Learning to Bounce Back? –

Current Evidence From a Systematic Cochrane Review on Resilience Interventions in Healthcare Professionals and a Pilot Study on a Mobile-Based Positivity Bias Training

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von

Angela Mareike Kunzler

aus Saarlouis

Mainz

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Dies betrifft alle Textabschnitte innerhalb des ersten Teils der Dissertation „Systematic Cochrane Review and Meta-analysis on Psychological Interventions to Foster Resilience in Healthcare Professionals“ sowie den dazugehörigen Anhang und digitalen Anhang. Teile aus der Publikation wurden wörtlich übernommen oder abgewandelt und für die vorliegende Monographie adaptiert. Die Publikation Kunzler et al. (2020) wurde entsprechend zitiert.

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Abstract

Background. There is an increasing interest in interventions to foster resilience, that is, the maintenance or quick recovery of mental health despite adversities. Healthcare professionals (HCP), exposed to various work-related stressors, are at an elevated risk of mental disorders and might benefit from such programs. Due to methodological weaknesses, previous reviews cannot answer the question of which interventions are really effective in HCP and how they should be implemented. Besides stressor exposure, biases in information processing may also determine mental health, with a focus on the reduction of negativity biases in previous research. Although a positivity bias (PB) is viewed as protective factor that might also facilitate a resilient response to stressors, training approaches to foster a PB have been neglected, especially at the level of action tendencies.

Objectives. Overall, this thesis addresses the question of whether interventions to foster resilience are effective in improving resilience and mental health by the following two objectives: 1) To synthesize the evidence on the efficacy of resilience training (i.e., trainings fostering one or several resilience factors) in HCP and to explore the potential moderating role of intervention characteristics (e.g., setting), in order to assess the meaning of these programs. 2) To develop a mobile-based positivity training at the level of action tendencies using various affective stimuli which solely focuses on the resilience factor of PB and whose feasibility and effects are evaluated. ***Methods.*** Two complementary approaches were used. First, a *systematic Cochrane review and meta-analysis* was conducted. CENTRAL, MEDLINE, Embase, 11 other databases, and three trial registries were searched from 1990 to June 2019, with reference lists being checked and researchers in the field contacted. Randomized controlled trials (RCTs) in adults aged ≥ 18 years who are employed as HCP, comparing any form of psychological intervention to foster resilience, hardiness, or posttraumatic growth against a comparator, were eligible. Primary outcomes were resilience and mental health, secondary outcomes were resilience factors. Study selection, data extraction, quality assessment, and the rating of the certainty of evidence were performed in duplicate. Random-effects meta-analyses were conducted along with preplanned subgroup and sensitivity analyses. Second, in the *empirical pilot study TRAIN$_4$Positivity* (single-group design), *N* = 41 healthy participants (university students) exposed to many microstressors underwent the three-week mobile-based intervention called “Breezly”. Based on

the modified Approach Avoidance Task (AAT) and using pictures from validated databases, “Breezly” included daily sessions of picture training. Using a pre- and postassessment with Ecological Momentary Assessment (EMA) throughout the intervention, perceived stress and the perceived severity of microstressors served as primary outcomes, with the latter being compared to a (matched) historical control group (*N* = 70). Secondary outcomes included implicit action tendencies, measures of resilience and well-being, psychological EMA outcomes (e.g., mood), cognitive reappraisal, and emotional experience. Data were analyzed using various methods (e.g., multilevel modeling). ***Results.*** Overall, both projects provided mixed results concerning the positive effects of interventions to foster resilience. In the *systematic review*, 44 RCTs were identified, with 39 solely in HCP (*N* = 6,974) and four in mixed samples (*N* = 1,000). Most studies investigated high-intensity group interventions, delivered face-to-face, and based on combined theoretical foundations. At postintervention, very-low certainty evidence indicated that, compared to controls, HCP receiving resilience training may report higher levels of resilience and lower levels of depression, and stress. There was little or no evidence for any effect on anxiety and well-being. Effect sizes were small to moderate, with the positive effects maintained within 3 months after training, but mostly not evident at longer follow-ups. Data on undesired events were only available for three studies, with none reporting any adverse effects. Subgroup analyses showed no consistent effect modifiers. Controlled for stressor exposure and the baseline attributional style, the *app-based study* found no evidence for any training effects on the above-mentioned outcomes, with the changes in action tendencies not moderated by attributional style. Only the ability to distance from negative stimuli partly improved. ***Conclusions.*** Both projects contribute to resilience intervention research. The *systematic review* provides very-low certainty evidence that resilience training may result in higher levels of resilience, lower levels of depression, stress, and higher levels of certain resilience factors at postintervention, with the effects mostly sustained in the short-term. The paucity of medium- and long-term data, restricted geographical distribution, and heterogeneous interventions limit the generalizability of results; thus, conclusions should be drawn carefully. A need for high-quality replications and improved study designs is implied. Second, the findings of the *pilot study* indicate that the app-based training is feasible, but did not change most of the psychological outcomes, partly attributable to (methodological, intervention)

limitations. The results arise the question of the suitability of the AAT as a measure of PB at the action level. Given the tendency for improvements for several outcomes, there is a need for further research to develop the training and examine its efficacy, for example in (stressor-exposed) individuals like HCP.

Keywords: systematic review, meta-analysis, resilience, interventions, healthcare professionals, positivity bias, mobile-based, Approach Avoidance Task, action tendencies, pilot study

Zusammenfassung

Hintergrund. An Interventionen zur Förderung von Resilienz, d.h. der Aufrechterhaltung oder schnellen Rückgewinnung der psychischen Gesundheit trotz Widrigkeiten, besteht ein wachsendes Interesse. Beschäftigte im Gesundheitswesen, die verschiedenen arbeitsbezogenen Stressoren ausgesetzt sind, haben ein erhöhtes Risiko für psychische Erkrankungen und könnten von entsprechenden Programmen profitieren. Aufgrund methodischer Schwächen sind bisherige Reviews nicht in der Lage, die Frage zu beantworten, welche Interventionen bei Gesundheitspersonal wirklich effektiv sind und wie diese implementiert werden sollten. Neben der Stressorexposition können auch Verzerrungen in der Informationsverarbeitung die psychische Gesundheit determinieren, wobei der Fokus der bisherigen Forschung auf einer Reduktion eines Negativitätsbias lag. Obwohl ein Positivitätsbias (PB) als protektiver Faktor angesehen wird, der eine resiliente Reaktion auf Stressoren ebenfalls erleichtern könnte, wurden Trainingsansätze zur Förderung eines PB bislang vernachlässigt, insbesondere auf der Ebene von Handlungstendenzen. ***Ziele.*** Insgesamt behandelt diese Dissertation die Frage nach der Wirksamkeit von Resilienzinterventionen auf Resilienz und die psychische Gesundheit, ausgedrückt durch die folgenden beiden Ziele: 1) die Evidenz zu den Effekten von Resilienztrainings (d.h. Trainings, die einen oder mehrere Resilienzfaktoren fördern) auf Gesundheitspersonal synthetisieren und die potenzielle moderierende Rolle von Interventionsmerkmalen (z. B. Setting) explorieren, um die Bedeutung dieser Trainingsprogramme zu beurteilen, 2) Entwicklung eines mobil-basierten Positivitätstrainings auf der Ebene von Handlungstendenzen unter Verwendung verschiedener affektiver Stimuli, welches sich ausschließlich auf den Resilienzfaktor PB fokussiert, und dessen Durchführbarkeit und Effekte evaluiert werden.

Methoden. Zwei komplementäre Ansätze kamen zum Einsatz. Zunächst wurde ein *systematisches Cochrane-Review mit Metaanalyse* durchgeführt. CENTRAL, MEDLINE, Embase, 11 weitere Datenbanken und Trialregister wurden zwischen 1990 und Juni 2019 durchsucht, wobei Referenzlisten geprüft und Forscher:innen in diesem Feld kontaktiert wurden. Berücksichtigt wurden randomisiert-kontrollierte Studien (RCTs) bei Erwachsenen im Alter von ≥ 18 Jahren, die als Fachkräfte im Gesundheitswesen beschäftigt sind, in denen jede Form einer psychologischen Intervention zur Förderung von Resilienz, Hardiness oder posttraumatischer Reifung mit einer Kontrollgruppe

verglichen wurde. Primäre Endpunkte waren Resilienz und die psychische Gesundheit, Resilienzfaktoren waren sekundäre Endpunkte. Die Studienselektion, Datenextraktion, Qualitätsbeurteilung und die Bewertung der Vertrauenswürdigkeit der Evidenz erfolgten doppelt. Random-effects Metaanalysen wurden neben Subgruppen- und Sensitivitätsanalysen durchgeführt. Zweitens nahmen an der *empirischen Pilotstudie TRAIN$_4$Positivity* (Eingruppen-Design), *N* = 41 gesunde Teilnehmer:innen (Studierende) mit vielen Mikrostressoren an der dreiwöchigen mobil-basierten Intervention „Breezly“ teil. Auf Basis der modifizierten Approach Avoidance Task (AAT) und unter Verwendung von Bildern aus validierten Datenbanken beinhaltete „Breezly“ tägliche Sitzungen eines Bildertrainings. Anhand einer Prä- und Posttestung mit Ambulantem Assessment (EMA) während der Intervention dienten wahrgenommener Stress und die wahrgenommene Belastung durch Mikrostressoren als primäre Endpunkte, wobei Letztere mit einer (gematchten) historischen Kontrollgruppe (*N* =70) verglichen wurde. Sekundäre Endpunkte umfassten implizite Handlungstendenzen, Maße zu Resilienz und Wohlbefinden, psychologische EMA-Maße (z. B. Stimmung), kognitive Neubewertung und das emotionale Erleben. Die Daten wurden mit Hilfe verschiedener Methoden (z. B. Multilevel-Analyse) analysiert. ***Ergebnisse.*** Insgesamt lieferten beide Projekte gemischte Resultate hinsichtlich der positiven Effekte von Interventionen zur Resilienzförderung. Im *systematischen Review* wurden 44 RCTs identifiziert, von diesen 39 ausschließlich in Gesundheitspersonal (*N* = 6974) und vier in gemischten Stichproben (*N* = 1000). Die Mehrzahl der Studien untersuchte Gruppeninterventionen von hoher Intensität, die face-to-face dargeboten wurden und auf einer Kombination theoretischer Ansätze basierten. Auf Basis einer sehr niedrigen Vertrauenswürdigkeit der Evidenz zum Posttest zeigten sich für Gesundheits-mitarbeiter:innen, die ein Resilienztraining erhielten, verglichen mit Kontrollprobanden, ein höheres Resilienzniveau und niedrigere Werte in Depression und Stress. Geringe bis keine Evidenz für Effekte lag in Bezug auf Angst und Wohlbefinden vor. Die Effektstärken waren gering bis moderat, wobei die positiven Effekte bis drei Monate nach dem Training anhielten, jedoch zu späteren Follow-ups meist nicht evident waren. Daten zu unerwünschten Ereignissen lagen nur für drei Studien vor, von denen keine nachteilige Effekte berichtete. Die Subgruppenanalysen ergaben keine konsistenten Moderatoren der Wirksamkeit. Unter Kontrolle der Stressorexposition und des Attributionsstils zur

Baseline erbrachte die *App-basierte Studie* keine Evidenz für mögliche Trainingseffekte auf die oben genannten Endpunkte, wobei die Veränderung in Handlungstendenzen auch nicht durch den Attributionsstil moderiert wurde. Lediglich die Fähigkeit, sich von negativen Stimuli zu distanzieren, verbesserte sich teilweise. ***Schlussfolgerungen.*** Beide Projekte tragen zur Resilienz-Interventionsforschung bei. Auf Basis einer sehr niedrigen Vertrauenswürdigkeit der Evidenz im *systematischen Review* können Resilienztrainings die Resilienz verbessern, Depression und Stress reduzieren und gewisse Resilienzfaktoren nach der Intervention stärken, wobei die Effekte überwiegend kurzfristig anhalten. Der Mangel an mittel- und langfristigen Daten, eine beschränkte geographische Verteilung und heterogene Interventionen limitieren die Generalisierbarkeit der Ergebnisse, weshalb Schlussfolgerungen vorsichtig erfolgen sollten. Das Review impliziert einen Bedarf an qualitativ hochwertigen Replikationen und verbesserten Studiendesigns. Zweitens indizieren die Ergebnisse der Pilotstudie, dass das App-basierte Training durchführbar ist, aber die meisten psychologischen Endpunkte nicht veränderte, was teilweise auf Limitationen (methodisch, Intervention) zurückzuführen ist. Die Ergebnisse werfen die Frage nach der Eignung der AAT als ein Maß für den PB auf der Handlungsebene auf. Angesichts der Verbesserungstendenzen für mehrere Endpunkte besteht weiterer Forschungsbedarf, um das Training weiterzuentwickeln und seine Wirksamkeit, beispielsweise bei (stressorexponierten) Personen wie Gesundheitspersonal, zu untersuchen.

Schlüsselwörter: Systematisches Review, Metaanalyse, Resilienz, Interventionen, Gesundheits-personal, Positivitätsbias, mobil-basiert, Approach Avoidance Task, Handlungstendenzen, Pilotstudie

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Abbreviations

α	significance level or Cronbach's alpha (internal consistency)
α_{corr}	Bonferroni-corrected significance level
AAT	Approach Avoidance Task
AAT-CS	Compatibility Score of the Approach Avoidance Task
ACT	Acceptance and Commitment Therapy
AIC	Akaike Information Criterion
AIT	Attention and Interpretation Therapy
ANOVA	analysis of variance
approx.	approximately
AS	attributional style
ASF-E	Attributionsstilfragebogen für Erwachsene
ASF-E-N	Total score of ASF-E for negative situations
ASF-E-P	Total score of ASF-E for positive situations
ASSIA	Applied Social Sciences Index & Abstracts ProQuest
B	unstandardized regression coefficient (except for standardized predictors)
BIC	Bayesian Information Criterion
BRS	Brief Resilience Scale
BRS_diff	Difference score of observed values at pre- and posttest in the BRS
BURTS	Bournemouth University resilience training for surgeons
CAR	Corporate Athlete® Resilience
CBM	Cognitive Bias Modification
CBM-A	CBM at the level of attention
CBM-I	CBM at the level of interpretation

CBT	Cognitive Behavioral Therapy
CDPLP	Cochrane Developmental, Psychosocial, and Learning Problems
CDSR	Cochrane Database of Systematic Reviews
CENTRAL	Cochrane Central Register of Controlled Trials
CERQ	Cognitive Emotion Regulation Questionnaire
CG	control group/control arm
CI	confidence interval
CINAHL	Cumulative Index to Nursing and Allied Health Literature
CONSORT	Consolidated Standards of Reporting Trials
corrected R^2	corrected determination coefficient
CS	Compound Symmetry
DARE	Database of Abstracts of Reviews of Effects
DASS-21	Depression Anxiety and Stress Scales-21
df	degrees of freedom
_diff	Difference score of observed values at pre- and posttest in the respective outcome
e.g.	for example
EM	Expectation Maximization
EMA	Ecological Momentary Assessment
Embase	Excerpta Medica Database
EMI	Ecological Momentary Intervention
ER	emotion regulation
ERIC	Education Resources Information Center Institute of Education Sciences
ERQ	Emotion Regulation Questionnaire
ES	effect size
ESRT	mindfulness-based enhanced stress resilience training
f^2	Cohen's f^2 (effect size)

F	*F* value (test statistic)
FU	follow-up (assessment)
G^2	G^2 statistic (heterogeneity)
GHQ-28	General Health Questionnaire-28
GRADE	Grading of Recommendations Assessment, Development, and Evaluation
HCP	healthcare professionals
I^2	I^2 statistic (heterogeneity)
IBS	International Bibliography of the Social Sciences ProQuest
ICC	intra-class correlation
i.e.	id est (that is/that means)
IG	intervention group/intervention arm
JGU	Johannes Gutenberg University Mainz
LE	Life events
LE Checklist	Life events Checklist
LOT-R	Life Orientation Test-Revised
M	mean
MAR	missing at random
MB-PBT	mobile-based positivity bias training
MBSR	Mindfulness-Based Stress Reduction
MCAR	missing completely at random
MD	mean difference
MHALO	mindfulness for health professionals building resilience and compassion
MI	multiple imputation
MIMIS	Mainz Inventory of Microstressors
MT-DEP	mobile application “Mindtastic Depression”
MVA	missing values analysis

N	sample size
n	size of subsample
η^2_p	partial eta^2 squared (effect size)
NB	negativity bias
NMA	network meta-analysis
NT	negative/negativity training
p	*p* value
PANAS	Positive and Negative Affect Schedule
PANAS-NA	PANAS negative affect subscale
PANAS-PA	PANAS positive affect subscale
PASTOR	Positive Appraisal Style Theory of Resilience
PB	positivity bias
PBT	positivity bias training
PP	Positive Psychology
PQDT	ProQuest Dissertations & Theses
PRISMA	Preferred Reporting Items for Systematic Reviews and Meta-Analyses
PS	propensity score
PSS-10	Perceived Stress Scale
PST	problem-solving therapy
PT	positive/positivity training
RCT	randomized controlled trial
r	Pearson correlation coefficient
R^2	determination coefficient
RE	replacement
RoB	risk of bias
RQ	research question

RT	reaction time
SD	standard deviation
SDS-17	Social Desirability Scale-17
SE	standard error
SIT	stress inoculation therapy
SMD	standardized mean difference
SoF	Summary of findings
SPF	Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie
STAXI	State-Trait Anger Expression Inventory
t	*t* value (test statistic)
τ^2	Tau^2 statistic (heterogeneity)
TAU	treatment as usual
TOL	tolerance factor
UMC	University Medical Center
VIF	variance inflation factor
vs.	versus
WHO-5	Well-being Index
WSR test	Wilcoxon signed-rank test
χ^2	Chi^2 value (test statistic)

1 Introduction

"We are all, always, in the dangerous river of life. The twin question is: How dangerous is our river? ***How well can we swim?****"* Aaron Antonovsky

Similar to Antonovsky's well-known metaphor (Antonovsky, 1996; Franke, 1997), humans are confronted with various stressors, ranging from minor stressors and challenging life circumstances to traumatic events that are a risk factor for the development of mental disorders (Kalisch et al., 2017). Each year, more than half a billion people suffer from mental disorders worldwide (Vos et al., 2015), with stress-related diseases (e.g., affective disorders) being most common (Jacobi et al., 2014, 2016; Möller et al., 2017; WHO, 2015). Through direct (e.g., healthcare costs) and indirect costs (e.g., productivity loss), they have an enormous impact, not only for the individual, but for the healthcare and social welfare systems as well (Bloom et al., 2011; Trautmann et al., 2016; Vos et al., 2015).

On the other hand, there is inter-individual variability concerning the ability to cope with stressors (Lepore & Revenson, 2006). This means, people differ in resilience, that is, the maintenance or quick recovery of mental health during or after adversities. Given the prevalence of mental disorders, the concept of resilience and the identification of factors that keep humans healthy is particularly important. As a concept of health promotion, research on resilience, its predictors, and underlying mechanisms offers innovative possibilities for the purpose of prevention, for which numerous calls have been prompted (e.g., Mental Health Global Action Program; WHO, 2002). In the wake of current definitions of resilience as a dynamic process, there is a growing interest in training resilience (e.g., "Health2020"; WHO, 2017). Overall, this thesis addresses the question of whether interventions to foster resilience are effective in improving resilience and mental health, by combining a systematic review of resilience trainings and an empirical study of a newly developed intervention.

Healthcare professionals (HCP; e.g., physicians, nurses) are at high risk of being exposed to many stressors with a potential negative impact on their mental health, making the concept of resilience important for this target group (e.g., Hart et al., 2014). To date, several systematic reviews have examined the efficacy of resilience interventions in various populations (e.g., Leppin et al., 2014),

including HCP (e.g., Lavin Venegas et al., 2019), suggesting positive effects. However, the available evidence is limited due to methodological weaknesses of previous reviews. Conclusions on adequate implementation are missing. Therefore, by addressing these limitations, one objective of this work is to synthesize the current evidence on the efficacy of psychological resilience interventions in HCP in a systematic Cochrane review with meta-analyses. Besides stressor exposure, the selective processing of affective information is a determinant of mental health. In line with pathogenesis, intensive research has been undertaken on biases for negative stimuli (i.e., negativity biases, NB) – a risk factor for the development of emotional stress-related disorders (e.g., Mathews & MacLeod, 2005). However, a positivity bias (PB), that is, the preferable processing of positive information and thus a potential resilience factor (Fox et al., 2009), which is modifiable by training, has rarely been examined. Although resilience-training programs with promising effects on mental health partly include elements to foster a PB at different levels (e.g., training of optimism; see also 2), only few interventions to train a PB *at the level of action tendencies* have been performed, none of them in relation to resilience. Based on reviews in the field of Cognitive Bias Modification (CBM), laboratory paradigms and self-report measures were primarily used to implement the interventions and assess their effects, resulting in unclear ecological validity. Thus, there is a gap regarding *mobile-based* interventions and *ambulatory assessment*, which is also shown by the Cochrane review in this thesis (see 2). The pilot study TRAIN$_4$Positivity in the second part of this work aims ad addressing these research gaps, by providing insights concerning the potential effects of a mobile-based positivity bias training (MB-PBT) at the action level. Using ambulatory assessment, the efficacy of a newly developed Ecological Momentary Intervention (EMI; called “Breezly”) is examined in healthy adults with many stressors.

Overall, this work aims at supplementing resilience intervention research twofold: First, to assess the evidence base on resilience trainings and their meaning (in HCP; see 2). Second, to evaluate a MB-PBT at the action level for the first time (see 3), to gain knowledge about its feasibility, and to possibly use it in individuals with elevated stressor exposure, such as HCP, in the future. For reasons of clarity, the two projects are presented separately, before combining them in concluding remarks.

2 Systematic Cochrane Review and Meta-Analysis on Psychological Interventions to Foster Resilience in Healthcare Professionals

2.1 Theoretical Background

2.1.1 Stressor Exposure in HCP and Effects on Mental Health

HCP are exposed to a substantial number of environmental and psychosocial stressors (Aiken et al., 2001; Hannigan et al., 2009; Jennings, 2008; Kumar, 2016; Lambert et al., 2004). *Patient-related stressors* may include pressure by patients and relatives up to physical or verbal aggression and exposure to suffering and death (Jackson et al., 2007; McAllister & McKinnon, 2009; McCann et al., 2013). *Work-related stressors* include, for example, time pressure, long and irregular working shifts, the responsibility of medical decision-making as well as social expectations (Lateef, 2011; McAllister & McKinnon, 2009; McCann et al., 2013). Moreover, healthcare staff can be exposed to many *organizational adversities*, such as multidisciplinary teamwork, inflexible hierarchies, staff downsizing, and technological changes (Jackson et al., 2007; McAllister & McKinnon, 2009; McCann et al., 2013; Zander-Jentsch et al., 2012).

This stressor exposure, often chronic in nature, can negatively affect the employees' mental health (see also Appendix D1.1). Physicians and other employees in the healthcare industry have been shown to report sleep disorders (M. S. Kim et al., 2018; Schlafer et al., 2014). Healthcare workers are at increased risk of developing burnout symptoms (e.g., increased emotional exhaustion compared to norms in 15.2%–43.2% of nurses over five European countries; Aiken et al., 2001). There is also considerable evidence for an increased risk of stress-related mental disorders (Gracino et al., 2016; Harvey et al., 2009; Robertson & Perry, 2010; Weinberg & Creed, 2000; Wieclaw et al., 2006) including, for example, symptoms of depression (Frank & Dingle, 2000; Gong et al., 2014; Tomioka et al., 2011) or posttraumatic stress disorder (Jonsson et al., 2003; Mealer et al., 2009; Ong et al., 2016). Due to emotional stressors, such as working with traumatized clients or patients, healthcare workers commonly reported secondary traumatic stress (e.g., Adams et al., 2006). Moreover, they have been shown to report higher rates of substance misuse (e.g., Horsfall, 2014) and increased perceived stress

(e.g., Leonelli et al., 2017). Finally, higher suicide rates for healthcare staff (e.g., physicians) compared to other disciplines have been demonstrated (Horsfall, 2014; Meltzer et al., 2008). In the face of chronic stressors and the negative impact on physical and mental health, healthcare workers have higher numbers of days absent due to illness (Michie & Williams, 2003; Moberly, 2018) and reported reduced job satisfaction (Kuburović et al., 2016; Lu et al., 2016). Overall, based on these findings, the concept of resilience has become increasingly important in the healthcare sector (Hart et al., 2014; McAllister & McKinnon, 2009; McCann et al., 2013; Robertson et al., 2016).

2.1.2 Definition of Resilience

Derived from the Latin "resilire" ("bounce back"), the concept of resilience has substantially changed over the past decades (Hu et al., 2015). *Trait resilience* refers to resilience defined as personal resources or static, positive personality characteristics that enhance an individual's adaptation (Block & Kremen, 1996; Nowack, 1989; Wagnild & Young, 1993). However, personality rather seems to be one of many factors that affect if a person maintains or regains mental health despite adversity (Bonanno & Diminich, 2013; Luthar et al., 2000). Therefore, although individuals with resilience-conducive traits may be more likely to have positive outcomes in view of adversity (Miller & Harrington, 2011), these traits should not be confounded with resilience (Kalisch et al., 2015).

According to an *outcome-oriented definition*, resilience is a positive outcome, that is, the maintenance or fast recovery of mental health despite significant adversity (Kalisch et al., 2017; Mancini & Bonanno, 2009). It can only be determined during or after the exposure to stressors (Chmitorz, Kunzler, et al., 2018). These may include short-term/acute or long-term/chronic stressors, micro- or macrostressors (e.g., daily hassles or critical life events and trauma), social, psychological, or physical stressors (Kalisch et al., 2017; Kalisch et al., 2015). Resilience as outcome is partially determined by resilience factors (Haglund et al., 2007; Iacoviello & Charney, 2014; Mancini & Bonanno, 2009; Southwick et al., 2005; Wu et al., 2013). The latter (see 2.1.3.2 and Appendix D2.1 and D2.2) describe (internal/external) resources protecting individuals from the potential negative impact of stressors (Chmitorz, Kunzler, et al., 2018; Fletcher & Sarkar, 2013; Rutter, 1985).

Most recently, based on a *process-oriented approach* (see also Appendix D1.2), resilience is considered as a multidimensional and dynamic process (Johnston et al., 2015; Kalisch et al., 2015; Kent et al., 2014; Southwick & Charney, 2012), characterized by either a trajectory of undisturbed mental health during or after adversities, or by temporary dysfunctions followed by successful recovery (APA, 2020a; Kalisch et al., 2015; Mancini & Bonanno, 2009; Norris et al., 2009; Rutten et al., 2013; Sapienza & Masten, 2011). Up to now, there is no general consensus on how to define resilience. However, researchers have agreed on two essentials: 1) the exposure to significant adversity, and 2) the successful adaptation to this stressor (Earvolino-Ramirez, 2007; Jackson et al., 2007; Luthar et al., 2000). This work is based on an outcome-and process-oriented resilience definition.

2.1.3 How Might Resilience Interventions Work? – Current Theories of Change

To date, there is no empirically validated framework outlining the mode of action of resilience training (Helmreich, Kunzler et al., 2017). This section suggests three models that might explain how the interventions work. Different theories of change are presented for various theoretical foundations.

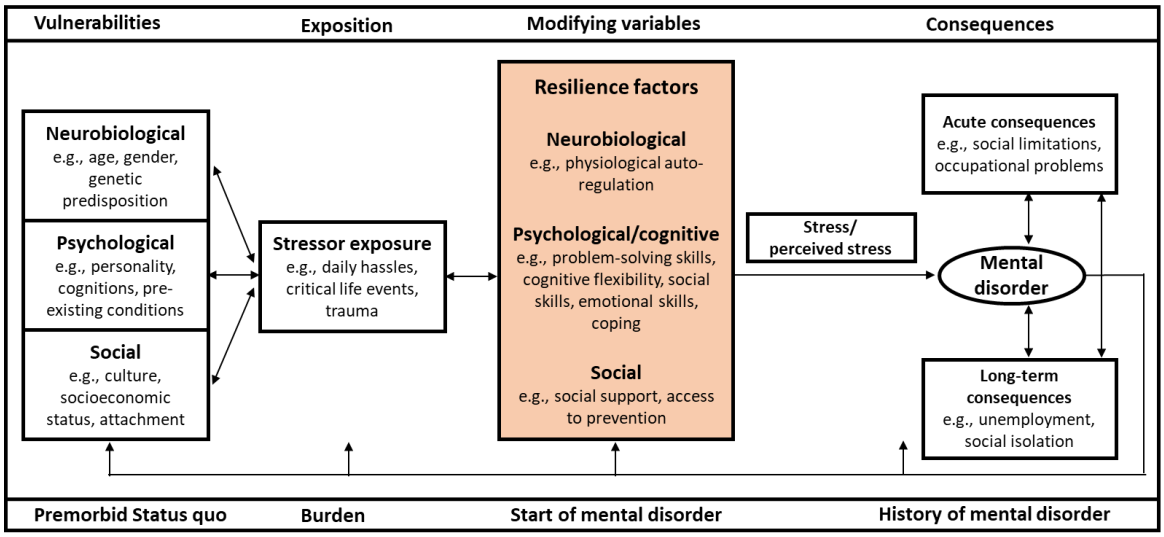
2.1.3.1 Diathesis-Stress Model

The diathesis-stress model (e.g., Berking, 2012; Wittchen & Hoyer, 2011) is a multifactorial model on the development of psychopathology (Figure 1). Mental disorders are considered as the result of predispositional vulnerabilities (i.e., diathesis) that interact with stressor exposure. According to the model, biological (e.g., age), psychological (e.g., cognitive styles), and social (e.g., socioeconomic status) vulnerabilities increase the risk of developing mental dysfunctions (Ingram & Luxton, 2005), that are finally triggered by occurring adversities. The latter may include acute and discrete events (e.g., accident), but also chronic stressors (e.g., illness). According to life-event theory, especially critical life events (e.g., death of partner) can contribute to mental illness (Holmes & Rahe, 1967). On the other hand, the long-term accumulation of daily hassles (e.g., traffic jam) may facilitate the development of mental disorders (Berking, 2012). However, vulnerabilities and stressors do not

necessarily lead to mental impairments. Instead, the model assumes a series of modifying variables (moderators) which can prevent or weaken ("buffer") the potential pathological effects (Wittchen & Hoyer, 2011).

Figure 1

Diathesis-Stress Model (Adapted From Berking, 2012; Wittchen & Hoyer, 2011)



These can include physiological factors (e.g., negative feedback mechanism finishing the release of stress hormone cortisol), psychological variables (e.g., cognitive flexibility), or social resources (e.g., social support). *Resilience factors* could also work as such modifying factors. In the face of adversity (e.g., patient-related stressors in clinical practice), well-developed resilience factors may contribute either to stable, undisturbed mental health or facilitate the recovery of mental health during or after stressors (see 2.1.2). Thus, resilience trainings might work by strengthening one or several resilience factors to foster resilience as outcome.

2.1.3.2 Resilience Factors

To date, various resilience factors have been investigated, with three levels of evidence to be differed (Helmreich, Kunzler et al., 2017). The current work focused on psychosocial resilience factors with strong evidence (level 1; see Appendix D2.1 and D2.2). Among level 1a-factors, *active coping* describes the tendency to use active coping styles (e.g., problem-solving) when being confronted with

stressors (Iacoviello & Charney, 2014; Kent & Davis, 2010). *Self-efficacy* refers to an individual's subjective beliefs about dealing with demanding situations through the use of own skills (Southwick, Litz, et al., 2011). *(Dispositional) Optimism* includes a stable tendency for positive expectations for the future and a positive explanatory style (Iacoviello & Charney, 2014; Southwick, Litz, et al., 2011). *Social support* refers to the (perceived) support available to individuals through their social network (Cohen & Syme, 1985; Ozbay et al., 2007). *Cognitive flexibility* describes the ability to react to changing environmental conditions by flexible thinking, including positive reappraisal or by accepting negative situations (Iacoviello & Charney, 2014; Kent & Davis, 2010). *Religiosity and religious coping* describe sharing religious beliefs and practices within a social community to connect with the sacred or God (Southwick, Litz, et al., 2011). *Spirituality* refers to an individual's dealing with questions of meaning in life, especially with respect to negative life events (Kent & Davis, 2010; Park, 2005; Southwick, Litz, et al., 2011). At level 1b, *positive emotions* include the regular experience of positive affect, also during or after adversities (Jackson et al., 2007; Kent & Davis, 2010; Southwick, Litz, et al., 2011). *Hardiness* describes a modifiable personality trait (Bonanno, 2004; Maddi, 2002) consisting of challenge (i.e., demanding situations are challenges allowing personal growth), commitment (i.e., full engagement in different areas) and control (i.e., perceived ability to control life events; Bartone et al., 1989; Kent & Davis, 2010). *Self-esteem* means an individual's positive self-appraisal (e.g., self-acceptance; Bengel & Lyssenko, 2012; Kent & Davis, 2010). As level 1c-factor, *meaning or purpose in life* describes a subjective feeling of meaningfulness, by living according to one's own values (Kent & Davis, 2010; Winger et al., 2016). *Sense of coherence* refers to the global orientation of viewing challenging life circumstances as comprehensive, meaningful, and manageable (Antonovsky, 1987; Eriksson, 2016).

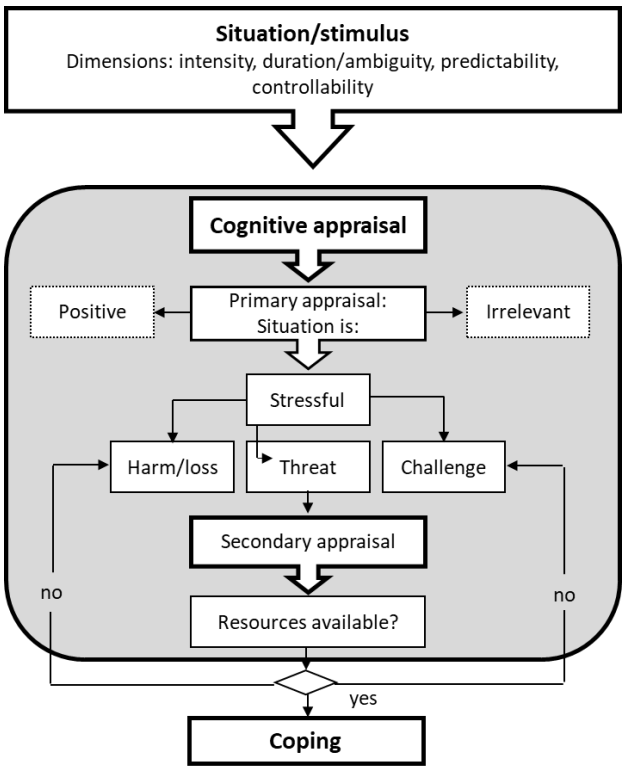
2.1.3.3 Transactional Model of Stress and Coping

The transactional model of stress and coping (Lazarus & Folkman, 1984) emphasizes the role of cognitive appraisal in relation to stressors. Stress is viewed as the result of a perceived imbalance between situational demands on the one hand and (internal/external) resources on the other. It is determined by two forms of appraisal (Figure 2). During *primary appraisal*, the motivational relevance

of a situation regarding personal well-being is judged with three possible categories (positive, irrelevant, stressful). Only if a situation is perceived as stressful, the stressor is further appraised as harm/loss, threat, or challenge. *Secondary appraisal* occurs in all three cases, meaning that individuals judge their coping repertoire and the availability of resources to successfully cope with the stressor (Lazarus & Folkman, 1984). Stress is experienced if an individual concludes of having no or limited resources to manage the demanding situation. Both forms of appraisal can be repeated and revised (i.e., reappraisal; Lazarus & Folkman, 1984). Negative outcomes of cognitive appraisal include either short-term (e.g., negative emotions) or long-term effects (e.g., mental disorder). In line with this model, resilience factors might work in three ways. First, the availability of resilience factors (e.g., self-efficacy) could positively affect primary appraisal. For example, compared to HCP with no or limited self-efficacy, individuals with strong self-efficacy beliefs might judge a demanding situation in clinical practice as less stressful or even as irrelevant or positive. Second, even if the primary appraisal was negative, resilience factors might contribute to successful coping by secondary appraisal. For instance, in case an individual judges a situation as stressful, but perceives enough social support (e.g., in multidisciplinary team), this might result in less perceived stress and mental health despite stressors. Third, even if both forms of appraisal were negative, the availability of resilience factors might facilitate later reappraisals of the situation itself and of an individual's repertoire of resources. To sum up, resilience training – through the training of resilience factors – might influence an individual's cognitive appraisal of stressors, thereby preventing or minimizing perceived stress and fostering resilience as outcome.

Figure 2

Transactional Model of Stress and Coping (Adapted/Translated From Franke, 2012)

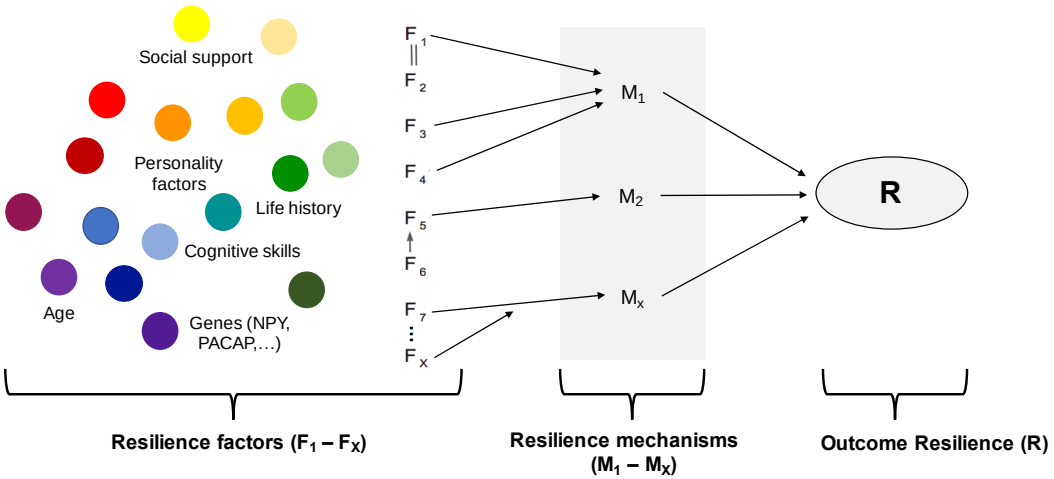


2.1.3.4 Positive Appraisal Style Theory of Resilience (PASTOR)

Since several resilience factors conceptually overlap and are likely to interact with each other, the existence of superordinate resilience mechanisms has been suggested (Kalisch et al., 2015; Luthar et al., 2000). A limited number of (neural, cognitive, or psychological) mechanisms may mediate the effect of resilience factors on resilience as outcome (Figure 3). In PASTOR, a positive appraisal style is assumed as one potential cognitive resilience mechanism (Kalisch et al., 2015). For example, social support may contribute to the positive (re-)appraisal of a stressor, which would in turn result in maintaining or regaining mental health. PASTOR presents an adaptation of the transactional model (see 2.1.3.3) as it also emphasizes the role of appraisal for the effects of adversities on mental health and assumes this appraisal to be influenced by resources (compare secondary appraisal). Thus, resilience trainings might enhance resilience by teaching resilience factors, thereby fostering the mediator positive (re-)appraisal.

Figure 3

Mediating Role of Resilience Mechanisms (Taken From Gilan, Kunzler et al., 2018)



Note. F = factor; M = mechanism; R = resilience.

2.1.3.5 Theories of Change Based on Theoretical Foundation of Resilience Interventions

Depending on the theoretical basis of resilience trainings, various theories are assumed on how resilience factors and hence resilience might be affected (Helmreich, Kunzler et al., 2017). From a cognitive-behavioral perspective, stress-related mental disorders are considered to be the result of dysfunctional thinking (Beck, 2011; Benjamin et al., 2011). In the face of adversity, people show maladaptive behavioral responses and/or experience negative emotions due to irrational cognitions (Beck, 1976; Ellis & Harper, 1975), which is in line with other stress (resilience) theories assuming that not the stressor itself, but its cognitive appraisal may lead to stress reactions (see 2.1.3.3, 2.1.3.4). Modifying cognitions into more adaptive patterns of thought might therefore also produce more adaptive responses to stress (Beck, 1964). By challenging an individual's maladaptive thoughts, cognitive-behavioral therapy (CBT)-based resilience trainings might be beneficial in promoting cognitive flexibility, for example. As one form of CBT, stress inoculation therapy (SIT) is based on the assumption that exposing individuals to mild forms of stress can strengthen coping strategies and the individual's confidence in using his/her coping repertoire (Meichenbaum, 2007). Resilience-training programs grounded in SIT might foster resilience by enhancing factors such as active coping and self-efficacy. Problem-solving therapy (PST), closely related to CBT, is based on problem-solving theory. Effective problem-solving can attenuate the negative effects of stressors and adversity on well-being

by moderating or mediating their effects on emotional distress (Nezu et al., 2016; Nezu et al., 2013). PST-based resilience interventions that enhance an individual's positive problem orientation and planful problem-solving (Nezu et al., 2013) might foster the participants' adjustment to stressors by increasing the resilience factors self-efficacy, optimism, and active coping.

According to Acceptance and Commitment Theray (ACT; Hayes et al., 2004; Hayes et al., 2006), psychopathology primarily results from psychological inflexibility (Hayes et al., 2006), which is also relevant when an individual is exposed to stressors. By teaching acceptance and mindfulness skills (e.g., being in contact with present moment) and commitment and behavior-change skills (e.g., values, committed action), several resilience factors might be fostered in ACT-based trainings (e.g., cognitive flexibility, purpose in life).

In mindfulness-based therapy (e.g., Mindfulness-based Stress Reduction [MBSR]; Kabat-Zinn, 1996), mindfulness is characterized by the non-judgmental awareness of the present moment and its accompanying mental phenomena (e.g., thoughts, emotions). Since practitioners learn to accept whatever occurs in the present moment, they are thought to adapt more efficiently to stressors (Grossman et al., 2004; Shapiro et al., 2005). As being more aware of the "here and now" possibly enhances the sensitivity for positive aspects in life, mindfulness-based interventions might also help participants to gain a brighter outlook for the future (i.e., optimism) or to experience positive emotions more regularly. Teaching mindfulness might also increase the participants' cognitive flexibility by learning to accept negative situations and emotions.

According to Attention and Interpretation Therapy (AIT), each behavior is influenced by attention and interpretation (Sood, 2010), which are automatically negatively biased (Sood et al., 2011). Using attention and interpretation training, AIT teaches subjects to delay negative judgements of their environment, to focus their attention on the world's novelty (Sood et al., 2011), and to take a more flexible perspective by learning higher-order principles (e.g., gratitude, acceptance, and meaning; Sood, 2010; Sood et al., 2011). Resilience trainings using an AIT approach could especially enhance the participants' cognitive flexibility (e.g., reappraisal), positive emotions, and meaning in life.

Coaching interventions usually include a helping relationship between clients (e.g., in executive coaching often with managerial responsibility; e.g., Grant et al., 2009) and a coach using different techniques (e.g., 360-degree feedback, problem-solving) to support the client(s) in identifying and meeting important goals in their personal and professional lives (APA, 2020b, 2020c). As coaching relies on models of problem-solving or related frameworks (e.g., Goal, Reality, Options, Way forward [GROW]; Grant et al., 2009), respective resilience trainings might increase active coping.

Positive Psychology (PP) refers, for example, to the study of positive psychological states and emotions as well as character strengths that enhance subjective well-being, enable individuals to thrive, and lead to meaningful lives (APA, 2020d; Positive Psychology Center, 2020; Seligman & Csikszentmihalyi, 2000). A number of constructs, such as happiness, meaning making, and optimism, is subsumed under this umbrella term (Luthar et al., 2014) and is focused in respective programs. Thus, resilience interventions using a PP approach might also foster various resilience factors, such as positive emotions and optimism. Further details are presented in Appendix D1.3.

2.1.4 What do we Know About Resilience Interventions? Findings From Previous Research

Effects on psychological outcomes in HCP over time. Previous systematic reviews (see 2.1.5) largely reported positive effects of resilience-training programs on psychological outcomes (e.g., resilience, mental health outcomes like stress and anxiety, resilience factors) in various adult populations (e.g., Macedo et al., 2014; Robertson et al., 2015) and healthcare workers (e.g., Cleary et al., 2018; Fox et al., 2018; Pezaro et al., 2017). Based on meta-analyses, evidence for immediate (i.e., posttest) and short-term (≤ 3 months) improvements by training was found for resilience (small effects; Joyce et al., 2018; Leppin et al., 2014), (perceived) stress (moderate effect; Leppin et al., 2014), anxiety, depression (small to moderate effects; Leppin et al., 2014 for trauma-focused trainings; Vanhove et al., 2016), well-being (Vanhove et al., 2016), and resilience factors (for details, see Appendix A1). Medium- and long-term follow-up (FU) periods were hardly investigated, with only evidence for maintained effects of training for the prevention of psychological deficits (> 1 month; Vanhove et al., 2016) or resilience at 6-month FU by mindfulness interventions (Joyce et al., 2018).

Impact of intervention setting. Resilience interventions might work differently depending on training setting (Robertson et al., 2015; Vanhove et al., 2016; e.g., mutual learning/support in group setting versus more attention for individual needs in one-on-one setting; for details, see Appendix A2). In general, research on group versus individual therapy of mental disorders is rather inconsistent, with similar effects (Cuijpers et al., 2011; McRoberts et al., 1998; Smith et al., 1980) versus (vs.) advantages of one of the two settings found (Hodgkinson et al., 2000; McDermut et al., 2001; Nietzel et al., 1987). In resilience research, positive effects of resilience training in HCP were found for group-based (e.g., Mache, Danzer, et al., 2015) as well as individual programs (e.g., Sood et al., 2011), although no trial has compared a group and individual format of the same intervention so far. In reviews or meta-analyses, the impact of setting on the efficacy of trainings has rarely been examined, with a lack of research concerning the comparison of group and individual settings with mixed interventions. In a meta-analysis on PP-based interventions – a field closely related to resilience research – Bolier et al. (2013) identified training setting as a moderator with larger effects found for interventions implemented on an individual basis. Vanhove et al. (2016) also showed a larger effect sizes (ES) for one-on-one compared to group formats, with the caveat of only a small number of studies.

Impact of delivery format. In addition to face-to-face resilience trainings, other delivery formats (e.g., online-/mobile-based, telephone, bibliotherapy) are increasingly used in resilience intervention research. Each of these formats has potential benefits and disadvantages (e.g., low access threshold and 24/7 availability in online-/mobile-based trainings vs. more direct contact with trainer and higher compliance in face-to-face interventions; for details, see Appendix A3). In general, positive effects for different deliveries were demonstrated in reviews on psychotherapy and other scopes like stress management (e.g., Anderson et al., 2005; Gregory et al., 2004; Heber et al., 2017; Okuyama et al., 2014; Portnoy et al., 2008). For psychotherapy, most meta-analyses concluded similar effects for different formats (e.g., Andersson et al., 2014; Carlbring, 2018; Cuijpers et al., 2010; J. Klein et al., 2016), whereas the evidence is rather inconsistent concerning other purposes (e.g., Kuster et al., 2017; Sitzmann et al., 2006). In resilience research, the role of delivery format was rarely focused,

neither in primary studies nor reviews. For PP trainings (including resilience programs), Bolier et al. (2013) found differences in favor of face-to-face delivery compared to self-help. Similarly, Vanhove et al. (2016) found larger ES for face-to-face trainings, but no evidence of effect for computer-based formats.

Impact of intensity. Resilience interventions might also work differently depending on training intensity (e.g., cost-effectiveness and maintained treatment motivation in low-intensity trainings vs. more possibilities to apply the learned strategies in daily life in high-intensity trainings; for details, see Appendix A4). In psychotherapy research, findings concerning the relationship between treatment dose (e.g., number of sessions) and outcome improvement were mixed (e.g., Cuijpers, Huibers, et al., 2013; Howard et al., 1986). Bolier et al. (2013), in a review on PP-based interventions, demonstrated benefits of longer trainings for depression. On the individual-study level, positive effects on resilience and mental health were found for low- (e.g., Sood et al., 2011), moderate- (e.g., Luthar et al., 2017), and high-intensity resilience-training programs (e.g., Mache, Danzer, et al., 2015). However, no previous review or meta-analysis has examined the role of training duration on the ES of resilience interventions.

Impact of theoretical foundation. Resilience trainings are based on various theoretical foundations (see 2.1.3.5). In general, therapy research concerning stress-related mental disorders mostly reported positive effects for each of the various approaches (e.g., A-Tjak et al., 2015; Cuijpers et al., 2016; Hofmann et al., 2010). With respect to comparisons *between* theoretical foundations, mixed findings were observed (e.g., Cuijpers, Berking, et al., 2013; Ruiz, 2012; for details see Appendix A5). For resilience intervention research, Joyce et al. (2018) identified a small positive effect of CBT-based interventions compared with control on resilience, whereas larger ES were shown for mindfulness-based or mixed trainings (i.e., combination of both). At 6-month FU, only the effect of mindfulness-based resilience programs remained significant. In a narrative review, Rogers (2016) concluded the strongest evidence for CBT and PST as well as combined programs in HCP. However, differences in the efficacy of resilience interventions depending on other theoretical foundations were

not considered so far, illustrating a current research gap.

2.1.5 Why it is Important to Conduct Another Review

To date, a considerable number of systematic reviews and meta-analyses have synthesized the efficacy of resilience-training programs in various adult populations (see Appendix D1.4 and 2.1.4) as well as HCP (see Appendix D1.6 and 2.1.4). In general, the publications mostly agree in their conclusion that resilience training can improve resilience, mental health (e.g., anxiety, perceived stress), and (job) performance in different adult populations and HCP in particular.

However, despite this positive evidence, the previous reviews and meta-analyses suffer from several methodological weaknesses that limit the robustness of their findings (see Appendix D1.7) and might also partly explain the large variety of ES that was found for psychological outcomes (see Appendix D1.5). These include heterogeneous eligibility criteria, for example, for study design (e.g., only RCTs vs. various designs) or types of interventions (see Appendix D1.4 and Appendix D1.6). While some reviews only included training programs with the stated intention to enhance resilience (e.g., Fox et al., 2018) or those teaching certain resilience factors (e.g., Vanhove et al., 2016), others did not describe the criteria concerning types of interventions in detail (e.g., Wright et al., 2017). Some reviews searched for resilience and associated constructs (e.g., hardiness; e.g., Foster et al., 2019), used specific terms of intervention (e.g., mindfulness; e.g., Gilmartin et al., 2017), and involved various terms for healthcare staff (e.g., Lavin Venegas et al., 2019), others, however, rather used a narrow search (e.g., only resilience/hardiness combined with training terms or terms for healthcare sector, e.g., Joyce et al., 2018; restriction to English language and no grey literature in most reviews), which may bias the search results. For the healthcare sector in particular, several reviews did not specify whether they had been conducted according to validated guidelines, such as PRISMA (e.g., Elliott et al., 2012; Gillman et al., 2015). Besides, the assessment and reporting of the quality (risk of bias, RoB) of included studies varied as the reviews often relied on different tools. The lack of a published protocol or pre-registration for most of these reviews also reduces the transparency and potentially restricts the evidence found. Meta-analyses for psychological outcomes have been only conducted in

three reviews in different adult populations (Joyce et al., 2018; Leppin et al., 2014; Vanhove et al., 2016). For HCP, only Lavin Venegas et al. (2019) was able to perform a meta-analysis that was restricted to burnout outcomes and mainly observational studies. Finally, the number of RCTs included in previous reviews on HCP is rather limited, and the search period covered is until June 2018 (Foster et al., 2019), preventing conclusions about the efficacy of interventions developed since then.

Overall, the different foci and methodological limitations of previous reviews do not allow reliable conclusions about the efficacy of resilience trainings, their long-term effects, or adequate forms of implementation, both in general and particularly concerning HCP. To address these metholodogical weaknesses, the first part of this work consists of synthesizing the current evidence on the efficacy of resilience interventions in HCP since 1990 in a systematic review with meta-analyses according to the Cochrane Collaboration standards (Kunzler et al., 2020). The review aims to provide detailed conclusions on which interventions are most likely to foster resilience in this target group. In the following, the research questions (RQs) and hypotheses are presented.

2.1.6 Research Questions and Hypotheses

As resilience intervention research is highly up-to-date, and given the increasing interest in health promotion for HCP, this thesis focuses on addressing research gaps concerning the efficacy of resilience trainings in this target group (see 2.1.4). The effects of such programs on different mental health outcomes and the impact of potential effect modifiers are investigated, leading to five RQs. Based on the previous research (see 2.1.4 and Appendix D1.4 to D1.6), the current work also assumes small positive effects of resilience training on psychological outcomes in HCP. Larger ES are expected in the short-term (≤ 3 months), while a decrease of ES over time is anticipated (see RQ1). In light of the heterogeneous evidence (see 2.1.4) concerning the role of intervention setting, delivery format, and theoretical foundation, the impact of these potential effect modifiers is focused in exploratory RQs. The insufficient state of research regarding the impact of training intensity on the efficacy of resilience trainings leads to another exploratory RQ.

Research question 1 (RQ1). Which effects do resilience interventions have on psychological

outcomes in HCP?

Hypothesis 1.1: Compared with a control group, there is evidence for at least small short-term (i.e., posttest, ≤ 3-month FU[1]) ES of resilience interventions in HCP on (1.1.1) resilience, (1.1.2) anxiety symptoms, (1.1.3) depressive symptoms, (1.1.4) (perceived) stress, (1.1.5) well-being, (1.1.6) social support, (1.1.7) optimism, (1.1.8) self-efficacy, (1.1.9) active coping, (1.1.10) self-esteem, (1.1.11) hardiness, and (1.1.12) positive emotions.

Hypothesis 1.2: At medium- (> 3 ≤ 6-months) and long-term FU (> 6 months), the ES of resilience interventions in HCP on the above outcomes decrease or there is no longer evidence for an effect.

RQ2 (exploratory). Are there any differences between resilience interventions using different settings (i.e., group, individual, combined, or unspecified setting) concerning the effects on (2.1) resilience, (2.2) anxiety symptoms, (2.3) depressive symptoms, (2.4) (perceived) stress, or (2.5) well-being in HCP?

RQ3 (exploratory). Are there any differences between resilience interventions using different delivery formats (i.e., face-to-face, online- or mobile-based, telephone, bibliotherapy, laboratory, multimodal, or unspecified delivery) concerning their effects on (3.1) resilience, (3.2) anxiety symptoms, (3.3) depressive symptoms, (3.4) (perceived) stress, or (3.5) well-being in HCP?

RQ4 (exploratory). Are there any differences between resilience interventions using different intensities (i.e., low intensity: ≤ 5 hours or ≤ 3 sessions; moderate intensity: > 5 hours to ≤ 12 hours or > 3 to ≤ 12 sessions; high intensity: > 12 hours or > 12 sessions; unspecified intensity) concerning their effects on (4.1) resilience, (4.2) anxiety symptoms, (4.3) depressive symptoms, (4.4) (perceived) stress, or (4.5) well-being in HCP?

RQ5 (exploratory). Are there any differences between resilience interventions based on different theoretical foundations (i.e., CBT, SIT, PST, ACT, mindfulness-based, AIT, coaching, PP, combination, or unspecified theoretical foundation) concerning their effects on (5.1) resilience, (5.2), anxiety symptoms, (5.3) depressive symptoms, (5.4) (perceived) stress, or (5.5) well-being in HCP?

[1] Follow-up consistently indicated with respect to the end of intervention

2.2 Methods

The methods of this review have been prespecified in a protocol (Helmreich, Kunzler et al., 2017). Certain eligibility criteria were modified in a postprotocol amendment. Differences between the protocol and the review are presented in Appendix D17.1. The following sections were largely taken from the corresponding Cochrane publication (Kunzler et al., 2020). All methods followed the Cochrane Handbook for Systematic Reviews of Interventions (Higgins, Thomas, et al., 2019).

2.2.1 Eligibility Criteria

2.2.1.1 Types of Participants

Adults aged ≥ 18 years, who are employed as *HCP*, were considered. These included healthcare workers delivering direct medical care (e.g., physicians, nurses, hospital personnel) and allied healthcare staff working in health professions, as distinct from medical care (e.g., psychologists, social workers, counselors, physical therapists, occupational therapists, speech therapists, medical assistants, medical technicians). Participants were included irrespective of health status. At the time of the intervention, individuals had to be exposed to potential risk or stressors, which was ensured by focusing on healthcare staff (see 2.1.1; Appendix D17.1). Studies involving *mixed samples*, that is, HCP and participants working outside of the healthcare sector (e.g., ambulance personnel and firefighters), were also included in the review. These studies were considered in meta-analysis (see 2.2.4.2) if data for HCP were reported separately or could be obtained by contacting the study authors.

2.2.1.2 Types of Interventions

Any psychological resilience intervention, irrespective of content, duration, setting, or delivery was included. Psychological resilience trainings were defined as follows: 1) interventions that explicitly stated the aim of fostering resilience or the related concepts of hardiness or posttraumatic growth, 2) by strengthening at least one well-evidenced resilience factor (level 1a-1c) that is assumed to be modifiable (see 2.1.3.2; Appendix D2.1). Since hardiness and posttraumatic growth are sometimes used synonymously to resilience, this review also considered studies focusing on these related

constructs. To ensure objective inclusion criteria, at least one of the terms had to be mentioned in a publication (Appendix D17.1). Regarding the criterion of fostering at least one level-1 factor, Appendix D2.2 provides training examples for these factors. Pharmacological (e.g., antidepressants) and physical (e.g., exercise) interventions or relaxation techniques (e.g., autogenic training) were only relevant if being part of a psychological resilience training. Studies merely investigating the efficacy of disorder-specific psychotherapy (e.g., CBT for depression) were excluded.

