stage of review at time of this submission

Stage	Started	Completed
Preliminary searches	Yes	Yes
Piloting of the study selection process	Yes	No
Formal screening of search results against eligibility criteria	No	No
Data extraction	No	No
Risk of bias (quality) assessment	No	No
Data analysis	No	No

Versions
22 January 2017
21 April 2017
12 December 2017

A.16 Search terms used in database searches

Cochrane

#1 Write or writing or wrote or written:ti,ab,kw (Word variations have been searched)
#2 "best possible sel*":ti,ab,kw (Word variations have been searched)
#3 optimism:ti,ab,kw (Word variations have been searched)
#4 happiness:ti,ab,kw (Word variations have been searched)
#5 positiv* near/4 (exercise* or future or activit* or psycholog*):ti,ab,kw (Word variations have been searched)
#6 writing near/4 goal*:ti,ab,kw (Word variations have been searched)
#7 #2 or #3 or #4 or #5 or #6
#8 #1 and #7 Publication Year from 2001 to 2017

MEDLINE and CINAHL

S8 S1 AND S7
S7 S2 OR S3 OR S4 OR S5 OR S6
S6 AB writing n4 goal* OR TI writing n4 goal*
S5 AB (positive* n4 (exercise* OR future OR activit* OR psychology*)) OR TI (positive* n4 (exercise* OR future OR activit* OR psychology*))
S4 AB happiness OR TI happiness
S3 AB optimism OR TI optimism
S2 AB "best possible sel*" OR TI "best possible sel*"
S1 AB (Write OR writing OR wrote OR written) OR TI (Write OR writing OR wrote OR written)

PsycInfo

(ab(Write OR writing OR wrote OR written) OR ti(Write OR writing OR wrote OR written) OR if(Write OR writing OR wrote OR written)) AND ((ab("best possible sel*") OR ti("best possible sel*") OR if("best possible sel*")) OR (ab(optimism) OR ti(optimism) OR if(optimism)) OR (ab(happiness) OR ti(happiness) OR if(happiness)) OR (ab(positiv* NEAR/4 (exercise* OR future OR activit* OR psycholog*)) OR ti(positiv* NEAR/4 (exercise* OR future OR activit* OR psycholog*)) OR if(positiv* NEAR/4 (exercise* OR future OR activit* OR psycholog*))) OR (ab(writing NEAR/4 goal*) OR ti(writing NEAR/4 goal*) OR if(writing NEAR/4 goal*))) AND pd(20010101-20171231)

Scopus

((TITLE-ABS-KEY ("best possible sel*")) OR (TITLE-ABS-KEY (optimism)) OR (TITLE-ABS-KEY (happiness)) OR (TITLE-ABS-KEY (positiv* W/4 (exercise* OR future OR activit* OR psycholog*))) OR (TITLE-ABS-KEY (writing W/4 goal*))) AND (TITLE-ABS-KEY
(write OR writing OR wrote OR written)) AND (LIMIT-TO (PUBYEAR , 2017) OR LIMIT-TO (PUBYEAR , 2016) OR LIMIT-TO (PUBYEAR , 2015) OR LIMIT-TO (PUBYEAR , 2014) OR LIMIT-TO (PUBYEAR , 2013) OR LIMIT-TO (PUBYEAR , 2012) OR LIMIT-TO (PUBYEAR , 2011) OR LIMIT-TO (PUBYEAR , 2010) OR LIMIT-TO (PUBYEAR , 2009) OR LIMIT-TO (PUBYEAR , 2008) OR LIMIT-TO (PUBYEAR , 2007) OR LIMIT-TO (PUBYEAR , 2006) OR LIMIT-TO (PUBYEAR , 2005) OR LIMIT-TO (PUBYEAR , 2004) OR LIMIT-TO (PUBYEAR , 2003) OR LIMIT-TO (PUBYEAR , 2002) OR LIMIT-TO (PUBYEAR , 2001))

A.17 Excluded studies and reasons for exclusion

Authors and full reference (where possible)[136]	Reason for exclusion
Antal, H. M., & Range, L. M. (2005). Psychological impact of writing about abuse or positive experiences. *Violence and Victims, 20*(6), 717-28.	Not a BPFS study
Appel, L., Labhart, L., Balczo, P., McCleary, N., Raley, M. & Winsett, R. P. (2013). A comparative study of a happiness intervention in medical-surgical nurses. *Medsurg Nursing, 22*(5), 319-324.	Not a BPFS study
Archer, S. & Yates, J. (2017). Understanding potential career changers' experience of career confidence following a positive psychology based coaching programme. *Coaching, 10*(2), 157-175.	Not experimental/ no control group/ portfolio
Aspegren, K. (2007). The difficult art of writing a good goal description. *Lakartidningen, 104*(38), 2698-2700.	Not available in English
Barber, S. J., Opitz, P. C., Martins, B., Sakaki, M., & Mather, M. (2016). Thinking about a limited future enhances the positivity of younger and older adults' recall: Support for socioemotional selectivity theory. *Memory and Cognition, 4*(6), 869-882.	Not a BPFS study
Bhullar, N., Schutte, N. S. & Malouff, J. M. (2011). Writing about satisfaction processes increases well-being. *Individual Differences Research, 9*(1), 22-32	Not a BPFS study
Chew, B. H., Lee, P. Y. & Ismail, I. Z. (2014). ''Personal mission statement'': An analysis of medical students' and general practitioners' reflections on personal beliefs, values and goals in life. *Malaysian Family Physician, 9*(2), 26-33.	Not experimental
Dellasega, C. A. (2001). Using structured writing experiences to promote mental health. *Journal of Psychosocial Nursing and Mental Health Services, 39*(2), 14-23.	Not a BPFS study
Dickerhoof, R. M. (2007). *Expressing optimism and gratitude: A longitudinal investigation of cognitive strategies to increase well-being* (Unpublished Doctoral Thesis). University of California, Riverside, California, USA.	Portfolio
D'Mello, S. & Mills, C. (2014). Emotions while writing about emotional and non-emotional topics. *Motivation and Emotion, 38*(1), 14-156.	Not a BPFS study
Drake, J. E. & Hodge, A. (2015). Drawing versus writing: The role of preference in regulating short-term affect. *Art Therapy, 32*(1), 27-33.	Not a BPFS study
DuBois, C.M., Millstein, R.A., Celano, C.M., Wexler, D.J., & Huffman, J.C. (2016). Feasibility and acceptability of a positive psychological intervention for patients with type 2 diabetes. *The Primary Care Companion for CNS Disorders, 18*(3), 1-7.	Not a BPFS study
Emanu, J., Avildsen, I., Starr, T., Kelman, J., Roth, A., Nelson, C. & Holland, J. (2015). Delivering the cancer and aging: Reflections for Elders (CARE) psychosocial intervention through expressive writing: A pilot study. *Psycho-oncology, 25,* 16- 17.	Not a BPFS study

[136] For some records the full reference was unavailable. However, the abstract or full text was available, unless otherwise stated as a reason for exclusion.