2.2.1.3 Types of Comparators

The comparators, that were considered in this review, included no intervention, wait-list control, treatment as usual (TAU), active control, and attention control. The term "attention control" was used for alternate treatments that mimicked the amount of time and attention received (e.g., by trainer) in the treatment group. Active controls were considered to involve an alternative treatment (no TAU; e.g., treatment developed specifically for the study), but that did not control for the amount of time and attention in the intervention group (IG) and was not attention control in a narrow sense.

2.2.1.4 Types of Outcomes

To date, there is no consensus about how to operationalize resilience (see 2.1.2). Therefore, in addition to resilience (1), as measured by improvements in "resilience scales" (e.g., Resilience Scale for Adults, Friborg et al., 2003; compare e.g., Pangallo et al., 2015; Windle et al., 2011), measures of mental health and well-being were also defined as *primary outcomes*. Consistent with the protocol (Helmreich, Kunzler et al., 2017), these outcomes were subsumed into four categories, as measured by increases in respective scales (e.g., Depression Anxiety and Stress Scale, DASS-21; Lovibond & Lovibond, 1995). They included anxiety (2), depression (3), (perceived) stress (4), and well-being (5; e.g., well-being, life satisfaction, (health-related) quality of life, vitality, vigor). Appendix D2.3 and D2.4 present example scales for these primary outcomes (Helmreich, Kunzler et al., 2017). Any adverse outcomes or events (6) were also examined as primary outcomes. *Secondary outcomes* referred to a range of psychosocial resilience factors (level 1a-1b; Appendix D2.1). They included social support (1),

optimism (2), self-efficacy (3), active coping (4), self-esteem (5), hardiness[2] (6), and positive emotions (7), as assessed by improvements in respective scales (e.g., Life Orientation Test-Revised for optimism; Scheier et al., 1994). Taken from the protocol (Helmreich, Kunzler et al., 2017), Appendix D2.5 shows examples of measures of these outcomes. Those were extracted and reported whenever assessed. Overall, self- and observer-rated (e.g., clinician) measures as well as study outcomes at all time frames were considered. The absence of the primary or secondary outcomes was not an exclusion criterion.

2.2.1.5 Types of Studies

RCTs including cluster-RCTs (e.g., hospitals as possible clusters) were considered.

2.2.2 Search Methods for Identification of Studies

2.2.2.1 Search Period, Search Terms, and Search Dates

The searches were not restricted regarding publication language, status, or format, but were limited to the period January 1990 onwards. The latter was chosen to account for the fact that the resilience concept and its operationalization have substantially developed over the past decades (Fletcher & Sarkar, 2013; Hu et al., 2015). Due to the heterogeneity in resilience definitions already shown for 1990 to 2014 (Robertson et al., 2015), the restriction to 1990 onwards served to identify resilience-training studies with similar resilience concepts and assessments. Moreover, the outcome- and process-oriented approach had only emerged in recent years and paved the way for the development of interventions (Leppin et al., 2014; Southwick, Pietrzak, et al., 2011)[3].

Search terms were chosen based on search strategies in previous reviews (Leppin et al., 2014; Macedo et al., 2014; Robertson et al., 2015; Vanhove et al., 2016)[4]. Appendix B1 includes further details on the components of the search syntax. The first searches were run in October 2016 based on the above-described strategy before changing the inclusion criteria of the review to focus on HCP

[2] Hardiness was conceptualized as resilience factor in the current review (see Appendix D2.1).

[3] The idea of fostering resilience by training is relatively new (Leppin et al., 2014); see also Macedo et al. (2014) who searched for resilience intervention studies every year until 2013 but only found RCTs published after 1990.

[4] No other published reviews/meta-analyses at the time of writing the review protocol

(Appendix D17.1). For top-up searches in June 2019, a new section was added to the original syntax, using search terms to limit the search to healthcare sector workers and students.

2.2.2.2 Electronic Searches

Relevant trials were retrieved from the following electronic databases (see Appendix B1 for the time frame contained in the respective databases or trial registers): Cochrane Central Register of Controlled Trials (CENTRAL; 2019 Issue 6) in the Cochrane Library, MEDLINE Ovid, Embase Ovid, PsycINFO Ovid, CINAHL EBSCOhost (Cumulative Index to Nursing and Allied Health Literature), PSYNDEX EBSCOhost, Web of Science Core Collection Clarivate, International Bibliography of the Social Sciences ProQuest (IBSS), Applied Social Sciences Index & Abstracts ProQuest (ASSIA), ProQuest Dissertations & Theses (PQDT), Cochrane Database of Systematic Reviews (CDSR; 2019 Issue 6) in the Cochrane Library, Database of Abstracts of Reviews of Effects (DARE) in the Cochrane Library, Epistemonikos, and ERIC EBSCOhost (Education Resources Information Center Institute of Education Sciences). In addition, the following trial registers were searched electronically from 1 January 1990 to 24 June 2019, respectively: Current Controlled Trials, ClinicalTrials.gov, and World Health Organization International Clinical Trials Registry Platform (WHO ICTRP). Appendix B2 and Appendix B3 contain the search strategy for MEDLINE. For this database and Embase, the Cochrane Highly Sensitive Search Strategy for RCTs (Lefebvre et al., 2019) was used. The terms and syntax for MEDLINE were adapted for other databases. The detailed search strategies are reported in Appendix D3.1 (up to 2016) and Appendix D3.2 for the revised inclusion criteria (2016 onwards). A further scoping search of four key databases (CENTRAL, CINAHL EBSCOhost, PsycINFO Ovid, ClinicalTrials.gov) was performed in June 2020 prior to the publication of the review (Kunzler et al., 2020). The results are awaiting classification and will be incorportated into the review at the next update.

2.2.2.3 Searching Other Sources

The reference lists of all included RCTs and relevant reviews were inspected for further eligible studies. Researchers in the field and authors of selected studies were contacted to check for any

unpublished or ongoing studies. If data were missing or unclear, the study authors were contacted.

2.2.3 Data Collection

In the successive parts, only the methods used in this review are described. Preplanned but unused methods are reported in Appendix D17.2.

2.2.3.1 Selection of Studies

Two reviewers (AK, IH) independently screened titles/abstracts to determine eligible studies. Clearly irrelevant papers were excluded immediately. At full text level, eligibility was also checked in duplicate by two reviewers (AK, IH), working independently. For records excluded at full text level, exclusion reasons (e.g., ineligible study design) were documented. If the eligibility criteria of this review could not be determined based on a full text or the status of a trial (e.g., study protocol) was unclear, the primary investigators were contacted to seek additional information (second inquiry if no response to first email). Inter-rater reliability using Cohen's kappa (κ; Cohen, 1960) was calculated at both stages (title/abstract, full text). Any disagreements in study selection were resolved by discussion. Where no consensus was found, a third reviewer (AC or KL) arbitrated. All decisions regarding the study selection were recorded in a PRISMA flow diagram (Moher et al., 2009). The feasibility of the selection criteria was assessed a priori by screening 500 records to attain acceptable inter-rater reliability (Appendix D17.1). There was a good agreement between reviewers (κ = .72) and thus no need to refine or clarify the criteria. For scientific reasons, the elibility criteria were adapted during review development (Appendix D17.1).

2.2.3.2 Data Extraction and Management

A data extraction sheet (Appendix B4) was developed based on the Cochrane guidelines (Li et al., 2019), consisting of the following aspects: source and eligibility for the review, study methods (e.g., design), allocation process, participant characteristics, interventions and comparisons, outcomes and scales, results (e.g., means and standard deviations [SDs] in any standardized scale), and

miscellaneous aspects. The extraction sheet was tested on 10 randomly-selected included studies, with a sufficient agreement between the reviewers achieved. Data of included RCTs were extracted in duplicate by two independent reviewers (AK, IH). Any disagreements in data collection were resolved by discussion. If necessary, the study authors were contacted (twice) to seek additional information. Where no consensus was reached, a third reviewer (AC or KL) arbitrated.

2.2.4 Data Analysis and Synthesis

2.2.4.1 Assessment of Risk of Bias (RoB) in Included Studies

Two reviewers (AK, IH) independently assessed the RoB of the included studies using the criteria in the Cochrane Handbook (Higgins, Altman, & Sterne, 2011). In accordance with Cochrane's tool for assessing RoB (Higgins, Altman, Gøtzsche, et al., 2011), the following domains were critically assessed (see Appendix B5): sequence generation and allocation concealment (selection bias), blinding of participants and personnel (performance bias), blinding of outcome assessment (detection bias), incomplete outcome data (attrition bias), and selective outcome reporting (reporting bias). Performance and detection bias were judged separately for objective (e.g., physiological variables) and subjective outcomes (e.g., self-report scales). In addition, the achieved baseline comparability between study conditions, which is not defined in the Cochrane Handbook, was considered as part of selection bias (random sequence generation). In the first part of the assessment, it was described what the authors reported to have done in the study for each domain, before assigning a judgement for the RoB (low, high, or unclear) for the entry. Any conflicts were resolved by discussion or by involving a third reviewer (AC or KL).

2.2.4.2 Data Synthesis and Measures of Treatment Effect

First, based on the extracted data (see 2.2.3.2), results of this review were synthesized narratively and in tabular form (Appendix D6), by describing the populations assessed, the resilience interventions, their theoretical concept, and the outcomes investigated. Study characteristics summarized here served to assess the clinical heterogeneity (see 2.2.4.5). Second, outcome measures

of included studies were combined through pairwise meta-analyses (any resilience training vs. control) to determine summary (pooled) intervention effects of resilience-training programs in HCP. The calculation of pairwise meta-analyses for the primary and secondary outcomes at different FU periods (see 2.2.4.3) served to investigate RQ1 (hypothesis 1.1 and 1.2) of this review (see 2.1.6). The decision to summarize numerical results of RCTs in meta-analyses depended on the number of studies found[5] as well as the homogeneity of included studies in terms of population (e.g., age, sex), resilience interventions (i.e., comparable content and modalities), comparisons, outcomes measured (i.e., same prespecified outcome albeit different assessment tools), and the methodological quality (RoB) of selected studies. Meta-analyses were conducted since intervention studies did not differ excessively regarding their content, outcomes (measures) were not too diverse, and there were no individual studies predominantly at high RoB. Because the included studies used different scales to assess resilience and related constructs (see Appendix D13.3 and Appendix D13.4), standardized mean difference (SMD) ES (Hedges' adjusted *g*) and their 95% confidence intervals (CIs) were used for continuous data in the pairwise meta-analyses. ES were calculated based on means, *SD*s and sample sizes for each study condition. In case respective data were not provided, Hedges *g* was computed from alternative statistics (e.g., *t* test, change scores). The magnitude of effect for continuous outcomes was assessed using the criteria for interpreting SMDs suggested in the Cochrane Handbook: a value of 0.2 indicates a small effect, a moderate effect is represented by 0.5, and 0.8 indicates a large effect (Schünemann, Vist, et al., 2019). The preplanned method for analyzing dichotomous outcomes (see Helmreich, Kunzler et al., 2017) was not needed as only two studies reported dichotomous data and both also provided continuous data to be combined in a meta-analysis.

The pooled analyses (i.e., for summary statistics) for continuous data were performed according to an inverse-variance random-effects model[6]. The latter was used due to a certain degree of heterogeneity between studies that was anticipated based on previous reviews (e.g., Leppin et al.,

[5] at least two studies for a specific outcome and time point

[6] assumes that different studies are estimating different, yet associated, treatment effects, and that the true effects are normally distributed (Borenstein et al., 2009; Deeks et al., 2019)

2014) and given the nature of included studies. The 95% prediction intervals from random-effects meta-analyses were calculated (see 2.2.4.5). As sensitivity analysis, fixed-effect meta-analyses were also performed (see 2.2.4.9). Continuous data reported as means and *SD*s, and outcomes where SMDs and the respective standard error (*SE*) were received from different data (e.g., independent *t* test) were analyzed separately, before combining these values using the generic inverse variance method. Studies in mixed samples (i.e., HCP and non-HCP) were included in meta-analyses if subgroup data for healthcare workers were reported separately or could be obtained from the study authors. If subgroup data were not available, the findings of these studies were presented narratively and separately (see Appendix D13.5) for each outcome. The same applied to single studies in HCP that provided insufficient quantitative data to be pooled with other studies, and were reported using statistical information from study reports (e.g., results of *F* test; see Appendix D13.5). Forest plots were generated to provide a graphical view of the ES of individual studies and the pooled ES.

For the primary outcomes of resilience and well-being as well as all secondary outcomes, positive values of the SMD (or mean difference, MD; see 2.3.4) indicate a higher (i.e., better) level of the corresponding outcome in the IG compared to the CG (e.g., higher resilience), whereas negative values refer to lower levels of this outcome in the intervention arm. For the remaining primary outcomes of anxiety, depression, and (perceived) stress, negative values indicate a lower (i.e., better) degree of these outcomes in the IG (e.g., fewer depressive symptoms) compared to the control arm, while positive values refer to a higher level in the IG versus CG.

All eligible studies measuring resilience used only one resilience scale. If a study reported more than one instrument for mental health and well-being outcomes or a resilience factor, the measure most often used across the included studies was used for ES calculation. With respect to the outcome of depression, depression scales were preferred over burnout scales if both measures were reported. For studies reporting both general measures of well-being and work-related assessments (e.g., job satisfaction), general measures were preferred. Once a summary of the evidence to date had been produced, and only if a pairwise meta-analysis (any resilience training vs. control) was possible, it

was examined whether the data were also suitable for a network meta-analysis (NMA). There was not enough evidence to perform a NMA. Statistical analyses were performed either in Review Manager 5.3 (The Cochrane Collaboration, 2014) or R 3.6.1 (R Core Team, 2019)[7], when appropriate. *p* values were reported exactly and where provided by study authors, unless *p* values were < .001, in which case they were expressed as $p < .001$. *t* and *p* values of Egger's tests were rounded.

2.2.4.3 Unit of Analysis Issues

Cluster-randomized trials. As allocation of individuals to conditions in resilience intervention studies partly occurs by groups (e.g., work sites, hospitals), it was intended to include cluster-RCTs along with individually-randomized trials. However, since no cluster-RCT was identified, only individually-randomized studies were considered in meta-analyses.

Repeated observations on participants. In case of longitudinal designs with repeated observations of participants, several outcomes were defined based on different periods of FU and separate analyses were conducted (Higgins, Li, et al., 2019). One analysis included all studies with measurements at the end of training (posttest), other analyses were based on the period of FU (short-term: ≤ 3 months; medium-term: > 3 to ≤ 6 months; long-term: > 6 months). Assessments were rated as postintervention if performed within one week after the training. Assessments at more than one week after the training were counted as short-term FU.

Studies with multiple treatment groups. If selected studies contained two or more IGs, two reviewers (AK, IH) determined which group was relevant to this review and the particular meta-analysis, based on the inclusion criteria for interventions (see 2.2.1.2). For all studies including several IGs, only one group was considered as relevant for the review (see 2.3.2.3).

2.2.4.4 Dealing With Missing Data

If there were any missing data in included studies (e.g., *SD*s) or where HCP had been combined

[7] libraries used: meta (Balduzzi, Rücker, & Schwarzer, 2019), metafor (Viechtbauer, 2010), and metasens (Schwarzer, 2019); R used for contour-enhanced funnel plots, Egger's test, and G^2

with other participants, the original authors were contacted to inquire if the missing data or subgroup (summary outcome) data were available. Following the recommendations in the Cochrane Handbook (Higgins, Li, et al., 2019), missing *SD*s of continuous outcomes were computed based on other statistical information (e.g., *SE*s, *t* values, *p* values). To obtain missing summary outcome data for studies solely conducted in HCP, the authors were also contacted twice to request the respective data (i.e., means, *SD*s, and sample sizes for the relevant study conditions or alternative information to calculate SMDs; see 2.2.4.2). In case of missing outcome data due to attrition, it was not asked for individual-level missing data and no reanalysis using imputation methods was performed. Studies with high levels of missing data (≥ 10%), that used no imputation methods, were judged at high risk of attrition bias (see 2.2.4.1). If the study authors had reported a complete-case analysis as well as imputed data, the summary outcome data based on the imputed data set (e.g., baseline observation carried forward or ideally expectation maximization, or multiple imputation) were used. Studies for which authors provided additional data not originally reported (e.g., number of participants analyzed) are described in detail in Appendix D6. Missing data and attrition levels for each included study were recorded in the RoB tables (Appendix D12). For primary outcomes, a sensitivity analysis was conducted to examine the consequences of excluding studies with high levels of missing data on the results and subsequent conclusions of the review (see 2.2.4.9).

2.2.4.5 Assessment of Heterogeneity

With respect to *clinical heterogeneity*, the study and study population characteristics across all eligible studies were compared (e.g., by generating descriptive statistics). In line with the Cochrane Handbook (Deeks et al., 2019), it was explored if studies were sufficiently homogeneous in terms of participant characteristics, interventions, and outcomes. *Methodological diversity* was assessed by inspecting included studies for variability in study design and RoB (e.g., method of randomization). Concerning *statistical heterogeneity* between included RCTs within each pairwise meta-analysis (i.e., heterogeneity in observed ES that exceeds sampling error alone), forest plots and statistical values were considered including Chi^2 test (χ^2), Tau^2 statistic (τ^2), and I^2 statistic (Deeks et al., 2019). To take

small-study effects into account, G^2 was considered (Rücker et al., 2011). It represents the proportion of unexplained variance, after having allowed for possible small-study effects (Rücker et al., 2011), with no statistical heterogeneity indicated by a G^2 value near zero. Regarding χ^2 test, significant statistical heterogeneity is indicated by a *p* value lower than .10. However, as eligible RCTs in the current and previous reviews often examined small sample sizes (e.g., Sood et al., 2014), it was acknowledged that the χ^2 test has only limited power. τ^2 also provides an estimate of the between-study variance in random-effects meta-analysis. I^2 reflects the percentage of total variation across studies that is due to heterogeneity rather than chance. In accordance with guidelines (Deeks et al., 2019), non-important heterogeneity was supposed for I^2 values of 0%–40%, moderate heterogeneity for values of 30%–60%, substantial heterogeneity for values of 50%–90%, and considerable heterogeneity if I^2 was between 75% and 100%. Finally, the 95% prediction intervals from random-effects meta-analyses (see 2.2.4.2; pooled analyses with more than two studies) were calculated to present the extent of between-study variation (Deeks et al., 2019). Where heterogeneity was observed (e.g., I^2 > 50%, with consideration of direction of effects and strength of evidence for heterogeneity [*p* value]), subgroup analyses were conducted to investigate potential explanations.

2.2.4.6 Assessment of Reporting Bias

Analyses for reporting bias were only performed if there were at least 10 studies for an outcome. Potential publication bias was assessed by inspecting (contour-enhanced) funnel plots (plotting effect estimates of trials against their standard errors on reversed scales; Page et al., 2019; Peters et al., 2008). When interpreting funnel plots, it was considered that funnel plot asymmetry does not necessarily reflect publication bias, but can stem from several reasons (Page et al., 2019). To differ between real asymmetry and chance, the recommendations of Page et al. (2019) were followed, by also using Egger's test (regression test; Egger et al., 1997) to check for funnel plot asymmetry.

2.2.4.7 Certainty of Evidence and "Summary of Findings" Table

The certainty of evidence found was assessed using the GRADE approach (Grading of

Recommendations Assessment, Development, and Evaluation), proposed by the GRADE Working Group (Schünemann et al., 2013; Schünemann, Higgins, et al., 2019). This rating resulted in a "Summary of findings" (SoF) table for the comparison resilience training versus control in HCP, that was created using the software GRADEpro: Guideline Development Tool (GRADEpro GDT, 2015) and included all primary outcomes (see 2.2.1.4) at posttest (Appendix D17.1). Five GRADE considerations (Appendix B7) were considered: *1) limitation*s in the design and implementation of the studies (i.e., unclear or high RoB of studies contributing to respective outcome; Guyatt, Oxman, Vist, et al., 2011), *2) indirectness* of evidence (i.e., included studies limited to certain participants, intervention types, or comparators; Guyatt, Oxman, Kunz, Woodcock, Brozek, Helfand, Alonso-Coello, Falck-Ytter, et al., 2011), 3) unexplained heterogeneity or *inconsistency* of results (i.e., heterogeneity exists but subgroup analyses fail to identify a plausible explanation; Guyatt, Oxman, Kunz, Woodcock, Brozek, Helfand, Alonso-Coello, Glasziou, et al., 2011), 4) *imprecision* of results (i.e., small number of participants included and wide CIs; Guyatt, Oxman, Kunz, Brozek, et al., 2011), and 5) high probability of *publication bias* (i.e., high risk of selective reporting bias for contributing studies; Guyatt, Oxman, Montori, et al., 2011). The assessment of the certainty of evidence was performed by two reviewers (AK, IH), working independently. Any disagreements were resolved by discussion or by consulting a third reviewer (AC, KL). The magnitude of effects was interpreted according to the Cochrane Handbook (Schünemann, Vist, et al., 2019; i.e., Cohen's d = 0.2: small effect, d = 0.5: moderate effect, d = 0.8: large effect[8]). The certainty of evidence was rated as high, moderate, low, or very low (Schünemann et al., 2013). High-certainty evidence indicates high confidence that the true effect lies close to that of the estimate of effect. Very-low certainty evidence indicates that there is very little confidence in the effect estimate and that the true effect is likely to be substantially different.

2.2.4.8 Subgroup Analyses and Investigation of Heterogeneity

To analyze the RQs 2 to 5 concerning potential effect modifiers and to examine the heterogeneity found for several outcomes (see 2.3.7), several subgroup analyses were performed

[8] These conventions can also be applied to Hedges g.

(Deeks et al., 2019). As outlined under 2.1.4, the potential moderators were selected based on previous reviews (e.g., Joyce et al., 2018). Where the necessary data could be extracted, the following subgroup analyses[9] were performed, classifying the RCTs as follows: 1) *setting* (RQ2; group vs. individual vs. combined vs. unspecified setting), 2) *delivery format* (RQ3; face-to-face vs. online or mobile-based vs. telephone vs. bibliotherapy vs. laboratory vs. multimodal delivery vs. unspecified delivery), 3) *intensity* (RQ4; low: ≤ 5 hours or ≤ 3 sessions vs. moderate: > 5 to ≤ 12 hours or > 3 to ≤ 12 sessions vs. high: > 12 hours or 12 sessions vs. unspecified intensity), 4) *theoretical foundation* (RQ5; CBT vs. SIT vs. PST vs. ACT vs. mindfulness vs. AIT vs. coaching vs. PP vs. combination[10] vs. unspecified training[11]). Pooled ES were calculated for each subgroup. Subgroup analyses[12] were restricted to primary outcomes with at least 10 studies included in a meta-analysis (Deeks et al., 2019).

2.2.4.9 Sensitivity Analyses

Sensitivity analyses were also restricted to primary outcomes with at least 10 studies in the meta-analysis. Sensitivity analyses were performed: 1) based on the underlying concept of resilience, by limiting pooled analyses to *scales assessing resilience as state-like outcome*, 2) by excluding studies at *high risk of attrition and reporting bias* (see 2.2.4.1), respectively, that were chosen as key bias domains (subgroup analyses conducted to test if studies judged at low and unclear risk of bias could be pooled in analysis), 3) by limiting the analyses to *registered studies*, with registration identified depending on whether a trial registration was found or whether the authors claimed to have registered a study (Appendix D6), 4) by limiting the analyses to those studies with *low levels of missing data* (less than 10% in the relevant primary outcome), 5) by restricting the analyses to studies with less than 10% missing primary outcome data and those where missing data had been imputed or accounted for by fitting a model for longitudinal data (*coping with missing data)*, and 6) using *fixed-effect* pairwise meta-analyses, to test the robustness of the findings.

[9] Except for training intensity (posthoc addition), all subgroup analyses were prespecified (Helmreich, Kunzler et al., 2017).

[10] that is, interventions based on two or more explicit approaches (e.g., CBT and mindfulness)

[11] fostering at least one resilience factor but without specifying any explicit theoretical foundation or where the underlying framework could not be assigned to a specific theoretical approach

[12] test for subgroup differences also based on random-effects models

2.3 Results

2.3.1 Results of the Search

The first searches for this review were run in October 2016 according to the protocol (Helmreich, Kunzler et al., 2017). The strategies in Appendix D3.1 were used to find studies in which participants included any adults aged 18 years and older. Due to the large number of potentially eligible studies, it was decided to split the review and the inclusion criteria were changed to focus on healthcare sector workers and students (see Appendix D17.1). Therefore, before running the top-up searches in June 2019, the original search strategy was revised by limiting the population to healthcare sector workers and students (Appendix D3.2). Following these searches, the inclusion criteria were further revised to HCP only, which is the focus of this review.

In total, the database searches retrieved 37,737 records. An additional 663 records were found by searching other resources. Following de-duplication, the remaining 24,703 records were screened by title and abstract. Of these, 21,629 records were deemed to be irrelevant and the full texts of the remaining 3,074 records were sought for further assessment. At the level of title/abstract screening, a good agreement (κ = .70) between reviewers was achieved for the original search, and an excellent agreement for the top-up searches (κ = .99). The full text screening resulted in excellent inter-rater reliability for both the original search (κ = .95) and the top-up searches (κ = 1). After revising the eligibility criteria to focus broadly on the healthcare sector (including HCP; see Appendix D17.1), 80 studies were identified that were performed in any of these groups. Nine ongoing studies and 29 studies awaiting classification were also found. Six additional reports of studies identified by the original search were found during the top-up searches. Finally, after revising the eligibility criteria to focus on *HCP*, these 118 studies (from 144 reports) were reassessed. In total, for HCP, 44 studies (from 59 reports) were included. A total of 3,000 full text reports was excluded[13]. Eight studies awaiting classification (see Appendix D7) and five ongoing studies (see Appendix D8). were identified. Appendix C1 includes the PRISMA flow diagram and results of both searches are presented in detail in

[13] This total includes the 16 reports (13 excluded studies), which needed to be examined in detail to determine eligibility, and which are described in the Appendix D9.

Appendix D5. From an updated (prepublication) search of four key databases in June 2020, 12 studies (from 12 reports) have been added to studies awaiting classification (Appendix D7). The results of these studies are not yet included and will be incorporated at the next update.

2.3.2. Included Studies

The characteristics of included studies are presented in Table 1 and in Appendix D6 and D10.

2.3.2.1 Study Design, Location, and Setting

Study design. All but one (NCT02603133) of the 44 included studies were parallel-group designs, published between 1997 and 2019; except for three completed, but unpublished trials (ISRCTN69644721; NCT02603133; NCT03645798). One study was termed a cluster-RCT in the report (Mistretta et al., 2018). However, according to additional information received from the authors, there were no multiple groups or clusters for each of the conditions. After discussion with a statistician (JK), the study was judged to be individually randomized with stratification for schedule availability.

Location. Most of the studies were conducted in North America (e.g., USA; 20 studies), followed by 12 European (e.g., Germany), and nine Asian studies (e.g., China; see Table 1).

Setting. Concerning the implementation sites of interventions (see Table 1), most programs (24/44) were performed in clinics or specific hospital departments (e.g., Department of Radiology). As four studies included online/mobile interventions, there was no concrete venue and subjects could participate regardless of location. One intervention took place in the laboratory. Two resilience trainings were conducted in mixed settings (e.g., online plus face-to-face sessions with unspecified venue).

2.3.2.2 Participants

Concerning *healthcare sectors*, most studies solely included nurses or physicians (see Table 1). Among the four studies with mixed samples (i.e., HCP combined with non-healthcare participants), relevant subgroups included: health service professionals (Cieslak et al., 2016), mental health workers (Gelkopf et al., 2008), and ambulance service personnel (ISRCTN69644721; Wild, 2016). Varker and

Table 1

Characteristics of Included Studies (Short Overview)

Study	Location	Venue of training	Sample	N_{random}	Gender	Age *M(SD)* in years	Mental health	Setting	Delivery	Intensity	Theoretical foundation	CG
Alexander 2015	USA	hospital	nurses	40	F>M	46.4 (10.2)	✓	G	FF	n.a.	unspecific	TAU
Berger 2011	Israel	hospital	nurses	80	F only	48.5 (7.3)	✓	G	FF	***	combi	WL
Bernburg 2016	Germany	hospital	physicians	54	F>M	27 (2.1)	n.a.	G	FF	***	combi	NI
Bernburg 2019	Germany	n.a.	nurses	92	F>M	32.0 (2.4)	n.a.	G	FF	***	combi	WL
Calder Calisi 2017	USA	hospital	nurses	53	F only	range: 27–60	✓	combi	FF	***	combi	WL
Chesak 2015	USA	hospital	nurses	55	F>M	28.2 (8.3)	✓	G	multi	*	AIT	AC
Cheung 2014	China	Medical Service	GMP	918	F>M	37.4 (11.8)	✓	G	FF	**	unspecific	WL
Cieslak 2016	Poland	no venue	mixed	253	F>M	37.5 (10.4)	✓	I	multi	**	CBT	ATC
Clemow 2018	USA	hospital	HP	92	F>M	48.5 (8.7)	✓	G	FF	**	CBT	TAU
Duchemin 2015	USA	hospital	HP	32	F>M	44.2	✓	G	multi	**	mindfulness	WL
Fei 2019	China	hospital	nurses	122	n.a.	32.2 (6.5)	n.a.	G	FF	**	combi	NI
Gelkopf 2008	Sri Lanka	NGO	mixed	62	F>M	48.7 (12.8)	n.a.	G	FF	***	combi	AC
Hosseinnejad 2018	Iran	n.a.	nurses	80	F>M	range: 24–45	n.a.	G	FF	**	unspecific	TAU
Ireland 2017	Australia	n.a.	physicians	44	F>M	26.9 (4.8)	✓	G	FF	**	combi	AC
ISRCTN69644721	UK	mixed	mixed	255	n.a.	n.a.	✓	combi	multi	**	unspecific	WL
Khoshnazary 2016	Iran	mixed	nurses	76	F>M	range: 24–55	n.a.	combi	multi	n.a.	unspecific	n.a.
Klatt 2015	USA	hospital	HP	34	n.a.	n.a.	n.a.	G	FF	**	mindfulness	WL
Lebares 2019	USA	n.a.	physicians	21	M>F	28.3 (2.4)	✓	G	FF	***	mindfulness	ATC
Lin 2019	China	n.a.	nurses	110	F>M	31.5 (6.9)	n.a.	G	multi	***	combi	WL
Loiselle 2018	USA	hospital	physicians	40	F>M	45.1 (10.5)	✓	combi	FF	**	mindfulness	WL
Luthar 2017	USA	hospital	physicians	40	F only	39.1 (5.5)	✓	G	FF	**	unspecific	NI
Mache, Danzer 2015	Germany	hospital	physicians	69	F>M	27 (2.5)	n.a.	G	FF	***	combi	NI
Mache, Vitzthum 2015	Germany	hospital	physicians	90	F>M	28	n.a.	G	FF	***	combi	NI
Mache 2016	Germany	hospital	physicians	76	F>M	33 (2.3)	n.a.	G	FF	***	combi	NI
Mache 2017	Germany	n.a.	physicians	80	F>M	27.5 (2.2)	✓	G	FF	***	combi	NI
Mealer 2014	USA	hospital	nurses	29	F>M	n.a.[a]	✓	combi	multi	***	combi	NI
Medisauskaite 2019	UK	n.a.	physicians	381	M>F	47.9 (11.2)	✓	n.a.	n.a.	n.a.	unspecific	NI

Study	Location	Venue of training	Sample	N_{random}	Gender	Age *M(SD)* in years	Mental health	Setting	Delivery	Intensity	Theoretical foundation	CG
Mirzaeirad 2019	Iran	hospital	nurses	80	F>M	52.5% < 31	n.a.	G	FF	n.a.	unspecific	n.a.
Mistretta 2018	USA	hospital	HP	60	F>M	46.0 (12.6)	✓	G	multi	**	combi	NI
NCT02603133	USA	no venue	HP	2650	n.a.	n.a.	✓	n.a.	online	*	unspecific	unclear
NCT03645798	China	no venue	Nurses	102	n.a.	n.a.	✓	combi	online	***	PP	TAU
Poulsen 2015	Australia	hospital	HP	80	F>M	range: 25–>45	n.a.	G	FF	**	unspecific	AC
Schroeder 2016	USA	hospital	physicians	33	F>M	42.8 (8.4)	✓	G	FF	***	mindfulness	WL
Smith 2019	Canada	n.a.	nurses	29	F>M	33	✓	G	multi	***	combi	NI
Sood 2011	USA	hospital	physicians	40	F=M	IG: 46.8 (8.3); CG: 50.2 (5.7)	✓	I	FF	*	AIT	WL
Sood 2014	USA	hospital	HP	26	M>F	47.8 (7.1)	✓	combi	multi	*	AIT	WL
Stetz 2007	USA	laboratory	GMP	63	M>F	60% < 30	✓	I	laboratory	*	stress inoc.	NI
Strijk 2011	Netherlands	hospital	HP	730	F>M	52.4 (4.9)	n.a.	combi	FF	***	coaching	AC
Tierney 1997	USA	hospital	nurses	62	n.a.	n.a.[b]	n.a.	G	FF	**	combi	NI
Varker 2012	Australia	n.a.	volunteers	82	F>M	28.4 (10.4)	✓	G	FF	*	combi	ATC
Villani 2013	Italy	no venue	nurses	30	F only	43 (8.8)	✓	I	mobile	*	stress inoc.	ATC
West 2014	USA	hospital	physicians	74	M>F	n.a.	✓	G	FF	***	combi	NI
West 2015	USA	n.a.	physicians	125	n.a.	n.a.	✓	G	FF	**	unspecific	WL
Wild 2016	UK	n.a.	mixed	430	F>M	41.4 (9.8)	✓	G	FF	***	combi	AC

Note. n.a. = not available; VENUE: mixed = mixed venue (e.g., online plus face-to-face sessions with unspecified venue); NGO = non-governmental organization; SAMPLE: HP = hospital personnel (e.g., physicians and other hospital personnel); GMP = general medical personnel; mixed = participants from healthcare and non-healthcare sector; N_{random} = sample size randomized (all study conditions); GENDER: F = female; M = male; F>M = women outnumbered men; M>F = men outnumbered women; F=M = comparable gender distribution; MENTAL HEALTH: ✓= mental health assessed at baseline; SETTING: combi = combined setting; G = group setting; I = individual setting; DELIVERY: FF = face-to-face; multi = multimodal (e.g., web-based intervention and daily diary); INTENSITY: * = low; ** = moderate; *** = high; THEORETICAL FOUNDATION: AIT = Attention and Interpretation Therapy; CBT = Cognitive-behavioral therapy; combi = combined theoretical foundation (e.g., CBT and mindfulness); PP = Positive Psychology; stress inoc. = stress inoculation; CONTROL GROUP (CG): AC = active control; ATC = attention control; NI = no intervention; TAU = treatment as usual; WL = wait-list; [a] Mealer et al. (2014): mean duration of practicing in intensive care unit of 5.35 years (*SD* 5.94); [b] Tierney and Lavelle (1997): staff nurses employed between 6 months and 2.5 years.

Devilly (2012; proof-of-concept study) evaluated a resilience intervention for emergency services personnel in the general population.

The total number of adults working as HCP randomized across 39 of the 44 included studies was 6,892. For the four mixed studies, the total number randomized or targeted was 1,000 participants (original number of HCP randomized not obtainable from study authors). Varker and Devilly (2012) randomized 82 volunteers from the general population. Overall, 11 studies randomized ≥ 100 participants (see Table 1) and five studies randomized ≤ 30 participants.

Participants were mainly of young to middle *age* (see Table 1), with (mean) age ranging from *M* = 26.9 to *M* = 52.4 years (*SD* range: 2.1 to 12.6 years) across 25 studies solely in HCP (average: *M* = 37.7 years, *SD* = 6.7), and from *M* = 37.5 to *M* = 48.7 years (*SD* range: 9.8 to 12.8 years) in three of the four mixed studies (average: *M* = 42.5 years, *SD* = 11). Study participants were predominantly *female* (see Table 1), with a proportion of 68.6% female participants in 32 studies with available data in HCP. Three mixed studies with available data included 64.9% female participants. The gender distribution varied between the studies.

From the 29 studies assessing *mental health at baseline* (see Table 1), all of them measured mental health using self-report (screening) measures that covered one or a small number of mental dysfunctions (e.g., DASS-21). None of the studies used a structured clinical interview. Fifteen studies provided no data about the sample's mental health. For four studies (ISRCTN69644721; NCT02603133; NCT03645798; Smith et al., 2019), the mental health status was unclear despite the pretest assessment. Five studies included only mentally healthy subjects (Chesak et al., 2015; Cheung, 2014; Sood et al., 2011; Sood et al., 2014) or participants with symptoms below the cut-off of a screening scale (Stetz et al., 2007). Lin et al. (2019) did not consider participants taking mood-modulating drugs, for Mirzaeirad et al. (2019), the lack of mental stress was an inclusion criterion.

2.3.2.3 Interventions

Six studies had multiple intervention arms (Cieslak et al., 2016; ISRCTN69644721; Medisauskaite & Kamau, 2019; Mistretta et al., 2018; Stetz et al., 2007; Tierney & Lavelle, 1997).

However, for each study, only one IG was considered to be relevant for this review and the meta-analyses (see Appendix D6 and Appendix D13.1). As presented in Table 1, most studies investigated *group* interventions (30/44) of *high training intensity* (18/44; > 12 hours/sessions), that were delivered *face-to-face* (29/44). Only three studies examined online- or mobile-based resilience trainings. Treatment duration varied between a single 40-minute intervention session (Varker & Devilly, 2012) and a total of 87 hours (Berger & Gelkopf, 2011) or 77 sessions (Strijk et al., 2011). Three interventions were provided over a 6-month period or even longer (NCT03645798; Strijk et al., 2011; West et al., 2015). With respect to the *theoretical foundations* (Table 1), most studies (19/44) included interventions based on a combination of two or more approaches. Unspecific training programs (11/44) fostered at least one prespecified resilience factor (see Appendix D2.1; level 1), but without indicating a concrete theoretical approach. Five studies evaluated mindfulness-based interventions using (modified) MBSR or meditation practices. Three trainings used AIT (e.g., attentional focus on novel aspects); stress inoculation was represented among two studies (e.g., acquisition of coping strategies before exposure to video clips of oncology patients). Two trainings solely included CBT elements (e.g., identification of thoughts, feelings, and behaviors in stressful situations). One study apiece was based on positive psychology (e.g., "three good things" exercise) and coaching (e.g., goal setting). Details are presented in Appendix D6 and Appendix D11.

2.3.2.4 Comparators

Forty-three studies involved only one comparator (see Table 1; control group [CG]), with a majority of no intervention (14/44) and wait-list controls (13/44). The uncertainty concerning the number and form of CGs for NCT02603133 could not be resolved by contacting the authors. Two studies used a design where a CG plus resilience training was compared to the comparator alone (Poulsen et al., 2015; Strijk et al., 2011). In six studies, *active CGs* included a lecture covering topics related to stress (Chesak et al., 2015), a befriending seminar with tools for emotional support (Gelkopf et al., 2008), an extra hour break time (Ireland et al., 2017), written educational material about recovery and self-care (Poulsen et al., 2015), written information about a healthy lifestyle (e.g., diet;

Strijk et al., 2011), and an online intervention with information on mental health (Wild, 2016). Of the four studies contrasting resilience training with *attention control*, two used a web-based educational intervention on coping with stress at work and indirect exposure to trauma (Cieslak et al., 2016), and a face-to-face group intervention on topics such as self-care, along with daily practice and a retreat hike (Lebares et al., 2019). Further attention controls included an accident management training (Varker & Devilly, 2012) and video clips representing natural environments by mobile phones (Villani et al., 2013). In two studies, *TAU* referred to minimally enhanced usual care by self-help materials (Clemow et al., 2018) or usual psychological instruction from the hospital (NCT03645798). Two studies did not further specify the content of TAU (Alexander et al., 2015; Hosseinnejad et al., 2018).

2.3.2.5 Outcome Measures

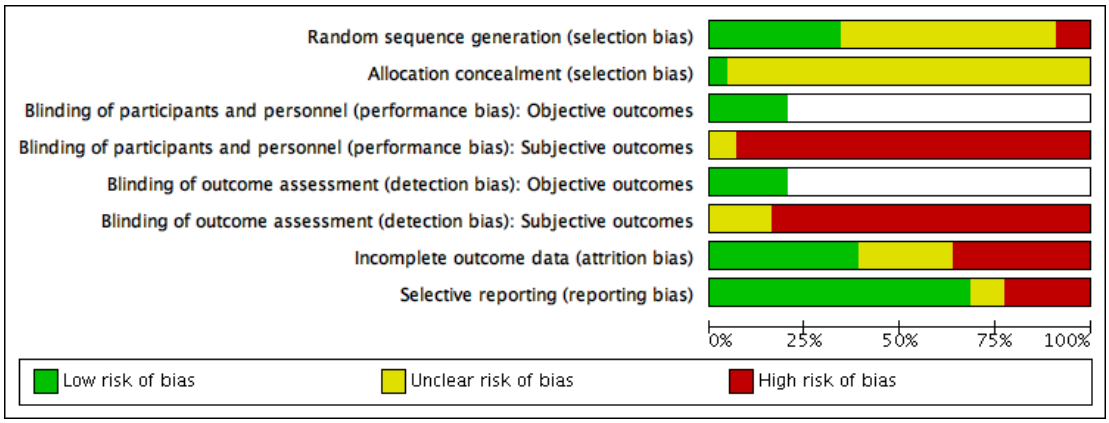
All outcomes were based on self-report and most studies used validated scales. For each of the *primary outcomes*, the studies used heterogeneous scales (see Appendix D13.3), although some of them measuring the same outcome (e.g., perceived stress) also used the same instrument (e.g., Perceived Stress Scale). Most of the 44 included studies (24 studies) assessed depressive symptoms, followed by (perceived) stress (22 studies), resilience using a resilience scale (21 studies), well-being (20 studies), and anxiety (12 studies). Heterogeneous measures were also used to assess the *secondary outcomes* (see Appendix D13.4). Most studies (11/44) assessed self-efficacy, followed by active coping (5/44). Social support, optimism, and positive emotions were assessed by three studies apiece, while both self-esteem and hardiness were outcomes in one study.

2.3.3 Risk of Bias in Included Studies

The overall RoB across the 44 studies was high, with main flaws (≥ 20% high risk) found in the domains of performance, detection, attrition, and reporting bias (see Figure 4; Appendix C5, Appendix D12, Appendix D13). For selection bias, a large number of studies provided insufficient information to judge the RoB adequately. Most variability across studies was identified for attrition and reporting bias. Most studies (41/44 studies) were rated at high risk of performance bias (see Appendix C5).

Figure 4

Risk of Bias Graph



Note. White domains: RoB only assessed for nine studies (about 20%) measuring objective outcomes (see Appendix C5).

2.3.4 Effects of Interventions

2.3.4.1 Effects of Resilience-Training Programs on Psychological Outcomes in HCP Over Time

Overall, 22 pooled (main) analyses combining at least two studies could be performed that served to investigate RQ1 regarding positive posttest and short-term effects (≤ 3 months) of resilience interventions in HCP (*hypothesis 1.1*) and the expected decrease of ES at medium- (> 3 to ≤ 6 months) and long-term FU (> 6 months; *hypothesis 1.2*). Except for resilience at posttest, the corresponding forest plots are presented in Appendix C6; Appendix D13.5 includes additional findings apart from the pooled analyses.

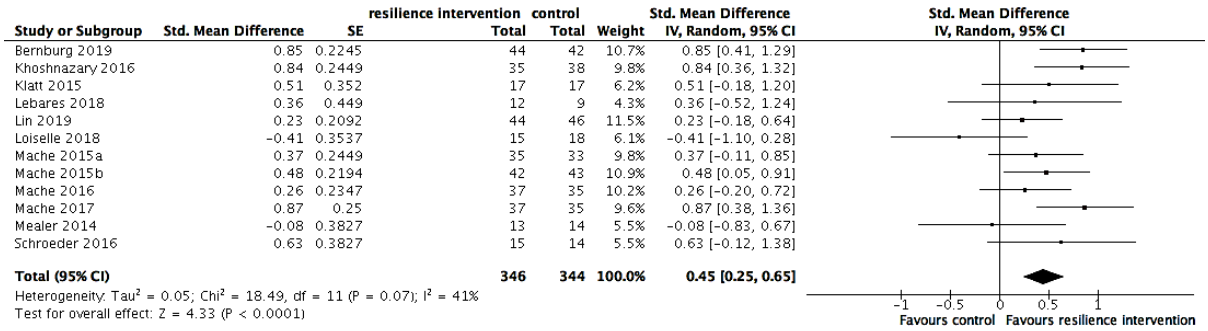
Primary outcomes

Resilience. Immediately postintervention, the pooled effect estimate for 12 studies suggested evidence for a moderate effect of resilience interventions on resilience, SMD 0.45, 95% CI [0.25, 0.65], $p < .001$, $I^2 = 41\%$, $\tau^2 = 0.05$, $p_{heterogeneity} = .07$ (690 participants; see Figure 5). At short-term FU, the pooled SMD on resilience was 0.42, 95% CI [0.17, 0.67], $p = .001$, $I^2 = 71\%$, $\tau^2 = 0.11$, $p_{heterogeneity} < .001$ (11 studies; 1325 participants), providing evidence for a moderate ES. At medium-term FU, the quantitative analysis of two studies (684 participants) showed little or no evidence for a difference between resilience intervention and control in resilience, SMD 0.35, 95% CI [−0.41, 1.11], $p = .37$, $I^2 = 87\%$, $\tau^2 = 0.27$, $p_{heterogeneity} = .005$. At long-term FU, only two studies (107 participants) assessed the

effects of resilience training on self-reported resilience and could be combined. The pooled SMD was 0.30, 95% CI [−0.08, 0.68], p = .12, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .97, indicating little or no evidence for an effect of training.

Figure 5

Forest Plot of Pairwise Meta-Analysis for Resilience at Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Anxiety. Immediately postintervention, the pooled effect estimate for five studies (231 participants) suggested little or no evidence for an effect of resilience training compared to control on self-reported anxiety, SMD −0.06, 95% CI [−0.35, 0.23], p = .67, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .99. At short-term FU, the pooled SMD for four studies (133 participants) on anxiety was −0.63, 95% CI [−0.98, −0.27], p < .001, I^2 = 0%, τ^2 = 0; $p_{heterogeneity}$ = .95, and revealed evidence for a moderate difference between groups favoring resilience training for this outcome (moderate ES). No study reported data on anxiety at medium-term and long-term FU.

Depression. Immediately postintervention, the pooled effect estimate for 14 studies (788 participants) suggested evidence for a small effect of resilience training on depression, SMD −0.29, 95% CI [−0.50, −0.09], p = .005, I^2 = 42%, τ^2 = 0.06, $p_{heterogeneity}$ = .05. At short-term FU, the pooled SMD on depression was −0.52, 95% CI [−0.81,−0.23], p < .001, I^2 = 50%, τ^2 = 0.08, $p_{heterogeneity}$ = .05 (8 studies; 545 participants), revealing evidence for a moderate difference between groups favoring resilience training. At medium-term FU, one study (Mache et al., 2017) assessed the impact of training compared to control on self-reported burnout using the emotional exhaustion subscale of the Maslach

Burnout Inventory (range 1 [best] to 6 [worst]). Based on data from 60 participants, Mache et al. (2017) found a mean difference (MD) of −0.40, 95% CI [−0.75, −0.05], p = .03, indicating evidence for a difference between resilience training and control groups. At long-term FU, two studies (87 participants) assessed the effects of resilience training on self-reported depression or burnout and could be combined. The pooled SMD was 0.09, 95% CI [−0.33, 0.51], p = .68, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .56, indicating little or no evidence for an effect of training.

Stress. At posttest, the pooled effect estimate for 17 studies (997 participants) suggested evidence for a moderate effect of resilience training on (perceived) stress, SMD −0.61, 95% CI [−1.07, −0.15], p = .01, I^2 = 90%, τ^2 = 0.79, $p_{heterogeneity}$ < .001. At short-term FU, the pooled SMD for self-reported (perceived) stress was −0.46, 95% CI [−0.67, −0.25], p < .001, I^2 = 53%, τ^2 = 0.08, $p_{heterogeneity}$ = .01 (14 studies; 788 participants), providing evidence for a moderate difference in favor of resilience training. At medium-term FU, perceived stress was only measured in one study (Mache et al., 2017). Using the Perceived Stress Questionnaire (range: 1 [best] to 4 [worst]) in 60 participants, the authors reported a significant difference (p < .01) in favor of the IG at 6-month FU (intervention arm: M = 2.8, SD = 0.7; control arm: M = 3.2, SD = 0.6). The MD also indicated evidence for an effect of resilience intervention on perceived stress, MD −0.40, 95% CI [−0.73, −0.07], p = .02. At long-term FU, three studies assessed the effects of resilience training on self-reported (perceived) stress and could be combined in analysis (173 participants). The pooled SMD was −0.39, 95% CI [−0.84, 0.05], p = .09, I^2 = 47%, τ^2 = 0.07, $p_{heterogeneity}$ = .15, indicating little or no evidence for an effect.

Well-being. At posttest, the pooled effect estimate for 13 studies (1494 participants) revealed little or no evidence for an effect of resilience training compared to control on self-reported well-being, SMD 0.14, 95% CI [−0.01, 0.30], p = .07, I^2 = 31%, τ^2 = 0.02, $p_{heterogeneity}$ = .13. At short-term FU, the pooled SMD for self-reported well-being was 0.07, 95% CI [−0.04, 0.18], p = .22, I^2 = 1%, τ^2 = 0, $p_{heterogeneity}$ = .43 (12 studies; 1413 participants), showing little or no evidence for an effect of training. At medium-term FU, the pooled SMD of three studies (1414 participants) comparing a resilience intervention with control suggested little or no evidence for an effect of training, SMD −0.08, 95% CI

[−0.31, 0.16], p = .52, I^2 = 73%, τ^2 = 0.03, $p_{heterogeneity}$ = .02. At long-term FU, only one study assessed quality of life (West et al., 2014). Using a single-item linear analogue question (range 0 [worst] to 10 [best]) in 66 participants, a non-significant increase in quality of life of 1.5% in the IG compared to 1.8% in the CG (p = .63) was found. The MD also indicated little or no evidence for an effect of training at 12 months postintervention, MD −0.20, 95% CI [−0.94, 0.54], p = .59.

Secondary outcomes

Social support. At short-term FU, the pooled SMD for two studies (825 participants) for self-reported social support was −0.07, 95% CI [−0.22, 0.08], p = .36, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .96, suggesting little or no evidence for an effect of resilience training. At medium-term FU, Cheung (2014; 624 participants) reported lower values of social support (Multidimensional Scale of Perceived Social Support; range 1 [worst] to 7 [best]) in the IG (M = 4.9, SD = 1.1) compared to wait-list control (M = 5.0, SD = 1.1) despite no significant Time × Treatment interaction (F = 0.85, p > .05). The MD for this outcome also indicated little or no evidence for a difference, MD −0.10, 95% CI [−0.27, 0.07], p = .25. No study assessed social support at long-term FU, at postintervention only one study in a mixed sample reported on perceived social support (see Appendix D13.5).

Optimism. At posttest, the pooled ES for three studies (169 participants) revealed evidence for a moderate effect on optimism favoring resilience training, SMD 0.41, 95% CI [0.10, 0.72], p = .009, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .93. At short-term FU, the pooled SMD for two studies (153 participants) was 0.44, 95% CI [0.12, 0.76], p = .008, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .72, suggesting evidence for a moderate effect of training. Optimism was not measured in any study at medium- and long-term FU.

Self-efficacy. Immediately at posttest, the pooled effect estimate for six studies (461 participants) suggested evidence for a moderate difference in favor of resilience training, SMD 0.43, 95% CI [0.25, 0.62], p < .001, I^2 = 0%, τ^2 = 0, $p_{heterogeneity}$ = .52. At short-term FU, the pooled SMD (seven studies; 1258 participants) for self-efficacy was 0.32, 95% CI [0.13, 0.51], p = .001, I^2 = 51%, τ^2 = 0.03, $p_{heterogeneity}$ = .06, indicating a small effect of resilience training. At medium-term FU, self-efficacy was only measured in one study (Cheung, 2014). Using a 13-item self-efficacy scale (range 1 [worst] to 4

[best]) in 624 participants, the authors reported a Time × Treatment interaction (F = 30.28, p < .001), with a sustained increase of self-efficacy in the treatment arm at 6-months FU compared to no change in the CG. The MD also suggested evidence for an effect of resilience training, MD 0.17, 95% CI [0.10, 0.24], p < .001. Similarly, at long-term FU, only Bernburg et al. (2019) compared the effects of resilience intervention with control on self-reported self-efficacy. Based on the Self-Efficacy, Optimism, and Pessimism scale (range 0 [worst] to 4 [best]) in 86 participants, the investigators reported a significant between-group difference at 9-month FU (p = .01), while the calculated MD indicated little or no evidence for an effect of training, MD 0.19, 95% CI [−0.02, 0.40], p = .08.

Active coping. At posttest, the pooled effect estimate of three studies (137 participants) suggested little or no evidence for an effect of resilience training for active coping, SMD 0.28, 95% CI [−0.31, 0.87], p = .35, I^2 = 52%, τ^2 = 0.14, $p_{heterogeneity}$ = .12. At short-term FU, Cheung (2014; 733 participants) reported similar scores of active coping (adaptive coping) assessed by the Brief Coping Orientations to Problems Experience (Brief COPE) scale (range for adaptive coping: 1 [worst] to 4 [best]) in the IG (M = 2.5, SD = 0.7) versus CG (M = 2.5, SD = 0.6). The MD indicated little or no evidence for an effect, MD −0.02, 95% CI [−0.11, 0.07], p = .67. Cheung (2014) was also the only study measuring active coping at medium-term FU. Again, using the Brief COPE adaptive coping subscale in 624 participants, similar scores of active coping (adaptive coping) were found in the IG (M = 2.6, SD = 0.6) than in the CG (M= 2.6, SD = 0.57). The authors reported a Time × Treatment interaction (F = 4.09, p < .05), with an increase of active coping in the CG compared to no change in the IG. The MD indicated little or no evidence for an effect of resilience intervention compared to wait-list control, MD −0.03, 95% CI [−0.12, 0.06], p = .53. No study assessed active coping at long-term FU.

Self-esteem. Self-reported self-esteem was only measured at short-term FU. Berger and Gelkopf (2011) compared the effects of resilience training to control using the Rosenberg Self-Esteem Scale (range 10 [worst] to 40 [best]) in 80 participants. The authors identified an increase of self-esteem in the IG after training (M = 37.4, SD = 3.5) compared to the control arm (M = 32.1, SD = 3.9), but no significant Time x Group interaction (F = 2.8, p > .05). The MD indicated evidence for an effect

of resilience intervention on self-esteem, MD 5.30, 95% CI [3.67, 6.93], $p < .001$.

Hardiness. Immediately postintervention, only one study assessed the effects of hardiness training compared to control on hardiness (Tierney & Lavelle, 1997). Using the Personal Views Survey in 43 participants, the study found higher values of hardiness in the IG (M = 78.2, SD = 7.0) compared to the CG (M = 74.6, SD = 8.7), with no significant difference in change scores between the conditions. The MD also showed little or no evidence for a difference in favor of the resilience intervention, MD 3.52, 95% CI [−1.19, 8.23], $p = .14$. Tierney and Lavelle (1997) was also the only study measuring self-reported hardiness at medium-term FU and showed similar scores in the two groups (IG: M = 75.7, SD = 5.9; CG: M = 75.5, SD = 7.4). The calculated MD indicated little or no evidence for an effect of training, MD 0.24, 95% CI [−3.74, 4.22], $p = .91$ (43 participants). None of the studies considered the participants' hardiness at short-term or long-term FU.

Positive emotions. At posttest, the pooled effect estimate of two studies (212 participants) suggested evidence for a large effect of resilience training compared to control on positive emotions, SMD 0.85, 95% CI [0.17, 1.53], $p = .01$, $I^2 = 82\%$, $\tau^2 = 0.20$, $p_{heterogeneity} = .02$. At short-term FU, only one study (Lin et al., 2019) assessed the effects of resilience intervention on self-reported positive emotions. Using the positive affect subscale of the Positive and Negative Affect Schedule (range: 10 [worst] to 50 [best]) in 90 participants, the investigators identified a significant Time x Group interaction on positive affect at 3-month FU ($F = 6.62$; $p < .01$) in favor of resilience training (IG: M = 33.2, SD = 7.4; CG: M = 29.0, SD = 5.6). The MD also indicated evidence for a positive effect of resilience training, MD 4.21 95% CI [1.49, 6.93], $p = .002$. No study measured positive emotions at medium- and long-term FU.

RQ1: Which effects do resilience interventions have on psychological outcomes in HCP?

Hypothesis 1.1: Compared with a CG, there is evidence for at least small short-term (i.e., posttest, ≤ 3-month FU) ES of resilience interventions in HCP on (1.1.1) resilience, (1.1.2) anxiety symptoms, (1.1.3) depressive symptoms, (1.1.4) (perceived) stress, (1.1.5) well-being, (1.1.6) social support, (1.1.7) optimism, (1.1.8) self-efficacy, (1.1.9) active coping, (1.1.10) self-esteem, (1.1.11) hardiness, and

(1.1.12) positive emotions. Hypothesis 1.1 is supported for (1.1.1) resilience, (1.1.3) depressive symptoms, (1.1.4) (perceived) stress, (1.1.7) optimism, (1.1.8) self-efficacy, and (1.1.12) positive emotions (only single study at short-term FU). Hypothesis 1.1. is partly supported for (1.1.2) anxiety symptoms (short-term FU) and (1.1.10) self-esteem (only testable for short-term FU)[14]. Hypothesis 1.1 is not supported for (1.1.5) well-being, (1.1.6) social support (only testable at short-term FU), (1.1.9) active coping, and (1.1.11) hardiness (only testable at posttest).