Ferguson, Y. L. & Sheldon, K. M. (2010). Should goal-strivers think about ‘’why’’ or ‘’how’’ to strive? It depends on their skill level. *Motivation and Emotion, 34(3),* 253-265.	Not a BPFS study
Fugh-Berman, A. (2005). Title unknown. *Focus on Alternative and Complementary Therapies*. Volume, issue and page numbers unknown.	Not a BPFS study
Ghodsbin, F., Safaei, M. Jahanbin, I., Ostovan, M. A. & Keshvarzi, S. (2015). The effect of positive thinking training on the level of spiritual well-being among the patients with coronary artery diseases referred to Imam Reza specialty and subspeciality clinic in Shiraz, Iran: A randomized controlled clinical trial. *ARYA Atherosclerosis, 11(6)*, 341-348.	Not a BPFS study
Gilrain, K. L. (2005). *Coping with bereavement through the use of optimistic emotional disclosure* (Unpublished doctoral thesis). Drexel University, Philadelphia, Pennsylvania, USA.	Not a BPFS study
Graham, J. E., Lobel, M., Glass, P. & Lokshina, I. (2008). Effects of written anger expression in chronic pain patients: making meaning from pain. *Journal of Behavioral Medicine, 31*(3), 201-212.	Not a BPFS study
Heimes, S. (2013). Does expressive writing about best possible future self have an influence on affect, coping, and self-efficacy? Publication title and details unknown.	Not available in English
Hill, E. D., Terrell, H. K., Arellano, A., Schuetz, B. & Nagoshi, C. T. (2015). A good story: Using future life narratives to predict present well-being. *Journal of Happiness Studies, 16*(6), 1615-1634.	Not experimental
King, L. A. & Raspin, C. (2004). Lost and found possible selves, subjective well-being, and ego development in divorced women. *Journal of Personality, 72*(3), 603-632.	Not experimental
King, L. A. & Smith, N. G. (2004). Gay and straight possible selves: Goals, identity, subjective well-being, and personality development. *Journal of Personality, 72*(5), 967-994.	Not experimental
Kreitler, C. M. (2011). *Evaluation of a cognitive tool for enhanced problem-solving and coping* (Unpublished doctoral dissertation). Texas Christian University, Texas, USA.	Not a BPFS study
Lambert D'raven, L. T. Moliver, N. & Thompson, D. (2015). Happiness intervention decreases pain and depression, boosts happiness among primary care patients. *Primary care research and development, 16*(2), 114-126.	No control group, portfolio study
Layous, K., Nelson, S. K., Kurtz, J. L. & Lyubomirsky, S. (2017). What triggers prosocial effort? A positive feedback loop between positive activities, kindness, and well-being. *The Journal of Positive Psychology, 12*(4), 385-398.	Portfolio
Layous, K. A. (2015). *Triggering kindness: Mechanisms and outcomes* (Unpublished doctoral thesis). University of California, Riverside, California, USA.	Portfolio
Lengelle, R., Meijers, F., Poell, R., Geijsel, F. & Post, M. (2016). Career writing as a dialogue about work experience: A recipe for luck readiness? *International Journal for Educational and Vocational Guidance, 16*(1), 29-43.	Not a BPFS study