Hypothesis 1.2: At medium- (> 3 ≤ 6-months) and long-term FU (> 6 months), the ES of resilience interventions in HCP on the above outcomes decrease or there is no longer evidence for an effect. Hypothesis 1.2 is supported for (1.1.1) resilience, (1.1.3) depressive symptoms[15], and (1.1.4) (perceived) stress[16]. Hypothesis 1.2 is partly supported for (1.1.5) well-being[17], (1.1.6) social support[18] (only testable for medium-term FU), (1.1.8) self-efficacy[18], (1.1.9) active coping[18], and (1.1.11) hardiness (only testable for medium-term FU)[18]. Hypothesis 1.2 is not testable for (1.1.2) anxiety symptoms, (1.1.7) optimism, (1.1.10) self-esteem, and (1.1.12) positive emotions due to the lack of studies measuring these outcomes at medium- and long-term FU.

2.3.4.2 Impact of Intervention Setting on the Efficacy of Resilience-Training Programs

With respect to the exploratory RQ2 regarding potential differences in the efficacy of resilience trainings in HCP depending on the intervention setting, there was no evidence for a difference between subgroups for any primary outcome at posttest or short-term FU (see tests for subgroup differences in Appendix C7; all *p*s > .07 and Appendix D14 for corresponding forest plots).

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[14] self-esteem: lack of studies at posttest; positive effect at short-term FU based on single study

[15] for medium-term FU: supported based on change in MD for Mache et al. (2017) between posttest (MD –1.13, 95% CI [–1.63, –0.63]), short-term FU (MD –0.90, 95% CI [–1.41, –0.39]), and medium-term FU (MD –0.40, 95% CI [–0.75, –0.05])

[16] for medium-term FU: supported based on change in MD for Mache et al. (2017) between posttest (MD –1.14, 95% CI [–1.64, –0.64]), short-term FU (MD –0.76, 95% CI [–1.10, –0.42]) and medium-FU (MD –0.40, 95% CI [–0.73, –0.07])

[17] see *hypothesis 1.1:* already no evidence for any effect at posttest and/or short-term FU (for hardiness, see change in MD for Tierney & Lavelle (1997) in 2.3.4.1)

[18] for long-term FU: supported based on change in MD for Bernburg et al. (2019) between posttest (MD 0.85, 95% CI [0.41, 1.29]), short-term FU (MD 0.47, 95% CI [0.04, 0.90]), and long-term FU (MD 0.19, 95% CI [–0.02, 0.40]); not supported based on change in MD for Cheung (2014) between short-term FU (MD 0.16, 95% CI [0.02, 0.30]) and medium-term FU (MD 0.17, 95% CI [0.10, 0.24])

Due to less than 10 studies measuring anxiety at posttest and short-term FU, no subgroup analysis regarding setting could be performed. The same applied to depression at short-term FU.

RQ2 (exploratory): Are there any differences between resilience interventions using different settings concerning the effects on (2.1) resilience, (2.2) anxiety symptoms, (2.3) depressive symptoms, (2.4) (perceived) stress, or (2.5) well-being in HCP? The exploratory RQ can be negated for (2.1) resilience, (2.3) depressive symptoms (only testable at posttest), (2.4) (perceived) stress), and (2.5) well-being, but could not be tested for (2.2) anxiety symptoms.

2.3.4.3 Impact of Delivery Format on the Efficacy of Resilience-Training Programs

Concerning the exploratory RQ3 whether the efficacy of resilience interventions in HCP depends on the delivery format of interventions, the respective subgroup analyses for resilience, depression (only posttest), (perceived) stress, and well-being resulted in no evidence for a subgroup difference in favor of a certain delivery (see tests for subgroup analyses in Appendix C7; all *p*s > .22 and Appendix D14, for forest plots). Again, no subgroup analyses could be conducted for anxiety and depression at short-term FU due to less than 10 studies included in the main analyses (see 2.2.4.8).

RQ3 (exploratory): Are there any differences between resilience interventions using different delivery formats concerning their effects on (3.1) resilience, (3.2) anxiety symptoms, (3.3) depressive symptoms, (3.4) (perceived) stress, or (3.5) well-being in HCP? The exploratory RQ can be negated for (3.1) resilience, (3.3) depressive symptoms (only testable at posttest), (3.4) (perceived) stress, or (3.5) well-being, but could not be tested for (3.2) anxiety symptoms.

2.3.4.4 Impact of Intensity on the Efficacy of Resilience-Training Programs

To answer the exploratory RQ3 regarding the potential role of training intensity for the effects of resilience interventions on the primary outcomes, subgroup analyses were also performed, the exceptions being anxiety (posttest and short-term FU) and depression (short-term FU), with less than 10 studies in the main analyses. For resilience at short-term FU, there was evidence for a difference between subgroups (χ^2 = 17.84, *df* = 2, *p* < .001, I^2 = 88.8%; Appendix C7), with a moderate ES found

for seven high-intensity trainings ($p < .001$) versus little or no evidence for an effect of training for low-intensity ($p = .12$) and moderate-intensity trainings ($p = .50$; 1 study). Across the remaining six analyses, no evidence for any subgroup difference was identified (see tests for subgroup differences in Appendix C7; all *p*s > .22 and Appendix D14, for forest plots).

RQ4 (exploratory): Are there any differences between resilience interventions using different intensities concerning their effects on (4.1) resilience, (4.2) anxiety symptoms, (4.3) depressive symptoms, (4.4) (perceived) stress, or (4.5) well-being in HCP? The exploratory RQ can be negated for (4.3) depressive symptoms (only testable at posttest), (4.4) (perceived) stress, and (4.5) well-being, but could not be tested for (4.2) anxiety symptoms. Regarding (4.1) resilience, the RQ can be negated for resilience at posttest. At short-term FU, there is evidence for a difference in the efficacy of trainings in favor of high-intensity resilience interventions.

2.3.4.5 Impact of Theoretical Foundation on the Efficacy of Resilience-Training Programs

Most subgroup analyses (forest plots in Appendix D14) to examine the potential role of theoretical foundation (RQ4) provided no evidence for a difference for the primary outcomes at posttest and short-term FU (Table 2; *p*s > .15). Again, this RQ could not be tested for anxiety and depression due to the small number of studies at posttest (anxiety) and/or short-term FU (both). For well-being at posttest, there was evidence for a subgroup difference ($\chi^2 = 10.79$, $df = 3$, $p = .01$, $I^2 = 72.2\%$; see Table 2), with a large ES found for two mindfulness-based trainings ($p = .001$) compared to little or no evidence for any effect of coaching ($p = .79$), combined theoretical foundations ($p = .10$), and unspecified training ($p = 1.00$). For resilience in the short-term, there was also evidence for a subgroup difference ($\chi^2 = 21.43$, $df = 3$, $p < .001$, $I^2 = 86.0\%$), with a large ES for one mindfulness-based intervention ($p = .01$) and a moderate ES for combined programs ($p < .001$) versus no evidence for an effect for AIT-based ($p = .12$) and one unspecified training ($p = .50$).

Table 2

Subgroup Analyses Concerning Theoretical Foundation[a]

Outcome/ subgroup	Studies	*N*	SMD random-effects, 95% CI	*p*	I^2	Interpretation (Cohen, 1988)
Resilience, posttest						
Test for subgroup differences: $\chi^2 = 3.02$, *df* = 2, *p* = .22, $I^2 = 33.7\%$						
mindfulness	3	83	0.17 [−0.48, 0.82]	.61	53%	ns
combined	8	534	0.46 [0.26, 0.67]	< .001	28%	moderate
unspecified	1	73	0.84 [0.36, 1.32]	< .001	/	large
Depression, posttest						
Test for subgroup differences: $\chi^2 = 2.44$, *df* = 3, *p* = .49, $I^2 = 0\%$						
mindfulness	3	83	0.01 [−0.42, 0.45]	.95	0%	ns
CBT	1	134	−0.34 [−0.68, 0.01]	.05	/	moderate
combined	6	293	−0.44 [−0.87, −0.01]	.05	58%	moderate
unspecified	4	278	−0.22 [−0.53, 0.08]	.15	33%	ns
(Perceived) stress, posttest						
Test for subgroup differences: $\chi^2 = 3.49$, *df* = 2, *p* = .17, $I^2 = 42.6\%$						
mindfulness	4	115	−0.18 [−0.63, 0.28]	.45	33%	ns
combined	12	843	−0.79 [−1.38 −0.20]	.009	93%	large
unspecified	1	39	−0.05 [−0.68, 0.58]	.88	/	ns
Well-being, posttest						
Test for subgroup differences: $\chi^2 = 10.79$, *df* = 3, *p* = .01, $I^2 = 72.2\%$						
mindfulness	2	66	0.83 [0.32, 1.33]	.001	0%	large
coaching	1	730	−0.02 [−0.17, 0.13]	.79	/	ns
combined	9	591	0.14 [−0.03, 0.31]	.10	0%	ns
unspecified	1	107	0 [−0.38, 0.38]	1.00	/	ns
Resilience, short-term FU (≤3 months postintervention)						
Test for subgroup differences: $\chi^2 = 21.43$, *df* = 3, *p* < .001, $I^2 = 86.0\%$						
mindfulness	1	26	1.05 [0.22, 1.88]	.01	/	large
AIT	3	98	0.53 [−0.14, 1.20]	.12	61%	ns
combined	6	468	0.43 [0.24, 0.62]	< .001	5%	moderate
unspecified	1	733	−0.05 [−0.20, 0.09]	.50	/	ns
(Perceived) stress, short-term FU (≤3 months postintervention)						
Test for subgroup differences: $\chi^2 = 5.28$, *df* = 3, *p* = .15, $I^2 = 43.2\%$						
mindfulness	1	26	−1.13 [−1.97, −0.29]	.008	/	large
AIT	3	97	−0.74 [−1.16, −0.32]	< .001	0%	large
combined	9	626	−0.34 [−0.60, −0.08]	.009	60%	moderate
unspecified	1	39	−0.69 [−1.34, −0.04]	.04	/	large
Well-being, short-term FU (≤3 months postintervention)						
Test for subgroup differences: $\chi^2 = 0.45$, *df* = 2, *p* = .80, $I^2 = 0\%$						
AIT	2	58	0.32 [−0.29, 0.93]	.30	23%	ns
combined	8	542	0.12 [−0.05, 0.28]	.18	0%	ns
unspecified	2	813	0.19 [−0.35, 0.73]	.49	82%	ns

Note. N = number of participants; SMD = standardized mean difference; CI = confidence interval; *p* = *p* value; I^2 = heterogeneity; χ^2 = Chi^2 value of test for subgroup differences; *df* = degrees of freedom; ns = not significant; CBT = Cognitive-Behavioral Therapy; FU = follow-up; AIT = Attention and Interpretation Therapy.

[a] For theoretical foundation, some subgroups were added (compared to the review protocol) based on the evidence found.

RQ5 (exploratory): Are there any differences between resilience interventions based on different theoretical foundations concerning their effects on (5.1) resilience, (5.2) anxiety symptoms, (5.3) depressive symptoms, (5.4) (perceived) stress, or (5.5) well-being in HCP? The exploratory RQ can be negated for (5.3) depressive symptoms (only testable at posttest) and (5.4) (perceived) stress, but could not be tested for (5.2) anxiety symptoms. Regarding (5.1) resilience, the RQ can be negated for resilience at posttest, while there is evidence for a difference in the efficacy of trainings at short-term FU in favor of mindfulness-based and combined interventions. Concerning well-being (5.5), the RQ can be negated for the short-term FU. At posttest, there is evidence for a difference in the efficacy of trainings in favor of mindfulness-based interventions.

2.3.5 Further Results – Sensitivity Analyses

With respect to sensitivity analyses at posttest and short-term FU (except for anxiety and depression), mostly no longer evidence for an effect of resilience training was found when excluding studies without trial registration or a published study protocol (see Appendix C8; forest plots in Appendix D15). The exclusion of studies at high risk of attrition bias, reporting bias, or with high levels of missing data, as well as sensitivity analyses related to the management of missing data and the use of fixed-effect instead of random-effects models partly led to changes in the evidence found (e.g., change from significant to non-significant finding or vice versa; increase from small to moderate ES or vice versa). Removing studies measuring resilience with a trait scale (posttest) did not change the evidence found for a moderate effect on resilience. The planned sensitivity analysis regarding the underlying concept of resilience scales at short-term FU was not possible since all studies used a state-oriented scale. In addition, for resilience at posttest and short-term FU, the planned sensitivity analysis concerning reporting bias could not be performed as all studies were judged at low RoB.

2.3.6 Further Results – Adverse Events

Only three studies assessed the potential adverse or undesired effects of resilience training in HCP, all of them reporting no such effects (Lebares et al., 2019; Loiselle, 2018; Strijk et al., 2011).

2.3.7 Further Results – Assessment of Heterogeneity

The heterogeneity across the different outcomes in HCP was mixed (see Appendix C9). Concerning I^2, nine analyses showed no heterogeneity (I^2 = 0% or near). In four analyses, heterogeneity was below 50%, whereas nine analyses showed substantial to considerable heterogeneity ($I^2 \geq 50\%$). Overall, I^2 ranged between 0% and 90% (stress at posttest). G^2, which takes small-study effects into account, indicated no heterogeneity (i.e., 0% or near) for nine analyses. For seven of these, the G^2 value of or near 0% was in line with I^2 values of 0% for the respective outcomes. Relatively similar values of G^2 and I^2 were also found for five analyses, with both indicating moderate or even higher heterogeneity. Inconsistent values of G^2 and I^2 (e.g., G^2 values indicating substantial or considerable heterogeneity vs. I^2 values below or near 50% or vice versa) were identified for ten analyses.

2.3.8 Further Results – Assessment of Reporting Bias

Based on funnel plots (visual analysis; see Appendix C10 and Appendix D16) and Egger's test, there was no visual and statistical evidence for asymmetry for depression (Egger's test: $t = -0.10$, $df = 12$, $p = .93$) and well-being (Egger's test: $t = 1.91$, $df = 11$, $p = .08$) at posttest. For resilience at posttest and (perceived) stress at posttest and short-term FU, there was slight visual evidence of asymmetry, respectively. However, no statistical evidence was identified based on Egger's test (resilience post: $t = -1.04$, $df = 10$, $p = .32$; (perceived) stress post: $t = -0.34$, $df = 15$, $p = .74$; short-term FU: $t = -1.32$, $df = 12$, $p = .21$). Visual (see Appendix D16) and statistical evidence for asymmetry were identified for resilience (Egger's test: $t = 4.01$, $df = 9$, $p = .003$) and well-being at short-term FU (Egger's test: $t = 2.43$, $df = 10$, $p = .04$; see 2.4.3).

2.4 Discussion

2.4.1 Summary of Main Results

The review aimed to synthesize the current evidence on the efficacy of psychological interventions to foster resilience in HCP. With respect to *RQ1 (hypothesis 1.1)* concerning the short-term effects of resilience trainings, the findings showed that training programs were more effective than control for improving resilience, self-reported symptoms of depression, (perceived) stress, optimism, self-efficacy, and positive emotions immediately postintervention. ES ranged from small to moderate. There was little or no evidence for an effect of training on anxiety. At short-term FU (≤ 3 months postintervention), the ES regarding the reduction of depressive symptoms further increased from small to moderate. The possible moderate effects for resilience and for (perceived) stress found at posttest were maintained. The same applied to the secondary outcomes of optimism, self-efficacy, and positive emotions. In contrast with the posttest, there was also evidence for a moderate effect in favor of resilience training on anxiety symptoms and self-esteem (only testable at short-term FU). Especially for the secondary outcomes, the evidence base at short-term FU partly consisted of a single study. There was little or no evidence for an effect of training on well-being, social support, active coping, and hardiness at posttest or short-term FU (for hardiness only testable at posttest).

At medium- and long-term FU, that is, more than 3 months postintervention (*RQ1, hypothesis 1.2*), this review identified no longer evidence for a difference between resilience training and control for resilience. Concerning depressive symptoms and (perceived) stress, the ES identified at posttest and short-term FU decreased in the medium-term based on a single study, also with no longer evidence for any effect at long-term FU. For only one secondary outcome, self-efficacy, the previously found positive effect was maintained in the medium-term, with no longer evidence of any effect in the long-term comparable to the primary outcomes. Consistent with earlier time periods, there was also no evidence for an effect of training on well-being, social support, active coping, and hardiness. Several primary and secondary outcomes were not assessed at medium- and long-term FU by any study (anxiety, optimism, self-esteem, positive emotions), preventing the review to test

hypothesis 1.2 regarding the sustainability of effects.

Subgroup analyses for the primary outcomes at posttest (not feasible for anxiety) and short-term FU (not feasible for anxiety and depression) to investigate the *RQs 2 to 5* indicated no consistent effect modifiers. The subgroup analysis of training intensity for resilience at short-term FU provided evidence for a difference in favor of high-intensity trainings. For theoretical foundation, the subgroup analyses for well-being at posttest and resilience at short-term FU showed evidence for a difference in favor of mindfulness-based interventions (both outcomes) and combined programs (resilience). Beyond that, however, little or no evidence for differences in the efficacy of resilience training was identified depending on setting, delivery, training intensity, and theoretical foundation.

At posttest (not feasible for anxiety) and short-term FU (not feasible for anxiety and depression), several sensitivity analyses were performed. These provided evidence for a substantial impact of the preregistration of included studies on the review's conclusions. Due to the relatively large number of studies judged at high risk of attrition and reporting bias and the variability between studies in these RoB domains, the conclusions were also partly affected by excluding studies at high risk of these biases. The same applied to the level of missing data, the coping with missing data, and the use of fixed-effect meta-analyses. This means, the conclusions of this review might not be robust against possible bias introduced, for example, by including studies at high risk of bias. The results of a sensitivity analysis regarding the underlying resilience concept (only testable at posttest) were consistent with the evidence found in the primary analysis. Adverse events were hardly assessed.

2.4.2 Overall Completeness and Applicability of Evidence

The review highlights some issues about the overall completeness and applicability of the evidence for the effects of resilience interventions in HCP (for details, see Appendix C11).

Participants. Since stress-related mental disorders are more prevalent in women (Kuehner, 2017; Li & Graham, 2017; Riecher-Rössler, 2017; WHO, 2020) and since women reported lower resilience (e.g., Kunzler, Chmitorz, et al., 2018), the high proportion of *women* among the study participants may be explained by a higher interest of women to participate in resilience interventions.

The applicability of the findings of this review to men may be limited, since gender differences in the prevalence of stress-related mental disorders may reflect differences in biological vulnerability, social roles, or stress reactivity (e.g., Nazroo et al., 1998; Verma et al., 2011; WHO, 2020), thereby causing a potentially different effect of resilience training in men and in women. Concerning the participants' *age*, middle-aged participants (50–65 years) before their retirement were rarely examined. Moran (1998) postulated a curvilinear association with a higher stressor exposure in emergency workers at the beginning and by the end of working life compared to a moderate level of work experience. Similarly, the period before retirement as an important transitional event may be stressful (e.g., Bossé et al., 1991; Selye, 1980). Thus, employees aged 50–65 years might benefit differently (e.g., to greater extent) from resilience trainings. The evidence from this review does not allow to answer this question. With respect to *healthcare sectors*, the included studies were mainly conducted in a hospital setting including physicians, nurses, and different hospital personnel, with various medical departments represented. About two-thirds of the 44 studies assessed *mental health at baseline*. The clinical relevance of mental symptoms, that is, whether symptom load justified a diagnosis of mental disorder, is unclear for most studies, since no study screened for mental disorders using a structured clinical interview. However, to get a clear picture of the participants' baseline mental health could be important, as the large ES in some studies (e.g., Bernburg et al., 2019) might in part also be explained by the inclusion of participants with pre-existing burden of mental symptoms or even clinical diagnoses. For *location*, the evidence was concentrated in North America, Europe, and Asia (including the Near East), with only three studies from Australia. The applicability of the findings to other locations and ethnicities (e.g., South America, Africa, Oceania) therefore remains unclear. Of the 44 included studies, 36 were conducted in *high-income countries* (e.g., USA) and eight in an upper-middle income countries (e.g., China). Therefore, the cross-cultural applicability of the evidence should also be considered with caution. In summary, the findings may be most applicable to the young and middle-aged, to female HCP, and to those living in high-income countries.

Interventions. Although the benefits of online- and mobile-based interventions (e.g., 24/7

availability) have recently been discussed (Cuijpers et al., 2017; Heber et al., 2017; Heron & Smyth, 2010), only three training programs delivered in this format were identified in this review. In addition, most of the interventions were of *high o*r *moderate intensity*, with treatment durations varying considerably. Except for ACT and PST, all *theoretical foundations* prespecified in the protocol (Helmreich, Kunzler et al., 2017) have been tested in RCTs found in this review. The number of RCTs varied, with most studies investigating combined theoretical foundations. Overall, the findings of this review are mostly applicable to group interventions of high intensity, delivered face-to-face, and using a combination of theoretical approaches.

Comparators. For *attention and active controls*, there was considerable heterogeneity in terms of setting, delivery format, and content, rendering comparability between single-study comparisons difficult. The primary use of no-intervention and wait-list controls, in particular, in the evidence found here could be problematic, since these CGs were demonstrated to yield inflated ES compared to active comparators in psychotherapy research (e.g., Mohr et al., 2014).

Outcomes. *Different measures for resilience* were used in this review (see Appendix D13.3). For the potential effect of the underlying concept of resilience, the exclusion of trait-based resilience measures did not modify the ES on resilience at posttest. However, this finding might be associated with only one study (Lebares et al., 2019) using a trait-based measure. A large *variety of assessments* was also admitted for the primary outcomes of mental health and well-being (e.g., burnout and depression scales for depression; see Helmreich, Kunzler et al., 2017). This diversity of measures should be considered as a potential source of heterogeneity in the meta-analyses, and might affect the interpretation of results. Although variables, such as social support, are discussed as well-evidenced *resilience factors* (Helmreich, Kunzler et al., 2017; see 2.1.3.2), relatively few of the included studies assessed these outcomes at the different periods of FU. Since most of the included studies had small samples, the *attrition bias* found for 16 studies has to be interpreted with caution. For psychotherapy, several possible adverse outcomes were discussed (e.g., emotional arousal; Berk & Parker, 2009; Moritz et al., 2019). As resilience trainings often include confronting participants with

individual problems (e.g., by teaching structured problem-solving), some of these programs might also have the potential to harm certain participants. However, given that potential *adverse effects* were not specified in most included studies (see 2.3.6), conclusions should be drawn cautiously. Lastly, very few studies had *medium- or long-term FU assessments*, limiting the ability to examine whether any benefits of resilience interventions are sustained in the long-term.

2.4.3 Certainty of the Evidence

Using the GRADE approach (Schünemann et al., 2013; Schünemann, Higgins, et al., 2019), the overall certainty of evidence at postintervention was rated to be very low, for several reasons: First, important *methodological limitations* reduced the certainty of the evidence offered by most included studies. There was unclear and high RoB for several domains across the studies, especially in methods of sequence generation, allocation concealment, blinding of participants and personnel, blinding of outcome assessment, and loss to FU. Selective outcome reporting was occasionally an issue. Second, with respect to the posttest considered in the SoF table, four outcomes had moderate (I^2 > 30%: resilience, depression, well-being) or substantial (I^2 > 50%; stress) levels of *unexplained heterogeneity* and only partially overlapping CIs, leading to inconsistency. Third, for all (primary) outcomes at postintervention, the evidence was *indirect*, as studies were limited to certain participants (e.g., young to middle-aged adults), particular versions of resilience training (e.g., group setting, face-to-face delivery, moderate and high training intensity, mindfulness-based and combined theoretical foundations), and certain comparators (e.g., no intervention, wait-list). Due to the small number of participants in the meta-analysis for anxiety (< 400 participants), inconsistent messages of the 95% CI for the treatment effect (anxiety, well-being), and the 95% CI encompassing both a very small intervention effect and crossing the threshold for appreciable benefit of the intervention (depression), *imprecision* was a problem for three outcomes at posttest (see Appendix C12).

It was not downgraded for *publication bias* for any of the primary outcomes at postintervention. Based on funnel plots (see Appendix C10) and Egger's test, there was no statistical or visual evidence of asymmetry (see Appendix D16). The funnel plots were mostly symmetrical in

shape, with only slight visual asymmetry for resilience and (perceived) stress. However, for both outcomes, studies appeared to be missing in areas of high statistical significance ($p < .01$); therefore, a publication bias can be assumed as unlikely according to the Cochrane Handbook (Page et al., 2019). If available, the results of grey literature for the primary outcomes of resilience and mental health (depression, [perceived] stress, well-being) did not differ from other published studies for the (non-) evidence or the direction of effect. Due to the scarcity of larger studies across the primary outcomes at posttest (with the exception of Strijk et al. (2011) for well-being), a small-study effect was difficult to assess and cannot not be ruled out completely. Nevertheless, an overestimation of effects in smaller studies seemed unlikely, since the meta-analyses mostly included small studies with statistically significant and non-significant results. Although the evidence was largely based on small studies, there was no indication of conflicts of interest of relevance for the posttest meta-analyses. Appendix C13 includes the results concerning the assessment of publication bias for three primary outcomes at short-term FU (not in SoF table).

Regarding *adverse events*, several GRADE domains (e.g., precision, publication bias) could not be assessed, due to the small number of studies documenting any adverse effects of study participation (e.g., by verbal feedback from participants; Lebares et al., 2019; Loiselle, 2018; Strijk et al., 2011). Based on the narrative reports in these studies, it was downgraded for study limitations and indirectness. Overall, the GRADE certainty rating as very low means that there is a high degree of uncertainty about the estimates of effect observed for all primary outcomes at posttest. Future research is very likely to substantially impact the effect estimates of resilience interventions.

2.4.4 Potential Biases in the Review Process

Search methods. Appendix C14 includes further information on how potential biases in the search methods were prevented for this review. Except for five completed but unpublished studies (ISRCTN69644721; NCT02603133; NCT03645798; Smith et al., 2019; West et al., 2015), the full texts for all included studies could be retrieved. In accordance with the Cochrane Developmental, Psychosocial, and Learning Problems (CDPLP) editorial team, alternative sources (e.g., trial register

entry) were considered for these studies. In eight cases (see Appendix C3; Appendix D7), no contact data from the investigators were found, no reply from the study authors was received, or the responses were inadequate to decide about a study's eligibility. It was attempted to conduct a comprehensive search; however, the fact that 12 studies have not yet been incorporated, and will only be added in the update of this review could be considered as potential source of bias.

Correspondence with the authors was required for 32 included studies. For three studies, for which it was aimed to double-check the available information (e.g., number of participants analyzed) or to receive unadjusted outcome data by contacting the authors, it was decided to rely on the reports and to include the studies in the meta-analyses despite the missing response (Fei, 2019; Loiselle, 2018; Medisauskaite & Kamau, 2019). For four studies, alternative statistical information was used to include them in quantitative analysis (Calder Calisi, 2017; Clemow et al., 2018; Hosseinnejad et al., 2018; Klatt et al., 2015). For three studies (ISRCTN69644721; NCT02603133; Smith et al., 2019), only the information was received that no data could be provided, as the studies were completed, but in the process of analysis or publication. For three further studies (NCT02603133; Stetz et al., 2007; Wild, 2016), the primary investigators responded to the first inquiry, but not to a second inquiry, or were not able to provide the relevant subgroup data at the time of data analysis.

Posthoc changes of eligibility criteria. A posthoc change was made to the eligibility criteria for the *Types of interventions* (see Appendix D17.1) by subsequently limiting the study selection to interventions that explicitly stated the aim of fostering resilience, hardiness, or posttraumatic growth. Although the change raises the possibility of bias in the review process, it was felt to be necessary to guarantee highly objective eligibility criteria and transparency. Thus, this departure from the protocol (Helmreich, Kunzler et al., 2017) should not cause a serious bias. Due to the focus on interventions with the mention of at least one of the three terms, general health-promoting interventions (e.g., well-being therapy, self-management trainings after negative life events) not meeting this criterion were excluded from this review. However, other psychological interventions in HCP, that are eventually more economical than theoretical approaches found in this review, might also foster

mental health despite stressors (i.e., resilience), although not being labeled as “resilience training”. A posthoc change was also made to the eligibility criteria for *Types of participants* (see Appendix D17.1) by limiting the review to *HCP*. Although the change raises the possibility of bias, it was assumed as necessary because the restriction to HCP guarantees a systematic review with sufficiently homogeneous comparisons.

Further potential biases. Even within each type of theoretical foundation, there was partial clinical heterogeneity (in terms of intervention setting, delivery, or intensity). However, as there is still no consensus or “gold standard” about how to design resilience-training programs leading to variety (see also, e.g., Leppin et al., 2014), it was decided to pool the data. This decision was taken as this review had a larger evidence base than previous meta-analyses and potential heterogeneity could be investigated by subgroup analysis. Beyond the five main results for the primary outcomes at posttest, the large number of the pooled analyses, subgroup, and sensitivity analyses in this review might have increased the probability of a type I error, potentially leading to false positive results. Another important limitation of this review refers to the unknown stressor or risk exposure in most included studies (see 2.4.7). Although employment in the healthcare sector might be associated with substantial stressors among participants of the included studies, a *proven* risk or stressor exposure was not applied as an inclusion criterion of this review (see 2.2.1.1). However, based on the definition of resilience (e.g., Windle, 2011), the effects of resilience interventions on resilience cannot be determined without ensuring a significant risk. The missing assessment of stressor exposure is a general problem of resilience intervention research (Chmitorz, Kunzler, et al., 2018).

2.4.5 Agreements and Disagreements With Other Studies or Reviews

2.4.5.1 Studies or Reviews in Different (Non-)Clinical Adult Populations

As mentioned under 2.1.5, the efficacy of resilience interventions for adult populations has been previously examined in 13 systematic reviews and five meta-analyses. In sum, the reviews largely found positive effects of resilience training on different outcomes (e.g., resilience, mental health). However, many of the reviews also considered *study designs other than RCTs* and focused on certain

target groups or *interventions* (see Appendix D1.4). The number of RCTs specifically on resilience training was therefore rather limited, making comparisons with the present review difficult.

Some of the previous reviews (Joyce et al., 2018; Leppin et al., 2014; Macedo et al., 2014; Robertson et al., 2015; Vanhove et al., 2016) used broader eligibility criteria and identified more RCTs compared to other reviews, facilitating comparisons with this work. Despite varying inclusion criteria, the findings of this review largely agree with this previous research. Macedo et al. (2014; seven RCTs), whilst not pooling any data, identified some degree of effectiveness of resilience-training programs. Similarly, Robertson et al. (2015; eight RCTs) found indications of benefit for personal resilience, mental health, well-being, and work performance in employees. With the exception of well-being and job performance (not examined here), these findings were confirmed by the present review.

Regarding the positive short-term effects for resilience and depressive symptoms up to 3 months postintervention, this review is largely consistent with several meta-analyses, which also found small-to-moderate positive effects of training on resilience up to 3 months postintervention (Joyce et al., 2018, 17 RCTs; Leppin et al., 2014, 25 RCTs) and small proximal effects (≤ 1 month postintervention) on psychological deficits (e.g., depressive symptoms; Vanhove et al., 2016; 14 RCTs). In contrast with Vanhove et al. (2016), who also identified positive effects on well-being (≤ 1 month postintervention), little or no evidence for an effect on this outcome was found here. On the other hand, this result is consistent with Leppin et al. (2014; quality of life), whereas this review found different evidence for training effects on depressive symptoms (small-to-moderate reduction) than Leppin et al. (2014; no evidence for effect). The delayed effect on anxiety between posttest and short-term FU in this work is comparable with Vanhove et al. (2016), who only found maintained effects of training for the prevention of psychological deficits at more than one month after training.

Based on subgroup analyses, the findings of previous reviews (Joyce et al., 2018; Vanhove et al., 2016), which identified advantages in favor of individual versus group settings (Vanhove et al., 2016), individual setting and classroom-based (group) format versus computer-based delivery (Vanhove et al., 2016), or positive effects of CBT-based, mindfulness-based, and mixed interventions

(Joyce et al., 2018) were not replicated. This inconsistency might be explained by the limited number of studies in some subgroup analyses of this review, the weighting of analyses for certain subgroups (e.g., group setting), and the lack of studies in certain subgroups (e.g., online-/mobile-based delivery). In addition, ”combined” interventions were defined differently from Joyce et al. (2018).

2.4.5.2 Studies or Reviews in HCP

With respect to healthcare staff, 13 systematic reviews and one meta-analysis have synthesized the efficacy of resilience-training programs in this target group so far (see Appendix D1.6). Comparable with this review, which considered various groups of HCP, four previous publications examined HCP in general (Cleary et al., 2018) or combined HCP and students (Gilmartin et al., 2017; Pezaro et al., 2017; Rogers, 2016). The majority, however, targeted *subgroups of healthcare workers* (e.g., Concilio et al., 2019; Fox et al., 2018), limiting comparisons with the current review. In contrast with the present work, which identified 44 RCTs, the number of RCTs on resilience training was rather limited (i.e., 0–9 RCTs among 5–33 included studies) in previous reviews as they also included *other study designs*. Furthermore, since the review questions of some of the 14 reviews *did not solely focus on the construct of resilience or on intervention studies*, the primary studies did not always explicitly mention the intention of fostering resilience, also rendering comparisons difficult. Keeping this inconsistency in mind, the findings of this work are nevertheless consistent with several previous reviews in HCP, that also reported improved resilience and mental health (e.g., reduced stress, burnout, or anxiety; Fox et al., 2018; Gilmartin et al., 2017; Pezaro et al., 2017; Rogers, 2016). However, as the current review only focused on RCTs and interventions explicitly stating the intention of fostering resilience or a related construct, its conclusions concerning the efficacy of resilience training might be more precise and meaningful.

Regarding the eligibility criteria, the current review is most comparable with Cleary et al. (2018), who also only included psychological interventions prospectively designed to enhance resilience and considered different groups of HCP. The RCTs on resilience training in physicians (Mache et al., 2016; Mache, Vitzthum, et al., 2015; Sood et al., 2011; Sood et al., 2014), nurses (Chesak et al.,

2015; Mealer et al., 2014), and different hospital personnel (Klatt et al., 2015), identified by Cleary et al. (2018), were also included in this review, except for Maunder et al. (2010) and Rowe (2006)[19]. Furthermore, many of the non-RCTs found in Cleary et al. (2018) were also identified during the current study identification process. Despite the limited comparability regarding study designs and the different number of included RCTs (10 vs. 44), the positive moderate effects on resilience in the short-term in this review review are partly in line with Cleary et al. (2018) who also summarized differences in favor of resilience training for four of seven RCTs measuring this outcome. In the only previous meta-analysis on resilience training in HCP (physicians; four RCTs), Lavin Venegas et al. (2019) did not perform pooled analyses for resilience due to heterogeneity. For burnout, meta-analyses were only performed for observational studies, while one RCT (Dyrbye et al., 2016)[20] measuring this outcome showed no evidence for an effect of training. Lavin Venegas et al. (2019) only conducted subgroup analyses for primary care physicians based on two non-RCTs, and found no evidence for a difference for burnout. Due to the different eligibility criteria, the findings of the (meta-) analyses are hardly comparable.

2.4.6 Implications for Practice

In sum, there is very uncertain evidence that resilience interventions are effective in improving resilience or certain resilience-related factors such as optimism, self-reported symptoms of depression, and (perceived) stress at posttest (small and moderate ES).

The generalizability and applicability of the available evidence is limited by the scarcity of studies with long-term FU, the divergent efficacy measures, the heterogeneous design and content of interventions (with a dominance of high-intensity face-to-face interventions delivered in a group setting), and the limited geographical locations (i.e., high-income countries). In addition, the certainty of the evidence from this review was rated as being very low across all primary outcomes at posttest.

[19] Maunder et al. (2010) not considered to be a RCT as the study involved a random assignment to three different doses of resilience intervention, but no CG; Rowe (2000) and Rowe (2006) excluded due to "ineligible intervention" since the focus of the training did not seem to be on fostering hardiness (i.e., hardiness only as correlate of burnout as main outcome)

[20] not included in this review as resilience or a related construct was not mentioned in the publication

Therefore, strong conclusions about the effects of resilience interventions cannot be drawn, as the true effect may be markedly different from the estimated effect. Little is known about the longer-term effects of resilience training on most outcomes, because few studies performed FU assessments. Booster sessions were not conducted in any of the included studies.

The limited evidence that resilience training improves well-being and several resilience factors might indicate the need to adapt the current intervention techniques used. Based on studies that achieved moderate to large effects on well-being and resilience factors, such contents might include, for example, mindfulness (e.g., informal and formal meditation; Duchemin et al., 2015; Klatt et al., 2015; Lin et al., 2019), attention training toward novel and positive aspects in the environment, and the strengthening of positive principles, such as gratitude (e.g., Sood et al., 2011). In addition, several studies also included specific modules on the identification of personal resources (e.g., Berger & Gelkopf, 2011; Gelkopf et al., 2008; Hosseinnejad et al., 2018). By training a positivity bias at the level of action tendencies, the second part of this thesis (see 3) also focuses on such positive aspects.

In this review, no effect modifiers regarding the design of resilience interventions were observed based on subgroup analyses, meaning that the effects did not depend from the form of implementation. This might be mainly due to the small number of studies in certain subgroups (e.g., CBT in analysis for theoretical foundation) and the weighting of analyses for one subgroup (e.g., group setting). Thus, subgroup analyses in this review might be underpowered and rather exploratory. With respect to the time point in the career of HCP or the working environments (e.g., healthcare sectors), this review provided no clear indication when and where healthcare workers benefit most from resilience interventions. Positive effects were identified for HCP working under relatively regular conditions (e.g., in hospital; Bernburg et al., 2019; Fei, 2019), those participating in a training before/after the transition to a new department (e.g., Chesak et al., 2015), healthcare workers at the beginning of their career (e.g., Mache et al., 2017), employees with special medical responsibilities (e.g., intensive care unit; Klatt et al., 2015), and HCP working under highly stressful circumstances (e.g., war, community emergency, disaster; Berger & Gelkopf, 2011; Cheung, 2014; Gelkopf et al.,

2008). Based on theories of stress and resilience (e.g., PASTOR; see 2.1.3.4), the individual stressor load and the subjective appraisal of those stressors might have affected the intervention effects. However, due to the lack of studies measuring the stressor exposure, respective conclusions are not possible.

In general, based on nine resilience-training programs for which evidence of moderate to large positive effects on at least two of all outcomes of this review was found (Berger & Gelkopf, 2011; Bernburg et al., 2019; Chesak et al., 2015; Fei, 2019; Lin et al., 2019; Luthar et al., 2017; Mache et al., 2017; Schroeder et al., 2016; Sood et al., 2011), several implications for intervention contents might be derived (for details, see Appendix C15). For example, HCP seem to benefit from modules on problem-solving, emotion regulation, conflict and anger management, goal-setting and planning for the future, and communication training. Furthermore, resilience interventions might include information on how to recognize stress and prevent burnout as well as to build and use social support and feedback. Several of the above programs also included techniques on mindfulness (e.g., mindfulness meditation) and (self-) compassion, strategies to challenge irrational beliefs, and to minimize rumination. Finally, positive attention training, the cultivation of flexible interpretations and positive re-framing of life experiences were focused in some interventions.

2.4.7 Implications for Research

The findings of this review point to the need for further research of high methodological quality to determine the efficacy of resilience interventions in HCP. For future research, a consensus on the definition of resilience and adequate outcome measures to be used consistently across the field would be important. Following the growing consensus on resilience as a dynamic outcome (e.g., Bonanno et al., 2015; Kalisch et al., 2017), intervention studies might be guided by this definition and examine resilience as a primary outcome (Chmitorz, Kunzler, et al., 2018). Due to only five studies measuring the participants' stressor exposure (Berger & Gelkopf, 2011; Cieslak et al., 2016; Gelkopf et al., 2008; Varker & Devilly, 2012; Wild, 2016), it remains unclear whether HCP really benefit from resilience training by being better able to cope with stressors. Future studies could therefore measure

resilience as a person's mental health in relation to individual stressor load. Only if the risk or stressor exposure (different from the subjective perception of stress) is assessed, may researchers gain knowledge about the changes in resilience by an intervention. In addition to the number of stressors, covariates such as the type of stressors (e.g., micro- vs. macrostressors, psychological vs. physiological stressors, acute vs. chronic stressors) or the perceived severity of stressors could be assessed.

With respect to study designs, there is a need for improved comparators, at least TAU or ideally active and attention control (Chmitorz, Kunzler, et al., 2018), to allow fair comparisons between resilience intervention and control. As already suggested (Chmitorz, Kunzler, et al., 2018), resilience-training programs could be implemented during or after the presence of a stressor. However, future studies should also use designs in which resilience training is provided prior to circumscribed stressors (e.g., rotation of a physician to an emergency ward), to draw conclusions on resilience effects of the intervention, and see whether the training does indeed improve resilience to the specific stress situation (Chmitorz, Kunzler, et al., 2018; Kalisch et al., 2015). In general, pre- and postassessments of the outcome indicators (e.g., for resilience) should be conducted, with future studies also filling the gap of longer FU periods and measuring the stressor exposure before, throughout, and after the intervention. Also, it could be interesting to investigate whether booster sessions might help maintaining the effects of training over time. To ensure sufficient statistical power, the use of adequate sample sizes based on a priori analyses seems to be an urgent need in this field. Intervention studies might also benefit from comprehensive baseline diagnostics of mental health (e.g., clinical interview) and better reporting of eligibility criteria for pre-existing mental symptoms. This would allow more precise conclusions about whether resilience training reduces (clinically relevant) mental symptoms. Furthermore, the conceptual implications of the resilience concept would require a baseline mental health assessment. To investigate the effects of interventions on resilience (i.e., mental health in relation to stressor load) and to determine a specific "resilience pattern or trajectory" under consideration, the status of psychological functioning (as an outcome of interest) at baseline is important. For example, when researchers are interested in testing

the effects of an intervention in stressor-exposed individuals on the resilience trajectory of sustained mental health (see also 2.1.2), they would have to prove a positive mental health level at baseline (and at postintervention). On the other hand, researchers considering a sample with elevated levels of mental symptoms at pretest would be able to investigate the resilience trajectory of recovery or even of posttraumatic growth (i.e., increased level of functioning compared to outset prior to stressors). Beyond RCTs, dismantling designs could be helpful in clarifying the efficacy of single components of resilience training. In general, there is a need for better reporting of intervention studies using international guidelines, such as the CONSORT statement (Schulz et al., 2010). To guarantee higher transparency, primary investigators could register trials or publish study protocols according to the SPIRIT guidelines (Standard Protocol Items: Recommendations for Interventional Trials; Chan, Tetzlaff, Altman, et al., 2013; Chan, Tetzlaff, Gøtzsche, et al., 2013).

Finally, future studies in this field should focus more on male participants and on employees above the age of 50 years. Research efforts should be intensified in low- and middle-income countries to reach more robust conclusions about the efficacy and effectiveness of training across various settings. For certain delivery formats of intervention (e.g., online-/mobile-based) and cultural contexts (e.g., non-Western countries, other continents), more studies would be desirable. For example, it would be interesting to examine if interventions that are implemented closer to everyday life (e.g., using a smartphone app) might result in larger positive effects than “artificial” training settings (e.g., face-to-face group setting) that were mostly found in this review (see 3 in this thesis).

In sum, there is still a need for additional evidence to answer the question about which resilience interventions are really effective in HCP and how they should ideally be implemented. Based on improved study designs of primary studies, future reviews in this field might then also focus on further RQs, such as the efficacy of resilience trainings in healthcare workers depending on stressor exposure or adequate time points of implementing trainings in the career of HCP.

3 TRAIN4Positivity – Development and Pilot Evaluation of a Mobile-Based Training of Positivity Bias at the Level of Action Tendencies

3.1 Theoretical Background

As demonstrated by previous reviews, including the systematic Cochrane review in this thesis (see 2), mobile-based interventions and ambulatory assessment have hardly been used in resilience intervention research. This also applies to the field of CBM and research on biases in affective information processing (see 1). Due to current gaps regarding trainings to foster a positivity bias (PB) at the level of action tendencies, that have not been focused in resilience studies, the following parts outline the procedures and results of a pilot study evaluating a MB-PBT at the action level, using EMA.

3.1.1 Selective Processing of Information and its Role for Emotion Regulation

The selective processing of affective information is an important determinant of emotional well-being (Clark & Beck, 2010). In line with the long-standing interest in pathogenesis, research focused on biases to negative stimuli (i.e., *negativity biases* [*NB*]), that present a risk factor for the development, maintenance, and recurrence of mental disorders (Hertel & Mathews, 2011; Mathews & MacLeod, 2005). They are assumed as especially relevant for (stress-related) emotional disorders (e.g., depression) that are characterized by emotion regulation (ER) deficits (e.g., Joormann & Gotlib, 2010; Salters-Pedneault et al., 2006). A NB may manifest at different levels of information processing (Mathews & MacLeod, 2005). Cognitive biases to positive, reward-related information (i.e., *positivity biases* [*PB*]), have received little attention. However, the variability between people concerning the processing of positive information is also recognized. Analogous to NBs, a PB describes the tendency to selectively process positive affective information and is manifested at four levels (see Table 3).

Table 3

Levels of Information Processing Relevant for NB and PB

		Negativity bias (NB)	Positivity bias (PB)
a) attention	definition	tendency of attending negative (NB) or positive stimuli (PB) when these compete for attention with neutral or positive (NB)/negative information (PB)	

		Negativity bias (NB)	Positivity bias (PB)
	examples	increased focus on threatening information in anxiety patients (Bar-Haim et al., 2007); attention bias toward negative dysphoric stimuli (Peckham et al., 2010) in depressive patients; longer focus of attention to negative aspects in depressive patients, due to problems in disengagement (Eizenman et al., 2003; Kellough et al., 2008; Sanchez et al., 2017)	attention bias toward positive aspects of a situation (Segerstrom, 2001) in people with higher levels of trait optimism; optimistic individuals, when looking at negative affective pictures of skin cancer, selectively detached their attention to focus on neutral skin areas, possibly to maintain their well-being (Isaacowitz, 2005); natural focus of attention to positive stimuli and away from negative ones (Isaacowitz, 2006a, 2006b) in older adults who are better able to regulate their emotions (Gross et al., 1997)
b) inter-pretation	definition	tendency to interpret ambiguous stimuli as negative (NB) or positive (PB)	
	examples	anxious individuals interpreted ambiguous sentences or social interactions as more threating than controls (Amir et al., 2005; Macleod & Cohen, 1993); dysphoric or depressive individuals evaluated ambiguous information in a more negative way (Lee et al., 2016; Mathews & MacLeod, 2005)	tendency to selectively impose non-threatening interpretations on ambiguous sentences in non-anxious individuals (Hirsch & Mathews, 2000; Macleod & Cohen, 1993)
c) memory	definition	preferential retrieval of negative (NB) or positive information (PB)	
	examples	preferable recall of mood-congruent information (Dalgleish & Watts, 1990; Deldin, Deveney, et al., 2001; Deldin, Keller, et al., 2001; Mathews & MacLeod, 2005) in depressive patients	negative events forgotten more easily by healthy people than events associated with positive affect (Walker et al., 2003)
d) action tendencies	definition	increased or decreased approach and avoidance tendencies to affective stimuli	
	examples	avoidance tendencies for smiling/angry faces in individuals with social phobia (Heuer et al., 2007); deficits in approach tendencies to positive stimuli and avoidance tendencies in depressive disorders (Trew, 2011)	approach motivation[21] to positive stimuli in healthy individuals whereas they tend to avoid negative stimuli (Chen & Bargh, 1999; Elliot, 2006; Elliot & Covington, 2001)

Different models provide an explanation for the origin of *negative* selective information processing and its role for ER and emotional disorders (e.g., depression), for example, Beck' s cognitive model of depression (Beck, 1967; Beck, 1976; Beck, 1987). As a result of early adverse events plus other (e.g., genetic) factors, underlying dysfunctional attitudes or beliefs (cognitive schemas) are developed (Beck, 2008). These are activated by stressor exposure and produce negative biases at

[21] approach motivation: "energization of behavior by, or direction of behavior toward positive stimuli (objects, events, possibilities)" (Elliot, 2006; p. 111); After automatic evaluations of stimuli, a behavioral predisposition (preparedness) for approach/avoidance behaviors is built that may lead to explicit behavior (Korn & Elliot, 2015).

different levels and depressive symptoms (e.g., sadness; Beck, 2008). By the repeated activation of cognitive schemas, negative patterns of affective information processing are consolidated (Beck, 2008), increasing negative thoughts and maintaining depression (Teasdale, 1988). According to Joormann and Vanderlind (2014), NBs can affect a person's attention to emotion-releasing aspects of an event and its appraisal, which, in turn, affect the emotional response. Later, deficits in cognitive control may limit the ability to offset biases (e.g., interpretation), finally leading to a more frequent use of maladaptive (e.g., rumination) and a restricted use of adaptive ER strategies (e.g., reappraisal).

Similarly, a *PB* may also affect an individual's ER (e.g., Wadlinger & Isaacowitz, 2011). PBs at different levels could affect a person's attention to emotion-eliciting aspects and their appraisal, which, in turn, might result in a more positive emotional response. At a later stage, PBs could facilitate the use of adaptive ER strategies (e.g., reappraisal). For example, optimism was shown to be related to the frequent use of cognitive reappraisal (Gross & John, 2003; John & Gross, 2007). Whereas healthy individuals preferably process positive information (e.g., Joormann & Gotlib, 2007; Pool et al., 2016), several studies demonstrated that emotional disorders – in addition to NBs (e.g., Bradley et al., 1997) – also include a missing or reduced PB (Hirsch & Mathews, 2000; Korn et al., 2014; Li et al., 2016; Liang et al., 2011; Moore & Fresco, 2012; Moser et al., 2012). This lack of PB was also found at the level of action tendencies (e.g., Elliot, 2006). Positive stimuli usually trigger approach motivation and behaviors (e.g., Elliot, 2006). For individuals with emotional disorders, however, deficits in this natural tendency were shown. The decreased reward sensitivity in depressive patients (Alloy et al., 2016; Eshel & Roiser, 2010; Pinto-Meza et al., 2006) is associated with reduced approach behaviors toward positive stimuli (e.g., less behaviors to make a reward more likely; Henriques & Davidson, 2000).

3.1.2 Always Look on the Bright Side of Life – Positivity Bias and Resilience

Due to the lack of PB in emotional disorders and potential positive effects for ER, a PB is increasingly discussed as *protective (resilience) factor* (Fox et al., 2009). Schick et al. (2013) found a positive interpretation bias (ambiguous cue-conditioning paradigm) to be negatively associated with rumination. Besides, in a prospective study, a positive interpretation bias predicted resilience as

outcome after a stressor (Kleim et al., 2014). Some studies showed that a training for PB can lead to reduced stress reactivity to and faster recovery of stressors (e.g., Johnson, 2009; Taylor et al., 2011). Resilience-promoting effects of a PB could also be assumed given the association between this bias and resilience-conducive traits, such as optimism. The above findings (see Table 3; Isaacowitz, 2005; Segerstrom, 2001) suggest a positive relationship between a positive attention bias and optimism. Concerning the interpretation of ambiguous information, Gordon et al. (2016) identified optimism as predictor of increased positive and decreased negative interpretations (modified sentence completion task). Relationships between a PB at the level of memory or action tendencies and optimism were hardly examined. For the former, the findings of Devitt and Schacter (2018) might provide evidence.

Optimism (see 2.1.3.2; Appendix K1.3) is one of the resilience factors most often investigated (e.g., Brissette et al., 2002; Kleiman et al., 2017). It includes a positive perspective, but without denying reality (Reivich & Shatté, 2002). Similarly, in a study on associations between optimism and biases in the emotional Stroop task, Segerstrom (2001) found a greater bias for positive stimuli and a smaller bias to negative ones in optimists compared to pessimists. However, both moderately and highly optimistic individuals showed an attentional bias for *both* forms of affective information, with the bias for negative stimuli assumed as being adaptive. Thus, a PB might also not consist of simply ignoring negative information, but of perceiving both, with an increased sensitivity for the positive.

In sum, a PB in information processing might play an important role for individual ER and function as resilience factor. Based on studies that found stronger attention and memory preferences toward positive stimuli in older individuals compared to younger people (Charles et al., 2003; Isaacowitz, 2006a, 2006b; Kennedy et al., 2004; Mather & Carstensen, 2005), a PB also seems to be dynamic over time. Therefore, the targeted modification of information processing toward positive stimuli by training might be a possible approach to foster mental health in the face of stressors (i.e., resilience). Overall, resilience might be fostered by reducing a NB and promoting the level of PB simultaneously. The following sections outline assessment methods for various biases in the field of CBM, introduce CBM training, and present the current state of research on CBM trainings for PB.

3.1.3 Cognitive Bias Modification (CBM) – Assessment and Training

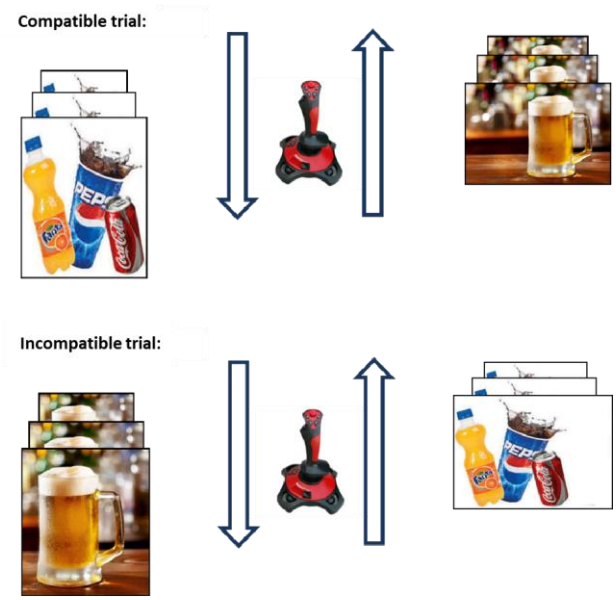
Computer-based CBM paradigms were developed to examine the associations between different biases of information processing (NB, PB) and symptoms of mental disorders (Hertel & Mathews, 2011), with a focus on the assessment of NBs (see Appendix K1.1 for examples). At the level of attention, biases toward affective information have been measured by simultaneously presenting neutral and affective material in filtering (e.g., emotional Stroop task), search paradigms (e.g., visual search), and cueing tasks (e.g., visual probe task). To measure interpretation bias, participants are usually asked to indicate their interpretation of ambiguous stimulus material, such as ambiguous sentences/scenarios (Ambiguous Scenario Test; e.g., Berna et al., 2011). Memory biases are assessed differently, depending on whether explicit (e.g., free recall) or implicit memory (e.g., completion of word stems after reading) is focused (Mitte, 2008).

At the level of action tendencies, the *Approach Avoidance Task (AAT)* is a prominent method to assess implicit (automatic) tendencies and was originally developed for the use in alcoholic patients (Wiers et al., 2009), but also used for other purposes (Seidel et al., 2010). Participants are presented with grey rectangles in different formats that they have to push away (landscape) or pull closer (portrait) as fast as possible (e.g., joystick, computer mouse). The picture size changes dynamically with every movement based on the respective reaction (i.e., pull: increase; push: decrease). This zooming effect produces the subjective feeling of approach or avoidance (Neumann & Strack, 2000). During the assessment, rectangles are replaced by alcohol-relevant (e.g., beer bottle) and neutral stimuli (e.g., soft drinks), that are also presented in two formats (Figure 6) equally often. Participants receive the same instruction resulting in *compatible trials* (i.e., push alcohol-relevant stimuli; pull neutral stimuli) and *incompatible trials* (i.e., pull alcohol-relevant stimuli; push neutral stimuli), and reaction times (RTs) are measured. Besides, a *Compatibility score (CS)* is calculated[22], with negative scores indicating faster reactions (i.e., bias) to alcoholic stimuli (Wiers et al., 2009).

[22] AAT-CS: median RT incompatible trials – median RT compatible trials

Figure 6

Approach Avoidance Task (AAT)



Note. Adapted from Wiers et al. (2009) and taken from Krayer (2019).

Using the same paradigms, CBM at various levels has also been used for interventions to *change* the selective processing of affective information (*CBM training*). Several meta-analyses provided mixed, but satisfactory evidence that CBM training may modify different biases and reduce symptoms of mental disorders (e.g., small to moderate ES for depression; Beard et al., 2012; Cristea et al., 2015; Hakamata et al., 2010; Hallion & Ruscio, 2011; Menne-Lothmann et al., 2014; Mogoaşe et al., 2014). The number of respective studies in meta-analyses (see Appendix K1.2) illustrates that CBM intervention research largely focused on the *modification of NBs* at the level of attention, interpretation, and memory in samples with mental disorders (e.g., Amir et al., 2009).

CBM trainings at the level of action tendencies were examined less frequently. Some studies provided evidence for positive effects of such trainings for different mental disorders (Eberl et al., 2013; Taylor & Amir, 2012; Wiers et al., 2011). Wiers et al. (2010) used a computer-based training version of AAT to modify the approach tendencies of alcoholic patients to alcohol-relevant content. To foster the avoidance of alcohol stimuli, 90% of alcohol content was presented in the landscape format (i.e., pushed), whereas 90% of neutral stimuli were shown as portraits (i.e., pull). The principles of AAT

were also used to modify other psychological symptoms, for example, using dysfunctional and functional statements in individuals with elevated depression scores (e.g., Lukas, 2019).

3.1.4 Current State of Research on CBM as Positivity Training and Limitations

In CBM trainings to foster a PB, especially the levels of attention and interpretation were focused (see Appendix E1 and Appendix K1.4). At the level of *attention* (for details, see Appendix E1), these included the modified visual probe task (Johnson, 2009; Taylor et al., 2011; Wadlinger & Isaacowitz, 2008), eye-tracking (Ferrari et al., 2016), and visual search paradigms (Dandeneau & Baldwin, 2009; Dandeneau et al., 2007), with positive effects shown for the emotional reactivity to (laboratory) stressors, for example. At the level of *interpretations*, CBM-I to foster a PB (e.g., modified Ambiguous Scenario Training) was demonstrated to increase resilience-congruent interpretations and a positive attributional style (AS), as well as to reduce the vulnerability and reactivity to stressors (Beadel et al., 2016; Peters et al., 2011; for details, see Appendix E1). A positive condition to train an attentional PB or benign interpretations has also been included in studies testing CBM-A or CBM-I in individuals with or at risk of mental problems (see Beard et al., 2012; e.g., Dandeneau & Baldwin, 2004; Li et al., 2008; see Menne-Lothmann et al., 2014). The same applies to the level of *action tendencies*, with Vrijsen et al. (2018) and Lukas (2019) showing a decrease of depressive symptom severity in depressive patients using an AAT paradigm with affective stimuli (see Appendix E1).

To date, there seem to be only few studies that evaluated an AAT-based PBT in individuals without a mental disorder (e.g., unselected individuals in Becker et al., 2016; Ferrari et al., 2018; see Appendix E1). For example, using the AAT with diverse emotional (positive vs. negative) pictures from standardized databases, Becker et al. (2016) aimed to enhance a PB and to simultaneously reduce a NB in the processing of affective information (for details of study procedure, see Appendix E1). In the positivity training (PT) group (i.e., pull closer all positive pictures, push negative ones) in *study 1* (unselected individuals), no significant change of action tendencies was found, while a pre-existing PB was reversed in the NT (negativity training) condition (i.e., pull negative pictues, push positive ones). Only in the PT group, a cross-over effect of PBT on attention bias (using trained and untrained pictures

in dot-probe task) was demonstrated, leading the authors to assume a generalization of training effects. For both conditions, no changes in mood scales or affective reactivity to stressor exposure were identified. Based on the non-significant findings for action tendencies in the PT group, a ceiling effect was concluded, as participants in both conditions had shown a PB before the training. In *study 2* (dysphoric and non-dysphoric subjects after negative mood induction), an increase of PB in the PT groups compared to sham training was found in both samples and the PT groups showed lower emotional reactivity in a stress anagram task. In a subsequent RCT (Becker et al., 2019) investigating the positivity-approach training in patients with mental disorders, depressive symptoms decreased.

The previous studies concerning a CBM-PBT, especially at the action level, reflect some limitations that could also explain the lack of significant findings. Becker et al. (2016) and other studies (see Appendix E1) used the AAT to operationalize changes in approach-avoidance tendencies by PBT. To date, this task has not been *validated* to measure a PB at the action level. Therefore, to draw meaningful conclusions about the efficacy of PBT at the level of action tendencies, the validation of the AAT seems needed.

Concerning the *training material*, previous research often focused on disorder-specific stimuli (e.g., social-evaluative words in socially anxious individuals; Taylor et al., 2011). Based on findings in children (Broeren & Lester, 2013), it is known that a PB is not limited to specific content. Given the high comorbidity of anxiety and affective disorders (Moses & Barlow, 2006), the use of diverse positive stimuli (e.g., humans, plants, animals, or objects) and a general PBT, as tested by Becker and colleagues, seems also reasonable. In previous CBM trainings, stimuli were identical for all subjects. However, to increase the personal relevance of interventions, personalized stimuli (e.g., by previous rating) could be used. The composition of training material based on norm values in standardized databases (e.g., International Affective Picture System [IAPS]; e.g., Becker et al., 2016; Ferrari et al., 2018) might have limited the intervention effects. For example, a picture rated as positive in the norm sample possibly had to be pulled closer although the subjects themselves would have assessed the same picture as negative. Furthermore, because several previous studies used the *same stimuli* for the

AAT training and the assessment at pre- and posttest, a generalization of training effects to other stimuli cannot be fully concluded.

Mobile-based CBM paradigms (e.g., Zhang et al., 2018) have not been evaluated to induce a PB. Several mobile apps aim at fostering a PB at the level of attention by using CBM paradigms, such as visual search (e.g., "Mood Mint"; "Happy Tap"). Scientific evaluations of these apps are missing. To date, CBM trainings are largely conducted as computer paradigms in laboratory settings. However, to allow maximum transfer of training effects on the participants' daily life, the use of EMI, that is, mobile technologies to conduct interventions, might be beneficial (Kubiak & Hermanns, 2001; Robbins & Kubiak, 2014). EMIs can be performed repeatedly and closely to the participants' everyday life, meaning that subjects can trigger a training when it is most needed (e.g., after stressor). In recent years, EMI has been used in several areas of health promotion (Kaplan & Stone, 2013). Thus, implementing a microintervention to foster a PB might also benefit from this methodology – especially since EMI was also hardly used in resilience intervention research so far (e.g., Joyce et al., 2018; Leppin et al., 2014; Vanhove et al., 2016). Due to the previous focus on laboratory paradigms, the *ecological validity* of CBM studies is limited. Therefore, to answer the question whether a CBM-based intervention affects the participants' daily life (e.g., coping with stressors), a combination of mobile-based training (EMI) with ambulatory assessment might be adequate.

3.1.5 Explanatory Approaches for the Effects of a PBT on Emotion Regulation and Mental Health

3.1.5.1 Action Tendencies and Information Processing

A bidirectional association between action tendencies and the cognitive processing of affective information is assumed (Ping et al., 2009). On the one hand, there is a compatibility effect between affect and action tendencies, meaning that stimuli with inherent positive or negative valence (e.g., happy vs. angry face) affect our action tendencies (Phaf et al., 2014). Stimuli elicit a valenced automatic evaluation which creates a predisposition for approach and withdrawal responses (Arnold, 1960; Elliot & Covington, 2001). Based on two motivational systems, the processing of positive stimuli might trigger an approach system, thereby fostering the compatible approach behavior to the

stimulus. An avoidance system, however, is probably activated by negative information, increasing the tendency to withdraw from the stimulus (Neumann & Strack, 2000). For example, Chen and Bargh (1999) found that evaluations of positive stimuli facilitated approach motor behaviors, by showing that participants were faster in responding to positive words when pulling a joystick, whereas evaluations of negative stimuli enhanced avoidance behaviors (i.e., faster when pushing the joystick). This compatibility effect has been replicated several times (see Phaf et al., 2014).

On the other hand, behavioral actions associated with approach and avoidance (e.g., arm movements, head nodding vs. head shaking) seem to affect an individual's valence judgements, both of already valenced stimuli and of neutral information (Ping et al., 2009; Trew, 2011). For this work, especially this converse link between approach/avoidance behaviors and information processing is relevant. According to Neumann and Strack (2000), the two motivational systems mentioned above are also activated whenever approach and avoidance behaviors are executed. As arm flexion (i.e., movement toward the body), for example, is closely coupled temporally with acquiring or consuming desired stimuli, the approach system should be activated. Arm flexion (i.e., approach behavior) should result in a positive (pleasant) emotional and an approach motivational orientation. Arm extension (i.e., movement away from body), however, is assumed to be temporally coupled with the onset of aversive stimuli and should activate the avoidance system. This movement (i.e., avoidance behavior) was suggested to be associated with a negative emotional and a withdrawal motivational orientation. Consistently, several studies (e.g., Cacioppo et al., 1993; Neumann & Strack, 2000) demonstrated that performing approach/avoidance movements by arm flexion/extension is associated with stimulus processing and affects the evaluation of respective stimuli (e.g., more positive attitudes to stimuli during arm flexion vs. more negative attitudes during arm extension; details in Appendix E2).

Furthermore, Neumann and Strack (2000) demonstrated the same effects using visual instead of proprioceptive cues, that is, if participants were only provided with the visual impression of moving toward a screen and vice versa. Besides arm movements, approach and avoidance behaviors may include many different behaviors (Neumann & Strack, 2000), such as vertical versus horizontal head

movements in nodding and head shaking (Wells & Petty, 1980). Finally, Woud et al. (2008) found similar results when experimentally inducing an implicit evaluation bias by approach versus avoidance joystick movements. Appendix K1.5 includes further information on approach/avoidance behaviors and neural structures of the motivational systems. Overall, performing approach versus avoidance movements to valenced stimuli during an intervention, that is, a training at the action level, might also affect (higher levels of) information processing of these stimuli, such as attention and interpretation.

3.1.5.2 Training of Action Tendencies, PB, and Emotion Regulation

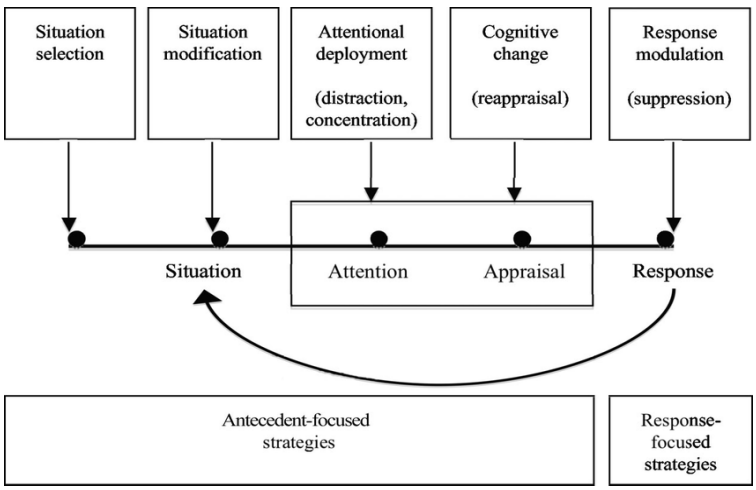
The modified AAT to foster a PB at the action level (e.g., Becker et al., 2016) consists of responding to positive and negative stimuli with approach versus withdrawal behaviors, thereby creating the impression of growing or shrinking pictures (zooming effect). It was developed to promote a PB consisting of approach tendencies (behaviors) to positive and avoidance/distancing tendencies to negative stimuli. Following the evidence concerning the connection between action tendencies and stimulus processing, these action tendencies could then affect other levels of information processing (see 3.1.5.1). For example, by activating their approach system, asking participants to consistently show approach behaviors to positive stimuli could improve the encoding of respective stimuli, foster an approach motivational orientation and a more positive attitude to positive information, as well as enhance their memory for such information. On the other hand – by triggering their avoidance system – instructing individuals to perform withdrawal behaviors to negative stimuli might lead to an improved encoding (e.g., faster categorization) of negative stimuli and an avoidance motivational orientation. Participants could be better prepared to distance from negative information. This would be in line with Becker et al. (2016) who demonstrated a cross-over effect from action tendencies to attentional processes (see 3.1.4). Similar effects of learned action tendencies on other cognitive processes (e.g., interpretation, memory) might be possible.

A PB with respect to action tendencies and other levels of information processing may then have beneficial effects on ER. With respect to the development of emotions, a situation-attention-

appraisal-response sequence was suggested: Beginning with a psychologically relevant situation, the situation is appraised regarding different aspects (e.g., familiarity, valence). Hence, not the situation itself, but an individual's evaluation of a situation generates emotion (Gross, 1999). Subsequently, these appraisals generate emotional responses including changes in several response systems (e.g., behavioral; Gross & Thompson, 2007). According to the "process model of emotion regulation" (Gross, 1998a, 1998b), the most popular ER model, emotions may be regulated at five time points during this emotion generative process: 1) selection of and 2) modification of the situation, 3) deployment of attention, 4) change of cognitions, and 5) modulation of responses. Figure 7 combines both models.

Figure 7

Emotion Generation and Emotion Regulation (Taken From Webb et al., 2012)



A PB at different levels might affect the emotion-generative process and ER in different ways. First, at the level of *attention*, individuals with a PB might focus more quickly on positive aspects in the environment, either from the start or as attentional deployment, to maintain their well-being. According to Wadlinger and Isaacowitz (2008), this would be in line with the selective inattention to negative pictures in optimists (Isaacowitz, 2005). Besides, older individuals, who tend to be better at regulating their emotions (Gross et al., 1997), showed natural attentional tendencies to positive stimuli and away from negative ones (Isaacowitz, 2006a, 2006b). This mood-regulatory function of a PB at the attention level was especially identified when (older) individuals were in bad mood

(Isaacowitz et al., 2008) or tried to resist mood declines (Isaacowitz et al., 2009; Xing & Isaacowitz, 2006). Finally, the essential role of PB for ER is also indicated by a missing PB in people at risk for or with emotional disorders (see 3.1.1).

Second, a PB might affect *appraisal processes.* An event (e.g., stressor) might be appraised more positively from the start, leading to more positive emotional responses. Moreover, PBs might facilitate cognitive change, that is, evaluating the situation to modify its emotional impact, either by changing how one thinks about the situation or about one's capacity to cope with it (Gross, 1999; Gross & Thompson, 2007). *Reappraisal* is an adaptive form of cognitive change (Gross, 1998a, 2002; Gross & John, 2003; Willroth & Hilimire, 2016). Several studies found associations between reappraisal as antecedent-focused strategy (i.e., starts early in emotion-regulative process; Gross, 2002) and positive affect and well-being as well as less negative emotions (e.g., Gross & John, 2003). By targeting the appraisal stage, reappraisal has the potential to address all aspects of emotional responses (Davis et al., 2011). Two types of cognitive reappraisal are differed. *Situation-focused reappraisal* refers to reinterpreting the nature of events or the situational context of affective stimuli (Ochsner et al., 2004; Willroth & Hilimire, 2016). Negative aspects of a stimulus are reinterpreted in neutral or positive terms (Kalisch et al., 2005; Ochsner et al., 2002). *Self-focused reappraisal* includes reinterpreting one's relationship to a stimulus, by "making one feel more or less connected to what is going on" (Ochsner et al., 2004; p. 484; Willroth & Hilimire, 2016). It is also described as (self-) distancing (e.g., Ochsner & Gross, 2008) or detachment (Kalisch et al., 2005). Both forms were shown to be adaptive (e.g., Ayduk & Kross, 2008; Hermann et al., 2017; Kross & Ayduk, 2008; Willroth & Hilimire, 2016).

Individuals with a PB might benefit concerning *both types of reappraisal*. First, participants who – as part of PB – have (learned) an approach tendency (motivation and behavior) to positive stimuli, perceive positive information faster and/or tend to interpret situations more positively, might have an improved situation-focused reappraisal. As they let oneself get affected more easily by positive aspects at cognitive and behavior levels, they could be more likely to find something positive in ambiguous or even negative situations, and experience less negative or more positive emotions.

Second, as individuals with PB have (learned) tendencies away from negative stimuli at different levels, they could be better able to distance from negative aspects to reduce the emotional impact (compare self-focused reappraisal). This is in line with the evidence for an association between trait optimism and reappraisal in view of stressors (e.g., Sears et al., 2003; Slattery et al., 2017) and a more frequent use of reappraisal in optimistic individuals (e.g., Gross & John, 2003; John & Gross, 2007).

Besides, a training with the modified AAT per se, that is, performing approach versus distancing movements to affective stimuli and the associated zooming effect, might immediately affect ER, especially situation modification and reappraisal. *Situation modification* involves changing an affective stimulus or its context to modify the emotion-eliciting impact. For example, the emotional response to affective stimuli was shown to be affected by the physical distance from stimuli (Davis et al., 2011; Williams & Bargh, 2008). The illusion of approaching unpleasant pictures increased both the negative valence and the arousal caused by the pictures compared to negative stimuli that were static or seeded to recede (Mühlberger et al., 2008). However, greater positivity was found when neutral pictures were imagined moving toward participants and growing (Davis et al., 2011). Similarly, the growing effect for positive pictures in the modified AAT might also result in increased positive emotional responses to these pictures. The shrinking effect for negative pictures might lead to a less negative emotional response. In addition to potential effects of a PB that, in turn, fosters reappraisal, both forms of reappraisal could also be directly enhanced by the movements in the modified AAT. The approach movements to positive pictures per se might facilitate situation-focused reappraisal: As individuals proverbially learn to “let positive things closer to them”, they might more easily retrieve something positive from negative situations. Simultaneously, distancing movements from negative pictures per se might increase self-focused reappraisal because individuals learn to distance from negative stimuli. This metaphorical detachment (“move away from the negative and don’t get affected by it”), as part of self-focused reappraisal, might also be transferred to real-life stressors.

3.1.5.3 Diathesis-Stress Model

The diathesis-stress model (see 2.1.3.1) postulates mental disorders as result of an interaction between predispositional vulnerabilities and the exposure to stressors. Modifying variables, such as resilience factors, can weaken potential pathological effects, resulting in the maintenance or fast recovery of mental health (i.e., resilience). A PB at the level of action tendencies, which generalizes to other cognitive levels (see 3.1.5.1 and 3.1.5.2), might also work as resilience factor (e.g., Fox et al., 2009) and hence moderate (i.e., reduce) the impact of vulnerabilities and stressors on mental health. People with a pronounced PB might be less vulnerable to stress-related mental disorders. Thus, an intervention fostering such a bias might also contribute to an individual's resilience.

3.1.5.4 Positive Appraisal Style Theory of Resilience (PASTOR)

According to PASTOR (see 2.1.3.4), the effects of resilience factors on resilience as outcome are mediated by a limited number of resilience mechanism (see Figure 3). For example, the resilience factor optimism could result in a positive (re-)appraisal of negative events, making an individual less vulnerable for stress-related mental disorders (Kunzler, Gilan, et al., 2018). Similarly, a PB at the action level, which generalizes to other cognitive levels (see 3.1.5.1 and 3.1.5.2) and is possibly related to optimism (see 3.1.2), might also help to maintain or regain mental health in the face of stressors due to the mediation by positive (re-)appraisal style (compare 2.1.3.4). Thus, individuals with an increased PB (e.g., by training) might be more resilient than those with a smaller or no such bias.

3.1.6 What do we Know About PBT and the AAT-CS? Findings From Previous Research

Effects of PBT on perceived stress and perceived microstressor severity. Concerning potential effects of CBM trainings on perceived stress, there is evidence for a reduction of perceived stress after a PBT at the level of attention and interpretation (Clarke, 2016; Dandeneau et al., 2007; for details, see Appendix E3). Besides, several studies demonstrated that the modification of biases in information processing to foster a PB can also have a positive impact on the emotional reactivity (e.g., less frustration) to (laboratory) stressors (e.g., attention: Johnson, 2009; Taylor et al., 2011; interpretation:

Peters et al., 2011). In contrast, at the level of action tendencies, Becker et al. (2016) (see 3.1.4) found no effects on the emotional vulnerability to stress (i.e., subjective stress and mood in response to anagram task) in unselected individuals. However, the study had some limitations (see 3.1.4), that might have restricted the effects. Moreover, based on the generally low stress scores, the authors suggested that the stress task might not have been adequate for inducing sufficiently high stress.

Effects of PBT on implicit action tendencies. There is evidence for the modifiability of action tendencies regarding affective and other stimuli from previous studies on CBM (e.g., Lukas, 2019; Rinck et al., 2013; Vrijsen et al., 2018; Wiers et al., 2009; for details, see Appendix E3). While Becker et al. (2016) found no effects of a PT in unselected individuals (study 1), they demonstrated an enhanced PB at the level of action tendencies[23] after a PT in samples of dysphoric (Becker et al., 2016; study 2) and depressed individuals (Becker et al., 2019). These results, together with the moderation of effects by initial depressive symptoms found in Becker et al. (2019), suggest that intervention effects might be larger in participants with a pre-existing NB or without a PB. To date, there is no validated measure of biases at the level of action tendencies. Thus, the participants' AS could serve as a proxy measure. It is part of optimism (see Appendix K1.3), that is probably related to a PB (see 3.1.2), but more dynamic than dispositional optimism.

Effects of PBT on resilience, ability to recover from stress, and well-being. A PB is probably related to optimism and is discussed as resilience factor itself (see 3.1.2). As mentioned above (3.1.5), a PB at the level of action tendencies could reduce or "buffer" negative effects of adversities, resulting in maintained or regained mental health despite stressors (i.e., resilience). Previous CBM studies primarily measured changes in different biases and/or mental health (e.g., Beard et al., 2012; Hallion & Ruscio, 2011; Menne-Lothmann et al., 2014; Mogoaşe et al., 2014). Even CBM trials directly focusing on resilience (e.g., Beadel et al., 2016; Peters et al., 2011) did not always measure it as an outcome or used "resilience scales" assessing a trait or the availability of putative resilience factors (Chmitorz, Kunzler, et al., 2018). If mental health was analyzed, the participants' stressor exposure was not

[23] shorter RTs in compatible trials (i.e., pull positive, push negative) compared with incompatible trials (i.e., push positive, pull negative); AAT-CS increased or became more positive, indicating a PB

controlled for. In addition, subjective well-being was rarely considered as outcome. Although several CBM trials tested intervention effects on the reactivity to stress inductions, the *recovery* from stressors has also rarely been examined (e.g., Rinck et al., 2013; for details, see Appendix E3). No previous CBM trial assessed the participants' (subjective) ability to recover from stress.

Effects of PBT on psychological EMA outcomes. EMA refers to one type of ambulatory assessment (Fahrenberg et al., 2007; Kubiak & Stone, 2012). In the form of *Experience Sampling*, subjects provide details about the occurrence of behaviors, experiences, and mood (e.g., using smartphones). By the repeated in-the-moment assessment under everyday conditions (in situ) in the same individual (Kaplan & Stone, 2013; Shiffman et al., 2008), EMA allows to generate longitudinal data (Bolger & Laurenceau, 2013). The method ensures a high level of ecological validity, reduces retrospective bias (Kubiak & Stone, 2012), and increases the reliability of assessments (e.g., Csikszentmihalyi & Larson, 1987). Thus, it is considered as gold standard to measure subjective outcomes (Heron & Smyth, 2010; Kaplan & Stone, 2013). To date, only few studies in resilience intervention research used EMA to assess the effects of training (e.g., Geschwind et al., 2011; McCraty & Atkinson, 2012). With respect to CBM research, most trials investigating the efficacy of CBM trainings also relied on self-report or laboratory paradigms (Beard et al., 2012; Cristea et al., 2015; Hakamata et al., 2010; Hallion & Ruscio, 2011; Menne-Lothmann et al., 2014; Mogoaşe et al., 2014). These measures were also mostly used by studies using mobile-based CBM interventions (e.g., Zhang et al., 2018), with few exceptions, such as Enock et al. (2014). As a consequence, it is largely unclear whether CBM trainings do also affect the participants' daily life, resulting in limited ecological validity.

Effects of PBT on cognitive ER (reappraisal) and emotional experience. It was suggested that approach and avoidance behaviors affect an individual's motivational orientation and the processing of affective stimuli (Cacioppo et al., 1993; Neumann & Strack, 2000; see 3.1.5.1). Therefore, a PBT at the level of action tendencies might also affect information processing (i.e., PB) at higher levels (e.g., attention, interpretation), which might then have a positive impact on (cognitive) ER. Individuals, who constantly perform approach movements to positive stimuli, could acquire an approach tendency

(e.g., motivation, behavior) to positive information and learn to get affected more easily by positive aspects. As a consequence, situation-focused reappraisal (see 3.1.5.2) could be enhanced. So far, withdrawal movements to negative stimuli were typically associated with avoidance and negative information processing. At the level of ER, however, self-focused appraisal is viewed as adaptive (see 3.1.5.2). The question if withdrawal behaviors are also associated with the dissociation from negative information and increased self-focused reappraisal, has not yet been examined. By performing distancing behaviors to negative stimuli during PBT, individuals could learn to reduce the personal relevance of these stimuli and to get affected less. Finally, based on potential effects of an action-level PBT for cognitive reappraisal, this training might also affect an individual's emotional experience. Reappraisal was shown to be associated with more positive and less negative emotions (e.g., Gross & John, 2003). Trials testing a CBM training with a positive condition also demonstrated positive effects on emotional experience, such as anger (Almoghrabi et al., 2017; Hawkins & Cougle, 2013) or general affect (Ferrari et al., 2016; Peters et al., 2011; Rinck et al., 2013; Taylor et al., 2011). Becker et al. (2016) partly found no effects of an AAT PB-training on mood, possibly due to study limitations (3.1.4).

Construct validity of the AAT as a measure of PB at the level of action tendencies. Despite its previous use (Becker et al., 2016; Becker et al., 2019), the modified AAT paradigm with affective stimuli has not yet been validated as a measure of PB at the action level. It is unclear whether the AAT-CS can depict an individual's tendency toward positive stimuli and how this score is related to other indicators of PB, such as optimism or the hereby associated positive AS (Seligman, 1990). People with high optimism tend to preferably process positive information at different levels (Devitt & Schacter, 2018; Gordon et al., 2016; Isaacowitz, 2005; Segerstrom, 2001). Some authors (e.g., Chang et al., 2009; Gibson & Sanbonmatsu, 2004) also concluded an increased motivation of optimists to approach positive stimuli than to avoid negative ones. Such motivational orientations could in turn be associated with approach and avoidance *behaviors* (Neumann & Strack, 2000). Given these findings, individuals with high trait optimism and a positive AS[24] could also show an increased tendency to

[24] positive events attributed to internal, global, and stable causes; negative events to external, specific, and variable causes

positive stimuli at the action level (i.e., increased AAT-CS). Optimism as resilience factor was shown to be positively associated with the ability to recover from stress and negatively with perceived stress (Kunzler, Chmitorz, et al., 2018). Similarly, a PB was suggested as resilience factor protecting from the development of emotional disorders (e.g., Fox et al., 2009; Thoern, 2016). Therefore, if the AAT presents a measure of PB at the level of action tendencies, there might also be associations between the AAT-CS on the one hand and the ability to recover from stress and perceived stress on the other.

3.1.7 Research Questions and Hypotheses

As outlined above, trainings to foster a PB suffer from methodological weaknesses, preventing robust conclusions concerning the modifiability of bias. The second part of this thesis aims to address these research gaps. Using a newly developed smartphone app, an EMI to foster a PB at the level of action tendencies is evaluated. In addition to training feasibility, the effects on outcomes, such as perceived stress, mental health, and action tendencies, including EMA measures, are examined. The validity of the AAT as a measure of PB is also explored, resulting in six RQs.

Given the findings mentioned above (see 3.1.6), this study tests the hypothesis of a decrease of perceived stress and explores a reduction of the perceived microstressor severity through the MB-PBT. To control for the sole impact of stressor monitoring during the training period, the effects are compared with a historical CG that only participated in EMA- and online-based stressor monitoring.

RQ1: Which effects does a MB-PBT have on perceived stress and perceived microstressor severity?

Hypothesis 1.1: Controlled for individual stressor exposure and the baseline AS, a MB-PBT at the action level decreases perceived stress from pre- to posttest within the IG.

Exploratory RQ 1.2: Compared to a historical CG (stressor monitoring), does a MB-PBT at the action level decrease the perceived microstressor severity at posttest?

Following from previous research (see 3.1.6), a PBT at the action level might result in larger approach tendencies to positive stimuli and increased withdrawal/distancing tendencies to negative stimuli. Thus, this study investigated the hypothesis that the intervention fosters a PB at the level of action tendencies and assumed a moderating role of the baseline AS.

RQ2: Which effects does a MB-PBT have on implicit action tendencies to affective stimuli?

Hypothesis 2.1: Within the IG, a MB-PBT at the action level results in a PB at the level of action tendencies from pre-to posttest, that is, the participants' AAT-CS increases.

Hypothesis 2.2: Within the IG, the baseline AS moderates the effects of training on implicit action tendencies, that is, the increase in the AAT-CS is larger for individuals with a negative AS at pretest.

Due to current research gaps (see 3.1.6), this study also explored the impact of a PBT using the AAT on resilience (Kalisch et al., 2015), the ability to recover from stress (i.e., a proxy measure of resilience; Kunzler, Chmitorz, et al., 2018), and well-being.

RQ3 (exploratory): Within the IG and controlled for individual stressor exposure and the baseline AS, does a MB-PBT at the action level have any effects on (3.1) resilience (change in mental health controlled for stressor load), (3.2) the ability to recover from stress, or (3.3) well-being?

Given the lack of research (see 3.1.6), the effects of a MB-PBT on several EMA-measured psychological outcomes during the three-week intervention were examined in an exploratory RQ.

RQ4 (exploratory): Within the IG, does a MB-PBT at the action level have any effects on the EMA-measured (4.1) current mood, (4.2) well-being, (4.3) the ability to distance from negative stimuli, or (4.4) the ability to get affected by positive stimuli?

As shown above (see 3.1.6), the effects of PBT at the level of action tendencies on cognitive ER and emotions have hardly been examined, leading to the following exploratory RQ.

RQ5 (exploratory): Within the IG and controlled for individual stressor exposure and the baseline AS, does a MB-PBT at the action level have any effects on (5.1) reappraisal, (5.2) positive affect, (5.3) negative affect, (5.4) state anger, or (5.5) anger expression (anger in, anger out, anger control)?

The current study aimed to explore the construct validity of the AAT as a measure of PB by investigating the bivariate correlations between the AAT-CS and the above variables for the first time.

RQ6 (exploratory): Within the IG at pretest, are there any bivariate associations between the AAT-CS and (6.1) trait optimism, (6.2) a positive AS (indicated by total score of attributions for positive and negative events), (6.3) the ability to recover from stress, or (6.4) perceived stress?

3.2 Methods

3.2.1 Study Design

To test the feasibility of the PBT, a single-group pre-post-design was primarily used, involving a three-week intervention, the pre- and postassessment and (weekly) EMA for the monitoring of microstressors during the training. Based on similar outcomes assessed, a matched CG design was used for one RQ. Participants of the TRAIN4Positivity study, who all received the intervention, were matched with a historical CG from the LifeStress project to control for the effects of stressor monitoring. In this 4-week trial, individuals aged 18–30 years (*N* = 70) had been examined using EMA for microstressors and the resulting perceived severity, without any intervention (see 3.2.5).

3.2.2 Sample

3.2.2.1 Recruitment

Participants of TRAIN4Positivity were recruited from students of various fields at the Johannes Gutenberg University (JGU) Mainz, Germany. Recruitment materials included 1) postings at notice boards of the JGU campus (e.g., library, Department of Psychology) and of the University Medical Center (UMC) of the JGU Mainz (e.g., library, cafeteria), 2) a recruitment email by the mailing list of the student body (Fachschaft) of the Department of Psychology at JGU, and 3) a posting in the Facebook group of JGU freshmen (winter term 2018/2019) (Appendix F1-Appendix F3). If potential participants expressed their interest in the study (e.g., by email or telephone), they were sent a *study brochure* by email (Appendix F4), which contained more details about study aims and procedures. In case individuals were still interested in the study after reading this brochure, they received an *informatory telephone call*. During the 10-minute call, a member of the study team outlined the study course in more detail and explained the inclusion and exclusion criteria. The students were informed that they could possibly not be considered if inclusion criteria were not fulfilled. During the call, an appointment for the 30-minute screening and eventually the subsequent pretest was agreed upon.

The personal *screening appointment* took place in a laboratory at the Department of Psychiatry and Psychotherapy of the UMC Mainz. Potential participants received all relevant

information about the study, in oral and written form, and provided a *written informed consent* (Appendix F6). Subsequently, the eligibility criteria (see 3.2.2.2) were tested using paper-pencil questionnaires (Appendix K2.1). Besides sociodemographic data, this screening included a medical history sheet, the assessment of mental health using the General Health Questionnaire-28 (GHQ-28; see 3.2.7.3), and the Mainz Inventory of Microstressors (MIMIS; see 3.2.7.1) for stressor exposure.

3.2.2.2 Inclusion and Exclusion Criteria

Important inclusion criteria of TRAIN4Positivity were 1) aged 18–30 years and 2) an increased stressor exposure to microstressors (sum score of microstressors ≥ 30 in MIMIS). Essential exclusion criteria involved 1) the previous/current diagnosis of a mental disorder (according to self-report and GHQ-28 screening questionnaire), 2) a previous/current psychotherapeutic or psychiatric treatment (according to self-report), and 3) the previous participation in an intervention to foster resilience or mental health during the past 3 months. A detailed overview of the inclusion and exclusion criteria, along with the eligibility criteria of LifeStress is provided in Appendix G1.

3.2.2.3 Sample Size Calculation

The required sample size was calculated using G*Power (Faul et al., 2007) for a two-tailed paired *t* test. A significance level of $\alpha = .05$ and a power of $\beta = .80$ were chosen. The ES was indicated based on Becker et al. (2016) who reported an effect of positivity training (AAT laboratory training) on the AAT-CS (paired *t* test) of $d = 0.62$ (study 2, non-dysphoric individuals). The sample size calculation resulted in a sample size of $N = 23$ (Appendix I1). Based on the inclusion criteria (mentally healthy, increased stressor exposure) and potential dropouts, more participants ($N = 56$) were recruited.

3.2.3 Study Course

After the recruitment and screening process, the study can be divided into a pre- and posttest assessment session and a three-week intervention period with EMA. If the subjects met the eligibility criteria of TRAIN4Positivity (see 3.2.2.2), the pretest was conducted immediately after the screening.

During the 1- to 1.5-hour *pretest assessment* (also in laboratory at UMC), participants were familiarized with the study procedures. They received a study smartphone (Motorola Moto G, third generation) and were informed about how to use the devices. Specifically, the aims and the functioning of the mobile application (app) "Breezly" for the PBT (see 3.2.4) and of the app "movisensXS" for the stressor monitoring (EMA) during the three-week study period (i.e., items and answer formats) were demonstrated. Participants were informed about the opportunity to contact the study team in case of any technical problems (email or study mobile phone). Based on contact data (initials), an individual code of *pseudonymization* was allocated to each participant (Appendix F7). Subsequently, the *pretest paper-pencil questionnaires* (Appendix K2.2) were distributed. These included the assessment of further sociodemographic (e.g., socioeconomic status) and psychological data (e.g., well-being) (see 3.2.7). To measure the participants' PB at the level of action tendencies, the *AAT mouse version* was performed on a study laptop.

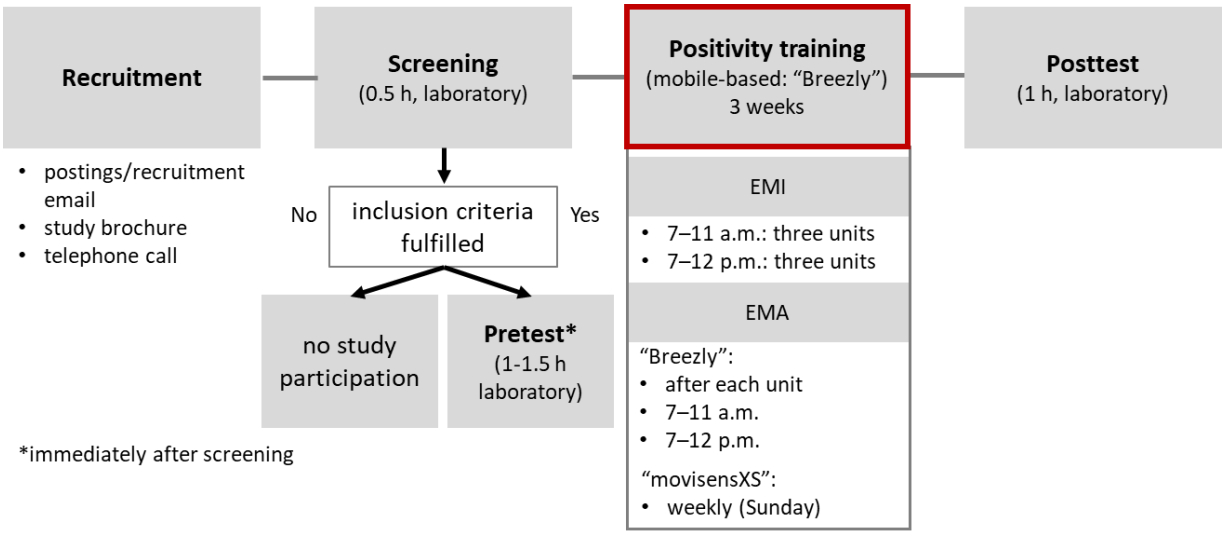
After the pretest assessment, the *MB-PBT* at the level of action tendencies was performed for three weeks using the newly developed "Breezly" app, that was installed on the study smartphones (see 3.2.4). During the training, participants were monitored with two components of *EMA*. First, within "Breezly", the subjects were asked to rate their current mood at the end of each training session. Besides, they answered different items, for example, concerning their current well-being at each training day (see 3.2.7). Second, an ambulatory assessment of microstressors was performed using the app "movisensXS", which measured the occurrence of and the perceived severity of occurred microstressors at the end of each week during the 3-week period (see 3.2.7).

At the end of the training, participants were invited to the 1-hour *posttest* at the Department of Psychiatry and Psychotherapy. With few exceptions (see Appendix G4), participants answered the same questionnaires (see Appendix K2.3) as for the pretest (including AAT paradigm) and returned the study smartphones. Participants indicated their choice of study reimbursement (see Appendix F10). In addition, they were asked for their qualitative feedback concerning the study procedures, especially the MB-PBT. All procedures of the study, including the functionality of the two apps ("Breezly",

"movisensXS" for EMA) and the assessments, were pretested in a sample of *n* = 4 participants. Figure 8 provides an overview of the study course.

Figure 8

Study Course



Note. EMA: Ecological Momentary Assessment; EMI = Ecological Momentary Intervention.

3.2.4 Mobile-Based Positivity Training

Based on the principles of the AAT paradigm (e.g., Becker et al., 2016), the mobile-based intervention "Breezly" aimed to induce a PB at the level of action tendencies. Using a picture training with affective stimuli, positive pictures had to be "swiped or pulled closer" (i.e., to lower edge of display), whereas negative pictures had to be "swiped or pushed away" (i.e., to top of smartphone screen). To generate the typical zooming effect of the AAT and to produce a subjective feeling of approach or avoidance (see 3.1.3, 3.1.4), the picture size changed dynamically with every movement, with positive pictures growing in size, while the size of negative images was reduced by the upward movement.

Before starting with the training, participants were required to rate *N* = 170 *pre-selected* pictures (see Appendix H1 and Appendix H2) on their smartphone device with regard to emotional valence (positive vs. negative), the triggered (e.g., emotional, physical) arousal, and a picture's personal relevance on a 9-level scale (analogous to Self-Assessment Manikin [SAM]; Bradley & Lang,

1994). Sliders for the ratings had variable starting points to prevent a systematic impact on participants. Depending on the individual valence and arousal ratings, an algorithm within "Breezly" allocated a personal training set of *n* = 50 pictures (25 positive, 25 negative) to each participant, considering the following criteria: 1) positive: arousal ≤ 8 and valence ≥ 6 in the SAM, 2) negative: arousal ≤ 8 and valence ≤ 4 in the SAM. After the picture rating, participants chose a personal avatar leading them through the training (Appendix H3).

In each 3-minute *training session* (see Appendix H4), *n* = 10 pictures from the available training set were randomly presented. Participants were instructed to attend a fixation cross in the center of the screen before this was replaced by a positive or negative picture (all pictures in same format). Depending on the (emotional) valence of each picture, the above-described swipe movement had to be performed resulting in the zooming effect. For each picture, the participants' RT (i.e., time between presentation of picture and first finger touch on screen) was assessed. At the end of each session, the current mood was measured using EMA (see 3.2.7.3; Appendix H4).

Based on the appearance (e.g., mood) of the avatars, "Breezly" also included a 7-level *rewarding system.* Starting with an avatar at the lower level (e.g., serious face expression, business outfit), the participants received points depending on the frequency of training sessions performed. With each further level achieved, the avatar seemed happier and wore more casual clothes (Appendix H5). To indicate the personal training progress, the avatar, the participants' score, and the number of training days were presented on the *home screen* (Appendix H6). Besides, the participants were congratulated on the number of completed sessions after each training (i.e., picture training and mood assessment). With respect to *training intensity* during the 3-week period, participants were asked to initiate and complete three training sessions (of 3 minutes each) during two time slots (morning: 7–11 a.m.; evening: 7–12 p.m.) each day, respectively (i.e., six recommended training sessions of 18 minutes in total per day). This recommendation was made to help participants to start the day with increased positive awareness and to assess experiences of the past day more positively. The theoretical basis and details of the rewarding system are provided in Appendix H5.

At the start and end of each training day ("*Tagesstart/Tagesabschluss"*), participants were invited to answer several items concerning psychological variables (see Appendix H7), including current well-being, sleep restfulness, vigor for the upcoming day, the ability to distance from negative stimuli, and to get affected by positive ones. Participants could *keep track* of their history of these measures (Appendix H7). "Breezly" also included a *reminder function* (e.g., for training sessions; Appendix H8). Finally, participants could read *additional information about the app* (Appendix H6).

3.2.5 Historical Control Group (LifeStress Study)

Participants in the LifeStress study (September 2016–March 2017) completed a 4-week EMA assessment (EMA version of MIMIS; see 3.2.7.1). Each day, participants reported the occurrence of microstressors since the last measurement, the perceived severity of occurred stressors, as well as their current affect at five random times between 9 a.m. and 8 p.m. using study smartphones. In addition, they completed retrospective assessments of microstressors using an online version of the MIMIS at the end of each day and of each week (Chmitorz, Kurth, et al., 2020). The main study of LifeStress (N = 70) was divided into several phases, including the recruitment and screening, the pretest in the laboratory, the stressor monitoring phase (EMA, retrospective assessments; 28 days), and the posttest assessment (laboratory). Details are provided in Appendix F11. Overall, the longitudinal study served to examine the ecological validity of the MIMIS by comparing the microstressors and perceived severity in this retrospective measure with EMA-assessed stressor data (Chmitorz, Kurth, et al., 2020). Participants of TRAIN4Positivity and LifeStress were comparable as they both completed end-of-week assessments concerning stressors.

3.2.6 Ethical Aspects

The study procedures corresponded to legal terms and ethical standards for conducting scientific studies and followed the Declaration of Helsinki (World Medical Association, 2013). Potential risks and burden for participants were minimized as far as possible. Only adults were admitted, whose mental and physical health had been thoroughly checked before study participation. For the MB-PBT,

only stimuli with arousal values ≤ 8 (see Appendix H1) and adequate motives were selected. Potential adverse events of the intervention, assessed at posttest, were compensated by the study team, by discussing them with the participants, noting their comments (see Appendix J11), and providing emotional support (if needed). The confidentiality of data was ensured and participants were only included after they had received detailed information about the study (e.g., procedures, voluntary nature, potential risks, data processing) and gave their written consent. All documents were formulated in an easy-to-understand language and according to the EU General Data Protection Regulation (GDPR). Further details are shown in Appendix F12-F13, including the study approval by the local Ethics Committee at the Rhineland-Palatine state chamber of physicians (No. 2018-13043-KliFo).

3.2.7 Measures

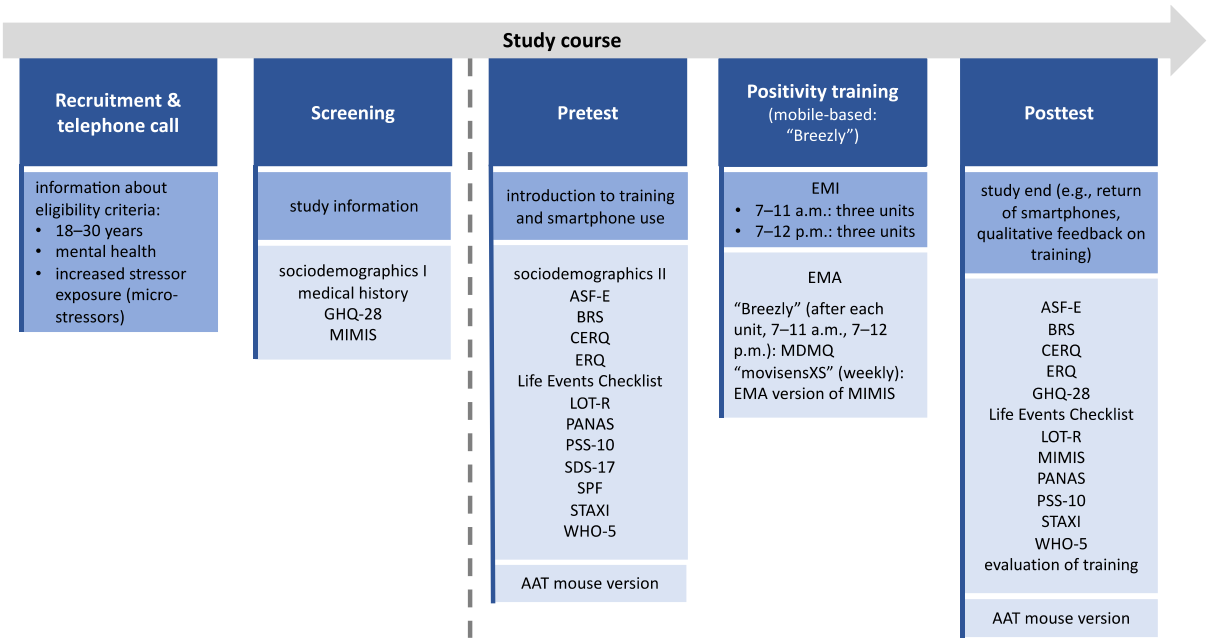
The following part presents the primary and secondary study outcomes and the corresponding scales. Except for sociodemographic, clinical and lifestyle data, empathy, and social desirability (no outcome measures), all variables from the screening and the pretest were also used at posttest. Dispositional optimism and the participants' AS were primarily investigated to verify the validity of the AAT as a measure of PB. Figure 9 gives an overview of the measures used. For all questionnaires, the current German adaptations were administered. The psychometric quality of the scales, example items, the recoding of items, and the calculation of total scores are presented in detail in Appendix G5. The reliabilities for all measures in this study are presented in Appendix G6.

3.2.7.1 Baseline Variables and Covariates

Sociodemographic, clinical, and lifestyle characteristics. Single items were used to assess sociodemographic, clinical, and lifestyle data at screening and pretest (Appendix G3). At screening, variables included, for example, age, gender, the participants' health status, alcohol and drug consumption, digital habits, and the previous participation in trainings to promote resilience or mental health. The pretest items focused on topics such as the participants' social situation and employment.

Figure 9

Study Assessments



Note. AAT = Approach Avoidance Task; ASF-E = Attributionsstilfragebogen für Erwachsene; BRS = Brief Resilience Scale; CERQ = Cognitive Emotion Regulation Questionnaire; EMA = Ecological Momentary Assessment; EMI = Ecological Momentary Intervention; ERQ = Emotion Regulation Questionnaire; GHQ-28 = General Health Questionnaire-28; LOT-R = Life Orientation Test-Revised; MDMQ = Multidimensional Mood Questionnaire; MIMIS = Mainz Inventory of Microstressors; PANAS = Positive and Negative Affect Schedule; PSS-10 = Perceived Stress Scale-10; SDS-17 = Social Desirability Scale-17; SPF = Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie; STAXI = State-Trait Anger Expression Inventory; WHO-5 = Well-being Index.

Life Orientation Test-Revised (LOT-R). To assess dispositional optimism, the German version of the LOT-R (Glaesmer et al., 2012; Scheier et al., 1994) was used. The two-dimensional self-rating scale consists of 10 items that are answered on a 5-point Likert scale (0 = *strongly disagree* to 4 = *strongly agree*). After recoding the items for pessimism, a total score (range: 0–24) was calculated, with higher optimism indicated by higher LOT-R scores.

Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie (SPF/IRI-S D). The participants' RT to positive and negative stimuli in the MB-PBT (e.g., crying child) could be influenced by their level of empathy. Thus, with an aim to consider this variable as covariate (see 3.2.8.2), the pretest included the SPF (Paulus, 2009, 2016). The self-report inventory includes 16 items describing

(generalized) human characteristics or reactions associated to emotions. On a 5-point Likert scale (1 = *never* to 5 = *always*), participants indicate to what extent the respective statement applies to them or not. A higher level of empathy is indicated by a higher sum score (range: 16–80).

Social Desirability Scale-17 (SDS-17). To control for potential social desirability of participants concerning their action tendencies to affective pictures, the SDS-17 (Stöber, 1999) was used at pretest. Using a dichotomous format (0 = *false*; 1 = *true*), participants indicate if they agree or disagree with 17 statements. Higher scores (range 0–1) indicate higher social desirability.

Attributionsstilfragebogen für Erwachsene (ASF-E). The German adaptation (ASF-E; Poppe et al., 2005) of the Attributional Style Questionnaire (Peterson et al., 1982) was used to assess inter-individual differences in the AS for positive and negative events. Based on 16 given situations (eight positive, eight negative), participants were asked to indicate the major cause they feel about a situation as if it happened to themselves using an open-ended format. Subsequently, they were asked to rate this major cause along three dimensions (internal–external, stable–variable, and global–specific) based on six items on a 7-point bipolar scale with varying anchors. Higher scores in two total scores of AS (range: 48–336 for each score) indicate internal, stable, and global attributions for either positive or negative events. Based on both total scores, a positive AS is indicated by a high total score for positive events (ASF-E-P) and a low total score for negative events (ASF-E-N).

Life Events (LE) Checklist. To determine the participants' stressor exposure, including macrostressors, the LE Checklist was used. Based on Caspi et al. (1996) and Canli et al. (2006), the items of this scale were modified and extended at the Leibniz Institute for Resilience Research (Chmitorz, Neumann, et al., 2020). It includes 27 critical LE that can be positive (e.g., marriage) or negative (e.g., job loss). For each stressor, participants indicate whether this stressor occurred and their respective age at this event (maximum of five age indications). For each occurrence, participants also rate the perceived stressor severity on a 5-point Likert scale (0 = *not at all severe* to 4 = *very severe*; not relevant for this study). At pretest, the LE Checklist in this study referred to the previous lifespan. At posttest, only the past 3 weeks were assessed, with no possibility of multiple answers.

Higher scores indicate more occurred macrostressors (pretest: range: 0–135; posttest: range: 0–27).

Mainz Inventory of Microstressors (MIMIS). The MIMIS (Chmitorz, Kurth, et al., 2020) was used for two reasons: First, to assess the number of microstressors at pre- and posttest as another component of the participants' stressor exposure (covariate). Second, to measure the perceived microstressor severity as primary study outcome (see 3.2.7.2). For 58 daily hassles (e.g., noise, traffic jam, disagreements), participants indicate if the respective stressor occurred during the past week using a 7-point scale (1 = *one day* to 7 = *seven days*). The rating of perceived microstressor severity is described in 3.2.7.2. The number of microstressors is determined by summing the number of days on which stressors occurred (range: 0–406).

EMA version of MIMIS. To measure the participants' exposure to microstressors during the 3-week training, the EMA version of the MIMIS was administered every Sunday. The scale was delivered through the mobile app "movisensXS" and had been developed and validated as part of the LifeStress study (see 3.2.5). In accordance with the above-described paper-pencil questionnaire, the EMA version measures the occurrence of 58 microstressors during the past week in a way that participants can select between 58 stressors and that multiple answers are possible. For each selected stressor, participants are also asked to rate the perceived severity (see 3.2.7.2). Every Sunday at 11 a.m. and 2 p.m., participants were reminded by "movisensXS" to complete this end-of-week assessment.

3.2.7.2 Primary Outcome Measures

The present study investigated two primary outcomes: perceived stress and the participants' perceived microstressor severity assessed using the MIMIS.

Perceived Stress Scale-10 (PSS-10). The PSS-10 (Cohen et al., 1983; Klein et al., 2016) measures the subjective perception of stress, that is, the extent to which situations in the participants' life were perceived as stressful in the past month. The self-report inventory consists of 10 items measuring perceived stress on a 5-point Likert scale (0 = *never* to 4 = *very often*). Higher scores (range: 0–40) indicate higher levels of perceived stress.

MIMIS (paper-pencil). The participants' perceived microstressor severity was also measured

using the MIMIS (see 3.2.7.1; Chmitorz, Kurth, et al., 2020). For this study, only the perceived severity at *posttest* was considered. For each microstressor, that participants indicated to have occurred in the past week, they were asked to rate the severity on a 5-point Likert scale (0 = *not at all severe* to 4 = *very severe*). Higher scores indicate a higher microstressor severity (range: 0–232).

3.2.7.3 Secondary Outcome Measures

General Health Questionnaire-28 (GHQ-28). The German GHQ-28 (Goldberg & Hillier, 1979; Klaiberg et al., 2004; self-report screening) was used to assess the participants' mental health. The 28 items are scored on a 4-point Likert scale with varying anchors (e.g., 0 = *not at all* to 3 = *much more than usual*). Better mental health is indicated by a lower GHQ-28 total score (range: 0–84; cut-off for clinical caseness: 23; Sterling, 2011). Based on an outcome definition of resilience (see 2.1.2; i.e., change in mental health controlled for individual stressor exposure), the GHQ-28 served to operationalize resilience in this study. For this purpose, the stressor data assessed with the LE Checklist (pre, post), the MIMIS (pre, post), and the end-of-week assessments using the MIMIS EMA version were considered when analyzing the GHQ-28 (see 3.2.8.2).

Brief Resilience Scale (BRS). The ability to recover from stress was assessed using the German BRS (Chmitorz, Wenzel, et al., 2018; Smith et al., 2008). Six items are rated on a 5-point Likert scale (1 = *strongly disagree* to 5 = *strongly agree*). Higher scores (range: 1–5) indicate a higher ability to recover from stress.

Positive and Negative Affect Scales (PANAS). In the PANAS (Watson et al., 1988), two mood scales with 10 adjectives assess positive (PANAS-PA) and negative affect (PANAS-NA). In this study, the instruction "How do you feel at the moment" was used. For each adjective, participants indicate the extent to which they experienced the respective mood state on a 5-point scale (1 = *very slightly or not at all* to 5 = *extremely*), with higher scores (range 10–50) indicating more positive/negative affect.

Well-being Index (WHO-5). The WHO-5 (Bech, 2004) is a self-report inventory measuring the well-being over the past two weeks with five positively phrased items on a 6-point Likert scale (0 = *at no time* to 5 = *all of the time*). Based on the means of linearly transformed index scores (range: 0–

100), higher well-being is indicated by higher scores.

State-Trait Anger Expression Inventory (STAXI). The German adaptation of the 44-item STAXI (Schwenkmezger et al., 1992; Spielberger, 1988) was used to assess the participants' state anger and different forms of anger expression (*anger in*: feelings of anger are suppressed; *anger out*: feelings of anger are expressed toward people/objects by attacking them; *anger control*: individual tries to control the anger). Based on a 4-point Likert scale (1 = *not at all* to 4 = *very much*), higher sum scores (range: 10–40 state anger; 8–32 anger expression scales) indicate higher anger (expression).

Emotion Regulation Questionnaire (ERQ). The ERQ (Gross & John, 2003), as self-report questionnaire, tests the ER strategies suppression and reappraisal. For this study, only the latter scale was used. Comparable to the original version, the German ERQ (Abler & Kessler, 2009) also includes six items for reappraisal, that are answered on a 7-point Likert scale (1 = *strongly disagree* to 7 = *strongly agree*). The mean score (range 1–7) of the six items is calculated.

Cognitive Emotion Regulation Questionnaire (CERQ). Another scale to assess the participants's use of cognitive ER was the German and abbreviated version of the CERQ (Garnefski et al., 2002; Loch et al., 2011). Using a 5-point Likert scale (1 = *[almost] never* to 5 = *[almost] always)*, the 27-item self-report scale measures nine dimensions of cognitive ER. For this study, only *positive reappraisal* was relevant, with higher scores (range: 3–15) indicating more frequent reappraisal.

EMA-assessed psychological measures ("Breezly"). To explore the effects of the training on the EMA-assessed well-being, the ability to distance from negative stimuli, and to get easily affected by positive ones, *self-developed (single) items* were used. The items were integrated into "Breezly" (evening rating; see 3.2.4, Appendix H7) and were answered on a bipolar scale (e.g., 0 = *not well at all* to 100 = *extremely well*). To assess the participants' current mood after each session, the German version of the Multidimensional Mood Questionnaire ([MDMQ]; Steyer et al., 1997), that had been adapted for EMA (Wilhelm & Schoebi, 2007), was implemented in "Breezly". Participants responded to six bipolar items (e.g., tired/awake; modified from original scale to 0–100). Three mood dimensions (positive valence, energetic arousal, calmness) were assessed, with higher scores indicating higher

values in the subscales.

Compatibility Score of the AAT. To measure the participants' implicit action tendencies to affective stimuli, the AAT assessment was used. The AAT mouse version using Inquisit 5 Lab (Millisecond Software, 2016) was conducted on a study notebook (Fujitsu LIFEBOOK notebook; wireless mouse Logitech M185) in the laboratory. Following Becker et al. (2016) and Becker et al. (2019), the original AAT was adapted as follows. Based on ratings of emotional valence (positive vs. negative), two categories of pictures (each of 13 images) were used, that had been selected from three databases (see Appendix G9). The 26 stimuli for the outcome assessment were different from pictures in the MB-PBT (see 3.2.4). Participants were instructed to react as fast as possible, but differently, depending on the picture format (landscape: push away/move mouse upward; portrait: pull closer/move mouse downward). During 10 practice trials, participants were presented with grey rectangles and received feedback for incorrect movements. In the assessment phase with 52 trials/pictures (2 categories [positive/negative] x 2 formats [landscape/portrait] x 13 repetitions/ pictures; inter-trial interval: 300 ms), positive and negative stimuli were presented randomly, with stimuli of both valence appearing equally often in each format. For each picture, the subjects' RT and the correctness of movement were measured, and the *AAT-CS* was calculated (see Appendix G10), with only RTs of correct responses being considered and the fastest and slowest 1% of RTs excluded.

3.2.8 Statistical Analyses

Separate data sets were created for the analysis (see Appendix I3). Statistical significance of effects was determined by *p* values of $p \leq .05$ or by 95% CIs. Due to the large number of 47 tests, analyses were adjusted for multiple testing using Bonferroni correction (Eid et al., 2017), resulting in a significance level of $\alpha = .001$ (see Appendix I4). The analyses were conducted in IBM SPSS 23v (IBM Corp., 2015) and Stata (StataCorp, 2019); see Appendix K3 for a detailed analysis plan.