Mackenzie, C. S., Wiprzycka, U. J., Hasher, L. & Goldstein, D. (2008). Seeing the glass half full: Optimistic expressive writing improves mental health among chronically stressed caregivers. *British Journal of Health Psychology, 13*(1), 73-76.	Not a BPFS study
Mann, T. (2001). Effects of future writing and optimism on health behaviors in HIV-infected women. *Annals of Behavioural Medicine, 23*(1), 26-33.	Not a BPFS study
McCarthy, M. (2011). Steps to happiness. *World of Irish Nursing and Midwifery*. No volume, issue or page numbers available.	Not a BPFS study
McWilliam, R. A. (2002). A cause for happiness. *Journal of Early Intervention, 25*(2), 75-77.	Not a BPFS study
Monroe, A. E., Ainsworth, S. E., Vohs, K. D. & Baumeister, R. F. (2017). Fearing the future? Future-oriented thought produces aversion to risky investments, trust, and immorality. *Social Cognition, 35*(1), 66-78.	Not a BPFS study
Morisano, D., Hirsh, J. B., Peterson, J. B., Pihl, R. O. & Shore, B. M. (2010). Setting, elaborating, and reflecting on personal goals improves academic performance. *Journal of Applied Psychology, 95*(2), 255-264.	Portfolio
Morisano, D. & Shore, B. M. (2010). Can personal goal setting tap the potential of the gifted underachiever? *Roeper Review, 32*(4), 249-258.	Not a BPFS study
Müller, R., Gertz, K., Molton, I., Terrill, A., Bombardier, C., Ehde, D. M. & Jensen, M. (2014). Pilot testing a positive intervention in individuals with chronic, disability-related pain. *Archives of Physical Medicine and Rehabilitation*, *95*(10), e9-e10.	Not a BPFS study
Müller, R., Gertz, K. J., Molton, I. R., Terrill, Alexandra, L., Bombardier, C. H., Ehde, D. M. & Jensen, M. P. (2016). Effects of a tailored positive psychology intervention on well-being and pain in individuals with chronic pain and a physical disability: A feasibility trial. *The Clinical Journal of Pain, 32*(1), 32-44.	Portfolio
Nath, P. & Pradhan, R. K. (2014). Does feeling happy contribute to flexible thinking: Exploring the association between positive emotions and cognitive flexibility. *Psychological Studies, 59*(2), 180-190.	Not a BPFS study
Ogilvy, J. (2014). Emergence, story, and the challenge of positive scenarios. *World Futures, 70*(1), 52-87.	Not a BPFS study
Oyserman, D., Destin, M., & Novin, S. (2016). The context-sensitive future self: Possible selves motivate in context, not otherwise. *Self and Identity, 14*(2), 173-188.	No control group
Panagopoulou, E., Montgomery, A. & Tarlatzis, B. (2009). Experimental emotional disclosure in women undergoing infertility treatment: Are drop outs better off? *Social Science and Medicine, 69*(5), 678-681.	Not a BPFS study
Panagopoulou, E. & Tarlatzis, B. (2013). Stress, and success of ARTs: Identifying the missing link(s). *Human Reproduction, 28*, 277-277.	Unable to access sufficient information to determine eligibility

Paradisi, A., Abeni, D., Finore, E., Di Pietro, C., Sampogna, F., Mazzanti, C., Pilla, M. A. & Tabolli, S. (2010). Effect of written emotional disclosure interventions in persons with psoriasis undergoing narrow band ultraviolet B phototherapy. *European Journal of Dermatology, 20*(5), 599-605.	Portfolio study
Pierce, J. B. (2009). Joy in the written word. *American Libraries.* No volume, issue or page number available.	Not a BPFS study
Pietrowsky, R. & Mikutta, J. (2012). Effects of positive psychology interventions in depressive patients: A randomised control study. *Psychology, 3*(12), 1067-1073.	Portfolio
Sergeant, S. & Mongrain, M. (2014). An online optimism intervention reduces depression in pessimistic individuals. *Journal of Consulting and Clinical Psychology, 82*(2), 263-274.	Not a BPFS study
Soliday, E., Garofalo, J. P & Rogers, D. (2004). Expressive writing intervention for adolescents' somatic symptoms and mood. *Journal of Clinical Child and Adolescent Psychology, 33*(4), 792-801.	Not a BPFS study
Suhr, M., Risch, A. K. & Wilz, G. (2017). Maintaining mental health through positive writing: Effects of a resource diary on depression and emotion regulation. *Journal of Clinical Psychology, 73*(12), 1586-1598.	Not a BPFS study
Surdey, J. F. (2015). *Is the self what's missing in college? Psychological well-being in college students* (Unpublished doctoral thesis). Binghamton University, New York, USA.	Not a BPFS study
Swigert, N. (2014). Patient outcomes, NOMS, and goal writing for pediatrics and adults. *Perspectives on Swallowing and Swallowing Disorders (Dysphagia), 23*(2), 65-71.	Not a BPFS study
Teismann, T., Het, S., Grillenberger, M, Willutzki, U. & Wolf, O. T. (2014). Writing about life goals: Effects on rumination, mood and the cortisol awakening response. *Journal of Health Psychology, 19*(11), 1410-1419.	Portfolio
Toussaint, L., Barry, M., Bornfriend, L. & Markman, M. (2014). Restore: The journey toward self-forgiveness: A randomized trial of patient education on self-forgiveness in cancer patients and caregivers. *Journal of Health Care Chaplaincy, 20*(2), 54-74.	Not a BPFS study
Trompetter, H. R., Bohlmeijer, E. T., Lamers, S. M. A. & Schreurs, K. M. G. (2016). Positive psychological wellbeing is required for online self-help acceptance and commitment therapy for chronic pain to be effective. *Frontiers in Psychology, 7,* 353.	Not a BPFS study
Ulbrecht, J. S., Trief, P. M., Wallston, K. A., Heron, K. E. & Smyth, J. M. (2013). Short-term effects of expressive writing as adjuvant treatment in T2DM on clinical status and patient well-being. *Diabetes, 62*, A2.	Not a BPFS study
Wang, Y., & Wang, Z. (2011). Effects of expressive writing positive emotion on improving well-being and coping style. *Chinese Journal of Clinical Psychology, 19*(1), 130- 132.	Not available in English