3.2.8.1 Data Preparation

Dealing with missing data. All variables were tested for the correctness of data entry. In

data set 1, used to analyze the RQs 1 (hypothesis 1.1), 2, 3, 5, and 6, the number and pattern of missing values were investigated by conducting a missing values analysis (MVA) including Little's MCAR test (Little & Rubin, 2002). Missing values, that eventually resulted from entry errors, were supplemented. A *p* value > .05 in the MCAR test confirms the assumption of *missing data completely at random* (Eid et al., 2017; Little & Rubin, 2002). Based on this pattern of missingness, the *Expectation Maximization (EM)* algorithm can be used to impute missing data (Dempster et al., 1977; Lin, 2010). Similarly, for *data set 2* including the data of TRAIN4Positivity and LifeStress, a MVA including Little's MCAR test was also conducted. Based on a non-significant MCAR result, missing data were imputed using the EM algorithm. For *data set 3 and 4* (long format) including the repeated observations on EMA outcomes, the number and pattern of missing data were analyzed in Stata. In case of data *not* missing completely at random, but only at random (MAR), an available-case analysis was performed (Stata default), which omits any observation with a missing value for the model variables. This method was chosen as mixed-effects models (see 3.2.8.2) are tolerant of missing data (Göllner et al., 2010). Nevertheless, participants with ≤ three assessments were excluded. As sensitivity analysis, missing data were then imputed using multiple imputation ("mi impute", MI) that accounted for the clustering (Allison, 2002; Goldstein et al., 2009; Graham, 2009; Stata support FAQs, n.d.), to control for a possible bias and to compare the results. MI is recommended for data missing at random (Grund et al., 2018) and provides unbiased estimates at any proportion of missing data (Lee & Huber, 2011; Madley-Dowd et al., 2019).

Calculation of total scores. Based on the recommendations in relevant publications (Appendix G5), a mean score was determined for the SDS-17, BRS, WHO-5, and ERQ. Sum scores were computed for the LOT-R, SPF, ASF-E, MIMIS (stressor occurrence and perceived severity; paper-pencil and EMA), LE Checklist, PSS-10, GHQ-28, CERQ, PANAS, and STAXI. For the ERQ and the CERQ, only the reappraisal subscale was considered. For three scales (ASF-E, PANAS, STAXI), total scores for several subscales were calculated. The AAT-CS was determined by subtracting median RTs (see Appendix E1).

Outliers and extreme values. An explorative data analysis was performed to identify potential

outliers and extreme values using Box-whisker plots (Eid et al., 2017). It was inspected whether such values resulted from errors in data entry or from misunderstandings (e.g., instruction). If an outlier or extreme value presented a participant' s actual value, the data point was considered for analysis since an exclusion is viewed as inappropriate (Eid et al., 2017), and given the small sample size.

3.2.8.2 Statistical Analyses Within the IG (Within-Subjects Design)

Central assumptions of the statistical analyses and their testing are presented in Appendix I2. To examine the RQs 1 (hypothesis 1.1), 2 (hypothesis 2.1), 3, and 5 concerning the change of different outcomes by the MB-PBT, a combination of paired *t* tests or Wilcoxon signed-rank (WSR) tests and multiple linear regression analyses was performed, respectively.

Paired *t* tests & Wilcoxon signed-rank tests. Nine *t* tests for paired samples were conducted to test for mean changes between pre- and posttest (outcomes: PSS-10, AAT-CS, BRS, WHO-5, ERQ-reappraisal, CERQ-reappraisal, PANAS-NA, STAXI-anger in, STAXI-anger control). For four outcomes (GHQ-28, PANAS-PA, STAXI state anger, STAXI anger out), WSR tests were conducted due to violations of the normality assumption (see Appendix J5). Due to the exploratory character of this study, two-tailed tests were performed for both the hypotheses (1.1, 2.1) and exploratory RQs (Field, 2015). Cohen's *d* (Eid et al., 2017) was calculated as ES (small effect: $d = |0.2|$, moderate effect: $d = |0.5|$, large effect: $d = |0.8|$; Cohen, 1988) for the *t* tests; for the WSR tests, *r* was calculated (Rosenthal, 1991) and converted to Cohen's *d* (Field, 2015).

Multiple linear regression analyses. This study also aimed at testing the potential impact of pre-existing tendencies in information processing and of individual stressor exposure on the efficacy of the PBT (see 3.1.7). Therefore, multiple linear regression analyses were performed to examine whether potential pre-post-changes in the above outcomes were possibly affected by the following variables: 1) macrostressors (pre, post), 2) microstressors (pre, EMA during training, post), 3) the baseline AS for positive events (ASF-E-P), and 4) for negative events (ASF-E-N). These variables served as predictors (included using *enter method*) in multiple linear regression. The latter was chosen as the enter method is considered as appropriate for hypothesis-testing (Field, 2015). Based on Eid et al.

(2017), all predictors were z-standardized before entering them. *Difference scores* of the above outcomes between pre- and posttest (e.g., AAT_diff) were the dependent variables (diff = post-pre). Due to the small sample size and the pilot character of this study, a parsimonious regression model using the four mentioned predictors was used. In case of statistically significant results for this model, another regression analysis for each outcome was planned, with z-standardized scores of SPF and SDS-17 as additional predictors (see Appendix I5). The determination coefficient R^2 and f^2 were used as ES (R^2 for proportion of variance explained: small: $|R^2| = .02$, moderate: $|R^2| = .13$, large: $|R^2| = .26$; small effect: $|f^2| = .02$, moderate effect: $|f^2| = .15$, large effect: $|f^2| = .35$; Cohen, 1988).

Three-way repeated-measures analysis of variance. To examine hypothesis 2.2 (RQ2), that is, the potential role of the baseline AS for the effects of "Breezly" on implicit action tendencies, a three-way (2 x 2 x 2) repeated-measures analysis of variance (ANOVA) was conducted. Between-subjects factors (grouping variables) were the AS for positive events (ASF-E-P) and negative events (ASF-E-N). Two categories (low vs. high) were created by median splits, respectively. Time (pre- vs. posttest) served as within-subjects (repeated) factor. Partial eta squared (η^2_p) was used as ES (small effect: $\eta^2_p = |.01|$, moderate effect: $\eta^2_p = |.06|$, large effect: $\eta^2_p = |.14|$; Sedlmeier & Renkewitz, 2013).

Multilevel analysis. Mixed-effects regression models in Stata were used to answer RQ4 concerning potential changes in EMA-measured psychological outcomes. Multilevel analyses are performed based on hierarchical data sets and are recommended for the analysis of longitudinal data, with the advantage of having less restrictive assumptions (Göllner et al., 2010; Richter & Naumann, 2002). They are increasingly used to evaluate interventions (O' Connell & Betsy McCoach, 2008). The multilevel structure in this study manifested in repeated daily observations (level 1), that were nested or clustered within participants (level 2). To examine RQ4, the main effect of time (level-1 predictor) on the above outcomes was tested, with the outcome change over time represented by regression slopes. For 2-level models, not aiming to analyze cross-level interactions, at least $n = 30$ units on the highest level are required (Göllner et al., 2010; Nezlek et al., 2006; here: $n = 41$). In this study, a multi-step approach (from null model to random-coefficients model; for details, see Appendix I7) was used

to test the effects of time (Appendix K3). This multi-step approach was performed twofold, as available-case analysis and based on the multiply imputed data set. To assess the model fit, several fit indices were used, such as the Akaike Information Criterion (AIC) and the Bayesian Information Criterion (BIC). The smaller these indices, the better is the model fit (Eid et al., 2017). Fitting plots of observed and predicted values were inspected. In addition, using the Likelihood-Ratio test (LR test; Snijders & Bosker, 2012), the goodness of fit between two subsequent models was compared, respectively. A significant difference of model deviances (χ^2 test) indicates better fit of the second compared to the previous model. In total, 36 multilevel models (six per outcome) were calculated for RQ4, with only the model with the best model fit being relevant for the testing of RQ4 (see Appendix I4). Data were converted (long format) before analyzing them in multilevel analyses. For each model tested (from the Random-intercept model onwards), the reduction of prediction error compared to the null model (R^2 on level 1) was calculated[25] (Hox, 2010; Luke, 2020).

Bivariate Pearson correlations. To investigate the construct validity of the AAT (RQ6), Pearson correlations between the AAT-CS and optimism (LOT-R), positive AS (ASF-E-P and ASF-E-N), the ability to recover from stress (BRS), and perceived stress (PSS-10) were calculated. The correlation coefficients were judged as follows: small: $|r| = .1$; moderate: $|r| = .3$; large: $|r| = .5$ (Cohen, 1988).

3.2.8.3 Statistical Analyses Between IG and (Historical) CG (Between-Subjects Design)

Propensity score matching and two-sample *t* test. To explore if the MB-PBT (IG), compared to a (historical) CG, decreases an individual's perceived microstressor severity (RQ1, exploratory RQ 1.2), a propensity score (PS) analysis and matching were performed. The analysis was chosen as PS analyses allow to evaluate interventions by considering pretest differences between non-randomized groups (Kuss et al., 2016). The details of the seven-step PS matching and analysis as preparation of the testing of RQ 1.2 are presented in Appendix I6. For the matched data, the perceived microstressor severity at posttest was compared between the groups using an independent *t* test.

[25] using the formula $R^2_{\text{level 1}} = ((\sigma^2_{e(1)} - \sigma^2_{e(2)}))/\sigma^2_{e(1)})$, with model 2 being the model with level-1 predictor time and model 1 being the null model (baseline model); σ^2_e = variance of residuals

3.3 Results

3.3.1 Data Preparation

Missing data. Based on the MVA in *data set 1* (*N* = 41), few data were missing at pre- and posttest, with the highest rate of missing values found for the STAXI with 14.6% missing values in item 20. No missing values resulted from errors in entering the data. A larger number of missing data was found for single items and the three sum scores of stressor exposure, as assessed using the MIMIS EMA version each Sunday (4.9%–17.1% in three sum scores). Since some participants missed or did not react to alarms of "movisensXS" on Sundays, they did not complete the MIMIS items, resulting in these items being counted as missing by the app. The Little's MCAR test for all metric variables showed no significant finding ($\chi^2 = 120.37$, $df = 18664$, $p = 1.00$) and confirmed the MCAR assumption (Little & Rubin, 2002). Therefore, missing data were imputed using the EM algorithm (Dempster et al., 1977). All further analyses (RQ 1/hypothesis 1.1, RQs 2, 3, 5, and 6) were performed based on the EM-imputed data set. For *data set 2* (*N* = 87), the MVA also showed almost no missing data at pre- and posttest for variables measured in both groups, with the highest rate found for the CERQ item 26 with 2.3% missing values. Due to the non-significant Little's MCAR test ($\chi^2 = 0$, $df = 2268$, $p = 1.00$) and the confirmed MCAR assumption, missing values were also imputed using the EM algorithm, with the PS matching and the group comparison conducted using the EM-imputed data set. As this study served to test the general acceptance of "Breezly" in daily life, the regular completion of EMA-measured variables (RQ4) could not be ensured, resulting in a large number of missing data in *data set 3* (38% missing data for current mood) and *data set 4* (47% missing data for end-of-day measures; for both). Given the analysis of the pattern of missing data, the MCAR assumption was not fulfilled. First, the multilevel modeling to examine RQ4 was performed based on an available-case analysis. Second, due to the study's pilot character, data were assumed as missing at random to also perform MI and to use the Stata "mixed" command with imputed data. As there is no threshold of missing data for the use of MI in longitudinal designs (see 3.2.8.1), this procedure was chosen despite the large proportion of missing values. To account for the clustering when imputing data, cluster indicators were included in

the imputation model due to the manageable number of clusters (*N* = 41; Allison, 2002; Goldstein et al., 2009; Graham, 2009; Stata support FAQs, n.d.).

Outliers and extreme values. Box-whisker plots (see 3.2.8.1; Appendix K4) indicated outliers for the ASF-E-N, MIMIS, AAT-CS, GHQ-28, BRS, WHO-5, ERQ, PANAS-PA, PANAS-NA, and STAXI (state anger, anger in) at pretest. (Additional) Extreme values were found for the PANAS-NA and STAXI (state anger, anger out). At posttest, the diagrams indicated outliers for the LOT-R, ASF-E-P, ASF-E-N, MIMIS, PSS-10, AAT-CS, GHQ-28, BRS, ERQ, PANAS-NA, and STAXI (all scales), with additional extreme values found for AAT-CS and STAXI (state anger). Concerning difference scores, outliers resulted for the PSS-10, GHQ-28, WHO-5, ERQ, CERQ, PANAS-PA, PANAS-NA, STAXI (all scales), with additional extreme values for the GHQ-28 and two STAXI scales (Appendix J1). Incorrect data entry or a lack of care in study completion could be excluded. Since the data represented the participants' *true* values, there were included in the analyses uncorrected (see 3.2.8.1).

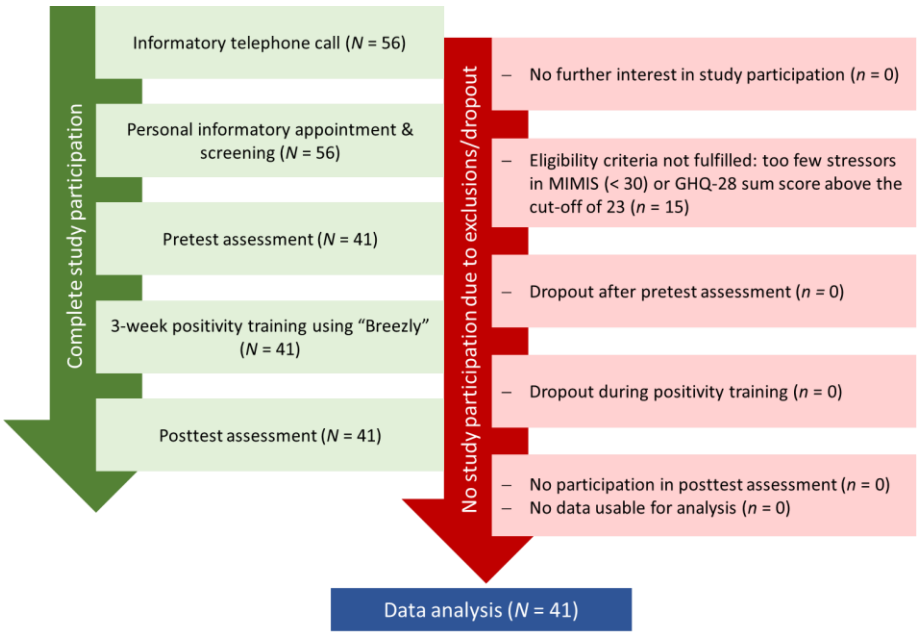
3.3.2 Descriptive Sample Description

For this study, 56 individuals were recruited, of whom 41 participants fulfilled the inclusion criteria. There were no dropouts or exclusions during the study course, resulting in *N* = 41 participants to be considered for the analysis. Figure 10 presents further information. The average age was 25 years (*SD* = 3.0; range: 20–30 years). Of the *N* = 41, 80.5% (*n* = 33) were female. All subjects were born in Germany and indicated German as mother tongue (*N* = 41). With respect to the educational level, 61% (*n* = 25) had a degree from university or University of Applied Sciences, for 34.1% (*n* = 14), A level was the highest school-leaving qualification, and 4.9% (*n* = 2) indicated a completed occupational training. Most participants (*n* = 26; 63.4%) were studying at the time of the study, *n* = 13 individuals (31.7%) were employed, and two participants (4.9%) indicated no information about their employment status. Appendix J2 includes the descriptive statistics of all sociodemographic, clinical, and lifestyle variables as well as a study flow diagram and a sample description for the historical CG LifeStress. Appendix J3 and Appendix J4 present a descriptive analysis of the baseline variables and covariates as well as the primary and secondary outcomes in TRAIN4Positivity. If available, these data

were classified with reference to published norms or descriptive statistics from other (representative) German samples. With respect to the variables age, gender, and education, a PS matching was performed between the participants of TRAIN4Positivity and LifeStress (see 3.3.3.1). Details of this procedure are presented in Appendix J8.

Figure 10

Study Flow Diagram (Adapted From Krayer, 2019)



Note. GHQ-28 = General Health Questionnaire-28; MIMIS = Mainz Inventory of Microstressors.

3.3.3 Testing of Hypotheses and Exploratory RQs

The following sections describe the findings for the hypotheses and exploratory RQs. Appendix J5 presents the examination of prerequisites of the statistical analyses. Due to non-significant findings for a parsimonious regression model (see 3.2.8.2), the model with six predictors was not examined.

3.3.3.1 Effects of PBT on Perceived Stress and Perceived Microstressor Severity

To investigate hypothesis 1.1 (RQ1) concerning the effects of PBT on perceived stress (PSS-10), a paired *t* test was conducted first. On average, the participants showed lower values of perceived stress after the PBT (M = 13.7, SD = 4.2) compared to the pretest (M = 15.3, SD = 5.0). Based on a Bonferroni-corrected α level (α_{corr} = .001), this difference was not significant

($t(40) = –2.28$, $p = .03$, $d = –0.36$) with Cohen's *d* indicating a moderate ES. Despite the non-significant mean difference in the *t* test, a multiple regression analysis on the difference score of perceived stress (PSS-10) was performed. This procedure was chosen based on the exploratory study design and to control for the potential impact of the participants' stressor exposure as well as their baseline AS. A parsimonious model with these four (z-standardized) predictors was calculated. In the regression on the PSS-10 change score, 4% of the variance were explained by entering the predictors ($R^2 = .04$, corrected $R^2 = –.06$, $f^2 = .04$), indicating a small ES (Cohen, 1988). The overall regression model was not significant, $F(4, 36) = 0.40$, $p = .81$. None of the predictors predicted the change in perceived stress. Table 4 illustrates the regression coefficients; the model summary is presented in Appendix J7.

Table 4

Regression Coefficients of Predictors to Predict Difference Score (Post-Pre) of Perceived Stress (PSS-10; Linear Regression; Enter Method)

Variable (z-standardized)	*B*	*SE*	*p*
Constant	–1.61	0.73	.03
Microstressors (MIMIS, MIMIS EMA)	0.24	0.92	.79
Macrostressors (LE Checklist)	–0.95	0.85	.27
ASF-E-P	0.36	0.76	.64
ASF-E-N	0.004	0.82	.996

Note. Dependent variable: change in PSS-10 between pre- and posttest; $N = 41$; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest and during the training; macrostressors = sum score of macrostressors at pre- and posttest; Bonferroni correction: $\alpha_{corr} = .001$ (two-tailed).

With respect to the exploratory RQ 1.2 if the PBT decreases the participants' perceived microstressor severity compared to a CG, a PS matching was performed (details in Appendix J8). Based on the matched data, the posttest comparison between TRAIN$_4$Positivity and LifeStress using an independent *t* test ($t(59) = –0.78$, $p = .44$, $d = 0.20$) showed no significant difference in perceived microstressor severity (small effect; Appendix J8.8).

Research Question 1 (RQ1): Which effects does a MB-PBT have on perceived stress and perceived microstressor severity?

Hypothesis 1.1: Controlled for individual stressor exposure and the baseline AS, a MB-PBT at the action level decreases perceived stress from pre- to posttest within the IG. Hypothesis 1.1 is not supported.

Exploratory RQ 1.2: Compared to a historical CG (stressor monitoring), does a MB-PBT at the action level decrease the perceived microstressor severity at posttest? The exploratory RQ can be negated.

3.3.3.2 Effects of PBT on Implicit Action Tendencies

The hypothesis concerning changes in the implicit action tendencies (AAT-CS; RQ2, hypothesis 2.1) was also tested by computing a paired *t* test. For two participants, the AAT data had to be excluded as they misapplied the instructions in the assessment after correct practice trials, leading to $n = 39$ participants in the analysis. On average, an increase of the AAT-CS after the training ($M = 42.4$, $SD = 129.1$) compared to the pretest ($M = 0.5$, $SD = 66.1$) was observed. Based on the Bonferroni-corrected significance level ($\alpha_{corr} = .001$), this difference was not significant ($t(38) = 2.31$, $p = .03$) with a moderate ES ($d = 0.37$). Subsequently, a multiple linear regression analysis was computed despite the non-significant finding in the *t* test (compare 3.3.3.1). In the multiple linear regression (parsimonious model; see 3.2.8.2), 6% of the variance in the change score of the AAT-CS were explained by the four predictors ($R^2 = .06$, corrected $R^2 = –.05$, $f^2 = .06$), indicating a small effect. The regression model was not significant, $F(4, 34) = 0.57$, $p = .69$. Neither the stressor exposure nor the AS at pretest significantly predicted the change in implicit action tendencies (see Appendix J7).

To investigate *hypothesis 2.2* if the participants' AS at pretest moderates the effects of the PBT on implicit action tendencies, a three-way repeated-measures ANOVA (dependent variable AAT-CS) was performed. Based on median splits, the attribution total scores for positive (ASF-E-P) and negative events (ASF-E-N) served as between-subjects factors, with time as repeated factor. Based on the Bonferroni-correction ($\alpha_{corr} = .001$), the three-way repeated-measures ANOVA showed no significant main effect of time, $F(1, 35) = 5.10$, $p = .03$, $\eta^2_p = .13$. In addition, there were no significant main effects of the ASF-E-P, $F(1, 35) = 1.04$, $p = .31$, $\eta^2_p = .03$, and the ASF-E-N, $F(1, 35) = 1.61$, $p = .21$, $\eta^2_p =$

.04. The two-way interaction between the repeated factor time and the ASF-E-P (Time x ASF-E-P: $F(1, 35) = 0.20$, $p = .66$, $\eta^2_p = .01$) as well as the ASF-E-N (Time x ASF-E-N: $F(1, 35) = 0.81$, $p = .38$, $\eta^2_p = .02$) were not significant, respectively. Neither the attribution score for positive situations nor for negative situations at pretest significantly moderated the change of the AAT-CS between pre- and posttest. The three-way interaction of the between-subjects factors ASF-E-P and ASF-E-N and the repeated factor time was also not statistically significant (Time x ASF-E-P x ASF-E-N: $F(1, 35) = 0.004$, $p = .95$, $\eta^2_p = 0$).

RQ2: Which effects does a MB-PBT have on implicit action tendencies to affective stimuli?

Hypothesis 2.1: Within the IG, a MB-PBT at the action level results in a PB at the level of action tendencies from pre-to posttest, that is, the participants' AAT-CS increases. Hypothesis 2.1 is not supported.

Hypothesis 2.2: Within the IG, the participants' AS at pretest moderates the effects of training on implicit action tendencies, that is, the increase in the AAT-CS from pre- to posttest is larger for individuals with a negative AS at pretest. Hypothesis 2.2 is not supported.

3.3.3.3 Effects of PBT on Resilience, the Ability to Recover From Stress, and Well-Being

To answer the exploratory RQ3 concerning the effects of "Breezly" on mental health (GHQ-28; one of the later indicators of resilience; see 3.2.7.3), the ability to recover from stress (BRS), and well-being (WHO-5), paired *t* tests and one WSR test (GHQ-28) were also used (see Appendix J5). On average, the participants showed slightly lower values of well-being and mental health (i.e., higher psychological distress in GHQ-28) between pre- and posttest, while there was no descriptive change in the ability to recover from stress. None of the differences was statistically significant (all *p*s > .05). The statistics of the two *t* tests, the WSR test, and the respective ES are illustrated in Appendix J6.

Despite the non-significant results in the *t* tests and the WSR test, multiple linear regression analyses were conducted (compare 3.3.3.1) to control for the potential impact of stressor exposure and the AS at pretest on changes in the above outcomes. The inclusion of the individual stressor load (i.e., micro- and macrostressors) was especially important to assess the effects of the PBT on resilience (i.e., change in mental health normalized by stressor exposure). With respect to *resilience*, two

multiple linear regression analyses were performed. First, a multiple linear regression on the difference score of GHQ-28 (post-pre) was computed by including two predictors (number of microstressors using the MIMIS and MIMIS EMA version; macrostressors using LE Checklist). This change in the GHQ-28 controlled for individual stressor load served as proxy measure of resilience. By entering the two predictors, 4% of the variance were explained (R^2 = .04, corrected R^2 = –.02, f^2 = .04), indicating a small effect. The regression model was not significant, $F(2, 38) = 0.70$, p = .51. The stressor exposure showed no significant impact on the change in mental health between pre- and posttest. The model summary and the regression coefficients are illustrated in Appendix J7. Subsequently, a second regression model was tested by including the baseline AS (ASF-E-P, ASF-E-N) as additional predictors in line with other outcomes of this study. The predictors explained 5% of the variance (R^2 = .05, corrected R^2 = –.06, f^2 = .05), indicating a small ES. The regression model was not significant, $F(4, 36) = 0.47$, p = .76. None of the four variables significantly predicted the change in mental health (see Appendix J7). By simultaneously entering the three predictors for stressor exposure and baseline AS in the multiple linear regression for the difference score of the *ability to recover from stress* (BRS), 6% of the variance were explained (R^2 = .06, corrected R^2 = –.01, f^2 = .06), indicating a small effect. The overall model was not significant, $F(3, 37) = 0.81$, p = .49. None of the variables (LE, ASF-E-P, ASF-E-N) significantly predicted the change in the ability to recover from stress (see Appendix J7). Concerning the change score of *well-being* (WHO-5), the four predictors explained 12% of the variance (R^2 = .12, corrected R^2 = .03, f^2 = .14), indicating a moderate ES. The regression model was not significant, $F(4, 36) = 1.26$, p = .31. Neither stressor exposure nor the pretest AS significantly predicted the change in well-being between pre- and posttest (see Appendix J7).

RQ3 (exploratory): Within the IG and controlled for individual stressor exposure and the baseline AS, does a MB-PBT at the action level have any effects on (3.1) resilience (change in mental health controlled for stressor load), (3.2) the ability to recover from stress, or (3.3) well-being? The exploratory RQ can be negated for (3.1) resilience (i.e., change in GHQ-28 controlled for stressor exposure), (3.2) the ability to recover from stress, and (3.3) well-being.

3.3.3.4 Effects of PBT on Psychological EMA Outcomes

Multilevel analysis was used to investigate potential changes in the EMA-measured outcomes current mood (subscales positive valence, energetic arousal, and calmness), the ability to get affected by positive things and to distance from negative things, and well-being (RQ4). For each of the mood scales, six participants with less than three observations (see Appendix K7.1) were excluded (i.e., n = 35). For the end-of-day measures, eight participants (Appendix K7.1) were excluded (i.e., n = 33).

For *positive valence*, the null model resulted in an ICC of 0.46, indicating that 46% of the variation in positive valence scores were due to between-participant variation (i.e., sufficient variation to perform multilevel modeling). Subsequently, different linear mixed-effects models (Random-intercept model; Random-coefficients model; Random-coefficients model with compound symmetry [CS] covariance matrix; Random-coefficients model with unstructured covariance matrix) were tested (see Appendix J9). The best model fit was found for the Random-coefficients model assuming an unstructured covariance matrix based on fit indices (AIC = 25808.78, BIC = 25845.21). The LR test comparing a Random-coefficients model (step 3) with a Random-intercept model (step 2) also suggested a better fit for a model assuming both random intercepts and random slopes (χ^2 = 385.55, p < .001). Besides, the fitting plot and the approximate normal distribution of standardized residuals suggested a good fit of the model with the unstructured covariance matrix (Appendix J9 and Appendix K7.3). Starting (i.e., intercept) from a positive valence score of 65.36 or 65.24 (imputed data), respectively, a statistically significant effect of the level-1 predictor time was found neither in the available-case analysis (B = 0.005, p = .87) nor for the imputed data (B = 0.009, p = .65). For *energetic arousal*, the null model resulted in an ICC of 0.23 (i.e., proportion of between-participant variation in total variance is 23%), indicating sufficient variation to perform multilevel modeling. Again, different linear mixed-effects models (Appendix J9) were tested and the best model fit was found for the Random-coefficients model with an unstructured covariance matrix based on fit indices (AIC = 27940.47, BIC = 27976.90). The LR test comparing a Random-coefficients model (step 3) with a Random-intercept model (step 2) also suggested a better fit for a model assuming both random

intercepts and random slopes (χ^2 = 66.61, p < .001). Besides, the fitting plot and the normality of standardized residuals indicated a good model fit (Appendix J9 and Appendix K7.3). Starting (i.e., intercept) from an energetic arousal score of 50.42 or 50.28 (imputed data), respectively, a significant effect of the level-1 predictor time was found neither in the available-case analysis (B = 0.002, p = .94) nor for the imputed data (B = 0.001, p = .95). For *calmness*, the null model resulted in an ICC of 0.38 (i.e., 38% of variance in calmness due to between-participant variation), suggesting sufficient variation to calculate mixed-effects regression models. Among the different linear models tested (Appendix J9), the best model fit was found for the Random-coefficients model with an unstructured covariance matrix based on fit indices (AIC = 26520.38, BIC = 26556.81). The LR test comparing a Random-coefficients model (step 3) with a Random-intercept model (step 2) also suggested a better fit for a model assuming both random intercepts and random slopes (χ^2 = 487.04, p < .001). Besides, the fitting plot and the normality of standardized residuals indicated a good model fit (Appendix J9 and Appendix K7.3). Based on (i.e., intercept) a calmness score of 63.55 or 63.56 (imputed data), respectively, a statistically significant effect of the level-1 predictor time was found neither in the available-case analysis (B = 0.01, p = .78) nor for the imputed data (B = 0.009, p = .73).

For *well-being*, in the null model an ICC of 0.25 was found, suggesting sufficient variation to calculate mixed-effects regression models. Among the different linear models tested (see Appendix J9), the best model fit was achieved for the Random-coefficients model with unstructured covariance matrix (AIC = 3699.04, BIC = 3723.36). The LR test comparing a Random-coefficients model (step 3) with a Random-intercept model (step 2) also suggested a better fit for a model assuming both random intercepts and random slopes (χ^2 = 9.14, p < .05). Besides, the fitting plot and the approximate normal distribution of standardized residuals suggested a good fit of the model with the unstructured covariance matrix (Appendix J9 and Appendix K7.3). Based on a value of 63.56 or 65.68 (imputed data) for well-being at time 0, no significant effect of the level-1 predictor time was found in the available-case analysis (B = 0.50, p = .07) or for the imputed data model (B = 0.04, p = .80). For the *ability to distance from negative stimuli*, the null model resulted in an ICC of 0.24, also indicating sufficient

variation to perform multilevel modeling. Among the different linear mixed-effects models tested (see Appendix J9), the best model fit was identified for the Random-intercept model (AIC = 3803.79, BIC = 3820.01). The LR test comparing this model (step 2) with the null model (step 1) also suggested a better fit for the Random-intercept model assuming a fixed slope (χ^2 = 17.19, p < .001). Besides, the fitting plot and the normal distribution of standardized residuals suggested a good fit of the Random-Intercept model (Appendix J9 and Appendix K7.3). Starting (i.e., intercept) from a score of 54.69 or 57.56 (imputed data), respectively, a statistically significant effect of the level-1 predictor time on the ability to distance from negative stimuli was found in the available-case analysis (B = 0.89, p < .001), but not for imputed data (B = 0.25, p = .15). For the *ability to get affected by positive stimuli*, the null model resulted in an ICC of 0.34, indicating sufficient variation to perform multilevel modeling. Again, different linear mixed-effects models were tested (see Appendix J9). The best model fit was found for the Random-coefficients model assuming an unstructured covariance matrix (AIC = 3651.78, BIC = 3676.10). The LR test comparing a Random-coefficients model (step 3) with a Random-intercept model (step 2) also suggested a better fit for a model assuming both random intercepts and random slopes (χ^2 = 5.43, p < .05). Besides, the fitting plot and the approximate normal distribution of standardized residuals suggested a good fit of the model with the unstructured covariance matrix (Appendix J9 and Appendix K7.3). Starting (i.e., intercept) from a score of 66.06 or 67.92 (imputed data), respectively, a significant effect of time on the ability to get affected by positive stimuli was found neither in the available-case analysis (B = 0.49, p = .07) nor for imputed data (B = 0.10, p = .52).

RQ4 (exploratory): Within the IG, does a MB-PBT at the action level have any effects on the EMA-measured (4.1) current mood, (4.2) well-being, (4.3) the ability to distance from negative stimuli, or (4.4) the ability to get affected by positive stimuli? The exploratory RQ can be negated for (4.1) current mood (i.e., positive valence, energetic arousal, calmness), (4.2) well-being, and (4.4) the ability to get affected by positive stimuli. Positive effects of a MB-PBT on the ability to distance from negative stimuli (available-case analysis) can be partly assumed, with an increase during training.

3.3.3.5 Effects of PBT on Cognitive ER (Reappraisal) and Emotional Experience

To answer RQ5 concerning potential effects of the intervention on reappraisal and the participants' emotional experience, paired *t* tests and WSR tests (PANAS-PA, STAXI state anger, and anger out) were conducted. On average, the subjects showed slightly higher values of positive reappraisal (CERQ), positive affect (PANAS-PA), state anger (STAXI), and anger control (STAXI) after the PBT. Slightly lower scores at posttest resulted for negative affect (PANAS-NA) and the STAXI scale anger out; the ERQ and the STAXI scale anger in were stable. None of the differences was significant (all *p*s > .05). Appendix J6 illustrates the statistics of the five *t* tests, three WSR tests, and the ES.

Despite the non-significant mean differences in the *t* tests and the WSR tests, multiple regression analyses on the difference scores (post-pre) of the above outcomes were performed for the aforementioned reasons. A parsimonious model with four (z-standardized) predictors (micro- and macrostressors, ASF-E-P, ASF-E-N) was calculated, respectively. By entering the predictors, 28% of the variance in the ERQ *reappraisal* difference were explained (R^2 = .28, corrected R^2 = .20, f^2 = .39), indicating a large ES (Cohen, 1988). Based on the Bonferroni correction (α_{corr} = .001), the regression model was not significant, $F(4, 36) = 3.46$, p = .02. In addition, based on the corrected α level, none of the four variables predicted the change in ERQ-assessed reappraisal. Appendix J7 illustrates the model summary and regression coefficients. For the change score of the CERQ subscale *positive reappraisal*, the four predictors explained 2% of the variance (R^2 = .02, corrected R^2 = –.10, f^2 = .02), indicating a small effect. The regression model was not significant, $F(4, 36) = 0.13$, p = .97. None of the predictors significantly predicted the change in CERQ-reappraisal (see Appendix J7). For the difference score of *positive affect* (PANAS-PA), 16% of the variance were explained by entering the four predictors (R^2 = .16, corrected R^2 = .07, f^2 = .19), indicating a moderate effect. The regression model was not significant, $F(4, 36) = 1.73$, p = .17. Given the corrected significance level, there was no significant predictor for the change in positive affect (see Appendix J7). Regarding the change score of the PANAS subscale *negative affect*, the four predictors explained 21% of the variance (R^2 = .21, corrected R^2 = .13, f^2 = .27), indicating a large ES. The overall regression model was not significant, $F(4, 36) = 2.44$, p = .07. Based on the Bonferroni correction, there was no significant predictor for the pre-post change in

negative affect (see Appendix J7). Concerning the STAXI subscale *state anger*, 12% of the variance were explained by entering the four predictors (R^2 = .12, corrected R^2 = .02, f^2 = .14), indicating a moderate effect. The regression model was not significant, $F(4, 36) = 1.17$, $p = .34$. None of the four variables significantly predicted the change in state anger (see Appendix J7). For the *STAXI subscale anger in*, the four variables explained 7% of the variance (R^2 = .07, corrected R^2 = –.04; f^2 = .08), indicating a small ES. The overall model was not significant, $F(4, 36) = 0.62$, $p = .65$. There was no significant predictor for the change in anger in over time (see Appendix J7). Regarding the STAXI scale *anger out*, the four predictors explained 27% of the variance (R^2 = .27, corrected R^2 = .19, f^2 = .37), indicating a large ES. Based on the Bonferroni correction, the regression model was not significant, $F(4, 35) = 3.29$, $p = .02$. In addition, given the corrected significance level, none of the four variables predicted the change in anger out (see Appendix J7). Concerning the STAXI subscale *anger control*, 1% of the variance was explained by entering the four predictors (R^2 = .01, corrected R^2 = –.10, f^2 = .01), indicating a small ES. Neither the regression model, $F(4, 36) = 0.13$, $p = .97$, nor any of the predictors was statistically significant (see Appendix J7).

RQ5 (exploratory): Within the IG and controlled for individual stressor exposure and the baseline AS, does a MB-PBT at the action level have any effects on (5.1) reappraisal, (5.2) positive affect, (5.3) negative affect, (5.4) state anger, or (5.5) anger expression (anger in, anger out, anger control)? The exploratory RQ can be negated for (5.1) reappraisal, (5.2) positive affect, (5.3) negative affect, (5.4) state anger, and (5.5) anger expression (anger in, anger out, anger control).

3.3.3.6 Construct Validity of the AAT as a Measure of PB at the Level of Action Tendencies

For the purpose of investigating the construct validity of the AAT (RQ6), bivariate correlations were calculated between the AAT-CS and optimism (LOT-R total, optimism score), positive AS (ASF-E-P, ASF-E-N), the ability to recover from stress (BRS), and perceived stress (PSS-10) at pretest. Consistent with RQ2, the analysis was only performed based on n = 39 subjects. The Pearson correlations are presented in Table 5. There were no significant associations between the AAT-CS on the one hand and the LOT-R (total and optimism score), ASF-E-P, BRS, and PSS-10 on the other (all ps >

.05). Only for the ASF-N, the bivariate correlation with the AAT-CS at pretest was statistically significant (p = .01). Appendix J10 contains the respective scatter plots.

RQ6 (exploratory): Within the IG at pretest, are there any bivariate associations between the AAT-CS and (6.1) trait optimism, (6.2) a positive AS (indicated by total score of attributions for positive and negative events), (6.3) the ability to recover from stress, or (6.4) perceived stress? The exploratory RQ can be negated for (6.1) trait optimism, (6.2) partly a positive AS (attributions for positive events), (6.3) the ability to recover from stress, and (6.4) perceived stress. Only a positive moderate correlation between the AAT-CS and a high total score for attributions of negative events (i.e., indicator of low positive AS) can be assumed.

Table 5

Bivariate Correlations

Variable	1	2	3	4	5	6	7
1. AAT-CS	1	–.001	–.02	.08	.39*	–.09	–.09
2. LOT-R – total	–	1	.87**	.36*	–.20	.55**	–.21
3. LOT-R – optimism	–	–	1	.19	–.23	.62**	–.19
4. ASF-E-P	–	–	–	1	–.05	.34*	–.28
5. ASF-E-N	–	–	–	–	1	–.24	.09
6. BRS	–	–	–	–	–	1	–.22
7. PSS-10	–	–	–	–	–	–	1

Note. AAT-CS = AAT Compatibility Score; LOT-R = Life Orientation Test-Revised; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; BRS = Brief Resilience Scale; PSS-10 = Perceived Stress Scale-10; N = 41 except for all correlations between AAT-CS and remaining variables (n = 39); * $p \leq .05$; ** $p \leq .01$ (two-tailed).

3.3 Discussion

3.4.1 Summary of Results

Using an EMI approach combined with EMA for stressor monitoring, this pilot study aimed at assessing the feasibility of a newly developed MB-PBT at the level of action tendencies. Potential effects on psychological outcomes were examined, while also exploring the validity of the AAT as a measure of PB at the action level. Healthy young adults with increased exposure to microstressors, who performed the training for three weeks, were primarily examined in a single-group design, while comparing the participants with a historical CG (only stressor monitoring) in one RQ using a matched control group design.

The findings showed that, controlled for stressor exposure and the baseline AS, the MB-PBT had no effect on perceived stress (*RQ1, hypothesis 1.1*). The same applied to the perceived microstressor severity at posttest, with no significant difference between subjects of TRAIN$_4$Positivity and matched controls of LifeStress who had only participated in stressor monitoring (e.g., EMA) for three weeks (*RQ1, exploratory RQ 1.2*). Regarding the effects of training on implicit action tendencies to affective stimuli (*RQ2*), this study found no statistically significant pre-post increase of the participants' PB at the action level, as assessed by the AAT-CS (*hypothesis 2.1*). The assumption of the baseline AS as a moderator for the effects of training on implicit action tendencies, with a larger increase of PB for individuals with a negative AS at pretest (*hypothesis 2.2*), was also not confirmed. This study found no statistically significant effects of the PBT on resilience, the ability to recover from stress, and well-being (*RQ3*). Concerning potential effects on psychological EMA outcomes, that were assessed immediately after training sessions or at the end of each day during the three-week period (*RQ4*), no change was demonstrated for current mood (positive valence, energetic arousal, calmness), well-being, and the ability to get affected by positive stimuli. The ability to distance from negative stimuli was partly shown to improve during the intervention, with the effect not being sustained after imputing missing data. Concerning *RQ5*, the pilot study found no significant pre-post changes regarding cognitive ER, specifically reappraisal, or emotional experience, including positive and

negative affect, state anger, and anger expression (anger in, anger out, anger control). When controlling for potential covariates, neither the number of micro- or macrostressors nor the baseline AS significantly predicted the change in the above outcomes (perceived stress, implicit action tendencies, mental health, ability to recover from stress, well-being, reappraisal and emotional experience) under the Bonferroni correction. With respect to the construct validity of the AAT as a measure of PB at the level of action tendencies (*RQ6*), no significant correlations between the AAT-CS and trait optimism, the ability to recover from stress, and perceived stress were found. For positive AS, the study showed no bivariate correlation with the total score for attributions of positive events. However, there was a significant positive relationship between the AAT-CS and the attribution score for negative events, indicating that a higher AAT-CS is associated with more internal, stable, and global attributions of negative events.

3.4.2 Interpretation of Results

In the following, the findings of this study are examined in more detail, with each RQ being discussed individually. The results are interpreted in the context of previous research.

3.4.2.1 Effects of PBT on Perceived Stress and the Perceived Severity of Microstressors

Despite the descriptive decrease of perceived stress in the current study, the pre-post change was not statistically significant. This finding is in contrast with previous studies that found stress-reducing effects of a PBT at the level of attention (e.g., Dandeneau et al., 2007) and interpretation (e.g., Clarke, 2016). The inconsistence might be explained by methodological differences, such as the level of intervention (attention/interpretation vs. action tendencies) and the stimuli. For example, in Dandeneau et al. (2007), participants were asked to identify a *single* positive picture (accepting/smiling face) among many negative pictures (rejecting faces) by tapping on the accepting face. In the current study, however, subjects had to perform approach or distancing movements to diverse pictures that could not be clearly evaluated as positive or negative by some participants (see 3.4.3.2). Thus, the training stimuli and the required response might have been unclear, possibly

reducing the training effects. For the same reason, the distancing movements to negative stimuli in "Breezly" might have affected the self-focused component of reappraisal and, in turn, the level of perceived stress, to a smaller extent. Therefore, this study can also not replicate the findings of Denny and Ochsner (2014), who showed that a training of self-focused reappraisal results in lower perceived stress (PSS). The three-week PBT might also have been experienced as an additional stressor (see 3.4.3.1), mitigating potential positive effects on perceived stress. The regression analysis to control for the impact of stressor exposure and baseline AS on the pre-post change showed no significant effect of any of the covariates. Taking into account the uncontrolled design, this result might indicate that the PBT, and not the number of stressors or the AS, did affect the descriptive decrease of perceived stress during the three-week period. However, the analysis could be biased by one influential data point (MIMIS).

Compared to a CG (LifeStress) with only the (ambulatory) assessment of microstressors, this study found no effect of the PBT on the perceived microstressor severity at posttest. Instead, based on descriptive statistics, participants of TRAIN$_4$Positivity even reported a higher perceived severity than the subjects of LifeStress. This finding is in contrast with previous research that reported less emotional vulnerability to stressors after PBTs at the level of attention (Johnson, 2009; Taylor et al., 2011), interpretation (Peters et al., 2011), and actions (Becker et al., 2016; study 2). Again, this might be explained by methodological aspects. All of the above-mentioned studies used stress tasks (e.g., stress anagram task) or a laboratory stressor (e.g., speech task) to examine the intervention effects on stress reactivity. Whereas they assessed the participants' responses to "artificial" stressors, the current study measured the reaction to *real-life* microstressors that had occurred in the participants' daily life. Furthermore, previous studies measured mood (e.g., Becker et al., 2016), frustration (e.g., Johnson, 2009), or anxiety (e.g., Taylor et al., 2011) after a stressor. The current study, however, relied on the subjective ratings of the severity of microstressors (MIMIS). Finally, while the current study included healthy participants with increased stressor exposure and found no effect on perceived microstressor severity, Becker et al. (2016; study 2) only reported reduced stress after a stress

anagram task for *dysphoric* individuals, who had received a negative mood induction. For unselected individuals and even non-dysphoric subjects after the mood induction, however, the authors also reported no effect on stress. Although the external circumstances were rather similar (e.g., use of study smartphone, "movisensXS"), higher demands in TRAIN4Positivity compared to LifeStress (e.g., in terms of time) might also explain the lack of evidence for an effect on the perceived microstressor severity. As for perceived stress, the combination of the pre-/posttest, a three-week PBT, EMA within "Breezly", and weekly EMA might have been experienced as stressful (see 3.4.3.1).

3.4.2.2 Effects of PBT on Implicit Action Tendencies

Based on previous literature in CBM, especially approach-avoidance modification trainings (e.g., Becker et al., 2016; Becker et al., 2019; Ferrari et al., 2018; Rinck et al., 2013), an increase of the AAT-CS was hypothesized, which should indicate a PB at the action level. Despite a descriptive tendency for an increase of the AAT-CS, the pre-post change was not statistically significant. This result is consistent with study 1 in Becker et al. (2016) who also found no change in action tendencies after a PBT in a (unselected) student sample. Except for Vrijsen et al. (2018), who also found no effect on the AAT-measured PB for a computerized add-on training in depressed inpatients, the current study does not replicate most previous resarch that showed action tendencies toward positive stimuli after an AAT-PBT in individuals at risk of or diagnosed with mental disorders (e.g., depressed individuals, Becker et al., 2019; socially anxious participants, Rinck et al., 2013) or (dysphoric and non-dysphoric) participants in sad mood after a negative mood induction (Becker et al., 2016; study 2).

The different results might be explained by methodological aspects. In contrast to laboratory AAT paradigms, this study used a mobile-based intervention to improve the ecological validity. As the participants' compliance with the training was not measured, they might have trained incorrectly or irregularly, leading to reduced training effects (see 3.4.3.2). In addition, by using EMI versus the computer-based AAT assessment at pre- and posttest, the approach and distancing movements had to be performed differently (e.g., swiping on display vs. moving mouse). Thus, to induce a measurable

difference in the AAT-CS, the three-week training with swiping movements might not have been intense enough.

When rating the pictures at the beginning of the study and during the training, some participants reported a limited ability to clearly categorize the stimuli as positive or negative (see 3.4.3.2). However, the pictures' valence and the associated movements are crucial for the PBT in this study. Similarly, the missing identification of a picture's valence could have affected the performance in the AAT assessment, resulting in a biased measurement of the AAT-CS at pre- and posttest.

The difference between *explicit* action tendencies, trained in the MB-PBT of this study, and the assessment of *implicit* action tendencies in the AAT is worth to be mentioned as it might have reduced the effects found. Participants in TRAIN$_4$Positivity were not blinded regarding the objectives of the PBT, that is, their explicit action tendencies were trained by instructing them to swipe affective pictures depending on the respective valence. In the above-mentioned studies (e.g., Becker et al., 2016; Becker et al., 2019; Rinck et al., 2013), however, the training followed on seamlessly from the assessment and vice versa, unbeknown to the participants (i.e., implicit action tendencies were trained and assessed). While the AAT assessment – as a RT measure – could be more difficult to be affected by the performance bias in the current study, the participants' training behavior might have been negatively affected (e.g., loss of interest due to triviality of the intervention).

Finally, although selected using the same criteria, the current study used different stimuli for the mobile-based intervention and the assessment to improve the generalizability of training (see 3.1.4), which is different from many of the above-named reference studies (e.g., Becker et al., 2016; Becker et al., 2019). Participants in these studies might have learned an association between pictures from the training and a specific movement (i.e., approach vs. avoidance). This, in turn, might have led to faster reactions in the AAT assessment. In addition to the activation of motivational schemes by the PBT at the action level, therefore, simple practice effects might have increased the AAT-CS in previous studies.

On the other hand, Ferrari et al. (2018) also found significant effects on action tendencies for

a PBT when using *different* pictures for the intervention and the pre-/posttest. The sample included unselected participants but – in contrast to the current study with rather no bias at pretest (M = 0.5) – started with an incompatibility bias (i.e., a tendency to avoid positive pictures) at baseline (M = -37 in PBT condition). During the training, Ferrari et al. (2018) demonstrated an increase of the AAT-CS (M = 39). This is in line with other studies that only showed the efficacy of an AAT-based PBT on the action tendencies of individuals with a pre-existing NB of information processing (Becker et al., 2019; Rinck et al., 2013) or with induced negative mood (Becker et al., 2016; study 2). Hence, the non-significant finding of the current study might be explained by a non-evident bias at the level of action tendencies at baseline (AAT-CS near 0). Based on the pretest scores of other measures, such as dispositional optimism (LOT-R; see Appendix J3), even a PB may have been present, which suggests a potential ceiling effect as in the first study of Becker et al. (2016).

Given the above-mentioned fndings that the effects of an AAT-based PBT are larger for individuals with a baseline NB, the current study controlled for the participants' AS at pretest when testing the effects on action tendencies as well as other outcomes. In the regression analysis, neither the baseline AS nor the stressor exposure predicted the change in action tendencies, indicating that the descriptive increase of the AAT-CS might have been affected by the training itself and not by covariates. Besides, a potential moderating role of the baseline AS was explored, with neither the AS for positive (ASF-E-P) nor negative events (ASF-E-N) affecting the effects of "Breezly" on action tendencies in the AAT. Thus, the (non-significant) change in the AAT-CS during the intervention did not depend on an individual's attributions for positive or negative situations before the training. This finding can be viewed in contrast with previous studies (e.g., Becker et al., 2016; Becker et al., 2019), but might also be due to some of the above-mentioned methodological differences.

Furthermore, the average values in both ASF-E total scores for positive and negative events at pretest could explain the lack of evidence for a moderating effect. As most participants showed no noticeable values of AS, probably also resulting in a limited variance, a significant interaction of time and AS might have been prevented. Because it is not recommended to calculate an overall score

(Poppe et al., 2005), both total scores for positive and negative events were used. However, the median split used to divide participants in those with low versus high values in the internality, globality, and stability of causal attributions might have been problematic (see 3.4.3.4).

In Becker et al. (2016; study 1), a pre-existing PB was assumed based on a positive AAT-CS at baseline in an unselected sample (*M* = 96). However, the current study found no significant correlations between the AAT-CS and optimism as well as the total attributional score for positive events (see 3.3.3.6 and 3.4.2.6). Instead, there was a significant *positive* association with the total attributional score for negative situations. Contrary to the assumptions of Becker et al. (2016), a positive AAT-CS might therefore not indicate a PB, which would also limit the conclusions concerning the moderating role of a pre-existing bias on the effects of training. Due to the AAT-CS being no validated measure for the assessment of a PB (or NB; see 3.1.6), the explanatory style (AS) was chosen as a proxy measure in this study, which is viewed as component of optimism (Seligman, 1990). This assumption is supported by the significant positive correlation between the ASF-E total score for positive events and trait optimism, also found in this study (see 3.3.3.6). On the other hand, no significant (negative) association was shown between trait optimism and the ASF-E total score for negative events, which partly questions the suitability of the ASF-E as a measure of PB in this pilot study.

Finally, the findings concerning the relationship between the AS (based on both total scores) and the AAT-CS indicate a limited comparability between the current work and previous studies (e.g., Becker et al., 2016). While there was no significant correlation between the ASF-E-P (positive events) and the AAT-CS, a significant correlation with the ASF-E-N (negative events) was found. Due to these limited associations, the measures used to indicate a pre-existing PB in the two studies might not be truly comparable.

3.4.2.3 Effects of PBT on Resilience, the Ability to Recover From Stress, and Well-Being

The non-significant changes in resilience (i.e., mental health normalized by stressor load) and the ability to recover from stress are consistent with Becker et al. (2016; study 1). The authors also

detected no effect of a general approach-avoidance training with diverse emotional pictures on the emotional vulnerability (mood) after a stress anagram task in unselected individuals (students). The findings are in constrast with Rinck et al. (2013) who found positive effects of an AAT training on mood recovery during a social stress task. However, the studies differed in the sample, training stimuli, and the outcome assessment, which limits the comparability of their results. While the current study included healthy participants with increased stressor exposure, that is, a group potentially at risk of mental disorders, Rinck et al. (2013) investigated socially anxious individuals. Both studies used an AAT training to induce a PB, with the current study including various positive and negative pictures, while participants of Rinck et al. (2013) were trained with disorder-specific stimuli (approach vs. avoid smiling faces). Besides, Rinck et al. (2013) assessed the mood after a social threat situation (video-taped self-presentation), showing that the disorder-specific training improved the recovery from disorder-specific stressors. This pilot study, however, included no acute stress induction. Resilience was operationalized by self-report questionnaires on mental health (GHQ-28) and stressor exposure (MIMIS, LE Checklist). With the BRS, a very economic measure of the ability to recover from stress was used, which is considered as proxy measure of resilience (Kunzler, Chmitorz, et al., 2018). All scales included general statements, whereas the mood ratings in Rinck et al. (2013) directly referred to the previous stress task. Given these differences, changes in resilience and the ability to recover from stress might have been more difficult to achieve in the current study, for example, due to the need of more general changes in the tendencies of information processing compared to disorder-specific changes in Rinck et al. (2013). Based on the negative correlation between the ability to recover from stress (BRS) and anxiety (Chmitorz, Wenzel, et al., 2018), the socially anxious participants of Rinck et al. (2013) probably had lower baseline scores in this construct than the subjects of TRAIN$_4$Positivity. This is in line with the observation that all participants of this study already had average or above-average scores in the BRS at pretest. For the answer format used in this study, no norm values for the GHQ-28 were available. However, all included participants were mentally healthy despite an increased exposure to microstressors (≥ 30 daily hassles). This fact, together with the ability to recover from

stress as proxy measure of resilience, supports the assumption that the participants already had a certain level of resilience at pretest. Similar to a pre-existing PB (see 3.4.2.2) and increased baseline levels in further constructs, that are associated with resilience (e.g., reappraisal; Appendix J4), thus, a ceiling effect might explain the non-significant effects on both outcomes (see Becker et al., 2016). In addition, some limitations concerning the outcome measures (see 3.4.3.4) might have reduced the potential training effects. These could include, for example, the unclear validity of mental health normalized by stressor exposure as operationalization of resilience (Kalisch et al., 2015).

Analogous to resilience, most previous CBM studies, including those on PBTs (e.g., Beadel et al., 2016; Becker et al., 2016; Rinck et al., 2013) did also not examine well-being due to their focus on disorder-specific outcomes. Thus, comparisons with the current study are difficult. However, the non-significant change in the WHO-5 found here is in contrast with the findings of Lukas (2019), who found positive effects in favor of a smartphone-based AAT training with the same SWIPE mechanism ("MT-Phoenix") in individuals with elevated depression scores. The lack of statistical significance for well-being might be interpreted in a similar way as the findings for resilience and the ability to recover from stress, including, for example, the general compared to disorder-specific training stimuli. As the WHO-5 includes general statements on well-being, a higher intensity of the mobile-based training might be necessary to trigger significant changes. As for the two outcomes above, the pre-post change in well-being was not predicted by the number of occurred stressors (or by the baseline AS). However, additional (non-measured) stressors that arose from the training as well as criticism concerning the design (see 3.4.3.1) might, at least in part, have reduced the particiants' well-being and limited potential effects. A ceiling effect is unlikely given that TRAIN$_4$Positivity subjects showed lower WHO-5 scores at pretest than a representative German sample (Brähler et al., 2007).

3.4.2.4 Effects of PBT on Psychological EMA Outcomes

Except for the ability to distance from negative stimuli, none of the psychological EMA outcomes that were assessed within "Breezly" after each training session (current mood subscales) or at the end of each day (ability to get affected by positive stimuli, well-being) significantly changed

during the training. However, due to the high proportion of missing data (38% for current mood, 47% for end-of-day measures), these findings might not be as meaningful as those for other study outcomes. As, to date, mobile-based positivity bias interventions were not investigated, they are also difficult to relate with previous research. The significant increase in the ability to distance from negative stimuli points to the importance of also including negative pictures in the PBT instead of an intervention that only fosters the approach of positive stimuli. This is consistent with the results of Ferrari et al. (2018) who concluded that only a PBT with both components (i.e., approach positive stimuli, avoid/distance from negative stimuli) effectively induced a PB. As assumed by Ferrari et al. (2018), a posthoc explanation might be the personal salience of negative stimuli due to evolutionary reasons (Rozin & Royzman, 2001).

The difference in the significance of findings between the ability to get affected by positive stimuli and the ability to distance from negative things might, at least in part, be explained by the problems of some participants to clearly assign a positive or negative valence to the training stimuli (see 3.4.3.2). As indicated by the answers of participants in the evaluation of the "Breezly" (see Appendix J11), the movement of "swiping away" negative pictures was equally associated with the distancing from such stimuli than the "swiping closer" reaction with the approach of positive stimuli, excluding a potential explanation for the different findings for these EMA outcomes.

The assessment of the six outcomes was integrated into "Breezly" and, thus, also part of the mobile-based intervention. As discussed in detail in 3.4.3.1 and 3.4.3.2, these regular assessments in addition to the training might also have been experienced as stressful by some participants, probably also resulting in the high proportion of missings. Finally, the items used to assess the end-of-day outcomes were self-developed for this study, with unclear psychometric quality (see 3.4.3.4).

3.4.2.5 Effects of PBT on Cognitive ER (Reappraisal) and Emotional Experience

Despite a descriptive increase of reappraisal (CERQ subscale) between pre- and posttest, there was no significant effect of the action-level PBT on this outcome. This RQ was based on the assumption of the activation of motivational systems by approach and distancing behaviors (Cacioppo

et al., 1993; Neumann & Strack, 2000), that might also affect other levels of information processing (e.g., Becker et al., 2016; Kawakami et al., 2007), including cognitive ER. As described in 3.4.2.2, the PBT did not affect the action tendencies in the AAT, possibly resulting in an unsufficient activation of motivational systems, that also hindered a transfer to other levels of information processing, like ER (reappraisal).

This study also assumed *direct* positive effects of the AAT training on reappraisal – situation-focused reappraisal due to the approach movements to positive stimuli and self-focused reappraisal given the distancing from negative stimuli. The reasons for the lack of evidence for effects on reappraisal might be similar to the ones discussed for action tendencies (see 3.4.2.2), such as the unclear valence of training stimuli (see 3.4.3.2), the lack of control for correct and regular training, or an unsufficient training intensity. Furthermore, the current study does primarly allow conclusions concerning the lack of any effects on *situation-focused* reappraisal, as measured by the CERQ and ERQ, that are also limited by the study design (see 3.4.3.1). Although the MB-PBT might have fostered self-focused reappraisal by distancing movements to negative stimuli, these changes could not have been sufficiently reflected (see 3.4.3.4). Furthermore, the average use of reappraisal at pretest was higher than in other German samples (Appendix J4). Thus, in line with other outcomes (see 3.4.2.2, 3.4.2.3), a ceiling effect might explain the non-significant results. Finally, as reported qualitatively (Appendix J11), the presentation of negative stimuli in part also induced negative emotions. This (unplanned) mood induction might have required a certain level of reappraisal during the intervention, limiting the effects on this ER strategy.

When controlling for the stressor exposure and the pretest AS in the analysis of reappraisal, none of these variables had a significant impact on the change in the ERQ. However, there was a tendency for a smaller change[26] in reappraisal for subjects who experienced more macrostressors. The relationship between (negative) life events, reappraisal, and mental health might explain this effect. Macrostressors were shown as a risk factor for the development of stress-related mental disorders

[26] based on descriptive statistics with two decimals, slight increase of reappraisal (ERQ) from *M* = 4.79, *SD* = 1.06 to *M* = 4.84, *SD* = 0.87

(e.g., Bonanno, 2004). Cognitive reappraisal, however, is associated with mental health despite stressors and was suggested as resilience mechanism in PASTOR (Kalisch et al., 2015). Based on this theory, especially individuals with many macrostressors, who maintained their mental health, should frequently use reappraisal. In these participants, the effects of the MB-PBT might have been reduced due to ceiling effects. However, given that this finding could not be replicated with the CERQ reappraisal subscale, it should be interpreted with caution.

The non-significant effects of the PBT on emotional experience might partly result from the missing effects on reappraisal, that was shown to affect positive and negative emotions (e.g., Gross & John, 2003). The current study is not consistent with some previous CBM intervention trials that demonstrated positive effects on anger (Almoghrabi et al., 2017; Hawkins & Cougle, 2013) and general affect (Ferrari et al., 2016; Peters et al., 2011; Rinck et al., 2013; Taylor et al., 2011). These differences might also be explained by methodological aspects (see 3.4.2.1; e.g., different training stimuli). On the other hand – although assessed differently (mood ratings, e.g., “happy”, “sad” vs. PANAS, STAXI in this study) – the findings are comparable with Becker et al. (2016) who also found no effects of an AAT-based PBT on mood in unselected individuals or subjects after negative mood induction. As for reappraisal, neither the stressor exposure nor the baseline AS affected the change in the emotional experience over time. However, as discussed above, the tendency for larger improvements in some of the constructs (e.g., larger increase of positive affect) in subjects experiencing more microstressors might be explained by the more frequent use of reappraisal.

3.4.2.6 Construct Validity of the AAT as a Measure of PB at the Level of Action Tendencies

The missing correlation between the AAT-CS and optimism in this study is consistent with Gordon et al. (2016) who demonstrated trait optimism (LOT-R) as no *unique* predictor of positive interpretation bias. On the other hand, the current result contradicts several findings of optimists having positive tendencies at different levels of information processing (Isaacowitz, 2005; Segerstrom, 2001; Sharot, 2011), which might be explained by methodological differences. Isaacowitz (2005) used skin cancer pictures as negative stimuli, as those are perceived as potentially threatening and relevant

by most participants. Besides, a better recall of personally relevant (e.g., health-related) compared with irrelevant information was demonstrated for optimists (Abele & Gendolla, 2007; Aspinwall & Brunhart, 1996). The current study, however, used pictures that had been, on average, rated as positive or negative in norm samples, but were not necessarily personally relevant (see 3.4.3.2). Thus, the selected pictures might not have been sufficiently positive or negative or personally relevant to activate the assumed approach or avoidance/distancing motivation in optimists and to result in measurable differences in the AAT compared to less optimistic participants. The limited activation of motivational systems might be responsible for the missing representation of an optimistic bias in the AAT. The current sample mostly included individuals with (above-) average scores in trait optimism at pretest, with the small variance in optimism possibly limiting a correlation between the AAT-CS and the LOT-R. Finally, the missing relationship between LOT-R and AAT-CS might be explained by the nature of approach/avoidance motivation and resulting action tendencies in optimists. According to Tennen and Affleck (1987) and Sharot (2011), optimistic individuals may not respond adequately to negative stimuli due to an "unrealistic optimism". Chang et al. (2009) discussed that approach motivation to positive stimuli (e.g., winning money) is more relevant for optimists than avoidance motivation from negative ones (e.g., not losing money). Following Neumann and Strack (2000), these motivational orientations are in turn associated with action tendencies. Thus, a PB in optimists might manifest in the faster pulling of positive pictures in the AAT, but not necessarily a faster pushing of negative stimuli, as the latter is less relevant. Similarly, they might be slower in pushing positive stimuli, but not necessarily differ from less optimistic people in pulling negative stimuli. To calculate the AAT-CS, however, all these RTs are considered, possibly resulting in a less positive AAT-CS.

Optimism is also associated with the ability to recover from stress (BRS; Chmitorz, Wenzel, et al., 2018). In addition, a positive AS, that is characterized, inter alia, by internal, stable, and global attributions of positive events (i.e., high ASF-E-P), is viewed as component of optimism (Seligman, 1990). Therefore, some of the above explanations could also apply to the missing correlations between the AAT-CS and these outcomes, including the lack of personal relevance of the AAT pictures

and the limited variance in both outcomes at pretest. Regarding the missing correlation between the AAT-CS and perceived stress, no previous research is available. The finding could be viewed as inconsistent with previous studies that found an improved recovery from stressors after a PBT, which also increased positive action tendencies (see 3.4.2.3; e.g., Rinck et al., 2013). On the other hand, the comparability is restricted since the PSS-10 measures perceived stress in general for the past month and is different from the recovery from stressors.

This study found a significant positive correlation between the AAT-CS and the ASF-E-N. Participants with more internal, stable, and global attributions for negative situations showed a higher (more positive) AAT-CS. Becker et al. (2019) demonstrated a negative AAT-CS for highly depressive patients (*M* = –196), who are characterized by maladaptive cognitive schemas and a negative AS (i.e., low ASF-E-P and high ASF-E-N; Beck, 1987). For *slightly depressed* patients, however, even a positive AAT-CS (*M* = 57) was found, although this group is also likely to have a tendency for a negative AS. Thus, instead of indicating a positive AS, a positive AAT-CS might also manifest a *less negative* AS. On the other hand, this is not consistent with the current sample who included (mentally) healthy participants with average pretest values in both ASF-E scores, but only showed a slightly positive AAT-CS (*M* = 0.53) at baseline. Overall, due to the these findings, the question arises whether the AAT is an adequate measure for a PB at the action level or if another paradigm is needed (see 3.4.3.4).

3.4.3 Critical Appraisal

In this chapter, the strengths and limitations of this study are discussed. The study design and the MB-PBT are examined before critically assessing the sample, the measures, and statistical analyses.

3.4.3.1 Study Design

As a positive aspect of TRAIN$_4$Posivity, the lack of dropouts during the three-week study period and the overall acceptance of the intervention (see Appendix J11) support the feasibility of the study design. Due to the pilot character, TRAIN$_4$Positivity primarily used a single-group pre-post

design, involving the MB-PBT and EMA for the stressor monitoring. For one RQ, a matched control group design with a historical CG was used. The lack of a comparator for most RQs does not allow causal conclusions. Interim events (e.g., positive events), that were eventually not assessed in the study, might have resulted in natural improvements of the outcomes, such as perceived stress. Therefore, the results should be interpreted with caution. Altough not equivalent to RCTs, the PS matching for RQ1 (exploratory RQ 1.2) at least offered the possibility of considering pretest differences in demographic variables between the groups (Kuss et al., 2016). The procedure creates covariance balance between groups (Elze et al., 2017). Nevertheless, the differences in study design between TRAIN$_4$Positivity and LifeStress (e.g., three weeks vs. four weeks, weekly stressor monitoring vs. daily and weekly stressor monitoring) might be a limitation.

Due to scheduling issues, the time periods between the screening and the pretest could not be standardized for all participants (i.e., screening not performed on same day as pretest for n = 7 [17.1%]). As a consequence, the variables surveyed (e.g., exposure to microstressors) might have changed, leading to a reduced comparability with the remaining sample. Similarly, in previous studies, that aimed to induce a PB (e.g., Becker et al., 2016; Ferrari et al., 2018), the pre- and posttest as well as the training were performed within *one* session. In the current study, however, the screening/pretest and the first training session were at least several hours up to one day (in single cases) apart; the same applied to the time period between the last training session and the posttest. In case of interim events (e.g., microstressors), especially the time lag between the end of training and the posttest might have reduced the training effects.

To avoid picture-specific practice effects, that could be assumed for previous studies (see 3.1.4), the current study used different stimuli for the intervention and the AAT assessment. However, the pictures were selected based on the same criteria to ensure comparability.
With the aim of improving the ecological validity compared to previous studies (e.g., Becker et al., 2016), TRAIN$_4$Positivity included the development of a mobile app to deliver the intervention. By using EMI, participants were enabled to train at any time and any location. Compared to laboratory studies,

that were mostly used in CBM training research (see 3.1.4), the self-guided PBT probably had a larger impact on the subjects' daily experiences, also facilitating the transfer to daily life.

To standardize the implementation of the intervention, the participants received identical study smartphones with the pre-installed "Breezly" app. It is worth mentioning that carrying the additional smartphone was experienced as a stressor by some participants. Several subjects reported to have felt under pressure to perform the training at specific day times. Based on findings that permanent availability is an important stressor in the German population (Techniker Krankenkasse, 2016), the latter should be critically viewed and might have partly reduced the effects of the PBT.

Furthermore, the pulling and pushing movement of affective pictures in previous studies (e.g., Becker et al., 2016; Becker et al., 2019; Ferrari et al., 2018; Rinck et al., 2013) was performed in the same way and using the same device for the assessments and the training. By using a mobile app to deliver the intervention and the computer assessment at pre- and posttest, however, the pictures were moved differently in TRAIN4Positivity. While approach and distancing movements in "Breezly" were performed by swiping on the display, participants had to pull a computer mouse toward them or push it away in the AAT assessment. Compared with intervention studies in the laboratory, especially the different approach and avoidance behaviors in "Breezly" might also have affected the participants' motivational orientation and the cognitive processing of positive and negative stimuli differently (Neumann & Strack, 2000). For example, in contrast with broader arm movements when pushing/pulling the computer mouse, the finger movements on the smartphone display might not have been intense enough to induce profound changes, possibly indicating the need of a higher intensity of the MB-PBT.

The different stressor exposure of individuals during the intervention might have been an important confounder in this study. Stressor exposure may activate maladaptive schemas and negatively affect the information processing (Beck, 1967; Beck, 1976; Beck, 1987; Eberhart et al., 2011). As such an effect might also reduce the efficacy of the PBT, the participants' stressor exposure was assessed (e.g., using weekly EMA) and considered as a covariate. The EMA stressor monitoring

asked participants to think of occurred microstressors in the previous week. It is likely that the subjects were re-exposed indirectly to these stressors and, by surveying the associated severity, were also required to deal with them emotionally. Thus, it cannot be ruled out that this assessment increased the participants' attention to negative aspects and counteracted the actual PBT. This might also explain the lack of statistical significance for training effects in all RQs that were answered based on the single-group design. On the other hand, regarding the exploratory RQ 1.2, there was no significant difference in the perceived microstressor severity between TRAIN$_4$Positivity (intervention + stressor monitoring) and LifeStress (only stressor monitoring). Instead, this comparison – controlling for potential negative effects of (weekly) stressor monitoring – indicated a tendency for a higher perceived severity in participants of the MB-PBT, that is, the partcicipants were rather sensitized to stressors in daily life. The latter might also be explained by certain stressors imposed by the intervention (see above).

3.4.3.2 Mobile-Based Positivity Training

Overall, based on the qualitative feedback, the MB-PBT, especially its easy handling and short duration, was rated positively. The avatars and the design were positively highlighted. Some subjects reported that the training made them think more about how to deal with positive and negative aspects in daily life. On the other hand, some participants noted that they found it difficult to clearly assign a (positive vs. negative) valence to the pictures, rendering the training difficult as it was not clear whether they should swipe a picture closer or away. Since the stimuli in the AAT assessment were selected using the same criteria, this criticism could possibly be transferred to the assessment of action tendencies. However, only by recognizing the valence of a stimulus, an individual's motivational orientation could be activated, resulting in action tendencies (see 3.1.5.1).

Furthermore, the presentation of negative stimuli was described as mood-pressing by some individuals. This induction of negative emotions caused by the training stimuli might have reduced the effects found. To avoid mood-inducing effects, some CBM studies to foster a PB in patients with mental symptoms (Becker et al., 2019; Rinck et al., 2013; Vrijsen et al., 2018) used neutral stimuli

instead of negative pictures. However, some studies (e.g., Ferrari et al., 2018) demonstrated that a PB was effectively induced in those trainings that included to push away negative stimuli – a finding that supports the use of ngative stimuli in this study. As another negative aspect, some participants remarked the missing personal relevance of the pictures. Thus, comparable with Lukas (2019), the efficacy of training might be improved by giving participants the possibility to individually select stimuli of personal relevance before the training. The swiping of personal positive and negative pictures might trigger a stronger reaction (e.g., faster RT, emotional response and, thus, cognitive processing of the stimulus) in participants, leading to better effects. However, letting the subjects collect individual stimuli before the training would be equivalent to a separate intervention. As this feasibility study primarily aimed to examine the effects of approach and distancing movements to affective stimuli, the individual selection of pictures was deliberately refrained from.

Negative feedback concerning the recommended training periods was also received by some participants. The periods were chosen to help participants to start the day with increased positive awareness and to assess experiences of the past day more positively. Due to various daily rhythms, however, some participants criticized the fact that the reward system assigned more points for performing training sessions between 7.00 to 11.00 a.m. and 7.00 to 12.00 p.m. One participant suggested to let the app users set own training periods based on individual daily rhythms. The missing control for the frequency and correctness of the MB-PBT might also be a limitation. To promote regular training, a reward system was integrated (see Appendix H5). In addition, to ensure the correctness of applying the PBT, participants were given detailed instructions at the beginning of the study and they received additional written information. Nevertheless, all participants were included in the analyses, irrespective of the number and correctness of performed sessions. Therefore, the lack of evidence for effects could at least partly be due to an inadequate and/or irregular training application.

Using the avatar that led participants through the training and the reward system, the training was designed to have a playful character. The reward system was based on the COR theory (Hobfoll et al., 2018; Appendix H5) and aimed to increase the participants' motivation. However, in contrast with

the original intention, many participants reported to feel rather demotivated by the minus points for omitted training sessions. This might have led to a decrease of the participants' mood and a negative attitude toward the intervention, in turn resulting in less regular training and smaller effects.

Finally, there were unpredictable technical difficulties which might have affected the efficacy of the training. For five participants, the mobile app stopped unexpectedly during a session, with further training only possible by deleting and reinstalling "Breezly". Since these participants also had to repeat the rating of pictures in the beginning, training sessions within the 21 days were not performed using the same picture set compared to the remaining sample.

3.4.3.3 Sample

The present work was a feasibility study, intended to provide initial indications of the efficacy of a PBT at the action level in healthy individuals with increased stressor exposure. Clinical diagnoses of mental disorders were an exclusion criterion, while an increased exposure to microstressors – a risk factor for the development of mental disorders (see 1) – was an inclusion criterion. The focus was not on investigating a representative sample. With $N = 41$, the current sample was small, limiting the analyses (see 3.4.3.5). Based on the age criterion of 18–30 years to ensure comparability with LifeStress, conclusions about the efficacy of the MB-PBT in older individuals are restricted.

Participants were recruited among students of various fields using different methods (see 3.2.2.1) to prevent selection effects. Nevertheless, due to the lack of randomization and given that most subjects were psychology students and 80.5% were female, a selection bias is rather likely. As demonstrated by previous research (see also 2.4.2), women seem to be more motivated to participate in interventions to foster their mental health in general. Furthermore, psychology students might have been more interested in participating in the PBT as the study aims were presented to be related with resilience and adaptive ER, that are also covered by psychology courses. The previous participation in an intervention to promote resilience was an exclusion criterion. Nevertheless, psychology students at least have basic knowledge about this concept. The expertise concerning psychological contents might also have affected their answers. Due to potential ceiling effects in outcome variables and, thus,

limited variance between subjects, training effects might have been reduced. This is in line with the finding that this sample showed higher pretest scores in resilience-related outcomes than other German samples, including optimism (LOT-R) and cognitive reappraisal (ERQ, CERQ), for example.

As discussed above (see 3.4.2.2), a PB at different levels might already have existed among the included participants at baseline. Even more restrictive eligibility criteria had been originally planned, including a lack of a PB. However, due to the pilot character and potential difficulties in recruiting participants, the criterion of no PB was omitted from the final study. Instead, the participants' AS at pretest was considered in the analyses. The restriction to healthy participants despite an increased stressor load might have been an important limitation and might affect the comparability with clinical samples that were mostly investigated in other CBM studies (see 3.1.4, 3.1.6). As the participants of TRAIN4Positivity were already resilient before the training, they probably had an optimistic attitude and a PB at different levels, leading to ceiling effects concerning the effects of training – a typical problem in resilience intervention research. Previous studies (e.g., Becker et al., 2016) also only found effects of an AAT-based PBT for unselected individuals with induced negative mood. Hence, the findings of this study arise the question of whether this intervention is meaningful in healthy individuals or if it should be rather offered to risk groups (e.g., with missing PB, increased perceived stress, and/or mental symptoms).

3.4.3.4 Measures

The stressor exposure was assessed using the MIMIS and the LE Checklist which was considered to operationalize resilience (see 3.2.7.3) and to control for the potential impact of stressor exposure. The perceived microstressor severity was only analyzed at posttest (RQ1, exploratory RQ 1.2). Nevertheless, it could also be important to precisely map an individual's stressor load and to be able to better control for it. To ensure an increased microstressor exposure, only participants with a MIMIS sum score of ≥ 30 were included. This value was determined by the study team (i.e., not theoretically founded). Thus, it is unclear whether a value ≥ 30 represents a high stressor exposure or if a higher cut-off should have been used.

Based on the suggested “Resilience score” (R score; Kalisch et al., 2015), resilience was operationalized as change in mental health, normalized by the individual stressor load. To date, the validity and reliability of this measure were not investigated, meaning that no statements can be made on wether this outcome really measures resilience and requiring that conclusions are drawn carefully. Although, in general, good reliabilities of the outcome measures used in this study had been found, for example for the BRS (α = .85; Chmitorz, Wenzel, et al., 2018), the internal consistencies in the current study at pre- or posttest (or both) were partly questionable (e.g., BRS: α = .66 at posttest). In addition, the psychological EMA outcomes (end-of-day measures) were assessed using self-developed items, resulting in unknown psychometric quality.

To assess potential changes in the use of reappraisal, the respective subscales of the ERQ and of the CERQ were used. Both measures rather seem to focus on situation-focused reappraisal (Abler & Kessler, 2009; Garnefski et al., 2002; Gross & John, 2003; Loch et al., 2011). Self-focused reappraisal, that might have been increased by distancing from negative stimuli in “Breezly”, was probably not represented in sufficient detail. However, to date, no self-report questionnaire to specifically measure the distancing component of reappraisal seems to be available.

In previous CBM studies at the level of action tendencies, a positive or negative AAT-CS was equated with the presence of differently biased action tendencies, respectively. As a secondary outcome, the current study also examined the AAT-CS to assess the potential effect of the MB-PBT on a PB at the action level. However, to date, the score not been validated as a measure of PB. The lack of statistically significant correlations between the AAT-CS and different measures related to a PB (e.g., optimism) initially provides no evidence for the construct validity of this measure. Thus, the values of the AAT-CS might not be equated with the presence of positively or negatively biased action tendencies, limiting the interpretation of the effects of the PBT on implicit action tendencies.

As a potential moderator of training effects and as a covariate, the participants’ AS was assessed using the ASF-E, measuring the internality, stability, and globality of attributions for positive and negative events. The calculation of one ASF-E total score is generally not recommended (Poppe et

al., 2005). Moreover, to artificially create a total score for AS based on a median split (e.g., positive AS: individuals with ASF-E-P scores ≥ median AND ASF-E-N scores < median) would have resulted in an unequal distribution of participants and a data loss for *n* = 20 participants who had either high or low values in both ASF-E total scores. Both of the suggested total scores for positive and negative events were used in the current analyses, rendering the interpretation of results partly difficult. Since median splits were applied, information might also have been lost, reducing the power of subsequent analyses (Cohen, 1983). On the other hand, median splits are also an accepted method (Iacobucci et al., 2015).

3.4.3.5 Statistical Analyses

Statistical analyses in this study were carefully planned and their prerequisites were tested. Nevertheless, some critical aspects have to be considered. Due to the pilot character and the small sample size, outliers were not removed from the analyses after checking the plausibility of values (Eid et al., 2017), possibly biasing the results in the small sample (Field, 2015).

To control for the potential impact of covariates on the pre-post changes in the outcomes, multiple linear regression analyses were conducted for exploratory reasons, although none of the preceding paired *t* tests or WSR tests provided evidence for an outcome change. However, as a sample size of 10 to 15 participants per predictor is recommended (Field, 2015), the statistical power of these analyses might have been limited. Moreover, when testing the assumptions of multiple linear regression, outliers and influential data points (mostly single values) were included for some outcomes (e.g., perceived stress), that might have biased the analysis. Due the exploratory design of this study, the small sample size, and the minimum exceeding of cut-off limits, these values were not excluded. The originally planned regression analyses with six predictors were not conducted due to the lack of statistical significance in the parsimonious model. Thus, the potential impact of the participants' level of empathy and social desirability on the effects of training could not be examined.

For the PS matching, a strength was the testing of different matching algorithms to identify the one with the best matching quality. Following the PS matching, an independent *t* test was used to compare both groups at posttest, which was chosen due to the pilot character of this study. However,

as some authors suggested to consider the matched data structure when performing post-matching tests (e.g., Austin, 2009), further analyses might be necessary (e.g., paired *t* tests, multilevel analysis).

With respect to the analysis of RQ4 regarding psychological EMA outcomes, the high proportion of missing data (see 3.3.1) can be explained by the feasibility character of this study. As the focus was on the acceptance of the mobile-based intervention, the participants' compliance with these assessments was not measured. The MI of missing data could be critically assessed. Besides, due to the large amount of missings and multicollinearity between the end-of-day measures, the MI could not be performed simultaneously for the three outcomes to be imputed.

Finally, the large number of single analyses (see Appendix I4), that were conducted given the exploratory character of this study, has to be mentioned. To counteract the problem of multiple comparisons (i.e., false-positive results), a Bonferroni correction was used. This conservative method resulted in a high level of significance ($\alpha = .001$), rendering it difficult to detect potential effects.

3.4.4 Implications for Practice

Based on the current findings, several implications for practice can be derived. First, given the qualitative feedback of participants, the use of personally relevant stimuli for the MB-PBT might result in a higher acceptance and more regular training, potentially also improving the intervention effects. For example, in a further development of "Breezly", participants might be instructed to collect a set of personally relevant positive and negative pictures, including motives that they are confronted with in their daily life. After this first component of training, which might already have positive effects (e.g., increased awareness for positive aspects), the app-based picture training could be conducted as usual. Despite the advantages of a low-threshold and cost-effective intervention using "Breezly", a longer training duration (i.e., > 3 weeks) but with less or even no recommended training periods, that is, more flexibility, might be appropriate. By allowing participants to perform training sessions when most needed (e.g., after a stressor) and by adapting the intensity to their individual rhythm, the benefits for psychological outcomes could be increased. Finally, the reward system of "Breezly" should be adapted by removing the punishment for omitted sessions. Combined with the avatar, the

exclusive reward for completed sessions (e.g., using graphical presentations, such as a pedometer indicating daily points and adding something to the avatar if the daily target was achieved) might even increase the game-like character of the training, with potential positive effects for acceptance and efficacy.

3.4.5 Implications for Research

This pilot study has also several implications for future research in this field. First, the validation of the AAT as a measure of PB should be further focused to allow reliable conclusions about the efficacy of a PBT at the action level. As this was a pilot study, primarily interested in the general feasibility and potential effects of the newly developed "Breezly" app, there is an urgent need for improved study designs. Larger sample sizes for increased statistical power in data analysis, FU assessments, and RCTs are indicated. Future studies should test the (adapted) training against a wait-list control first and then an attention control (e.g., alternative mobile-based training), to allow fair comparisons. Based on additional components of the app-based training (see 3.4.4), dismantling designs might be desirable to detect the most effective element (e.g., selection of personal pictures vs. AAT-based picture training). Given that participants of this study were included based on a rather arbitrary cut-off for the exposure to microstressors, upcoming studies might benefit of an empirically founded cut-off, ideally based on (representative) norm values for the MIMIS. The current study also arises the question about whether the perceived severity of micro- and macrostressors should be focused more in future research to better control for the individual stressor exposure in the analyses. To date, there seems to be no questionnaire that specifically measures the distancing component of reappraisal. However, to allow conclusions about the efficacy of "Breezly" on situation- *and* self-focused reappraisal, different scales for both forms of reappraisal are needed. Given the rather negative feedback regarding the use of a separate study smartphone, future studies should consider the delivery by personal smartphones. Finally, compared to healthy individuals examined here, future research should deal with the question whether this newly developed intervention would be more effective in participants who are more likely to have no PB at pretest, such as individuals reporting

mental symptoms or with a clinical diagnosis of mental disorder. Provided that the general efficacy of the MB-PBT is proved in these groups, the intervention effects might also be examined in individuals aged above 30 years and specific risk groups (e.g., HCP; see 2.1.1).

3.4.6 Conclusion

Based on the potential protective role of a PB for stress-related mental disorders, this pilot study examined the feasibility and efficacy of a MB-PBT at the level of action tendencies in healthy participants with many microstressors. Controlled for stressor load and the AS at baseline, the intervention showed no significant effects on perceived stress, the perceived microstressor severity compared to a matched CG (only stressor monitoring), and implicit action tendencies to affective stimuli, with the training effect on action tendencies not moderated by the baseline AS. There was also no evidence for any effect on resilience, the ability to recover from stress, well-being, or psychological EMA outcomes (e.g., current mood). Finally, the study found no significant change in cognitive reappraisal or the participants' emotional experience by the training. Only the ability to distance from negative stimuli was partly shown to improve during the intervention. Given the mostly non-significant correlations with possibly related (e.g., trait optimism) and unrelated constructs (e.g., perceived stress) in this study, the construct validity of the AAT as a measure of PB was not supported. Despite non-significant results, a tendency for improvements for several outcomes (e.g., perceived stress, AAT-CS, anger control) was demonstrated, indicating potential positive effects of the PBT. Thus – considering the above-mentioned critical points and the implications for research and practice – the current study indicates the need for further studies concerning the efficacy of the MB-PBT in different populations.

4 General Conclusion

By using two complementary approaches (systematic review, empirical study), this thesis makes several contributions to resilience intervention research. First, weaknesses of current primary studies were revealed by the *systematic Cochrane review* on resilience interventions in HCP. In finding positive effects of resilience-training programs, the review is mostly consistent with previous reviews, although hardly any conclusions can be made regarding the maintenance of effects over time, given the lack of long-term data. Probably due to the unbalanced weighting of studies in subgroup analyses, no effect modifiers were found. In addition, because of the limited methodological rigor in many studies, the certainty of evidence was very low, meaning that future studies in the field might change the pooled effect estimates. Nevertheless, the review identified several promising approaches that may support HCP to maintain their mental health despite the exposure to stressors. Cautious conclusions regarding the need of adapting training contents (e.g., stronger focus on resources) and innovative delivery are possible. The second part of this thesis took this up by evaluating an app-based training to foster the resilience factor PB. The *pilot study* indicates that a MB-PBT, conducted as microintervention several times a day, is feasible. Although an improvement of the app (e.g., technical aspects) and stimuli (e.g., personal pictures) seems needed, the picture training was largely accepted. By using a (newly developed) smartphone app – a delivery not used to foster a PB at the level of action tendencies to date – and the combination of different assessments (e.g., EMA), the study closes a gap that has been demonstrated by previous reviews in the field of CBM and the Cochrane review in this thesis. Based on the qualitative feedback, especially the implementation of training in everyday life was probably improved compared to “artificial” settings by using the “Breezly” app. Furthermore, TRAIN$_4$Positivity distinguishes from other studies by controlling for the individual stressor load. Given the pilot character, several limitations highlighted by the Cochrane review were also evident here. The non-significance of results for most RQs might partly result from the large number of outcomes and the correction for multiple testing. Thus, the study is a starting point for the future development of MB-PBT. To sum up, both projects clearly indicate the need for further (high-quality) research.

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List of Appendices

Appendix Systematic Cochrane Review and Meta-Analysis on Psychological Interventions to Foster Resilience in Healthcare Professionals

Appendix A Details Concerning the Theoretical Background of the Cochrane Review – Previous Research on Resilience-Training Programs

Appendix A1 Effects of Resilience-Training Programs on Psychological Outcomes in HCP

Previous reviews and meta-analyses largely reported positive effects of resilience interventions on psychological outcomes in adults. Certain reviews used wider eligibility criteria and identified more RCTs than others (Joyce et al., 2018; Leppin et al., 2014; Macedo et al., 2014; Robertson et al., 2015; Vanhove et al., 2016). Among these, Macedo et al. (2014; seven RCTs in non-clinical samples of adults), whilst not pooling the data, concluded overall promising effects of resilience-training programs on resilience and resilience factors. Similarly, Robertson et al. (2015; eight RCTs) found benefits for personal resilience, mental health, well-being, and work performance in employees.

Concerning short-term effects of resilience training, small positive effects on resilience at posttest and within 3 months postintervention were identified (Joyce et al., 2018, 17 RCTs; Leppin et al., 2014, 25 RCTs; both in diverse adult populations). Besides, Vanhove et al. (2016; 14 RCTs, focus on workplace) showed small proximal effects (≤ 1 month after training) on well-being and a decrease of psychological deficits (e.g., depressive symptoms). Leppin et al. (2014) also demonstrated moderate short-term effects on depressive and stress symptoms for trauma-focused, but not for generalized (stress-directed) resilience trainings.

Among systematic reviews in HCP, of whom most did not solely focus on resilience interventions (i.e., limited number of RCTs), certain benefits of training were also concluded (e.g., Cleary et al., 2018; Fox et al., 2018). For example, Pezaro et al. (2017) reported positive outcomes for work-based resilience workshops (e.g., McDonald et al., 2013) in midwives (e.g., improved well-being and resilience, reduced stress and anxiety). Rogers (2016) identified evidence for resilience-enhancing effects of resilience workshops, cognitive behavioral, and problem-solving trainings in healthcare workers.

Regarding longer follow-up (FU) periods in previous reviews, Vanhove et al. (2016) determined maintained effects of resilience training only for the prevention of psychological deficits at more than 1 month after the training, while Joyce et al. (2018) only found evidence for a positive effect of mindfulness interventions on resilience at 6-month FU.

Appendix A2 Impact of Intervention Setting of Resilience-Training Programs

Advantages of group settings may include mutual learning and support (Stangier et al., 2003) and the possibility to receive feedback from others (Shaffer et al., 1981). By listening to group members with similar problems, participants could recognize their problems as not individual and gain a deeper insight into own issues (Yalom & Leszcz, 2008). Group-based resilience training might indirectly enhance the participants' social support and hence resilience (e.g., Toseland et al., 1990). On the other hand, individual problems might receive less attention than in one-on-one settings (Chen et al., 2003; Stangier et al., 2003). Besides an increase of avoidance behaviors, group-based interventions might consolidate anxieties and dysfunctional thoughts by mutual reinforcement (Stangier et al., 2003). In individual settings, however, trainers might also be better able to attend specifically to participants' needs and provide more feedback (Vanhove et al., 2016).

Appendix A3 Impact of Delivery Format of Resilience-Training Programs

Other delivery formats that are increasingly used in addition to face-to-face trainings include online-/mobile-based (e.g., Bekki et al., 2013) or telephone interventions (e.g., Farchi & Gidron, 2010), bibliotherapy (e.g., Sharma et al., 2014), or multimodal delivery (Loprinzi et al., 2011). The possibility of personalization, training diversity, and higher treatment motivation were highlighted as advantages of online- and mobile-based trainings (Konrath, 2014; Portnoy et al., 2008; Sitzmann et al., 2006). Due to higher anonymity, participants might also be more willing to engage and share personal information (Tate & Zabinski, 2004). Further advantages include 24/7 availability, cost-effectiveness, and low access threshold (Cuijpers et al., 2017; Lehr et al., 2018). Telephone interventions are also characterized by increased accessibility, flexibility, and anonymity compared to face-to-face settings (Mozer et al., 2008). The benefits of bibliotherapy (i.e., self-help books) are primarily seen in its non-stigmatizing and easy use (Cuijpers, 1997). The following disadvantages of these formats might be viewed as positive aspects of face-to-face delivery. Especially the use of modern technologies and bibliotherapy requires certain skills (e.g., internet skills, reading/writing skills) and pronounced self-reflection ability (Erbe et al., 2017). Furthermore, the non-binding character could affect intervention effects negatively (Tate & Zabinski, 2004). For instance, Robertson et al. (2015) reported high dropout rates for online-delivered resilience trainings. Vanhove et al. (2016) emphasized that interventions implemented face-to-face could work better due to the more direct trainer/participant contact.

Appendix A4 Impact of Intensity of Resilience-Training Programs

Advantages of low-intensity training programs might include cost-effectiveness and maintenance of treatment motivation, with positive effects on compliance and intervention effects. High-intensity trainings might become counterproductive as the same training techniques are repeated for a long period of time, whereas interventions of short duration require both participants and intervention providers to establish and maintain a focus throughout the intervention process (Juul et al., 2019). On the other hand, subjects might benefit from longer resilience trainings as they have more possibilities to apply the learned strategies in daily life, to deepen acquired skills (e.g., to cope with stressors), and to modify more basic aspects of their lifestyle (e.g., social relationships) or general attitudes.

Appendix A5 Impact of Theoretical Foundation of Resilience-Training Programs

For example, Ruiz (2012) identified no evidence for differences between CBT and ACT for depression and anxiety, but found a positive trend for ACT on depression outcomes. Cuijpers, Berking, et al. (2013) found CBT to be more effective for depression than PST. However, the effects of certain theoretical approaches were not contrasted (e.g., ACT vs. PST, CBT vs. AIT).

In resilience intervention research, the efficacy of training programs depending on their theoretical basis has also been examined (e.g., Dray et al., 2017 in children and adolescents; Joyce et al., 2018). Only few individual studies – none of them in HCP – directly compared the effects of different theoretical approaches. For example, Hallowell (2010) compared bibliotherapy based on cognitive therapy and ACT with a wait-list control in teachers. In this RCT, changes in outcomes (e.g., depression) over time did not differ by treatment group.

Appendix B Methodological Details of the Cochrane Review

Appendix B1 General Aspects Concerning the Search Strategy

The original search syntax (Helmreich, Kunzler et al., 2017) was divided into four parts. Part 1 included terms related to the concept of resilience (e.g., "resil*", "hardiness", "posttraumatic growth"; linked with Boolean operator "OR"). This part also contained broader terms associated with resilience (e.g., "positive adjustment", "bouncing back"). Part 2 referred to different forms of therapy and intervention (e.g., cognitive behavior therapy; linked with "OR"). Part 1 and 2 were linked using "AND". The third block combined "resilience" and "hardiness" with several training terms (e.g., "intervention") and was connected with the first two parts using "OR". In part 4, the search was restricted to RCTs, humans, and the time period 1990 onwards.

The following databases and trial registers were searched, with the time periods in parentheses reflecting the time frame contained in the respective database or trial register. Cochrane Central Register of Controlled Trials (CENTRAL; 2019 Issue 6[28]) in the Cochrane Library (searched 26 June 2019), MEDLINE Ovid (1946 to 21 June 2019), Embase Ovid (1974 to 2019 Week 25), PsycINFO Ovid (1806 to June Week 3 2019), CINAHL EBSCOhost (Cumulative Index to Nursing and Allied Health Literature; 1981 to 24 June 2019), PSYNDEX EBSCOhost (1977 to 24 June 2019), Web of Science Core Collection Clarivate[29] (1970 to 26 June 2019), International Bibliography of the Social Sciences ProQuest (IBSS; 1951 to 25 June 2019), Applied Social Sciences Index & Abstracts ProQuest (ASSIA; 1987 to 24 June 2019), ProQuest Dissertations & Theses (PQDT; 1743 to 24 June 2019), Cochrane Database of Systematic Reviews (CDSR; 2019 Issue 6) in the Cochrane Library (searched 26 June 2019), Database of Abstracts of Reviews of Effects (DARE; 2015 Issue 4) in the Cochrane Library (final issue; searched 27 October 2016), Epistemonikos (all available years), and ERIC EBSCOhost (Education Resources Information Center Institute of Education Sciences; 1966 to 26 June 2019). In addition, the following trial registers were searched electronically from 1 January 1990 to 24 June 2019: Current

[28] including the Cochrane Developmental, Psychosocial, and Learning Problems Specialized Register

[29] (Science Citation Index [SCI]; Social Science Citation Index [SSCI]; Conference Proceedings Citation Index-Social Science & Humanities [CPCI-SSH]; Conference Proceedings Citation Index-Science [CPCI-S])

Controlled Trials (controlled-trials.com), ClinicalTrials.gov (clinicaltrials.gov), World Health Organization International Clinical Trials Registry Platform (WHO ICTRP; who.int/trialsearch).

Appendix B2 Search Strategies January 1990 to October 2016 (1st Search)

To get a comprehensive understanding of the evidence in the field of psychological resilience interventions, and to identify training programs that can really be assumed to enhance resilience in adults based on scientific findings, a literature search was performed that combined and complemented the search approaches from previous reviews and meta-analyses. In contrast to the search strategy of previous publications (Joyce et al., 2018; Leppin et al., 2014; Robertson et al., 2015; Vanhove et al., 2016), who used very narrow search terms (e.g., "resilience program" or "hardiness training"), broader intervention terms were also searched. These were based on the search performed by Macedo et al. (2014), but were also supplemented by new terms (e.g., "acceptance and commitment therapy", "stress management", "mindfulness"). As example, the search strategy for MEDLINE Ovid based on the original inclusion criteria for this review (see Appendix D17.1) is presented below. The search strategies for the remaining databases are presented in Appendix D3.

MEDLINE OVID

Searched 28 October 2016 [6723 records]

1 Resilience, Psychological/
2 social adjustment/
3 Adaptation, Psychological/
4 (post-traumatic growth or posttraumatic growth or stress-related growth).tw,kf.
5 (positiv$ adj1 (adapt$ or adjust$)).tw,kf.
6 (psychol$ adj1 (adapt$ or adjust$)).tw,kf.
7 (resilien$ or hardiness$).tw,kf.
8 (cope or coping).tw,kf.
9 ((withstand$ or overcom$ or resist$ or recover$ or thriv$ or adapt$ or adjust$ or bounc$ back) adj5 (stress$ or trauma$ or adversit$)).tw,kf.
10 or/1-9
11 exp psychotherapy/
12 Stress, Psychological/th
13 (psychotherap$ or psycho-therap$).tw,kf.
14 (behav$ adj3 (intervention$ or program$ or therap$)).tw,kf.
15 ((cognit$ or cognitive behavior$ or CBT) adj3 (intervention$ or program$ or therap$)).tw,kf.
16 (psycho$ adj3 (intervention$ or program$ or therap$)).tw,kf.
17 relaxation.tw,kf.
18 mindful$.tw,kf.
19 (counsel?ing or coaching).tw,kf.

20 (third wave adj (psycho$ or therap$)).tw,kf.
21 cognit$ restructur$.tw,kf.
22 positive psychology.tw,kf.
23 (reframe$ or re-fram$ or reapprais$).tw,kf.
24 (stress adj1 (inoculation or manag$ or reduc$ or resist$)).tw,kf.
25 (anxiety adj3 manage$).tw,kf.
26 "acceptance and commitment ".tw,kf.
27 Combined Modality Therapy/
28 (multimodal or multi-modal or combined modal$).tw,kf.
29 exp Health promotion/
30 (health adj3 (educat$ or promot$)).tw,kf.
31 or/11-30
32 10 and 31
33 (resilien$ adj5 (train$ or program$ or intervention$ or promot$ or prevent$ or enhanc$ or learn$ or teach$ or educat$ or increas$ or develop$ or manag$ or therap$ or protocol$ or treat$)).tw,kf.
34 (hardiness$ adj5 (train$ or program$ or intervention$ or promot$ or prevent$ or enhanc$ or learn$ or teach$ or educat$ or increas$ or develop$ or manag$ or therap$ or protocol$ or treat$)).tw,kf.
35 or/32-34
36 randomized controlled trial.pt.
37 controlled clinical trial.pt.
38 randomi#ed.ab.
39 placebo$.ab.
40 drug therapy.fs.
41 randomly.ab.
42 trial.ab.
43 groups.ab.
44 or/36-43
45 exp animals/ not humans.sh.
46 44 not 45
47 35 and 46
48 limit 47 to yr="1990 -Current"

Appendix B3 Search Strategies October 2016 Onwards (2nd Search)

MEDLINE OVID

Searched 25 June 2019 [725 records]

1 Resilience, Psychological/
2 social adjustment/
3 Adaptation, Psychological/
4 (post-traumatic growth or posttraumatic growth or stress-related growth).tw,kf.
5 (positiv$ adj1 (adapt$ or adjust$)).tw,kf.
6 (psychol$ adj1 (adapt$ or adjust$)).tw,kf.
7 (resilien$ or hardiness$).tw,kf.
8 (cope or coping).tw,kf.
9 ((withstand$ or overcom$ or resist$ or recover$ or thriv$ or adapt$ or adjust$ or bounc$ back) adj5 (stress$ or trauma$ or adversit$)).tw,kf.
10 or/1-9
11 exp psychotherapy/
12 Stress, Psychological/th
13 (psychotherap$ or psycho-therap$).tw,kf.
14 (behav$ adj3 (intervention$ or program$ or therap$)).tw,kf.
15 ((cognit$ or cognitive behavior$ or CBT) adj3 (intervention$ or program$ or therap$)).tw,kf.
16 (psycho$ adj3 (intervention$ or program$ or therap$)).tw,kf.
17 relaxation.tw,kf.
18 mindful$.tw,kf.
19 (counsel?ing or coaching).tw,kf.
20 (third wave adj (psycho$ or therap$)).tw,kf.
21 cognit$ restructur$.tw,kf.
22 positive psychology.tw,kf.
23 (refram$ or re-fram$ or reapprais$).tw,kf.
24 (stress adj1 (inoculation or manag$ or reduc$ or resist$)).tw,kf.
25 (anxiety adj3 manage$).tw,kf.
26 "acceptance and commitment ".tw,kf.
27 Combined Modality Therapy/
28 (multimodal or multi-modal or combined modal$).tw,kf.
29 exp Health promotion/
30 (health adj3 (educat$ or promot$)).tw,kf.
31 or/11-30
32 10 and 31
33 (resilien$ adj5 (train$ or program$ or intervention$ or promot$ or prevent$ or enhanc$ or learn$ or teach$ or educat$ or increas$ or develop$ or manag$ or therap$ or protocol$ or treat$)).tw,kf.
34 (hardiness$ adj5 (train$ or program$ or intervention$ or promot$ or prevent$ or enhanc$ or learn$ or teach$ or educat$ or increas$ or develop$ or manag$ or therap$ or protocol$ or treat$)).tw,kf.
35 or/32-34

36 randomized controlled trial.pt.
37 controlled clinical trial.pt.
38 randomi#ed.ab.
39 placebo$.ab.
40 drug therapy.fs.
41 randomly.ab.
42 trial.ab.
43 groups.ab.
44 or/36-43
45 exp animals/ not humans.sh.
46 44 not 45
47 35 and 46
48 Health personnel/
49 (health$ adj3 (personnel or profession$ or worker$ or practitioner$ or provider$ or staff)).tw,kf.
50 ((medical care adj3 (personnel or profession$ or worker$ or practitioner$ or provider$ or staff)) or (medical adj3 (personnel or profession$ or worker$ or practitioner$ or provider$ or staff))).tw,kf.
51 (care adj1 (personnel or profession$ or worker$ or practitioner$ or provider$ or staff)).tw,kf.
52 (doctor$ or physician$ or general practitioner$ or (primary care adj2 practitioner$) or surgeon$).tw,kf.
53 (nurse$ or (nursing adj3 assistant$) or (nursing adj3 staff)).tw,kf.
54 nursing.tw,kf.
55 ((hospital or ambulance) adj1 personnel).tw,kf.
56 ((intensive adj2 care) or ICU or (intensive adj2 care adj2 unit adj3 personnel$)).tw,kf.
57 ((allied health$) adj2 (personnel or profession$ or worker$ or practitioner* or provider$ or staff)).tw,kf.
58 (psychologist$ or psychotherapist$ or psychiatrist$ or (mental health adj2 clinician$) or (mental health adj2 profession$) or (mental health adj2 worker$)).tw,kf.
59 (social worker$).tw,kf.
60 (paramedic$ or ambulance or medic$ or ((first or emergency or disaster) adj1 (response or responder$))).tw,kf.
61 (professional adj1 (caregiver$ or care-giver$)).tw,kf.
62 ((physical therapist$) or physiotherapist$ or occupational therapist$ or recreational therapist$ or music therapist$ or art therapist$ or dietitian$ or nutritionist$ or ((speech and language) adj1 therapist$) or speech pathologist$ or audiologist$ or exercise physiologist$ or osteopath$ or sonographer$ or radiographer$ or radiotherapist$ or ((radiology or radiation) adj1 (therapist$ or technician$ or technologist$ or assistant$ or scientist$)) or respiratory therapist$ or ((anesthesia or anesthesiologist) adj1 (technician$ or assistant$)) or dental hygienist$ or (surgical adj1 (technician$ or technologist$)) or orthotist$ or orthoptist$ or podiatrist$ or perfusionist$).tw,kf.
63 counsel?or$.tw,kf.
64 ((clinical or clinical laboratory or medical$ or medical$ laboratory) adj1 (technician$ or technologist$ or assistant$ or scientist$)).tw,kf.
65 ((human or health) adj1 service adj3 profession$).tw,kf.
66 (public health adj2 (service or agency)).tw,kf.
67 (secondary traumati?ation or (work$ adj2 (trauma survivor$))).tw,kf.
68 ((nursing or medical or premedical or paramedic or psychology or physical therapy or occupational therapy) adj2 student$).tw,kf.

69 (college adj2 student$).tw,kf.
70 ((nurs$ adj1 (graduate$ or education)) or (medic$ adj1 train$) or (student adj1 nurse$)).tw,kf.
71 or/48-70
72 47 and 71
73 limit 72 to yr="1990 -Current"
74 limit 73 to yr="2016 -Current"

Appendix B4

Data Collection/Extraction Sheet (Items According to Li et al., 2019)

Category	Extracted data
Source	• Study ID (created by review author) • Report ID (created by review author) • Review author ID (created by review author) • Citation and contact details
Eligibility	• Confirm eligibility for review • Reason for exclusion
Methods	• Study design • Total study duration • Sequence generation[a] • Allocation sequence concealment[a] • Blinding[a] • Other concerns about bias:[a] - analyses to assure baseline comparability of groups for sociodemographic characteristics and outcomes of interest; and - selection of comparison group
Participants	• Total number • Setting • Diagnostic criteria • Age • Sex • Country • Comorbidity • Sociodemographics • Date of study
Interventions	• Total number of intervention groups • For each intervention and comparison group of interest: - specific intervention; and - intervention details (sufficient for replication, if feasible)
Outcomes	• Outcomes and time points (1) collected; (2) reported[a] • For each outcome of interest: - outcome definition (with diagnostic criteria, if relevant) - unit of measurement (if relevant) • For scales: upper and lower limits and whether high or low score is good
Results	• Number of participants allocated to each intervention group • For each outcome of interest: - sample size - missing participants[a] - summary data for each intervention group (e.g., *M*s and *SD*s for continuous data at baseline and any time point after treatment; change); - estimate of effect with standard error, 95% CI and *p* value - subgroup analyses • Potential adverse effects
Miscellaneous aspects	• Funding source • Declaration of interests for the primary investigators • Key conclusions of the study authors • Miscellaneous comments from the study authors • References to other relevant studies • Correspondence required

Category	Extracted data
	• Miscellaneous outcomes by the review authors

Note. ID = identifier; CI = confidence interval; SD = standard deviation. Taken from review Kunzler et al. (2020) and review protocol (Helmreich, Kunzler et al., 2017).

[a] Full description required for standard items in RoB tool.

Appendix B5

Criteria for Risk of Bias Assessment in Included RCTs (According to Higgins, Altman, & Sterne, 2011)

Item	Judgment	Description
1. Random sequence generation (selection bias). We will describe the method used to generate the allocation sequence in sufficient detail for each included trial to allow an assessment of whether it should produce comparable groups.[a]	Low risk	The investigators describe a random component in the sequence generation process such as: • random number table; • computer random number generator; • coin tossing; • shuffling cards or envelopes; • throwing dice; • drawing of lots; or • minimization (minimization may be implemented without a random element [treatment sums are equal], and this is considered to be equivalent to being random).
	High risk	The researchers describe a (systematic or non-systematic) non-random component in the sequence generation process such as: • systematic, non-random approach - generating the sequence by, for example: o odd or even date of birth; o date (or day) of admission; o hospital or clinic record number; or o alternation. • non-systematic, non-random approach - allocating the participant by, for example: o judgement of the clinician; o preference of the participant; o results of a laboratory test or a series of tests; or o availability of the intervention.
	Unclear risk	Insufficient information to permit a judgment of “Low risk” or “High risk”.
2. Allocation concealment (selection bias). For each RCT, we will describe the method used to conceal the allocation sequence in sufficient detail to determine whether intervention allocations could have been foreseen in advance of, or during, enrolment.	Low risk	Participants and investigators enrolling participants could not foresee assignment because one of the following, or an equivalent method, was used to conceal allocation: • central allocation (including telephone, web-based and pharmacy-controlled randomization); • sequentially numbered drug containers of identical appearance; or • sequentially numbered, opaque, sealed envelopes.
	High risk	Participants or investigators enrolling participants could possibly foresee assignment and thus introduce selection bias because one of the following methods was used: • open random allocation schedule (e.g., a list of random numbers); • assignment envelopes without appropriate safeguards (e.g., if envelopes were unsealed or non-opaque or not sequentially numbered) • alternation or rotation; • date of birth; • case record number; or • any other explicitly unconcealed procedure.
	Unclear risk	Insufficient information to permit judgement of “Low risk” or “High risk”. This is usually the case if the method of concealment

Item	Judgment	Description
		is not described or not described in sufficient detail to allow a definite judgment (e.g., if the use of assignment envelopes is described, but it remains unclear whether envelopes were sequentially numbered, opaque, and sealed).
3. Blinding of participants and personnel (performance bias): objective outcomes. For each included trial, we will describe all methods used to blind trial participants and personnel from knowledge of which intervention a participant received. We will provide any information relating to whether the intended blinding was effective. We will assess blinding separately for different classes of outcomes. Outcomes will be divided into objective (e.g., cortisol) and subjective (e.g., self-reported resilience and other psychological outcomes). We will consider the same outcomes at different time points.	Low risk	Any one of the following: • no blinding or incomplete blinding, but the review authors judge that the outcome is not likely to be influenced by lack of blinding; or • blinding of participants and key study personnel ensured, and unlikely that the blinding could have been broken.
4. Blinding of participants and personnel (performance bias): subjective outcomes. For each included trial, we will describe all methods used to blind trial participants and personnel from knowledge of which intervention a participant received. We will provide any information relating to whether the intended blinding was effective. We will assess blinding separately for different classes of outcomes. Outcomes will be divided into objective (e.g., cortisol) and subjective (e.g., self-reported resilience and other psychological outcomes). We will consider the same outcomes at different time points.	Low risk	Blinding of participants and intervention providers, and unlikely that the blinding could have been broken.
	High risk	Any one of the following: • no blinding or incomplete blinding, and the outcome is likely to be influenced by lack of blinding; or • blinding of key study participants and personnel attempted, but likely that the blinding could have been broken; and the outcome is likely to be influenced by the lack of blinding.
	Unclear risk	Insufficient information to permit a judgment of “Low risk” or “High risk”.
5. Blinding of outcome assessors (detection bias): objective outcomes. For each included trial, we will describe all methods used to blind outcome assessors from	Low risk	Any one of the following: • no blinding of outcome assessment, but the review authors judge that the outcome measurement is not likely to be influenced by lack of blinding; or • blinding of outcome assessment ensured, and unlikely that

Item	Judgment	Description
knowledge of which intervention a participant received. We will provide any information relating to whether the intended blinding was effective. We will assess blinding separately for different classes of outcomes. Outcomes will be divided into objective (e.g., cortisol) and subjective (e.g., self-reported resilience and other psychological outcomes). We will consider the same outcomes at different time points.		the blinding could have been broken.
6. Blinding of outcome assessors (detection bias): subjective outcomes. For each included trial, we will describe all methods used to blind outcome assessors from knowledge of which intervention a participant received. We will provide any information relating to whether the intended blinding was effective. We will assess blinding separately for different classes of outcomes. Outcomes will be divided into objective (e.g., cortisol) and subjective (e.g., self-reported resilience and other psychological outcomes). We will consider the same outcomes at different time points.	Low risk	Any one of the following: • no blinding of outcome assessment, but the review authors judge that the outcome measurement is not likely to be influenced by lack of blinding; or • blinding of outcome assessment ensured, and unlikely that the blinding could have been broken.
	High risk	Any one of the following: • no blinding of outcome assessment, and the outcome measurement is likely to be influenced by lack of blinding; or • blinding of outcome assessment, but likely that the blinding could have been broken, and the outcome measurement is likely to be influenced by lack of blinding.
	Unclear risk	Insufficient information to permit a judgment of “Low risk” or “High risk”.
7. Incomplete outcome data (attrition bias). For each RCT, we will describe the completeness of outcome data for each main outcome, including attrition and exclusions from the analysis. We will state whether attrition and exclusions were reported, the numbers included at each stage (compared with the total number of participants randomized), reasons for attrition or exclusions (where reported), whether missing data were balanced across groups or were related to outcomes, and	Low risk	Any one of the following: • no missing outcome data; • reasons for missing outcome data unlikely to be related to true outcome (for survival data, censoring unlikely to be introducing bias); • missing outcome data balanced in numbers across intervention groups, with similar reasons for missing data across groups; • for dichotomous outcome data, the proportion of missing outcomes compared with observed event risk not enough to have a clinically relevant impact on the intervention effect estimate; • for continuous outcome data, plausible effect size (difference in means or standardized difference in means) among missing outcomes not enough to have a clinically

Item	Judgment	Description
the adequacy of dealing with missing data. Where sufficient data are reported, or can be provided by the trial authors, we will re-include missing data in the analyses.		relevant impact on observed effect size; • missing data have been imputed using appropriate methods; or • intention-to-treat; all randomized participants are analyzed in the group to which they were allocated by randomization, irrespective of noncompliance and co-interventions.
	High risk	Any one of the following: • reasons for missing outcome data likely to be related to true outcome, with either imbalance in numbers or reasons for missing data across intervention groups; • for dichotomous outcome data, the proportion of missing outcomes compared with observed event risk is enough to induce clinically relevant bias in intervention effect estimate; • for continuous outcome data, plausible effect size (difference in means or standardized difference in means) among missing outcomes enough to induce clinically relevant bias in observed effect size; • potentially inappropriate application of simple imputation; or • "as-treated" analysis done with substantial departure of the intervention received from that assigned at randomization.
	Unclear risk	Insufficient reporting of attrition or exclusions to permit a judgement of "Low risk" or "High risk" (e.g., number randomized not stated, no reasons for missing data provided, number of dropouts not reported for each group).
8. Selective outcome reporting (reporting bias). For each included trial, we will describe how the possibility of selective outcome reporting was examined and what was found.	Low risk	Any of the following: • the study protocol is available and all of the study's prespecified (primary and secondary) outcomes that are of interest in the review have been reported in the prespecified way; or • the study protocol is not available, but it is clear that the published reports include all expected outcomes, including those that were prespecified (convincing text of this nature may be uncommon).
	High risk	Any one of the following: • not all of the study's prespecified primary outcomes have been reported; • one or more primary outcomes is reported using measurements, analysis methods, or subsets of the data (e.g., subscales) that were not prespecified; • one or more reported primary outcomes were not prespecified (unless clear justification for their reporting is provided such as an unexpected adverse effect); • one or more outcomes of interest in the review are reported incompletely so that they cannot be entered in a meta-analysis; or • the study report fails to include results for a key outcome that would be expected to have been reported for such a study.
	Unclear risk	Insufficient information to permit a judgment of "Low risk" or "High risk".