Wong, Y. J., Owen, J., Gabana, N. T., Brown, J. W., Mcinnis, S., Toth, P. & Gilman, L. (2016). Does gratitude writing improve the mental health of psychotherapy clients? Evidence from a randomized controlled trial. *Psychotherapy Research, 28*(2), 192- 202.	Not a BPFS study
Wong, Y. J. (2008). *The potential benefits of expressive writing for male college students with varying degrees of restrictive emotionality* (Unpublished doctoral thesis). The University of Texas at Austin, Texas, USA.	Not a BPFS study
Zahaluk, D. (2010). Are we (still) on track for 2010? *Podiatry Management, 29*(5), 239-240.	Not a BPFS study
King, K. T. (2012). *The spiral staircase: Developing a happiness increasing training program for workers* (Unpublished doctoral thesis). The Chicago School of Professional Psychology, Chicago, USA.	Not a BPFS study

A.18 Sample data extraction form

Publication characteristics
Authors' names:
Title of publication:
Year of publication:
Country where research took place:

Authors' aims and hypotheses:
Study design:
Randomisation strategy if applicable:

Participants
Where were participants recruited from?
Any explicit exclusion criteria?
Number of participants at each stage of the study:
Gender split:
Ethnicity:
Other characteristics:

Intervention
Study setting:
Number, length and spacing of writing sessions:
BPFS writing instructions:
Control group:
Any imagery?

Outcome variables
What were the dependent variables?
What measurement instruments were used?
When was each outcome measured?

Findings
How were data analysed?
What were the findings for each dependent variable?

Quality
Were there any quality issues?

Other notes:

A.19 Systematic review ethics checklist and approval letter

A.19.1 Ethics checklist

Sheffield Hallam University

RESEARCH ETHICS CHECKLIST (SHUREC1)

This form is designed to help staff and postgraduate research students to complete an ethical scrutiny of proposed research. The SHU Research Ethics Policy should be consulted before completing the form.

Answering the questions below will help you decide whether your proposed research requires ethical review by a Faculty Research Ethics Committee (FREC). In cases of uncertainty, members of the FREC can be approached for advice.

Please note: staff based in University central departments should submit to the University Ethics Committee (SHUREC) for review and advice.

The final responsibility for ensuring that ethical research practices are followed rests with the supervisor for student research and with the principal investigator for staff research projects.

Note that students and staff are responsible for making suitable arrangements for keeping data secure and, if relevant, for keeping the identity of participants anonymous. They are also responsible for following SHU guidelines about data encryption and research data management.

The form also enables the University and Faculty to keep a record confirming that research conducted has been subjected to ethical scrutiny.

- For postgraduate research student projects, the form should be completed by the student and counter-signed by the supervisor, and kept as a record showing that ethical scrutiny has occurred. Students should retain a copy for inclusion in their thesis, and staff should keep a copy in the student file.
- For staff research, the form should be completed and kept by the principal investigator.

Please note if it may be necessary to conduct a health and safety risk assessment for the proposed research. Further information can be obtained from the Faculty Safety Co-ordinator.