Note. RCT = randomized controlled trial. Taken from review Kunzler et al. (2020) and review protocol (Helmreich, Kunzler et al., 2017).

[a] The achieved baseline comparability between study conditions was considered as part of selection bias (random sequence generation).

Appendix B6

Further Methodological Details of the Cochrane Review

Category	Methodological details
Assessment of heterogeneity	Clinical heterogeneity; it is explored if studies are sufficiently homogeneous in terms of: • participant characteristics (e.g., similar target group, age, and gender) • interventions (e.g., theoretical foundation of interventions, setting, delivery format, and intensity) • outcomes (e.g., outcomes measured and respective scales)
Data synthesis and analysis	• Inverse variance approach: the study weights of individual studies in a meta-analysis are adjusted to be the inverse of the variance of the effect size; that is, larger studies have smaller *SE*s and receive more weight than smaller studies (with larger *SE*s). This choice of weights minimizes the imprecision (uncertainty) of the pooled effect estimate (Deeks et al., 2019). • Random-effects meta-analyses instead of meta-analyses according to the fixed-effect model as the assumption of the same true effect size in all studies is not plausible in many systematic reviews (Borenstein et al., 2009)
Subgroup analyses and investigation of heterogeneity	• Since substantial heterogeneity was detected for several outcomes (see 2.3.4.1; 2.3.7), subgroup analyses were conducted to examine intervention characteristics possibly associated with this diversity. • Four subgroup analyses performed for four of five primary outcomes at postintervention, the exception being anxiety. For resilience, (perceived) stress, and well-being, four subgroup analyses were also conducted at short-term FU.
Sensitivity analyses	• Six sensitivity analyses were performed for the primary outcomes of depression (at postintervention only), (perceived) stress, and well-being at postintervention and short-term FU. For resilience, a sensitivity analysis of the underlying resilience concept was conducted at posttest only; it was not possible at short-term FU since all studies used a state-oriented scale. In addition, the planned sensitivity analysis for reporting bias was not possible for resilience, due to all studies being at low risk of reporting bias. No sensitivity analysis was performed for anxiety.

Note. SE = standard error; FU = follow-up.

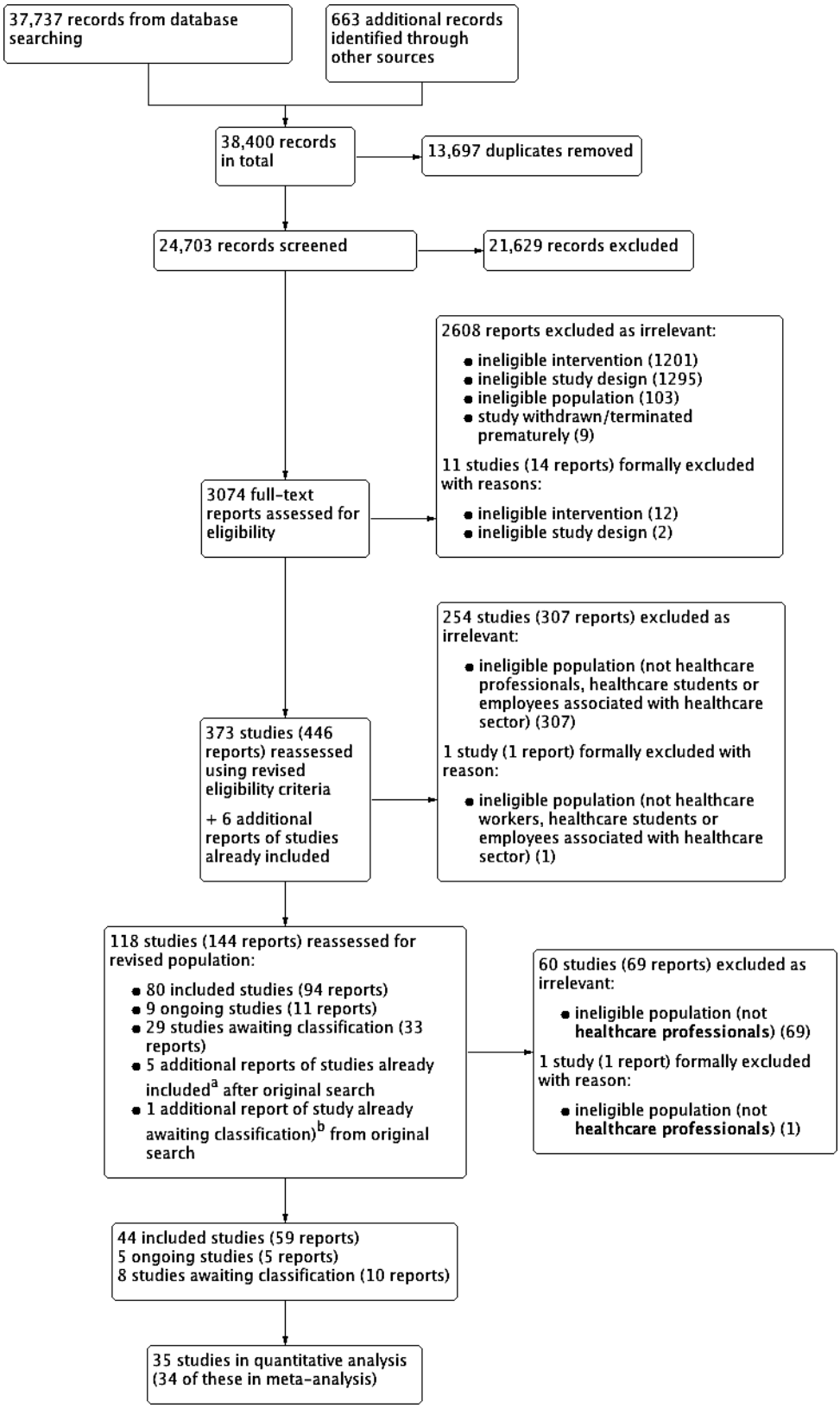
Appendix B7 GRADE Approach (Summary of Findings Table)

Outcomes were downgraded for study limitations if the majority of studies had unclear or high risk of bias (Guyatt, Oxman, Vist, et al., 2011). It was downgraded for indirectness if included studies were limited to certain participants, particular versions of resilience intervention (e.g., setting, theoretical foundation), or certain comparators (Guyatt, Oxman, Kunz, Woodcock, Brozek, Helfand, Alonso-Coello, Falck-Ytter, et al., 2011). Inconsistency was determined by the heterogeneity of results based on variation of effect estimates between studies, overlapping CIs, the statistical test of heterogeneity and I^2 (Guyatt, Oxman, Kunz, Woodcock, Brozek, Helfand, Alonso-Coello, Glasziou, et al., 2011). Imprecision was assessed by the number of participants included in an outcome and the width of CIs (e.g., consistent messages of upper and lower CI limits concerning the effect of intervention; CIs encompassing both small treatment effect and crossing the threshold of appreciable benefit at SMD of (±) 0.5; Guyatt, Oxman, Kunz, Brozek, et al., 2011). According to the GRADE system, for sufficient statistical precision, meta-analyses for continuous outcomes should include sample sizes of at least 400 participants. Where there was both substantial inconsistency ($I^2 \geq 60\%$) for an outcome *and* imprecision, it was *not* downgraded for imprecision as the heterogeneity might have influenced the CI (i.e., precision), in order not to double downgrade for the same problem. Publication bias was assessed based on funnel plot asymmetry (e.g., publication of null findings or not), statistical tests for asymmetry (Egger's test), different results for an outcome between published versus unpublished studies, and whether the evidence consisted of many small studies with potential conflicts of interest (Guyatt, Oxman, Montori, et al., 2011).

Appendix C Results of the Cochrane Review

Appendix C1

Study Flow Diagram Combining Both Searches



Note. [a] Duchemin 2015; Mistretta 2018; Schroeder 2016; [b] Van Berkel 2014.

Appendix C2 Excluded Studies

Based on the subsequent alteration of eligibility criteria for this review (see Appendix D17.1) and the focus on HCP, a total of 3,000 irrelevant full text reports were excluded for the following reasons: 1) ineligible study design/not a primary study (e.g., no RCT; 1,297), 2) ineligible intervention (1,213), 3) ineligible population (e.g., no healthcare professionals; 481), and 4) study withdrawn/terminated prematurely (9).

Thirteen studies (from 16 reports) were excluded that seemed to merit inclusion but on closer inspection did not (see Appendix D9). Nine of these studies were excluded because they did not explicitly state the aim of fostering resilience, hardiness, or posttraumatic growth through the intervention or the study authors advised that resilience was not the primary focus of the study, or both (i.e., ineligible intervention; Chang, 2008; Dyrbye et al., 2016; Imamura et al., 2019; NCT03753360; NCT03914898; Rowe, 1999; Speckens et al., 2019; Strauss et al., 2018; Watanabe et al., 2019). Chang (2008) only mentioned the concept of resilience in the discussion section, but did not explicitly state the aim of promoting resilience. Speckens et al. (2019), who tested a mindfulness intervention for medical residents of different fields, identified resilience as one issue in qualitative interviews but did also not aim to foster the participants' resilience. For two study protocols (Imamura et al., 2019; Strauss et al., 2018), the information was obtained from the study authors that resilience was not the primary focus and not measured in these studies. Two studies were excluded that mentioned resilience in the trial registration, study protocol, or a publication reporting baseline results of a RCT, but not in the final report (Dyrbye et al., 2016; Watanabe et al., 2019). The study by Rowe (1999) and the corresponding FU reports (Rowe, 2000, 2006) were not considered, since hardiness – although mentioned several times in the reports – was only examined as a correlate of burnout (main study outcome) and not the primary aim of the intervention. According to the primary investigators of the completed, but unpublished study NCT03914898, the content of the intervention program did include resilience elements. Nevertheless, as the corresponding report (currently under review but provided by the study authors) only mentioned the term resilience when referring to other studies, it was decided against including the study in this review. For NCT03753360, the information was

obtained from the investigators that fostering resilience was not the primary focus of the intervention, but rather a secondary outcome. Therefore, this study was also excluded due to the ineligible intervention.

Two studies were excluded due to *ineligible study design*. Lahn (2014) was excluded because the "heartfelt emotion" condition (p. 9), which trained the resilience factor positive emotions and would have been relevant for this review, only served as a second control condition and not as the intervention arm in this study. Maunder et al. (2010) was excluded as the study involved a random assignment to three different doses of resilience intervention, but no control group.

Since they did not examine HCP, two studies were excluded for *ineligible population*. NCT02417051 was conducted in a sample of active disaster responders that participated in relief efforts after Hurricane Sandy but included no HCP. Bian et al. (2011) was excluded for a similar reason, as it evaluated the effectiveness of a coping training intervention for the Chinese Special Service Military Personnel as civil emergency responders. Although the training was provided partially by soldiers from a medical military team, HCP were not among the participants.

Appendix C3 Studies Awaiting Classification

Based on the subsequent alteration of eligibility criteria for this review (see Appendix D17.1), and the focus on HCP, eight studies (from 10 reports) were identified as awaiting classification. For four studies, resilience was only rarely mentioned in the reports, that is, the focus of the intervention on fostering resilience, hardiness, or posttraumatic growth was unclear and could not be obtained from the study authors (Aranda Auserón et al., 2018; NCT03613441; NCT03781336; Ruehl, 2013). For example, Ruehl (2013) measured posttraumatic growth as an exploratory measure following a written emotional expression intervention in a diverse group of nursing staff, but it was unclear whether the study primarily focused on fostering this construct. Similarly, Aranda Auserón et al. (2018) examined a mindfulness and self-compassion program in primary-care health professionals to reduce stress and prevent burnout, but it was unclear if resilience was the primary focus of the study (resilience was mentioned only once). With respect to Ouyang et al. (2017), the study design could not be clearly determined since the full text was not available and no response was received from the investigators. The same applied to Mainwaring (2018; available as conference abstract), for which no response from the authors was obtained. For two studies, it was unclear whether the sample also included healthcare workers (J. I. Kim et al., 2018; Van Berkel et al., 2014a; Van Berkel et al., 2014b). Details of these studies can be found in the Appendix D7.

The 12 studies from the updated search in June 2020 (Almén et al., 2020; Chesak et al., 2019; Dyrbye et al., 2019; Grabbe et al., 2020; Heath et al., 2020; Moffatt-Bruce et al., 2019; NCT04368676; NCT04372303; NCT04373382; NCT04384861; Rodgers, 2018; Yeo et al., 2019) were also added to the studies awaiting classification (see Appendix D7). They will be incorporated into this review at the update stage.

Appendix C4 Ongoing Studies

Five ongoing studies (from five reports; see Appendix D8) were found that are likely to meet the inclusion criteria, all of them being RCTs with parallel assignment. ACTRN12617000290392 compared the 6-week online program “Doctors Working Well” (e.g., modules on stress management, emotion monitoring, and regulation) to an active control (protected individual study time) in junior medical doctors. In a Japanese trial (JPRN-UMIN000031435), medical professionals working in the field of oncology or palliative care (or both) were randomized to either the MHALO program (mindfulness for health professionals building resilience and compassion) or a no-intervention control. NCT03518359 compared the mindfulness-based enhanced stress resilience training (ESRT; six weekly classes plus daily homework and retreat; e.g., sustained attention, emotion regulation, meta-cognition) with active control (stress management) in medical interns from different departments. NCT03645512 included critical care nurses at Florida Hospital to determine whether the Corporate Athlete® Resilience (CAR) training program (a holistic approach focusing on moving between stress and strategic recovery) had a significant impact on the nurses' resilience and stress mindset compared to a wait-list control. The Bournemouth University resilience training for surgeons (BURTS), based on ACT (ACTr; Flaxman et al., 2013), was contrasted with a wait-list control in trainee and consultant surgeons (NCT03759795). In contrast with other ongoing studies, the IG did not receive one treatment, but followed a maximum of three intervention periods (e.g., mindfulness training) of eight weeks each.

Appendix C5 Risk of Bias Assessment of Included Studies

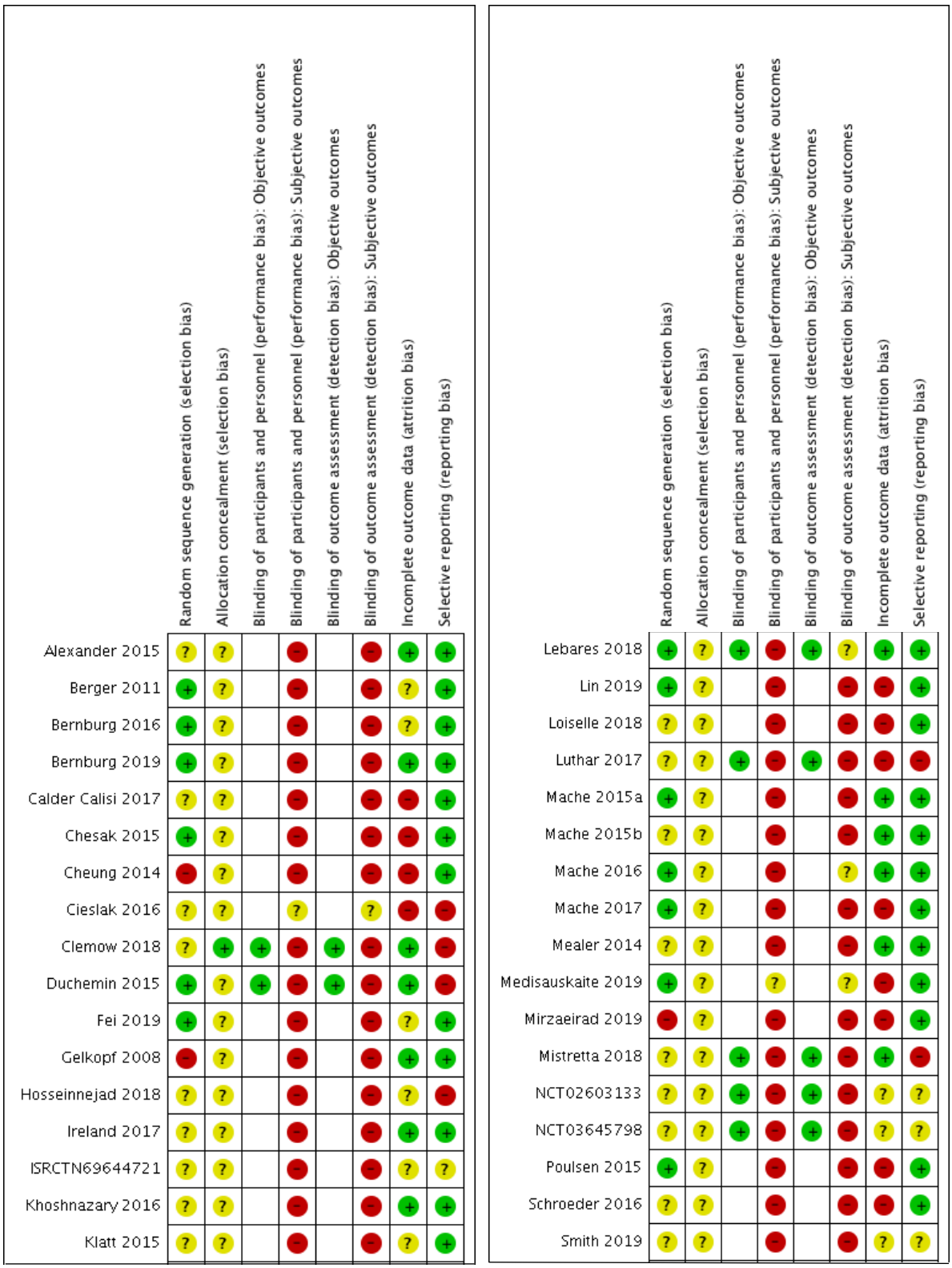
Most studies (41/44 studies) were rated at high risk of performance bias. For two of these, this was the only domain at high RoB. However, 15 studies were also at high RoB in another domain. Thus, one study (Sood et al., 2014) was also rated at high risk of selection bias, 13 studies (Alexander et al., 2015; Berger & Gelkopf, 2011; Bernburg et al., 2016; Bernburg et al., 2019; Fei, 2019; Ireland et al., 2017; ISRCTN69644721; Klatt et al., 2015; Mache, Danzer, et al., 2015; NCT02603133; NCT03645798; Smith et al., 2019; Tierney & Lavelle, 1997) at high risk of performance and detection bias, and one study at high risk of performance and reporting bias (Stetz et al., 2007).

Eighteen studies were also at high RoB in two other domains: Thirteen were also at high risk of detection and attrition bias (Calder Calisi, 2017; Chesak et al., 2015; Khoshnazary et al., 2016; Lin et al., 2019; Loiselle, 2018; Mache et al., 2017; Mache, Vitzthum, et al., 2015; Mealer et al., 2014; Poulsen et al., 2015; Schroeder et al., 2016; Sood et al., 2011; West et al., 2015; Wild, 2016), and five studies were also at high risk of detection and reporting bias (Clemow et al., 2018; Duchemin et al., 2015; Hosseinnejad et al., 2018; Mistretta et al., 2018; Strijk et al., 2011).

Six studies were also at high RoB in three other domains. Besides performance bias, three studies were judged at high risk of selection, detection, and attrition bias (Cheung, 2014; Gelkopf et al., 2008; Mirzaeirad et al., 2019), and three studies at high risk of detection, attrition, and reporting bias (Luthar et al., 2017; Varker & Devilly, 2012; West et al., 2014).

Figure C5.1

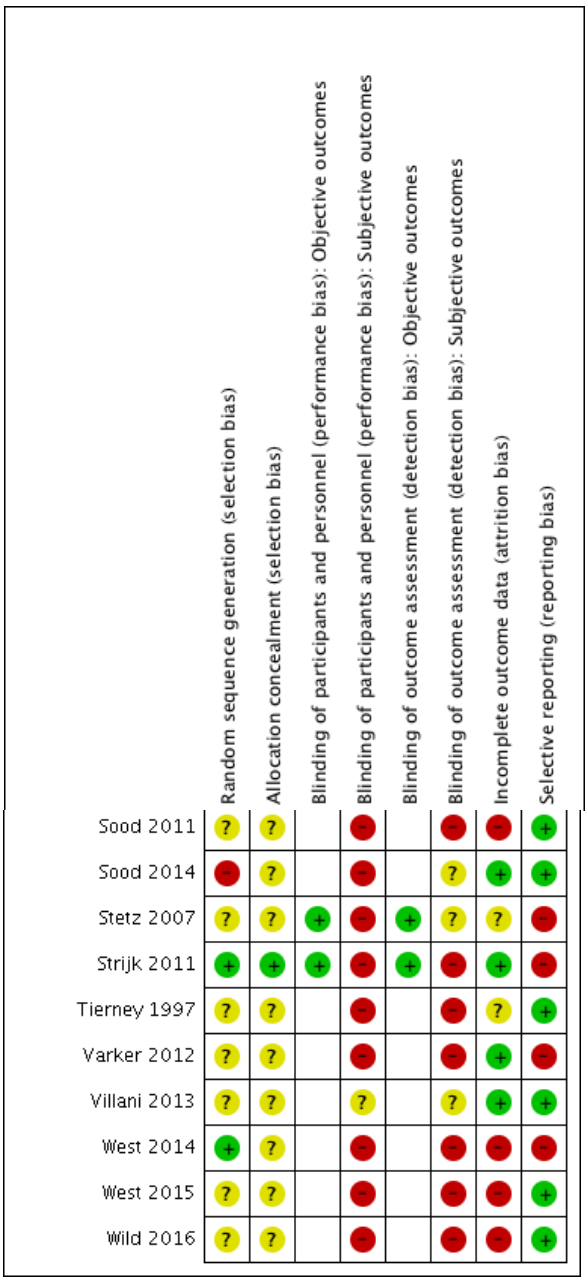
RoB Summary (Judgements About Each Risk of Bias Item for Each Included Study) – Part 1



Note. Mache 2015a = Mache, Danzer, et al. (2015); Mache 2015b = Mache, Vitzthum, et al. (2015).

Figure C5.2

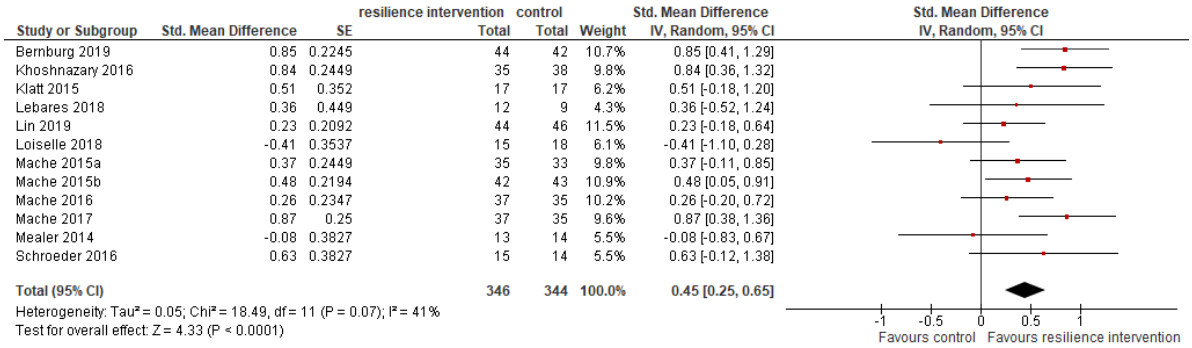
RoB Summary (Judgements About Each Risk of Bias Item for Each Included Study) – Part 2



Appendix C6 Forest Plots – Pooled (Main) Analyses

Figure C6.1

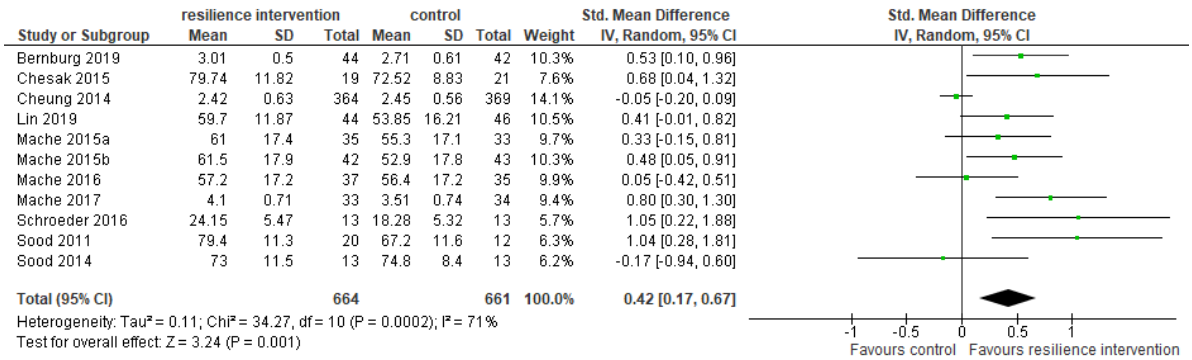
Forest Plot Resilience Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.2

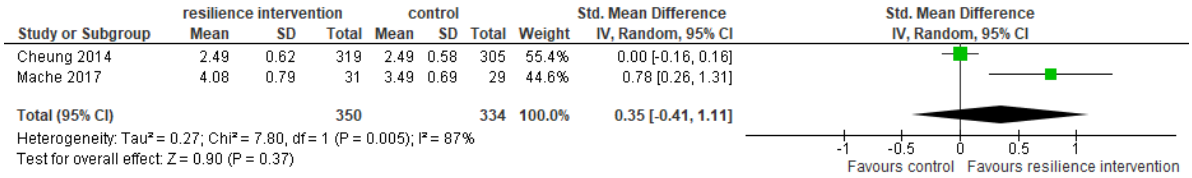
Forest Plot Resilience Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.3

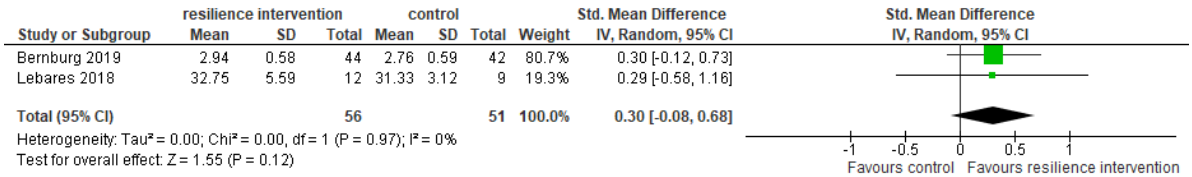
Forest Plot Resilience Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.4

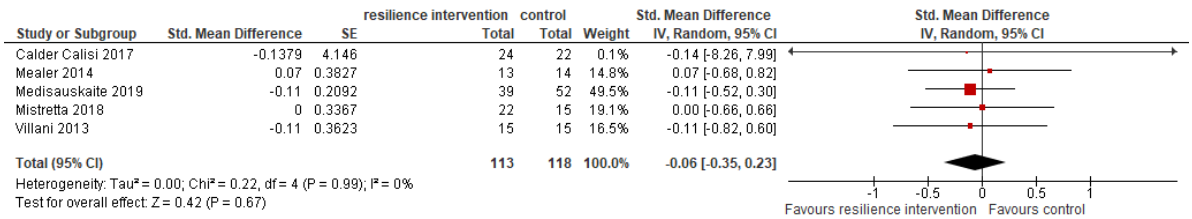
Forest Plot Resilience Long-Term FU (> 6 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.5

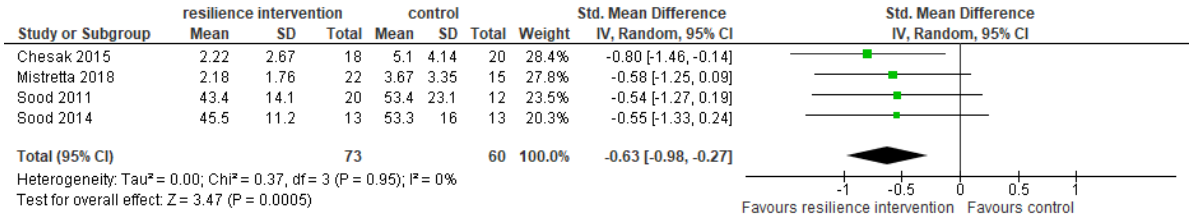
Forest Plot Anxiety Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.6

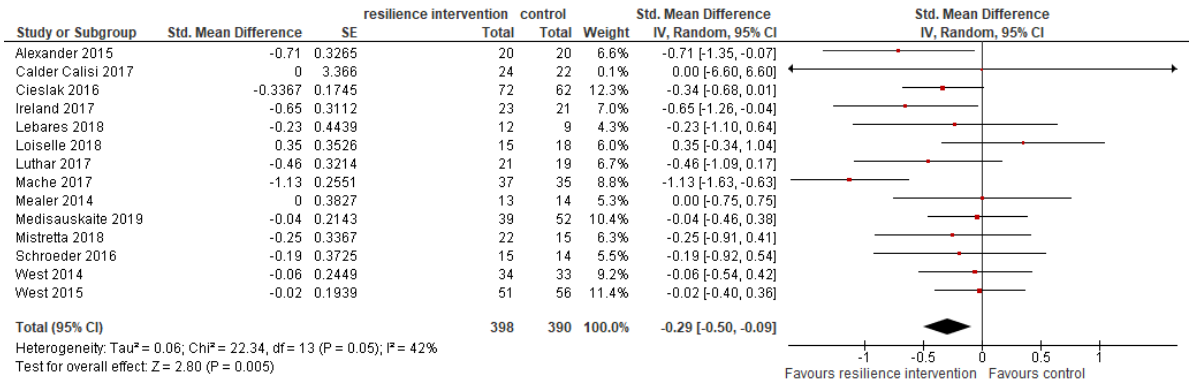
Forest Plot Anxiety Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.7

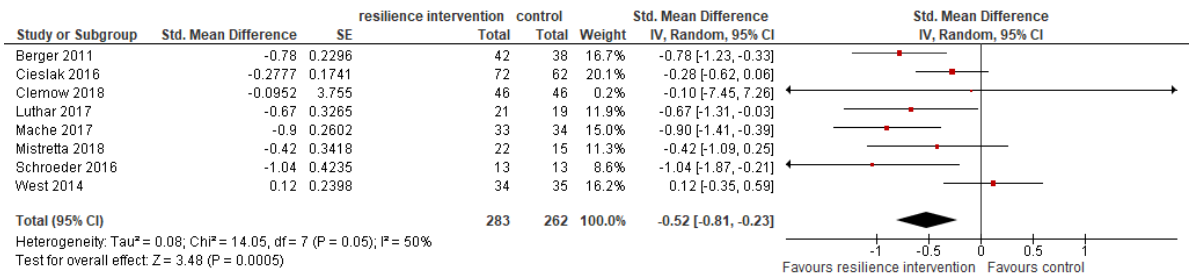
Forest Plot Depression Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.8

Forest Plot Depression Short-Term FU (≤ 3 Months Postintervention)

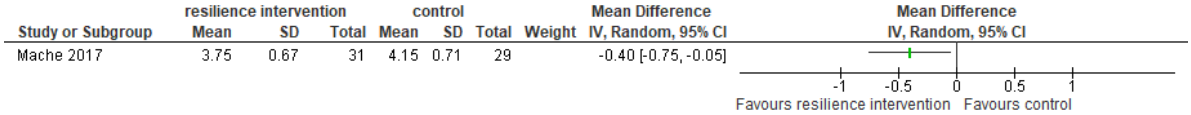


Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* =

standard error; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.9

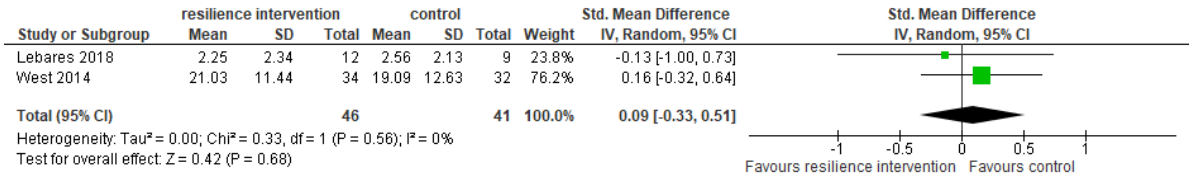
Forest Plot Depression Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.10

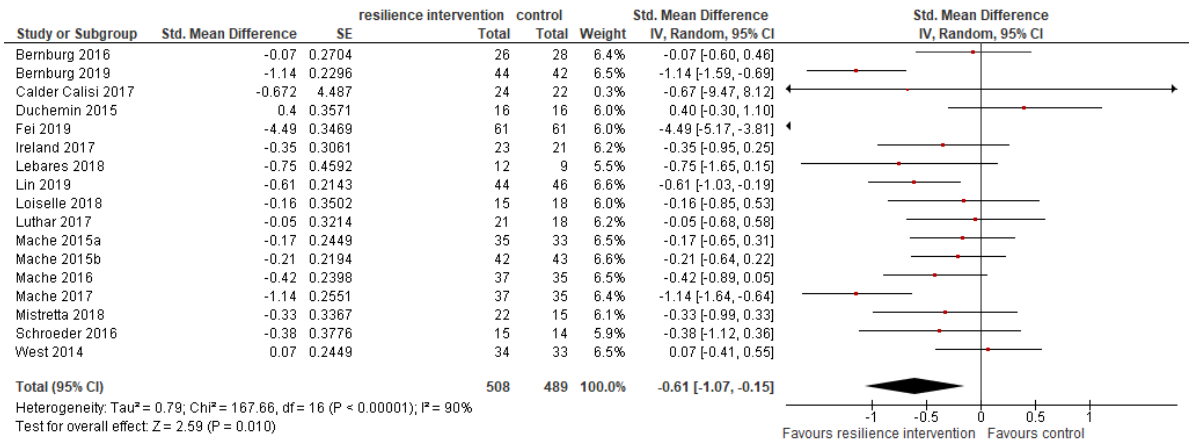
Forest Plot Depression Long-Term FU (> 6 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.11

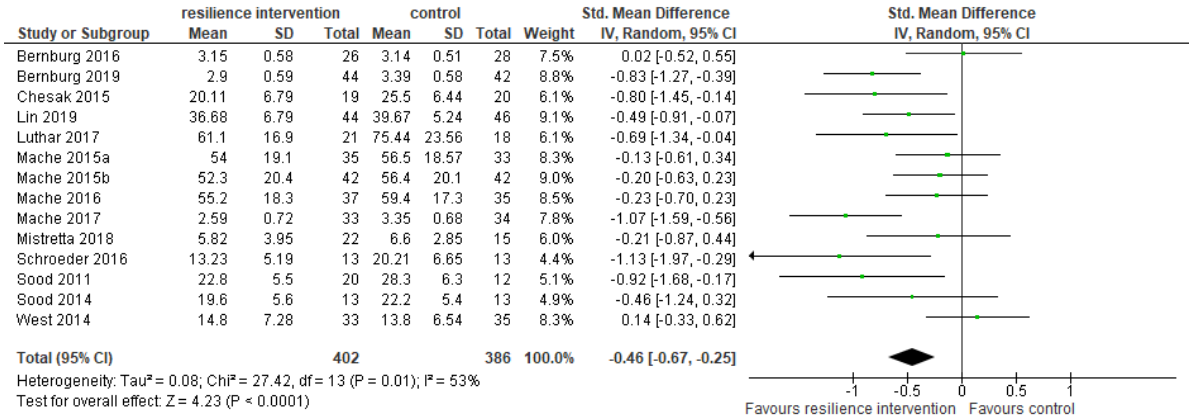
Forest Plot (Perceived) Stress Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.12

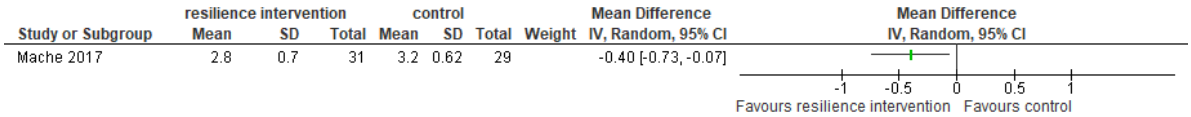
Forest Plot (Perceived) Stress Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.13

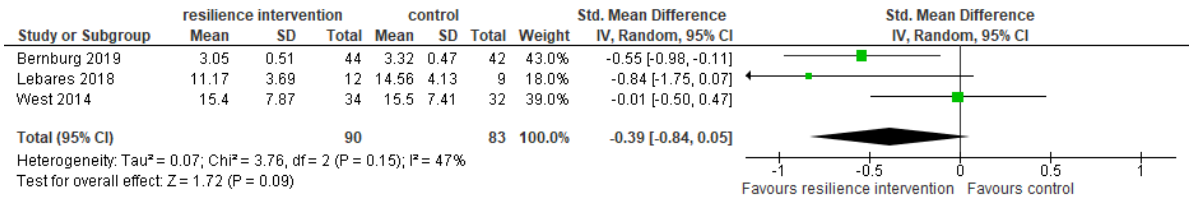
Forest Plot (Perceived) Stress Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.14

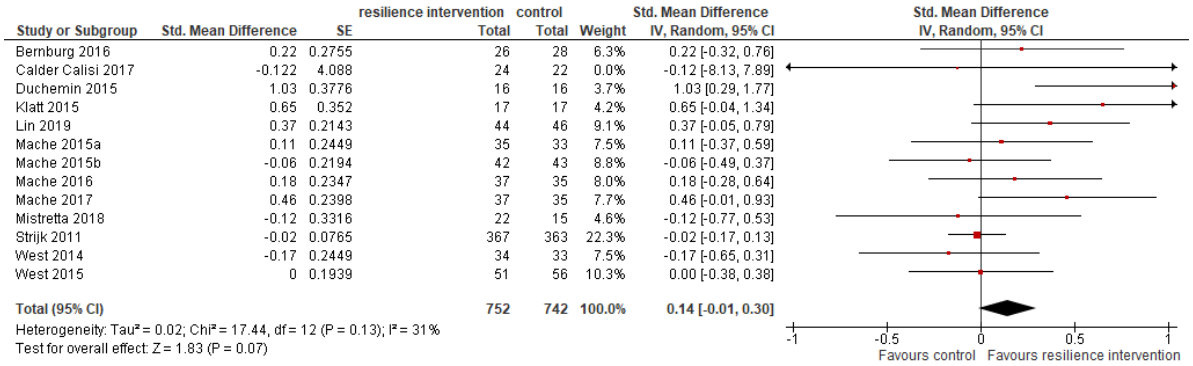
Forest Plot (Perceived) Stress Long-Term FU(> 6 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.15

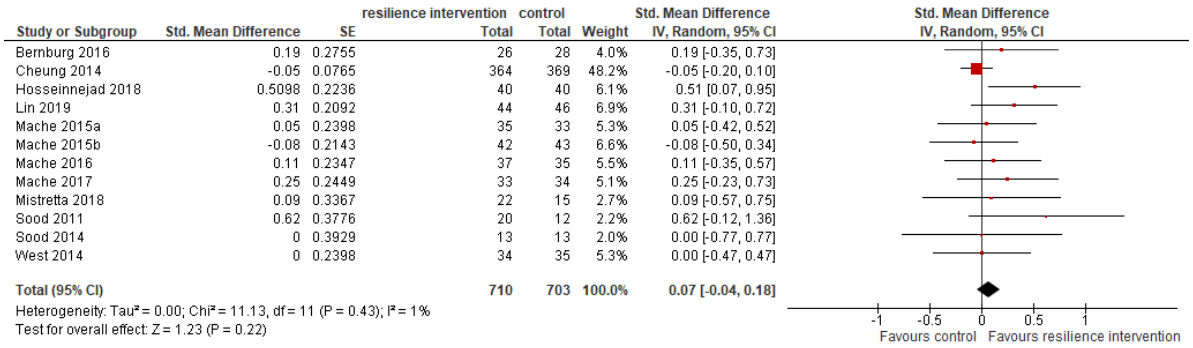
Forest Plot Well-being Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.16

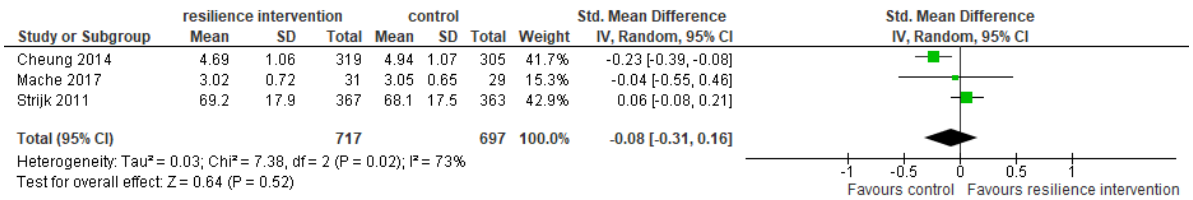
Forest Plot Well-Being Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.17

Forest Plot Well-Being Medium-term FU (> 3 ≤ 6 Months Postintervention)

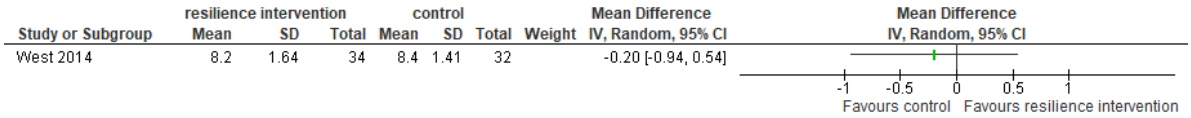


Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z*

value.

Figure C6.18

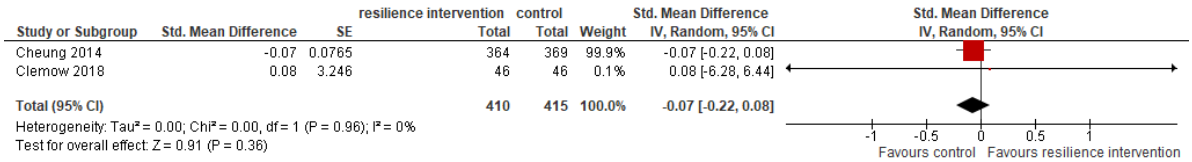
Forest Plot Well-Being Long-Term FU (> 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.19

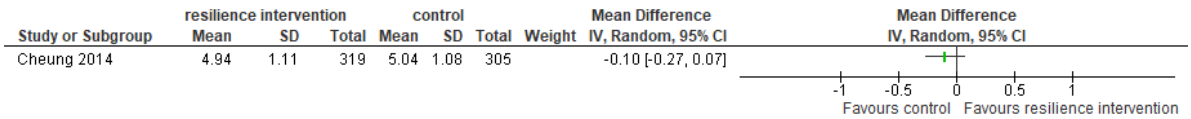
Forest Plot Social Support Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.20

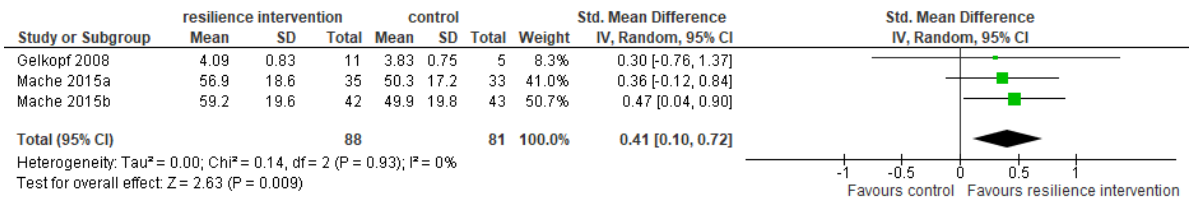
Forest Plot Social Support Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.21

Forest Plot Optimism Posttest

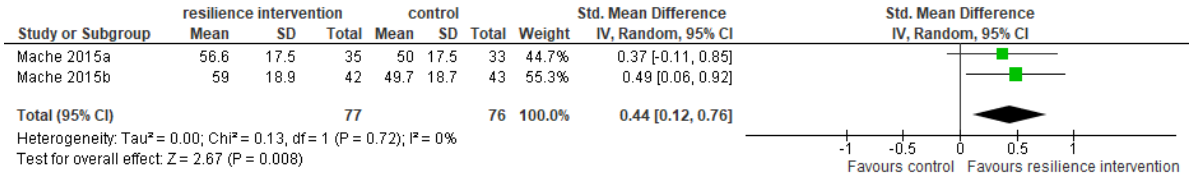


Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* =

standard deviation; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.22

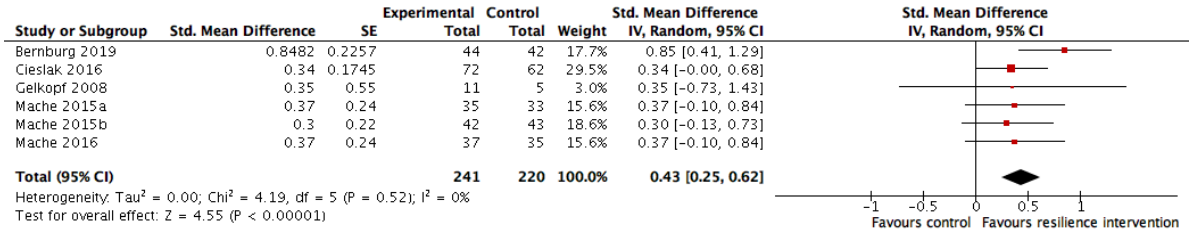
Forest Plot Optimism Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.23

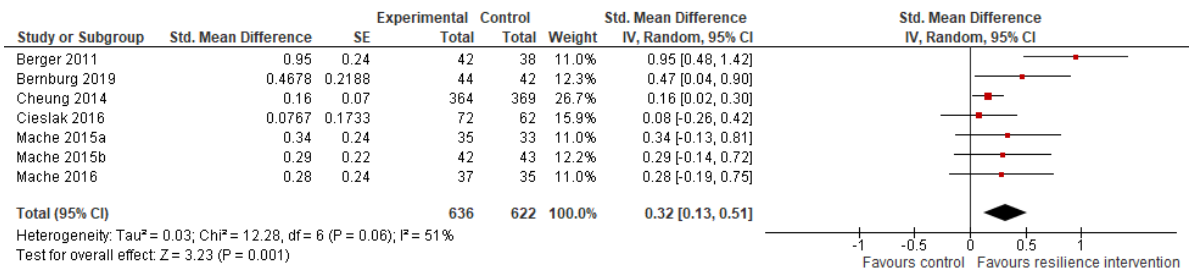
Forest Plot Self-Efficacy Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.24

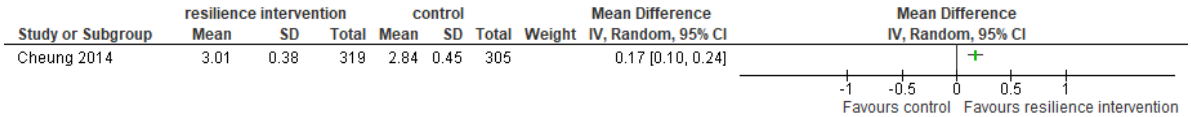
Forest Plot Self-Efficacy Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SE* = standard error; Std. = standardized; Tau² = Tau² value (τ²; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.25

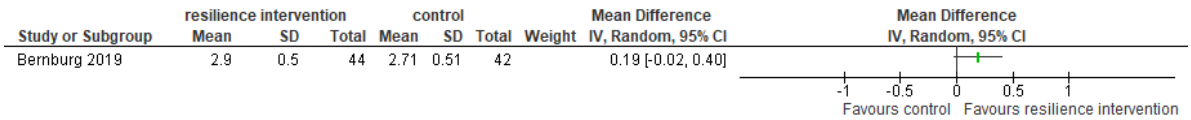
Forest Plot Self-Efficacy Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.26

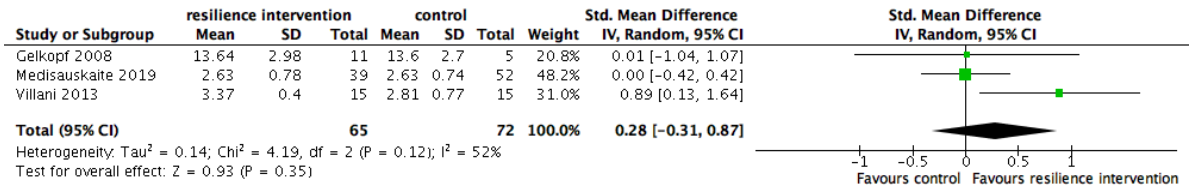
Forest Plot Self-Efficacy Long-Term FU (> 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.27

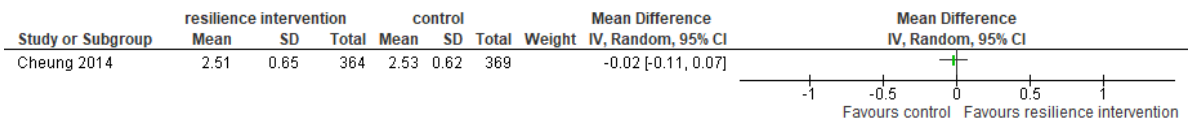
Forest Plot Active Coping Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau² = Tau² value (τ^2; heterogeneity); Chi² = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.28

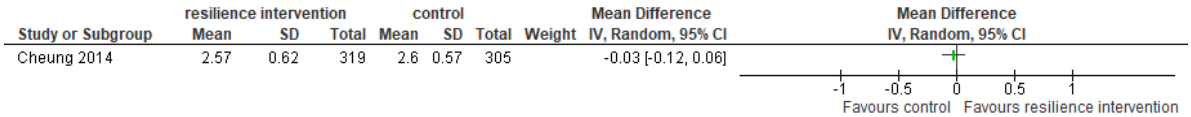
Forest Plot Active Coping Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.29

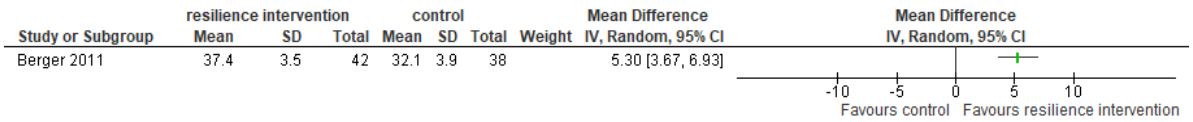
Forest Plot Active Coping Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.30

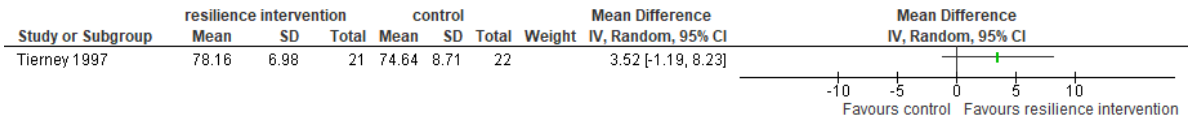
Forest Plot Self-Esteem Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.31

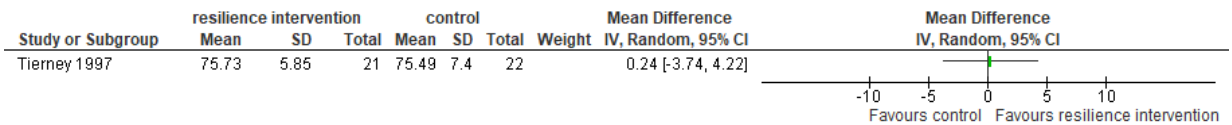
Forest Plot Hardiness Posttest



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.32

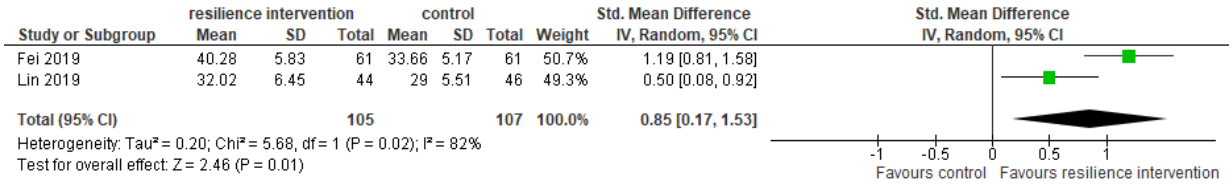
Forest Plot Hardiness Medium-Term FU (> 3 ≤ 6 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Figure C6.33

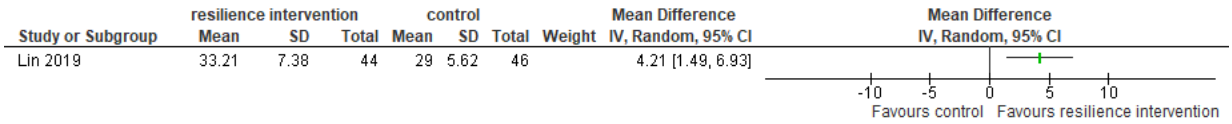
Forest Plot Positive Emotions Posttest



Note. CI = confidence interval; *df* = degrees of freedom; I^2 = I^2 value (heterogeneity); IV = inverse variance; P = *p* value; *SD* = standard deviation; Std. = standardized; Tau^2 = Tau^2 value (τ^2; heterogeneity); Chi^2 = χ^2 value (test for heterogeneity); Z = *z* value.

Figure C6.34

Forest Plot Positive Emotions Short-Term FU (≤ 3 Months Postintervention)



Note. CI = confidence interval; IV = inverse variance; *SD* = standard deviation.

Appendix C7 Subgroup Analyses

Table C7.1

Subgroup Analyses Concerning Intervention Setting

Outcome/ subgroup	Studies	*N*	SMD random-effects, 95% CI	*p*	I^2	Interpretation (Cohen, 1988)
Resilience, posttest						
Test for subgroup differences: χ^2 = 0.68, *df* = 1, *p* = .41, I^2 = 0%						
group	9	557	0.50 [0.33, 0.67]	< .001	0%	moderate
combined	3	133	0.15 [−0.66, 0.97]	.72	80%	ns
Depression, posttest						
Test for subgroup differences: χ^2 = 5.34, *df* = 3, *p* = .15, I^2 = 43.9%						
group	9	457	−0.41 [−0.69, −0.13]	.004	50%	moderate
individual	1	134	−0.34 [−0.68, 0.01]	.05	/	ns
combined	3	106	0.19 [−0.32, 0.69]	.47	0%	ns
unspecified	1	91	−0.04 [−0.46, 0.38]	.85	/	ns
(Perceived) stress, posttest						
Test for subgroup differences: χ^2 = 1.22, *df* = 1, *p* = .27, I^2 = 17.8%						
group	15	918	−0.64 [−1.12, −0.15]	.01	92%	moderate
combined	2	79	−0.16 [−0.85, 0.52]	.64	0%	ns
Well-being, posttest						
Test for subgroup differences: χ^2 = 3.17, *df* = 1, *p* = .07, I^2 = 68.5%						
group	11	718	0.19 [0.01, 0.37]	.04	29%	small
combined	2	776	−0.02 [−0.17, 0.13]	.79	0%	ns
Resilience, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: χ^2 = 4.81, *df* = 2, *p* = .09, I^2 = 58.5%						
group	9	1267	0.41 [0.15, 0.68]	.002	72%	moderate
individual	1	32	1.04 [0.28, 1.81]	.008	/	large
combined	1	26	−0.17 [−0.94, 0.60]	.66	/	ns
(Perceived) stress, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: χ^2 = 1.46, *df* = 2, *p* = .48, I^2 = 0%						
group	12	730	−0.44 [−0.67, −0.20]	< .001	57%	moderate
individual	1	32	−0.92 [−1.68, −0.17]	.02	/	large
combined	1	26	−0.46 [−1.24, 0.32]	.25	/	ns
Well-being, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: χ^2 = 2.23, *df* = 2, *p* = .33, I^2 = 10.5%						
group	10	1355	0.05 [−0.06, 0.16]	.34	0%	ns
individual	1	32	0.62 [−0.12, 1.36]	.10	/	ns
combined	1	26	0 [−0.77, 0.77]	1.00	/	ns

Note. N = number of participants; SMD = standardized mean difference; CI = confidence interval; *p* = *p* value; I^2 = heterogeneity; χ^2 = Chi^2 value of test for subgroup differences; *df* = degrees of freedom; ns = not significant; FU = follow-up.

Table C7.2

Subgroup Analyses Concerning Delivery Format[a]

Outcome/ subgroup	Studies	*N*	SMD random-effects, 95% CI	*p*	I^2	Interpretation (Cohen, 1988)
Resilience, posttest						
Test for subgroup differences: $\chi^2 = 0.12$, *df* = 1, *p* = .73, $I^2 = 0\%$						
face-to-face	9	500	0.47 [0.24, 0.71]	< .001	37%	moderate
combined	3	190	0.37 [−0.13, 0.88]	.15	63%	ns
Depression, posttest						
Test for subgroup differences: $\chi^2 = 1.42$, *df* = 2, *p* = .49, $I^2 = 0\%$						
face-to-face	10	499	−0.35 [−0.65, −0.05]	.02	55%	moderate
combined	3	198	−0.27 [−0.55, 0.01]	.06	0%	ns
unspecified	1	91	−0.04 [−0.46, 0.38]	.85	/	ns
(Perceived) stress, posttest						
Test for subgroup differences: $\chi^2 = 1.34$, *df* = 1, *p* = .25, $I^2 = 25.2\%$						
face-to-face	14	838	−0.70 [−1.26, −0.15]	.01	92%	large
combined	3	159	−0.23 [−0.81, 0.35]	.44	66%	ns
Well-being, posttest						
Test for subgroup differences: $\chi^2 = 1.51$, *df* = 1, *p* = .22, $I^2 = 33.9\%$						
face-to-face	10	1335	0.04 [−0.07, 0.16]	.43	0%	ns
combined	3	159	0.40 [−0.16, 0.95]	.16	62%	ns
Resilience, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: $\chi^2 = 0.12$, *df* = 1, *p* = .72, $I^2 = 0\%$						
face-to-face	8	1169	0.45 [0.14, 0.77]	.005	76%	moderate
combined	3	156	0.36 [−0.04, 0.76]	.08	30%	ns
(Perceived) stress, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: $\chi^2 = 0.03$, *df* = 1, *p* = .87, $I^2 = 0\%$						
face-to-face	10	596	−0.46 [−0.74, −0.18]	.002	65%	moderate
combined	4	192	−0.49 [−0.78, −0.20]	< .001	0%	moderate
Well-being, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: $\chi^2 = 0.45$, *df* = 1, *p* = .50, $I^2 = 0\%$						
face-to-face	9	1260	0.09 [−0.06, 0.23]	.23	17%	ns
combined	3	153	0.21 [−0.11, 0.52]	.20	0%	ns

Note. *N* = number of participants; SMD = standardized mean difference; CI = confidence interval; *p* = *p* value; I^2 = heterogeneity; χ^2 = Chi^2 value of test for subgroup differences; *df* = degrees of freedom; ns = not significant; FU = follow-up.