General Details

Name of principal investigator or	Megan Bean
SHU email address	A9022330@my.shu.ac.uk
Name of supervisor (if applicable)	Dr. Katie Cutts, Dr. John Reidy, and Mrs. Melanie Gee
email address	John Reidy: ssljgr@exchange.shu.ac.uk Katie Cutts: sslkc@exchange.shu.ac.uk Melanie Gee: slsmdg@exchange.shu.ac.uk
Title of proposed research	Is writing about a best possible future self beneficial for physical and psychological well-being? A systematic review of methodological variations.
Proposed start date	January 2017
Proposed end date	January 2018
Brief outline of research to include, rationale & aims (500 -750 words).	Writing about a best possible future self has been found to have positive impacts on depression, expectancies for positive outcomes, relatedness to others, positive affect, physical illness, self-criticism, experiences of pain intensity and sustained dampening of negative affect (Hanssen, Peters, Vlaeyen & Vancleef, 2012, King, 2001; Layous, Nelson & Lyubomirsky, 2013; Sheldon & Lyubomirsky, 2006; Harrist et al., 2007; Peters, Flink, Boersma & Linton, 2010; Troop, Chilcot, Hutchings & Varnaite, 2013). Although the literature widely suggests that the intervention is promising, there are discrepancies in findings, and effects are inconsistent. For example, Lyubomirsky, Dickerhoof, Boehm and Sheldon (2011) found no benefits of writing about a best possible future self. It is possible that Lyubormirsky et al's (2011) null findings arose from asking participants to write about a different aspect of their best possible future self (romantic life, educational attainment, hobbies or personal interests, family life, career, social life, community involvement, and health) in 8 writing sessions, whereas a large amount of studies (e.g. King, 2001; Boselie, Ng, 2016; Boselie, Vancleef, Smeets & Peters, 2014) require that participants write about their general best possible future self- with no further prompts or restrictions with regards to what they should write about. Nevertheless, Layous et al. (2013) also used a more structured protocol, with writing instructions tailored to a different element of a best possible future self for each of 4 sessions; academic, social, career and health, and reported benefits to positive affect and flow. It is difficult to compare the results of

	Lyubomirsky et al's (2011) and Layous et al.'s (2013) studies directly, due to further methodological differences which could account for discrepancies in findings. For example, Layous et al. (2013) used 4 writing days, whereas Lyubomirsky et al. (2011) used 8. It is possible that 8 writing days was too high a 'dose', possibly leading to boredom which may have dampened any beneficial effects of writing which could have been present after the fourth writing day. Multiple differences make comparisons difficult, and as such render it impossible to isolate the factors which may account for the null findings reported by Lyubomirsky et al. (2011). Methodological and procedural inconsistencies present a problem in the wider best possible future self literature; they confound comparisons of results of studies and render accurate interpretation of findings across investigations difficult. Therefore, it remains impossible to isolate the methodological parameters within which best possible future self writing tasks do- and do not- work. Research into the effects of alterations to the best possible future self paradigm is critical in order to identify the parameters within which it is effective. It appears sensible to conduct a systematic review in order to identify confounds arising from multiple methodological discrepancies across studies, which currently render it impossible to isolate the methodological factors which impact therapeutic power and accurately compare findings. In the current study, a systematic review of both published and unpublished best possible future self intervention studies will be conducted. The aims of the review are to identify all of the methodological variations which occur across best possible future self intervention studies, and- if the evidence is available- to explore which of these variations appear to impact therapeutic power.
Where data is collected from human participants, outline the nature of the data, details of anonymisation, storage and disposal procedures if these are required (300 -750 words).	No data to be collected
Will the research be conducted with partners & subcontractors?	**Yes/No** (If **YES**, outline how you will ensure that their ethical policies are consistent with university policy.)

1. Health Related Research involving the NHS or Social Care / Community Care or the Criminal Justice System or with research participants unable to provide informed consent

Question	Yes/No
1. Does the research involve? • Patients recruited because of their past or present use of the NHS or Social Care • Relatives/carers of patients recruited because of their past or present use of the NHS or Social Care • Access to data, organs or other bodily material of past or present NHS patients • Foetal material and IVF involving NHS patients • The recently dead in NHS premises • Prisoners or others within the criminal justice system recruited for health- related research* • Police, court officials, prisoners or others within the criminal justice system* • Participants who are unable to provide informed consent due to their incapacity even if the project is not health related	No
2. Is this a research project as opposed to service evaluation or audit? *For NHS definitions please see the following website* http://www.nres.nhs.uk/applications/is-your-project-research/	N/A

If you have answered **YES** to questions **1 & 2** then you **must** seek the appropriate external approvals from the NHS, Social Care or the National Offender Management Service (NOMS) under their independent Research Governance schemes. Further information is provided below.

NHS https://www.myresearchproject.org.uk/Signin.aspx

* Prison projects may also need National Offender Management Service (NOMS) Approval and Governor's Approval and may need Ministry of Justice approval. Further guidance at:
http://www.hra.nhs.uk/research-community/applying-for-approvals/national-offender-management-service-noms/

NB FRECs provide Independent Scientific Review for NHS or SC research and initial scrutiny for ethics applications as required for university sponsorship of the research. Applicants can use the NHS proforma and submit this initially to their FREC.

2. Research with Human Participants

Question		Yes/No
1.	Does the research involve human participants? This includes surveys, questionnaires, observing behaviour etc. *Note* *If YES, then please answer questions 2 to 10* *If NO, please go to Section 3*	No
2.	Will any of the participants be vulnerable? *Note* *'Vulnerable' people include children and young people, people with learning disabilities, people who may be limited by age or sickness or disability, etc. See definition*	N/A
3	Are drugs, placebos or other substances (e.g. food substances, vitamins) to be administered to the study participants or will the study involve invasive, intrusive or potentially harmful procedures of any kind?	N/A
4	Will tissue samples (including blood) be obtained from participants?	N/A
5	Is pain or more than mild discomfort likely to result from the study?	N/A
6	Will the study involve prolonged or repetitive testing?	N/A
7	Is there any reasonable and foreseeable risk of physical or emotional harm to any of the participants? *Note* *Harm may be caused by distressing or intrusive interview questions, uncomfortable procedures involving the participant, invasion of privacy, topics relating to highly personal information, topics relating to illegal activity, etc.*	N/A
8	Will anyone be taking part without giving their informed consent?	N/A
9	Is it covert research? *Note* *'Covert research' refers to research that is conducted without the knowledge of participants.*	N/A
10	Will the research output allow identification of any individual who has not given their express consent to be identified?	N/A

If you answered **YES only** to question **1,** you must complete the box below and submit the signed form to the FREC for registration and scrutiny.

Data Handling

Where data is collected from human participants, outline the nature of the data, details of anonymisation, storage and disposal procedures if these are required (300 -750 words).

N/A

If you have answered **YES** to any of the other questions you are **required** to submit a SHUREC2A (or 2B) to the FREC. If you answered **YES** to question **8** and participants cannot provide informed consent due to their incapacity you must obtain the appropriate approvals from the NHS research governance system.

3. Research in Organisations

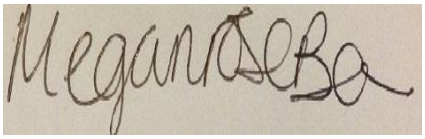
Question		**Yes/No**
1	Will the research involve working with/within an organisation (e.g. school, business, charity, museum, government department, international agency, etc.)?	No
2	If you answered YES to question 1, do you have granted access to conduct the research? *If YES, students please show evidence to your supervisor. PI should retain safely.*	N/A
3	If you answered NO to question 2, is it because: A. you have not yet asked B. you have asked and not yet received an answer C. you have asked and been refused access. *Note You will only be able to start the research when you have been granted access.*	N/A

4. Research with Products and Artefacts

Question	Yes/No
1. Will the research involve working with copyrighted documents, films, broadcasts, photographs, artworks, designs, products, programmes, databases, networks, processes, existing datasets or secure data?	Yes
2. If you answered YES to question 1, are the materials you intend to use in the public domain? *Notes 'In the public domain' does not mean the same thing as 'publicly accessible'.* − *Information which is 'in the public domain' is no longer protected by copyright (i.e. copyright has either expired or been waived) and can be used without permission.* − *Information which is 'publicly accessible' (e.g. TV broadcasts, websites, artworks, newspapers) is available for anyone to consult/view. It is still protected by copyright even if there is no copyright notice. In UK law, copyright protection is automatic and does not require a copyright statement, although it is always good practice to provide one. It is necessary to check the terms and conditions of use to find out exactly how the material may be reused etc.* *If you answered YES to question 1, be aware that you may need to consider other ethics codes. For example, when conducting Internet research, consult the code of the Association of Internet Researchers; for educational research, consult the Code of Ethics of the British Educational Research Association.*	Mainly yes, but articles not in the public domain (i.e. unpublished manuscripts) only to be used with permission.
3. If you answered NO to question 2, do you have explicit permission to use these materials as data? *If YES, please show evidence to your supervisor. PI should retain permission.*	Yes

4.	If you answered NO to question 3, is it because: A. you have not yet asked permission B. you have asked and not yet received and answer C. you have asked and been refused access.	**A/B/C**
Note	*You will only be able to start the research when you have been granted permission to use the specified material.*	

Adherence to SHU policy and procedures

Personal statement	
I can confirm that: − I have read the Sheffield Hallam University Research Ethics Policy and Procedures − I agree to abide by its principles.	
Student / Researcher/ Principal Investigator (as applicable)	
Name: Megan Bean	Date:24/07/17
Signature: 	
Supervisor or other person giving ethical sign-off	
I can confirm that completion of this form has not identified the need for ethical approval by the FREC or an NHS, Social Care or other external REC. The research will not commence until any approvals required under Sections 3 & 4 have been received.	
Name: Melanie Gee	Date:24/07/2017
Signature: 	
Additional Signature if required:	
Name:	Date:
Signature:	

Please ensure the following are included with this form if applicable, tick box to indicate:

	Yes	No	N/A
Research proposal if prepared previously	☐	☐	☒
Any recruitment materials (e.g. posters, letters, etc.)	☐	☐	☒
Participant information sheet	☐	☐	☒
Participant consent form	☐	☐	☒
Details of measures to be used (e.g. questionnaires, etc.)	☐	☐	☒
Outline interview schedule / focus group schedule	☐	☐	☒
Debriefing materials	☐	☐	☒
Health and Safety Project Safety Plan for Procedures	☐	☐	☒
Data Management Plan*	☐	☐	

If you have not already done so, please send a copy of your Data management Plan to rdm@shu.ac.uk
It will be used to tailor support and make sure enough data storage will be available for your data. **Completed form to be sent to Relevant FREC. Contact details on the website.**

A.19.2 Approval letter

Sheffield Hallam University

Our Ref AM/SW/43-BEA(b)

Ms M Bean
12 Trent Port Road
Marton
Gainsborough
DN21 5AP

24th July 2017

Dear Megan,

Request for Ethical Approval of Research Project

Your research ethics checklist (SHUREC1) entitled "**Is writing about a best possible future self beneficial for physical and psychological well-being? A systematic review of methodological variations**" has been submitted for ethical review to the Faculty's rapporteurs and I am pleased to confirm that they have approved your project.

I wish you every success with your research project.

Yours sincerely

Professor A Macaskill
Chair
Faculty Research Ethics Committee

Office address :
Business Support Team
Faculty of Development & Society
Sheffield Hallam University
Unit 4, Sheffield Science Park
Howard Street, Sheffield, S1 1WB
Tel: 0114-225 3308
E-mail: DS-ResearchEthics@shu.ac.uk