[a] For delivery format, some subgroups were added (compared to the review protocol) based on the evidence found.

Table C7.3

Subgroup Analyses Concerning Training Intensity

Outcome/ subgroup	Studies	*N*	SMD random-effects, 95% CI	*p*	I^2	Interpretation (Cohen, 1988)
Resilience, posttest						
Test for subgroup differences: $\chi^2 = 2.99$, $df = 2$, $p = .22$, $I^2 = 33.1\%$						
moderate	2	67	0.05 [−0.85, 0.95]	.91	71%	ns
high	9	550	0.47 [0.27, 0.66]	< .001	20%	moderate
unspecified	1	73	0.84 [0.36, 1.32]	< .001	/	large
Depression, posttest						
Test for subgroup differences: $\chi^2 = 0.26$, $df = 2$, $p = .88$, $I^2 = 0\%$						
moderate	6	395	−0.23 [−0.47, 0.01]	.06	24%	ns
high	6	262	−0.35 [−0.82, 0.11]	.14	57%	ns
unspecified	2	131	−0.33 [−0.98, 0.32]	.32	66%	ns
(Perceived) stress, posttest						
Test for subgroup differences: $\chi^2 = 0.23$, $df = 1$, $p = .63$, $I^2 = 0\%$						
moderate	6	307	−0.83 [−2.24, 0.58]	.25	96%	ns
high	11	690	−0.48 [−0.75, −0.20]	< .001	62%	moderate
Well-being, posttest						
Test for subgroup differences: $\chi^2 = 1.15$, $df = 1$, $p = .28$, $I^2 = 13.3\%$						
moderate	4	210	0.34 [−0.16, 0.84]	.19	64%	ns
high	9	1284	0.06 [−0.06, 0.17]	.34	0%	ns
Resilience, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: $\chi^2 = 17.84$, $df = 2$, $p < .001$, $I^2 = 88.8\%$						
low	3	98	0.53 [−0.14, 1.20]	.12	61%	ns
moderate	1	733	−0.05 [−0.20, 0.09]	.50	/	ns
high	7	494	0.46 [0.26, 0.66]	< .001	18%	moderate
(Perceived) stress, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: $\chi^2 = 1.74$, $df = 2$, $p = .42$, $I^2 = 0\%$						
low	3	97	−0.74 [−1.16, −0.32]	< .001	0%	large
moderate	2	76	−0.46 [−0.93, 0.01]	.06	3%	ns
high	9	615	−0.40 [−0.68, −0.12]	.005	66%	moderate
Well-being, short-term FU (≤ 3 months postintervention)						
Test for subgroup differences: $\chi^2 = 0.40$, $df = 2$, $p = .82$, $I^2 = 0\%$						
low	2	58	0.32 [−0.29, 0.93]	.30	23%	ns
moderate	3	850	0.15 [−0.23, 0.53]	.43	65%	ns
high	7	505	0.12 [−0.06, 0.29]	.19	0%	ns

Note. *N* = number of participants; SMD = standardized mean difference; CI = confidence interval; *p* = *p* value; I^2 = heterogeneity; χ^2 = Chi^2 value of test for subgroup differences; *df* = degrees of freedom; ns = not significant; FU = follow-up.

Another subgroup analysis for study comparator is presented in the corresponding Cochrane publication (Kunzler et al., 2020) but was not considered in this thesis.

Appendix C8

Sensitivity Analyses

Outcome/ subgroup	Studies	*N*	SMD random-effects, 95% CI	*p*	I^2	Interpretation (Cohen, 1988)
Posttest						
Resilience						
Original analysis: SMD 0.45, 95% CI [0.25, 0.65], $p < .001$, $I^2 = 41\%$ (12 studies; 690 participants; moderate ES)						
Resilience scale	11	669	0.45 [0.24, 0.67]	< .001	46%	moderate
Attrition bias	8	466	0.50 [0.30, 0.71]	< .001	13%	moderate
Reporting bias	/	/	/	/	/	/
Trial registration	**2**	**54**	**−0.07 [−0.82, 0.67]**	**.84**	**45%**	**ns**
Level missing data	8	466	0.50 [0.30, 0.71]	< .001	13%	moderate
Coping missing data	7	393	0.45 [0.25, 0.65]	< .001	0%	moderate
Fixed-effect	12	690	0.47 [0.31, 0.62]	< .001	41%	moderate
Depression						
Original analysis: SMD −0.29, 95% CI [−0.50, −0.09], $p = .005$, $I^2 = 42\%$ (14 studies; 788 participants; small ES)						
Attrition bias	**5**	**169**	**−0.41 [−0.72, −0.11]**	**.008**	**0%**	**moderate**
Reporting bias	**10**	**510**	**−0.30 [−0.61, 0.01]**	**.06**	**57%**	**ns**
Trial registration	**6**	**289**	**−0.10 [−0.33, 0.14]**	**.41**	**0%**	**ns**
Level missing data	**6**	**239**	**−0.34 [−0.60, −0.09]**	**.009**	**0%**	**moderate**
Coping missing data	8	410	−0.33 [−0.53, −0.14]	< .001	0%	small
Fixed-effect	14	788	−0.28 [−0.43, −0.14]	< .001	42%	small
(Perceived) stress						
Original analysis: SMD −0.61, 95% CI [−1.07, −0.15], $p = .01$, $I^2 = 90\%$ (17 studies; 997 participants; moderate ES)						
Attrition bias	**10**	**621**	**−0.75 [−1.47, −0.02]**	**.04**	**94%**	**large**
Reporting bias	**13**	**822**	**−0.81 [−1.38, −0.25]**	**.005**	**92%**	**large**
Trial registration	**5**	**197**	**−0.15 [−0.43, 0.14]**	**.31**	**0%**	**ns**
Level missing data	**11**	**690**	**−0.64 [−1.30, 0.02]**	**.06**	**94%**	**ns**
Coping missing data	12	727	−0.62 [−1.23, −0.01]	.05	93%	moderate
Fixed-effect	17	997	−0.56 [−0.70, −0.42]	< .001	90%	moderate
Well-being						
Original analysis: SMD 0.14, 95% CI [−0.01, 0.30], $p = .07$, $I^2 = 31\%$ (13 studies; 1494 participants; ns)						
Attrition bias	8	1112	0.15 [−0.06, 0.35]	.17	41%	ns
Reporting bias	**9**	**628**	**0.20 [0.03, 0.36]**	**.02**	**0%**	**small**
Trial registration	3	834	−0.04 [−0.18, 0.10]	.56	0%	ns
Level missing data	7	412	0.20 [−0.06, 0.46]	.14	41%	ns
Coping missing data	9	1179	0.11 [−0.08, 0.29]	.27	37%	ns
Fixed-effect	13	1494	0.08 [−0.02, 0.19]	.13	31%	ns
Short-term FU (≤ 3 months postintervention)						
Resilience						
Original analysis: SMD 0.42, 95% CI [0.17, 0.67], $p = .001$, $I^2 = 71\%$ (11 studies; 1325 participants; moderate ES)						
Resilience scale	/	/	/	/	/	/
Attrition bias	**5**	**337**	**0.31 [0.09, 0.54]**	**.007**	**9%**	**small**
Reporting bias	/	/	/	/	/	/

Outcome/ subgroup	Studies	*N*	SMD random-effects, 95% CI	*p*	I^2	Interpretation (Cohen, 1988)
Trial registration	**1**	**733**	**−0.03 [−0.12, 0.06]**	**.50**	**/**	**ns**
Level missing data	4	311	0.36 [0.13, 0.58]	.002	0%	moderate
Coping missing data	**5**	**337**	**0.31 [0.09, 0.54]**	**.007**	**9%**	**small**
Fixed-effect	**11**	**1325**	**0.18 [0.07, 0.29]**	**.001**	**71%**	**small**
(Perceived) stress						
Original analysis: SMD −0.46, 95% CI [−0.67, −0.25], $p < .001$, $I^2 = 53\%$ (14 studies; 788 participants; moderate ES)						
Attrition bias	**7**	**427**	**−0.30 [−0.52, −0.07]**	**.009**	**24%**	**small**
Reporting bias	11	644	−0.52 [−0.75, −0.29]	< .001	50%	moderate
Trial registration	**3**	**144**	**−0.22 [−0.71, 0.28]**	**.39**	**52%**	**ns**
Level missing data	**7**	**471**	**−0.27 [−0.53, −0.00]**	**.05**	**51%**	**small**
Coping missing data	**9**	**534**	**−0.27 [−0.49, −0.06]**	**.01**	**36%**	**small**
Fixed-effect	14	788	−0.43 [−0.58, −0.29]	< .001	53%	moderate
Well-being						
Original analysis: SMD 0.07, 95% CI [−0.04, 0.18], $p = .22$, $I^2 = 1\%$ (12 studies; 1413 participants; ns)						
Attrition bias	7	422	0.14 [−0.05, 0.33]	.16	0%	ns
Reporting bias	9	1227	0.03 [−0.08, 0.14]	.59	0%	ns
Trial registration	4	919	0.10 [−0.17, 0.36]	.46	47%	ns
Level missing data	5	348	0.04 [−0.17, 0.25]	.70	0%	ns
Coping missing data	7	411	0.04 [−0.15, 0.24]	.66	0%	ns
Fixed-effect	12	1413	0.06 [−0.04, 0.17]	.24	1%	ns

Note. *N* = number of participants; SMD = standardized mean difference; CI = confidence interval; *p* = *p* value; I^2 = heterogeneity; ns = not significant; FU = follow-up; text in bold: different finding in sensitivity analysis compared to original pooled analysis for the respective outcome.

Appendix C9

Results on Statistical Heterogeneity

Outcome, time point, number of studies, SMD/MD (95% CI)	Statistical indicators of heterogeneity	Interpretation of statistical heterogeneity
Primary outcomes		
Resilience, postintervention, 12 studies: SMD 0.45, 95% CI [0.25, 0.65]	• I^2 = 41% • τ^2 = 0.05 • *p* for heterogeneity (χ^2 test): .07 • G^2 = 66.9% • 95% prediction interval [−0.25, 1.14]	moderate heterogeneity
Resilience, short-term FU, 11 studies: SMD 0.42, 95% CI [0.17, 0.67]	• I^2 = 71% • τ^2 = 0.11 • *p* for heterogeneity (χ^2 test): < .001 • G^2 = 85.4% • 95% prediction interval [−0.40, 1.24]	substantial heterogeneity
Resilience, medium-term FU, two studies: SMD 0.35, 95% CI [−0.41, 1.11]	• I^2 = 87% • τ^2 = 0.27 • *p* for heterogeneity (χ^2 test): .005 • G^2 = 0% • 95% prediction interval: incalculable due to only two studies	mixed; considerable (e.g., I^2) versus no heterogeneity (G^2)
Resilience, long-term FU, two studies: SMD 0.30, 95% CI [−0.08, 0.68]	• I^2 = 0% • τ^2= 0 • *p* for heterogeneity (χ^2 test): .97 • G^2 = 0% • 95% prediction interval: incalculable due to only two studies	no heterogeneity
Anxiety, postintervention, five studies: SMD −0.06, 95% CI [−0.35 to 0.23]	• I^2 = 0% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .99 • G^2 = 0.5% • 95% prediction interval [−0.19, 0.06]	no heterogeneity
Anxiety, short-term FU, four studies: SMD −0.63, 95% CI [−0.98 to −0.27]	• I^2 = 0% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .95 • G^2 = 0% • 95% prediction interval [−1.40, 0.15]	no heterogeneity
Depression, postintervention, 14 studies: SMD −0.29, 95% CI [−0.50 to −0.09]	• I^2 = 42% • τ^2 = 0.06 • *p* for heterogeneity (χ^2 test): .05 • G^2 = 89.2% • 95% prediction interval [−0.95, 0.37]	mixed; moderate (I^2) versus substantial heterogeneity (e.g., χ^2 test, G^2)
Depression, short-term FU, eight studies: SMD −0.52, 95% CI [−0.81 to −0.23]	• I^2 = 50% • τ^2 = 0.08 • *p* for heterogeneity (χ^2 test): .05 • G^2 = 81.6% • 95% prediction interval [−1.33, 0.29]	substantial heterogeneity
Depression, medium-term FU, one study: MD −0.40, 95% CI [−0.75, −0.05]	/	/
Depression, long-term FU, two studies: SMD 0.09, 95% CI [−0.33, 0.51]	• I^2 = 0% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .56	no heterogeneity

Outcome, time point, number of studies, SMD/MD (95% CI)	Statistical indicators of heterogeneity	Interpretation of statistical heterogeneity
	• G^2 = 0% • 95% prediction interval: incalculable due to only two studies	
(Perceived) stress, postintervention, 17 studies: SMD −0.61, 95% CI [−1.07, −0.15)	• I^2 = 90% • τ^2 = 0.79 • *p* for heterogeneity (χ^2 test): < .001 • G^2 = 98.7% • 95% prediction interval [−2.86, 1.65]	substantial to considerable heterogeneity
(Perceived) stress, short-term FU, 14 studies: SMD −0.46, 95% CI [−0.67, −0.25]	• I^2 = 53% • τ^2 = 0.08 • *p* for heterogeneity (χ^2 test): .01 • G^2 = 99.1% • 95% prediction interval [−1.14, 0.22]	mixed; moderate to substantial heterogeneity (I^2) versus substantial heterogeneity (e.g., G^2)
(Perceived) stress, medium-term FU, one study: MD −0.40, 95% CI [–0.73, −0.07]	/	/
(Perceived) stress, long-term FU, three studies: SMD −0.39, 95% CI [−0.84, 0.05]	• I^2 = 47% • τ^2 = 0.07 • *p* for heterogeneity (χ^2 test): .15 • G^2 = 94.0% • 95% prediction interval [−4.85, 4.07]	mixed; moderate (e.g., I^2, χ^2 test) versus substantial heterogeneity (G^2)
Well-being, postintervention, 13 studies: SMD 0.14, 95% CI [−0.01, 0.30]	• I^2 = 31% • τ^2 = 0.02 • *p* for heterogeneity (χ^2 test): .13 • G^2 = 93.4% • 95% prediction interval [−0.41, 0.75]	mixed; moderate (e.g., I^2) versus substantial heterogeneity (G^2)
Well-being, short-term FU, 12 studies: SMD 0.07, 95% CI [−0.04, 0.18]	• I^2 = 1% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .43 • G^2 = 78.1% • 95% prediction interval [−0.21, 0.46]	mixed; no important (e.g., I^2) versus substantial heterogeneity (G^2)
Well-being, medium-term FU, three studies: SMD −0.08, 95% CI [−0.31, 0.16]	• I^2 = 73% • τ^2 = 0.03 • *p* for heterogeneity (χ^2 test): .02 • G^2 = 97.7% • 95% prediction interval [−2.71, 2.56]	substantial heterogeneity
Well-being, long-term FU, one study: MD −0.20, 95% CI [−0.94, 0.54]	/	/
Secondary outcomes		
Social support, postintervention, one *mixed* study	/	/
Social support, short-term FU, two studies: SMD −0.07, 95% CI [−0.22, 0.08]	• I^2 = 0% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .96 • G^2 = 0% • 95% prediction interval: incalculable due to only two studies	no heterogeneity
Social support, medium-term FU, one study: MD −0.10, 95% CI [−0.27, 0.07]	/	/
Optimism, postintervention, three studies: SMD 0.41, 95% CI [0.10, 0.72]	• I^2 = 0% • τ^2 = 0	no heterogeneity

Outcome, time point, number of studies, SMD/MD (95% CI)	Statistical indicators of heterogeneity	Interpretation of statistical heterogeneity
Optimism, short-term FU, two studies: SMD 0.44, 95% CI [0.12, 0.76]	• *p* for heterogeneity (χ^2 test): .93 • G^2 = 0% • 95% prediction interval [−1.58, 2.40] • I^2 = 0% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .72 • G^2 = 0% • 95% prediction interval: incalculable due to only two studies	no heterogeneity
Self-efficacy, postintervention, six studies: SMD 0.43, 95% CI [0.25, 0.62]	• I^2 = 0% • τ^2 = 0 • *p* for heterogeneity (χ^2 test): .52 • G^2 = 45.8% • 95% prediction interval [−0.006, 0.88]	mixed; no (e.g., I^2) versus moderate heterogeneity (G^2)
Self-efficacy, short-term FU, seven studies: SMD 0.32, 95% CI [0.13, 0.51]	• I^2 = 51% • τ^2 = 0.03 • *p* for heterogeneity (χ^2 test): .06 • G^2 = 69.7% • 95% prediction interval [−0.30, 0.96]	moderate to substantial heterogeneity
Self-efficacy, medium-term FU, one study: MD 0.17, 95% CI [0.10, 0.24]	/	/
Self-efficacy, long-term FU, one study: MD 0.19, 95% CI [−0.02, 0.40]	/	/
Active coping, postintervention, three studies: SMD 0.28, 95% CI [−0.31, 0.87]	• I^2 = 52% • τ^2 = 0.14 • *p* for heterogeneity (χ^2 test): .12 • G^2 = 93.6% • 95% prediction interval [−5.85, 6.41]	moderate to substantial heterogeneity
Active coping, short-term FU, one study: MD −0.02, 95% CI [−0.11, 0.07]	/	/
Active coping, medium-term FU, one study: MD −0.03, 95% CI [−0.12, 0.06]	/	/
Self-esteem, short-term FU, one study: MD 5.30, 95% CI [3.67, 6.93]	/	/
Hardiness, postintervention, one study: MD 3.52, 95% CI [−1.19, 8.23]	/	/
Hardiness, medium-term FU, one study: MD 0.24, 95% CI [−3.74, 4.22]	/	/
Positive emotions, postintervention, two studies: SMD 0.85, 95% CI [0.17, 1.53]	• I^2 = 82% • τ^2 = 0.20 • *p* for heterogeneity (χ^2 test): .02 • G^2 = 0% • Prediction interval: incalculable due to only two studies	mixed; substantial to considerable (e.g., I^2) versus no heterogeneity (G^2)
Positive emotions, short-term FU, one study: MD 4.21, 95% CI [1.49, 6.93]	/	/

Note. SMD = standardized mean difference; MD = mean difference; CI = confidence interval; I^2 = I^2 value (heterogeneity); τ^2= Tau^2 (heterogeneity); *p* = *p* value; χ^2 = Chi^2 test for heterogeneity; G^2 = G^2 value (heterogeneity); FU = follow-up.

Appendix C10 Contour-Enhanced Funnel Plots

Figure C10.1

Contour-Enhanced Funnel Plot of Comparison Resilience Intervention Versus Control, HCP, Resilience: Postintervention.

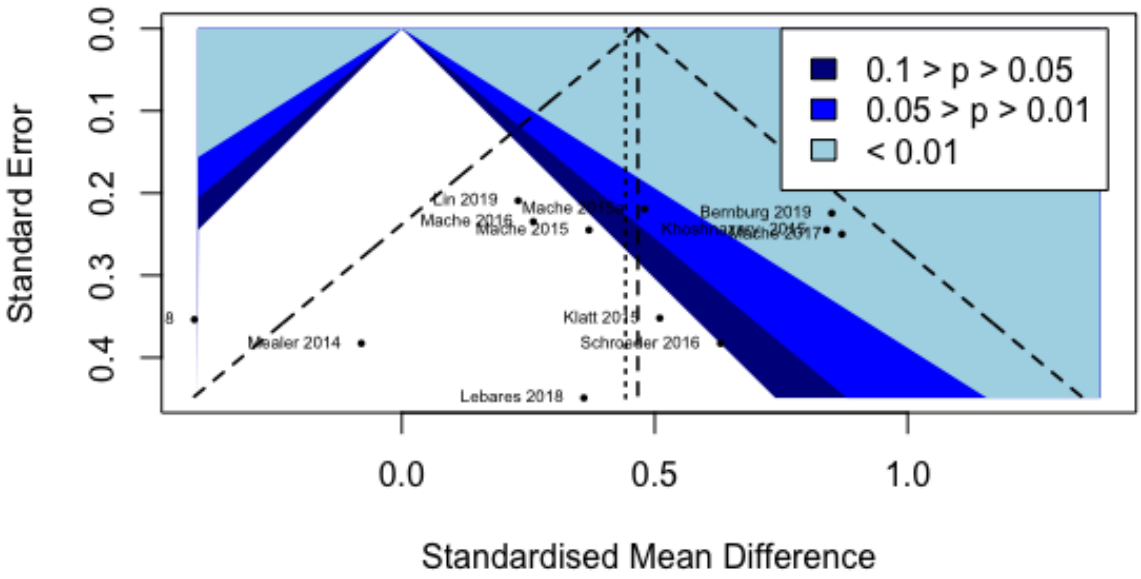


Figure C10.2

Contour-Enhanced Funnel Plot of Comparison Resilience Intervention Versus Control, HCP, Depression: Postintervention.

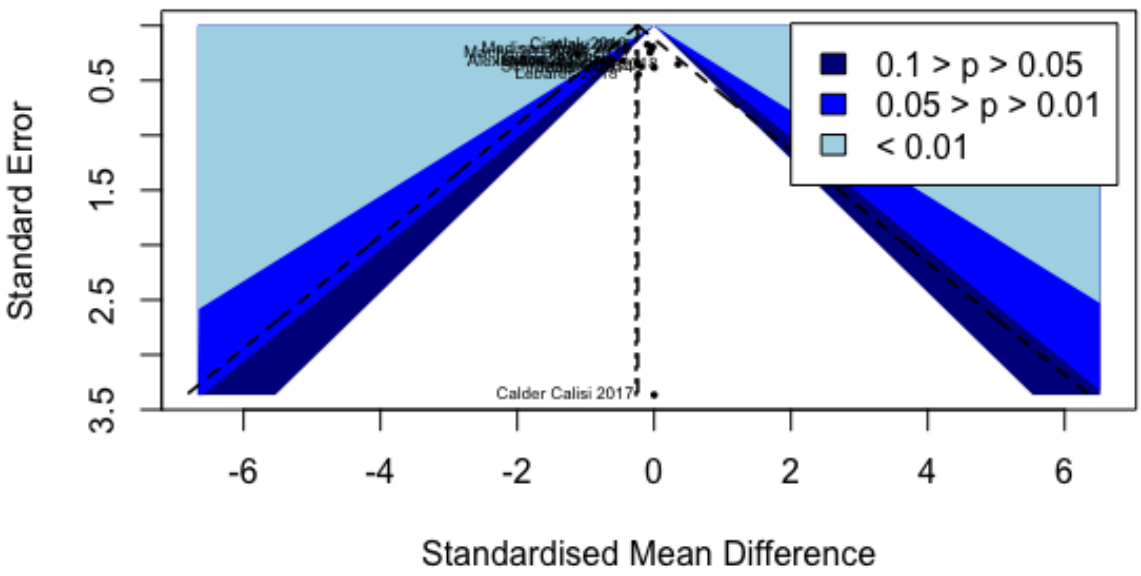


Figure C10.3

Contour-Enhanced Funnel Plot of Comparison Resilience Intervention Versus Control, HCP, (Perceived) Stress: Postintervention.

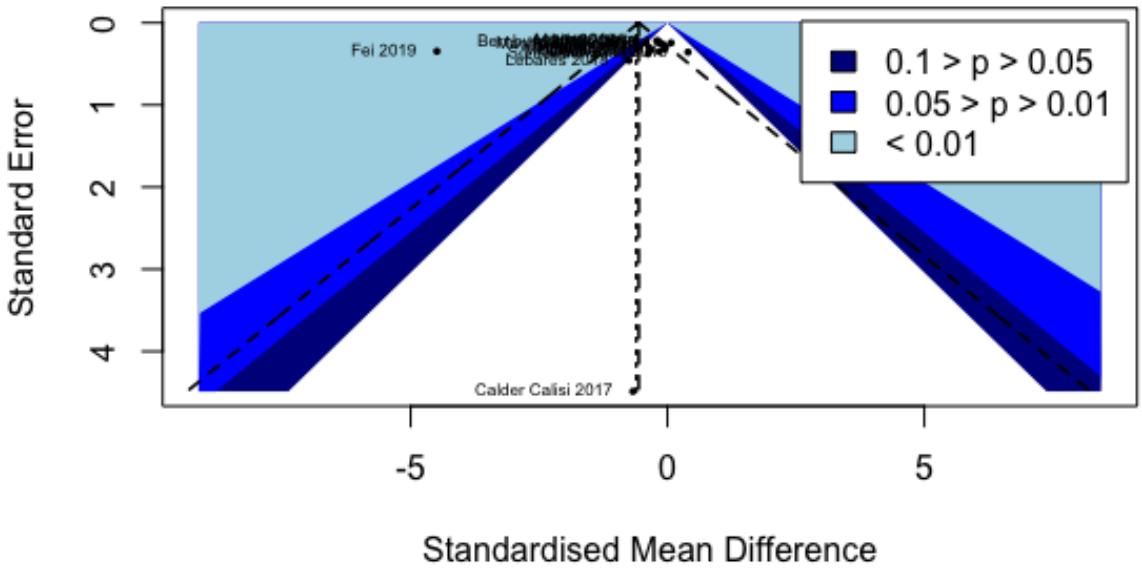
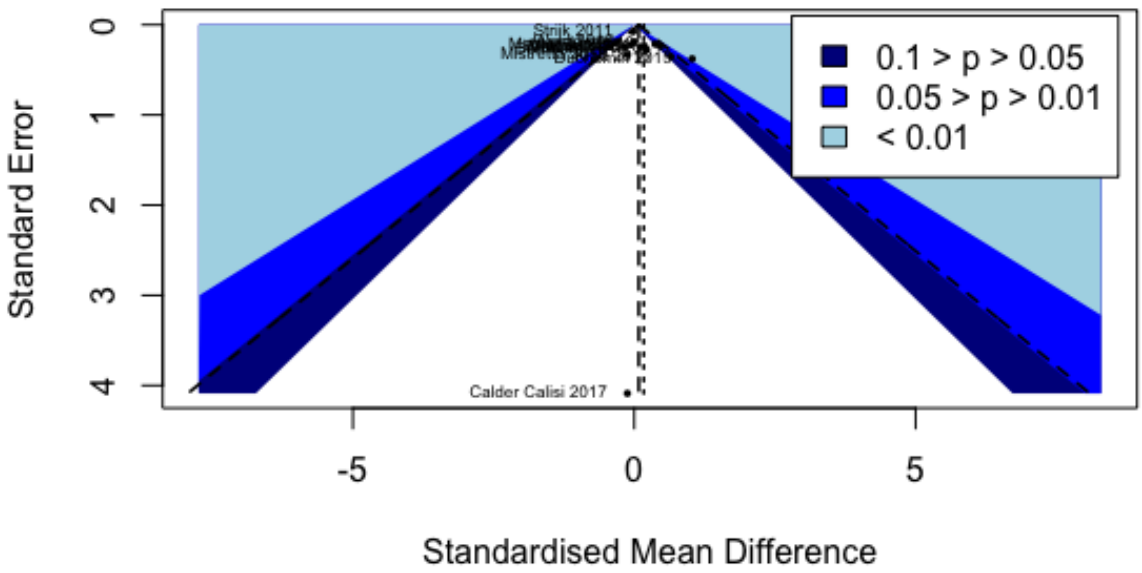


Figure C10.4

Contour-Enhanced Funnel Plot of Comparison Resilience Intervention Versus Control, HCP, Well-being: Postintervention.



Appendix C11 Further Details on Overall Completeness and Applicability of Evidence

- **Participants:**
  - *Gender:* Most study participants were female (e.g., proportion of female participants in 32 studies solely conducted in healthcare staff and reporting total numbers for participants' sex: 68.6%).
  - *Age:* The included studies mainly considered young professionals (e.g., junior physicians; Bernburg et al., 2016) and middle-aged employees up to approximately 50 years of age (Strijk et al., 2011); for example, mean age across 25 studies solely conducted in healthcare workers and reporting age: *M* = 37.7 years, *SD* = 6.7
  - *Healthcare sectors:*
    - The studies were mainly conducted in a hospital setting (37/44 studies)
    - Various medical departments were represented (e.g., Psychiatry, Cardiology, Emergency, Surgery, Intensive Care Unit), with no clear majority of a certain healthcare sector.
  - *Mental health at baseline:*
    - 15 of the 44 studies provided no data about the mental health status of the sample or only reported to include healthy participants.
    - All studies measuring mental functioning used self-report (screening) measures covering one or a small number of mental dysfunctions.
    - Comprehensive baseline diagnostics of mental health by the use of a structured clinical interview were not conducted.
    - Five studies only included mentally healthy participants or individuals showing symptoms below a cut-off in a screening measure. Two studies only considered participants without mental stress (not further specified) or without taking mood-modulating drugs.
    - Overall, drawing from those studies assessing mental health, the severity of impairment ranged between (mostly) no mental symptoms (Strijk et al., 2011; Wild, 2016) to moderate and high levels of mental dysfunctions at least in a certain part of the sample,

for example, compared to norm samples (Cheung, 2014; Mealer et al., 2014; West et al., 2014)

- *Study location:*
  - North America (20 studies), Europe (12 studies), Asia (including the Near East; 9 studies), only three studies from Australia
  - High-income countries in 36 studies: Australia, Canada, Germany, Israel, Italy, Poland, the Netherlands, UK, and USA
  - Upper-middle-income countries in eight studies: China, Iran, and Sri Lanka

- **Interventions:**
  - All 44 studies examined the effects of a psychological intervention to foster resilience, hardiness, or posttraumatic growth compared to a control condition in HCP.
  - The evidence found is restricted to certain types of intervention settings, delivery formats, training intensities, and theoretical foundations.
  - Thirty of the 44 studies assessed the efficacy of resilience interventions in group setting, whereas only eight were conducted in combined settings and four in individual settings (unclear setting for two studies).
  - The same pattern was seen for the delivery format of interventions with the majority of studies (29/44) investigating face-to-face delivery, followed by multimodal delivery (10/44; e.g., web-based intervention and daily diary). Only three studies delivered in an online- or mobile-based format were identified.
  - Most of the interventions were of relatively high or moderate intensity (high: 18/44: > 12 hours or sessions; moderate: 15/44) compared to low-intensity trainings (7/44) and studies with unclear training intensity (4/44). Treatment durations ranged considerably from a 40-minute single session to 87 hours or 77 sessions in total.
  - Except for ACT and sole problem-solving approaches, all theoretical foundation**s** prespecified (Helmreich, Kunzler et al., 2017) have been tested in the RCTs found in this review. The

number of RCTs varies, with most studies investigating combined theoretical foundations (e.g., CBT and mindfulness).

- **Outcomes:**
  - Different measures for resilience in this review (see Appendix D13), ranging from resilience scales measuring resilience as trait (e.g., Resilience Scale-14), as summary of resilience factors (e.g., Connor-Davidson Resilience Scale), or as outcome (e.g., Brief Resilience Scale)
  - Although there is still no consensus about the *definition of resilience*, two aspects are viewed as essential: the exposure to substantial risk or adversity and the maintenance or fast recovery of mental health despite this adversity (e.g., Earvolino-Ramirez, 2007; Kalisch et al., 2017). By considering studies of HCP – a population group often exposed to significant stressors – that assessed resilience or another measure of psychological adjustment, a greater homogeneity between the included studies was ensured.

Appendix C12

Summary of Findings Table

Resilience interventions compared to control condition for healthcare professionals

Patient or population: healthcare professionals including healthcare staff delivering direct medical care (e.g., nurses, physicians, hospital personnel) and allied healthcare staff (e.g., social workers, psychologists); aged 18 years and older; irrespective of health status
Setting: Any healthcare sectors (e.g., psychiatric departments, intensive care unit, surgery, family medicine, internal medicine)
Intervention: Any psychological resilience intervention focused on fostering resilience or the related concepts of hardiness or posttraumatic growth by strengthening well-evidenced resilience factors that are thought to be modifiable by training (see Appendix D2.2); irrespective of content, duration, setting, or delivery mode
Comparison: no intervention, wait-list control, treatment as usual (TAU), active control, attention control

	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	№ of participants (studies)	Certainty of the evidence (GRADE)	
Outcomes	Risk with control	Risk with resilience interventions				Comments
Resilience Measured by: investigators measured resilience using different instruments; higher scores mean higher resilience Timing of outcome assessment: postintervention	See comment	The mean resilience score in the intervention groups was, on average, 0.45 standard deviations higher (0.25 higher to 0.65 higher)	-	690 (12 RCTs)	⊕⊖⊖⊖ Very low[a]	SMD of 0.45 represents a moderate effect size (Cohen, 1988).
Mental health and well-being: anxiety Measured by: investigators measured anxiety using different instruments; lower scores mean lower anxiety Timing of outcome assessment: postintervention	See comment	The mean anxiety score in the intervention groups was, on average, 0.06 standard deviations lower (0.35 lower to 0.23 higher)	-	231 (5 RCTs)	⊕⊖⊖⊖ Very low[b]	SMD of 0.06 represents a small effect size (Cohen, 1988).
Mental health and well-being: depression Measured by: investigators measured depression using different instruments; lower scores mean lower depression Timing of outcome assessment: postintervention	See comment	The mean depression score in the intervention groups was, on average, 0.29 standard deviations lower (0.50 lower to 0.09 lower)	-	788 (14 RCTs)	⊕⊖⊖⊖ Very low[c]	SMD of 0.29 represents a small effect size (Cohen, 1988).

Outcomes	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	№ of participants (studies)	Certainty of the evidence (GRADE)	Comments
	Risk with control	Risk with resilience interventions				
Mental health and well-being: (perceived stress) Measured by: investigators measured (perceived) stress using different instruments; lower scores mean lower (perceived) stress Timing of outcome assessment: postintervention	See comment	The mean (perceived) stress score in the intervention groups was, on average, 0.61 standard deviations lower (1.07 lower to 0.15 lower)	-	997 (17 RCTs)	⊕⊖⊖⊖ Very low[d]	SMD of 0.61 represents a moderate effect size (Cohen, 1988).
Mental health and well-being: well-being Measured by: investigators measured well-being using different instruments; higher scores mean higher well-being Timing of outcome assessment: postintervention	See comment	The mean well-being score in the intervention groups was, on average, 0.14 standard deviations higher (0.01 lower to 0.30 higher)	-	1494 (13 RCTs)	⊕⊖⊖⊖ Very low[e]	SMD of 0.14 represents a small effect size (Cohen, 1988).
Adverse events	There were no adverse events reported in association with study participation in three studies.		-	784 (3 RCTs)	⊕⊖⊖⊖ Very low[f]	-

*The risk in the intervention group (and its 95% CI) is based on the assumed risk in the comparison group and the relative effect of the intervention (and its 95% CI).

GRADE Working Group grades of evidence
High certainty: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate certainty: we are moderately confident in the effect estimate; the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low certainty: our confidence in the effect estimate is limited; the true effect may be substantially different from the estimate of the effect
Very low certainty: we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect

Note. CI: confidence interval; RCT: randomized controlled trial; SMD: standardized mean difference.

[a] Downgraded two levels due to study limitations (unclear risk of selection and detection bias, high and unclear risk of attrition bias, high risk of performance bias), one level due to unexplained inconsistency (I^2 = 41%), and one level due to indirectness (studies limited to certain participants [young and middle-aged adults], interventions [e.g., group setting, face-to-face delivery, moderate and high intensity, mindfulness-based training and combination], and comparators [no intervention, wait-list]).

[b] Downgraded two levels due to study limitations (unclear risk of selection and detection bias, unclear and high risk of performance bias, high risk of attrition bias), one level due to indirectness (studies limited to certain participants [middle-aged adults]), and two levels due to imprecision (< 400 participants; 95% CI wide and inconsistent).

[c] Downgraded two levels due to study limitations (unclear risk of selection and detection bias, unclear and high risk of performance bias, high risk of attrition bias), one level due to unexplained inconsistency (I^2 = 42%), one level due to indirectness (studies limited to certain participants [middle-aged adults], interventions [e.g., group setting, face-to-face delivery, moderate and high intensity, mindfulness-based training and combination], and comparators [no intervention]), and one level due to imprecision (95% CI wide and inconsistent).

[d] Downgraded two levels due to study limitations (unclear risk of selection and detection bias, high and unclear risk of attrition bias, high risk of performance bias), one level due to unexplained inconsistency (I^2 = 90%), and one level due to indirectness (studies limited to certain participants [young and middle-aged adults], interventions [e.g., group setting, face-to-face delivery, moderate and high intensity, mindfulness-based training and combination], and comparators [no intervention, wait-list]).

[e] Downgraded two levels due to study limitations (unclear risk of selection and detection bias, unclear and high risk of attrition bias, high risk of performance bias), one level due to unexplained inconsistency (I^2 = 31%), one level due to indirectness (studies limited to certain participants [young and middle-aged adults], interventions [e.g., group setting, face-to-face delivery, moderate and high intensity, mindfulness-based trainings and combination], and comparators [no intervention, wait-list]), and one level due to imprecision (95% CI wide and inconsistent).

[f] Downgraded two levels due to study limitations (unclear risk of selection and detection bias, high and unclear risk of attrition and other bias [no or unclear systematic and validated assessment of adverse events], high risk of performance bias), and one level due to indirectness (studies limited to certain interventions [e.g., combined setting, face-to-face delivery, high intensity, mindfulness-based training]).

Appendix C13 Results Concerning Publication Bias for Three Outcomes at Short-Term FU

For three primary outcomes, for which funnel plots and Egger's test were also examined at *short-term FU* (> 10 studies), similar results were found for (perceived) stress, with no indication of publication bias. Despite statistical and (slight) visual evidence of funnel plot asymmetry for resilience and well-being at short-term FU, a publication bias was also not assumed for these outcomes for several reasons: “Negative” (i.e., statistically non-significant) studies had also been published and studies appeared to be missing in the area of high statistical significance ($p < .01$), making a publication bias unlikely according to the Cochrane Handbook (Page et al., 2019). In addition, the results of one unpublished study in both meta-analyses (Cheung, 2014) did not differ from other published studies. For both outcomes, the evidence was based on a multitude of small studies (resilience: 10/11, well-being: 11/12 studies). In such cases, according to the GRADE approach, publication bias should be suspected if most of these studies have been commercially funded or when conflicts of interest are assumed (Guyatt, Oxman, Montori, et al., 2011). For resilience and well-being, a potential conflict of interest was indicated from the authors or likely for three studies (Chesak et al., 2015; Sood et al., 2011; Sood et al., 2014). However, as these studies represented a minority and several of them also included non-significant results, there was insufficient evidence for publication bias. Other forms of selection bias (language bias, location or database bias, multiple publication bias, provision of data bias, citation bias, outcome reporting bias) could not explain the funnel plot asymmetry. For both outcomes, the non-significant finding of Cheung (2014) as an unpublished study could indicate a potential time-lag bias. Again, small-study effects were difficult to assess for both outcomes due to the lack of larger studies, but were unlikely, as both significant and non-significant results were reported by small studies. Besides, the ES did not differ according to study size due to true heterogeneity (Page et al., 2019), as there were no consistent clinical (e.g., population, setting, or delivery format of resilience intervention) or methodological differences between studies of different size. Finally, it must be considered that alternative explanations of funnel plot asymmetry for both outcomes might refer to artefacts due the use of SMDs or chance (Page et al., 2019).

Appendix C14 Prevention of Potential Biases by the Search Methods of This Review

Extensive searches of relevant databases were performed, reference lists were checked, and grey literature (e.g., conference abstracts, unpublished dissertations, and theses) was considered. The search process was designed in conjunction with, and supervised by, the Cochrane Developmental, Psychosocial, and Learning Problems (CDPLP) Information Specialist, to minimize bias in the acquisition of potentially relevant references. The authors of (included) studies were contacted to ask for, for example, full texts or additional data where reported data were insufficient or missing. In all phases of the review process, it was repeatedly (at least twice) tried to contact the study authors by email, when needed. Regarding data analysis, correspondence with the authors was required for 32 included studies. For 19 studies, the replies received allowed to include these studies in quantitative analysis (e.g., West et al., 2014).

Appendix C15

Implications for Practice – Training Elements of Successful Interventions

Intervention content	Studies[a]
Problem-solving	e.g., work-related situations (Bernburg et al., 2019; Mache et al., 2017)
Emotion regulation	e.g., recognition and evaluation of own emotions; emotional management by mindfulness (Bernburg et al., 2019; Fei, 2019; Lin et al., 2019; Mache et al., 2017)
Conflict and anger management	e.g., Bernburg et al., 2019; Fei, 2019; Luthar et al., 2017; Mache et al., 2017
Goal setting and planning for the future	e.g., Berger & Gelkopf, 2011; Bernburg et al., 2019
Communication training	e.g., Bernburg et al., 2019; Lin et al., 2019; Mache et al., 2017
Information on how to recognize stress and prevent burnout	e.g., „say no“ (Berger & Gelkopf, 2011; Fei, 2019)
Build and use social support and feedback	e.g., Berger & Gelkopf, 2011; Luthar et al., 2017
Techniques on mindfulness (e.g., mindfilness meditation) and (self-)compassion	e.g., Klatt et al., 2015; Lin et al., 2019; Luthar et al., 2017; Schroeder et al., 2016; Sood et al., 2011
Strategies to challenge irrational beliefs and to minimize rumination	e.g., ABCDE technique (Fei, 2019; Luthar et al., 2017)
Positive attention training, cultivation of flexible interpretations, and positive re-framing of life experiences	e.g., Chesak et al., 2015; Sood et al., 2011

Note. ABCDE technique = Activating Event, Belief, Consequences, Disputation, Effects.

[a] Nine studies with evidence for moderate to large positive effects on at least two of all outcomes of this review (Berger & Gelkopf, 2011; Bernburg et al., 2019; Chesak et al., 2015; Fei, 2019; Lin et al., 2019; Luthar et al., 2017; Mache et al., 2017; Schroeder et al., 2016; Sood et al., 2011).

Appendix D Digital Appendix Cochrane Review

D1. Background – Stressor Exposure in HCP, Definition of Resilience, Theories of Change, and Resilience Intervention Research

- D1.1 Further Information on the Stressor Exposure in HCP and its Consequences
- D1.2 Further Information on Definition of Resilience (Especially Process-Oriented Approach)
- D1.3 Theories of Change Based on Theoretical Foundations of Resilience Interventions (Detailed)
- D1.4 Previous Reviews/Meta-Analyses on Resilience Interventions in (Non-)Clinical Populations
- D1.5 Effect Sizes Found in Previous Reviews on Resilience Interventions in (Non-)Clinical Populations
- D1.6 Previous Reviews/Meta-Analyses on Resilience Interventions in Healthcare Workers
- D1.7 Summary of Methodological Characteristics of Previous Reviews and Meta-Analyses

D2. Methods – Preparation of the Cochrane Review (Prespecified in Review Protocol Helmreich, Kunzler et al., 2017)

- D2.1 Evidence Rating of Modifiable Resilience Factors
- D2.2 Examples of Training Methods to Address Resilience Factors With Evidence Levels 1a-1c
- D2.3 Potential (Prespecified) Scales to Assess Psychological Resilience
- D2.4 Potential (Prespecified) Scales to Measure Mental Health and Well-Being
- D2.5 Potential (Prespecified) Scales to Assess Resilience Factors

D3. Methods – Search Strategies

- D3.1 Search Strategies January 1990 to October 2016 (1st Search)
- D3.2 Search Strategies 2016 Onwards (2nd Search)

D4. Methods – R Scripts – Heterogeneity and Reporting Bias

D5. Results – Detailed Search Results of Both Searches (With Separate Flow Charts)

D6. Results – Detailed Characteristics of Included Studies

D7. Results – Detailed Characteristics of Studies Awaiting Classification

D8. Results – Detailed Characteristics of Ongoing Studies

D9. Results – Detailed Characteristics of Excluded Studies (Exclusion Reasons)

D10. Results – Summary Description of Included Studies

D11. Results – Intervention Content of Included Studies Depending on Theoretical Foundation

D12. Results – Risk of Bias Assessment of Included Studies

D13. Results – Additional Results on Included Studies (Multiple Treatment Groups, Primary and Secondary Outcome Scales, Quantitative Analysis and Risk of Bias, Funding Sources)

D13.1 Rationale for Selection of Relevant Intervention Groups in Studies With Multiple Treatment Groups

D13.2 Risk of Bias Assessment of Included Studies

D13.3 Primary Outcome Scales of Included Studies

D13.4 Secondary Outcome Scales of Included Studies

D13.5 Additional Results of Main Analyses (Non-Pooled Studies)

D13.6 Funding Sources of Included Studies

D14. Results – Forest Plots for Subgroup Analyses (RQ2–5)

D15. Results – Forest Plots for Sensitivity Analyses

D16. Discussion – Details on Assessment of Reporting/Publication Bias

D17. Discussion – Additional Information

D17.1 Differences Between Protocol and Review (Including Postprotocol Amendment) – Taken From the Cochrane Review (Kunzler et al., 2020)

D17.2 Unused Methods

D18. Glossary

D19. Review Manager file for the Cochrane review Kunzler et al. (2020)_including data analyses (rm5 file)

D20. Kunzler et al. (2020)_Final publication_Psychological interventions to foster resilience in healthcare professionals

Appendix TRAIN$_4$Positivity – Development and Pilot Evaluation of a Mobile-Based Training of Positivity Bias at the Level of Action Tendencies

Appendix E Details Concerning the Theoretical Background of TRAIN$_4$Positivity

Appendix E1 Previous Studies on CBM Positivity Training at Different Levels

CBM interventions to foster an *attentional PB* largely relied on the modified visual probe task. For instance, Johnson (2009) found that individuals who were given the goal to deploy their attention to positive stimuli exhibited less emotional reactivity to a stressor than participants without any attention goal. In a RCT, Wadlinger and Isaacowitz (2008) also used a probe detection paradigm to induce selective attention to positive information and proved the role of positive attentional bias for stress reactivity. Similar findings were provided by Taylor et al. (2011) who demonstrated that participants with the greatest change in attentional allocation to positive stimuli also displayed the least anxiety reactivity to a laboratory stressor. Ferrari et al. (2016) used eye-tracking to compare a positive condition of CBM-A (CBM at the level of attention) to a negative training group. Besides, some studies examined visual search paradigms to foster an attention PB (Dandeneau & Baldwin, 2009; Dandeneau et al., 2007).

At the level of *interpretations*, Beadel et al. (2016) investigated a resilience-enhancing interpretation bias modification (CBM-I; Ambiguous Scenario Training) in people at risk of panic disorder, founding increased resilience-congruent interpretations, reduced anxiety sensitivity, and less intense panic symptoms during a stressor after training. Peters et al. (2011) also used CBM-I to modify the attributional style (AS) and influence stress vulnerability. Compared to a vulnerability condition, individuals in the resilience group (fostering a positive AS) exhibited a greater tendency for positive attributions of poor performance in a stress anagram test and reported less depressed mood.

At the *level of memory*, for example, Visser et al. (2020) used a smartphone application (“movisensXS”) to implement a memory bias modification (MBM; positive, negative, or sham memory training). Subjects were prompted a total of 10 times per day by a study smartphone to think of a recent positive event (positive condition), an unpleasant event (negative condition), or an event

related to you or study (sham condition). While the MBM significantly increased positive memory bias (i.e., change in proportion of positive memories) in the positive condition and facilitated the recall of positive autobiographical events (i.e., frequency) compared to the other two groups, positive memory bias as well as other outcomes (e.g., symptoms) did not differ between conditions.

At the level of *action tendencies*, Vrijsen et al. (2018) investigated a CBM training (pull positive pictures, push negative ones) using the AAT compared to positive CBM-A (dot-probe training) and TAU in depressive patients, and identified a decrease of depressive symptom severity. Similar findings with an AAT using functional versus dysfunctional statements were found by Lukas (2019). Becker et al. (2016) also examined the effects of CBM to enhance a PB and simultaneously reduce a NB in the processing of various affective information. In two studies, the AAT training version was adapted using diverse emotional pictures of positive and negative valence from standardized databases. After excluding high-arousal pictures, gender-non-specific images of equivalent arousal were organized into positive or negative pictures based on database ratings. In study 1, *unselected individuals* (students) were randomly assigned to a positive (PT) or negative training (NT). In both conditions, they were instructed to respond to each picture's tilt as fast as possible by pushing right-tilted pictures and pulling left-titled ones using a joystick. The picture size changed dynamically with every movement to create the zooming effect. In training trials, all positive pictures had to be pulled closer and negative ones had to be pushed away (PT). In the NT group, however, the opposite pattern was implemented. At baseline and posttest, action tendencies were measured using the AAT, including the same pictures previously used as training stimuli. Compatible trials referred to trials where positive pictures had to be pulled and negative pictures were pushed. Incompatible trials included those with reversed contingence (i.e., push positive, pull negative pictures), resulting in the CS[30]. To identify cross-over effects on attention bias, the visual probe task (dot-probe task) was conducted after training, with trained and untrained material used. Participants completed mood scales and the anagram stress task to measure stress reactivity. In study 2, the study design was modified by investigating dysphoric (Self-

[30] Compatibility Score (CS) = median RT incompatible trials [RT push positive + RT pull negative] – median RT compatible trials [RT pull positive + RT push negative]. CS > 0 (i.e., faster reactions in compatible trials): PB; CS = 0: no bias; CS < 0 (i.e., faster reactions in incompatible trials): NB.

Rating Depression Scale > 40) and non-dysphoric individuals, who had received negative mood induction and were randomized to PT or sham training (AAT assessment). Based on the same AAT paradigm (including same picture stimuli) as in Becker et al. (2016), Ferrari et al. (2018) examined which training component (i.e., pull positive pictures, push negative pictures, or both) in the AAT training is the active mechanism to change action tendencies and stress responses. Using the joystick AAT in a sample of unselected individuals (student sample), four conditions were compared: a general positivity training (Positive-Negative group [PN]; i.e., pull positive and push negative pictures), an Avoid-Negative group (AvN; i.e., push negative pictures and pull empty picture), an Approach-Positive group (ApP; i.e., pull positive pictures and push empty picture), and a sham-training (i.e., continued assessment; all picture types are again pulled and pushed equally often). After the AAT training, participants were exposed to a stressful speech task. In the AAT assessment (pre- and posttest), both groups that were trained to avoid (push) negative pictures (PN and AvN) showed a stronger increase in positive approach-avoidance tendencies compared to the other two groups (ApP and sham). However, only subjects in the *general positivity training* demonstrated significant within-group changes in positive bias. No between-group differences in self-report (or cardiovascular) measures of anxiety in response to the stress task were found. The authors concluded the importance of *both* approaching positive pictures and avoiding negatives ones for modifying an approach-avoidance bias and especially to induce a positive bias.

Appendix E2 Link Between Approach/Avoidance Behaviors and Information Processing

Cacioppo et al. (1993) asked participants to either perform arm flexion or arm extension to novel, unrelated stimuli (Chinese ideographs) in six experiments. Ideographs, to which participants were exposed during arm flexion, were rated more positively than ideographs presented during arm extension (e.g., study 1, 2). Besides, arm flexion – in contrast to extension – was associated with an approach motivational orientation (e.g., study 4) and resulted in more positive attitudes to the ideographs, whereas extension led to more negative attitudes (study 6).

Neumann and Strack (2000) also used arm flexion and extension in the word-evaluation task. During exposure to adjectives on a screen, participants had to decide as fast as possible if the displayed word was positive or negative, being either in the flexion or extension condition. Based on the assumption of two motivational systems involved, the approach behavior (flexion) was expected to facilitate the processing of positive concepts, whereas avoidance behavior (extension) would facilitate the processing of negative concepts. In line with this hypothesis, positive words were categorized more quickly than negative words during flexing the arm, while participants categorized negative words faster than positive ones when contracting their tensor muscle in arm extension.

Appendix E3 Previous Research on the Effects of PBT

Effects of PBT on perceived stress and perceived microstressor severity. Dandeneau et al. (2007) provided some evidence for decreased stress after an attentional training (Find-the-smile visual search task) in two studies. Clarke (2016) also found a reduction of perceived stress after a CBM training fostering positive interpretations of ambiguous stimuli. Several studies demonstrated that the modification of biases in information processing can also have a positive impact on the emotional reactivity to (laboratory) stressors. Johnson (2009), who instructed participants to selectively attend to happy faces and avoid angry faces in a dot-probe task, found less frustration (i.e., mood reactivity) in response to a stressful anagram task. Similarly, Taylor et al. (2011) showed the least emotional responses to a laboratory stressor for participants with the greatest change in attentional PB following CBM. Peters et al. (2011) found less depressed mood in a stress anagram test for a CBM-I intervention fostering a positive AS (resilience group) compared to a group that aimed on negative AS (vulnerability group).

Effects of PBT on implicit action tendencies. Originally, Wiers et al. (2009), using an AAT-training, demonstrated decreased approach tendencies to alcohol-relevant stimuli in alcoholic patients. Several studies also adapted the AAT paradigm to examine or train approach versus avoidance tendencies to affective stimuli (e.g., Lukas, 2019; Vrijsen et al., 2018). For example, Rinck et al. (2013) induced an approach versus avoidance tendency to smiling faces in high socially anxious individuals. Participants in the approach-faces condition did approach faces faster than the avoid-faces group. In study 1 of Becker et al. (2016) in unselected students, a negativity training reversed a pre-existing PB, whereas no changes were found for the positivity training (PT) – possible due to methodological weaknesses of the study (see 3.1.4) and a ceiling effect in the PT group. On the other hand, Ferrari et al. (2018), who also investigated unselected individuals using the same pictures as Becker et al. (2016), found a significant change of implicit action tendencies (i.e., increase of AAT-CS) for a general positivity training (i.e., pull positive pictures and push negative ones).

Effects of PBT on resilience, the ability to recover from stress, and well-being. Rinck et al.

(2013) tested an AAT training (approach-faces versus avoid-faces condition) and implemented a social stress task with mood assessments both after the task instruction and after the stressor to measure stress recovery. Only after the stressor, the authors reported more positive mood and lower anxiety in the positive compared to the negative group, suggesting that the training especially affected mood recovery.

Appendix F Recruitment and General Information

Appendix F1 Poster Recruitment (Notice Boards)

JOHANNES GUTENBERG UNIVERSITÄT MAINZ JG|U DRZ MAINZ JG|U UNIVERSITÄTSmedizin. MAINZ

Studienteilnehmer gesucht! Train4Positivity – Smartphone-basiertes Training zum Positivity bias

Für die Studie **TRAIN4Positivity** der Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz in Kooperation mit der Leuphana Universität Lüneburg suchen wir Studienteilnehmerinnen und -teilnehmer.

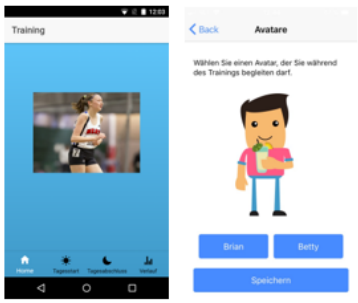
Worum geht es?

Täglich trifft eine wahre „Flut" von Umweltreizen auf uns ein. Die Tendenz, sich auf positive Aspekte in der Umgebung zu konzentrieren oder Situationen positiver zu interpretieren, kann sich hierbei förderlich auf unser emotionales Wohlbefinden und unsere psychische Gesundheit auswirken.

Ziel der TRAIN4Positivity-Studie ist die Untersuchung eines **Smartphone-basierten Trainings** zur Stärkung einer **Positivitätstendenz („positivity bias") auf der Handlungsebene**.

Wer kann teilnehmen?

Wir suchen **gesunde** Erwachsene im Alter von **18 bis 30 Jahren**, die in ihrem Leben mit einer **großen Anzahl von stressreichen Momenten**, Unannehmlichkeiten und Ärgernissen des täglichen Lebens konfrontiert sind (z. B. Pendeln zur Arbeit, Prüfungen).



Was wird gemacht?

Die Studienteilnahme umfasst ein **dreiwöchiges Training** mit einer Smartphone-App (täglich 6 Trainingseinheiten à 3 min.), das **Ausfüllen von Fragebögen** zu zwei Zeitpunkten und am Ende jeder Trainingswoche (sonntags) sowie die zweimalige Bearbeitung eines **Laborparadigmas**. Die Teilnahme besteht aus drei Terminen, Termin 1 und 2 können nach Möglichkeit kombiniert werden.

Was haben Sie davon?

- Kostenfreie Teilnahme an verschiedenen Interventionsangeboten zur Förderung von Resilienz und der psychischen Gesundheit (z. B. 1-tägiger Resilienz-Workshop)
- **Aufwandsentschädigung i. H. v. 50€ im Falle einer dreiwöchigen Studienteilnahme**

Wenn Sie die Voraussetzungen erfüllen und an einer Studienteilnahme interessiert sind, kontaktieren Sie bitte unser Studienteam unter:

Train4Positivity@uni-mainz.de

Bei Rückfragen zur Studie wenden Sie sich bitte an die Versuchsleitung, Dipl.-Psych. Angela Kunzler, Deutsches Resilienz Zentrum (DRZ) gGmbH Mainz, E-Mail: angela.kunzler@drz-mainz.de

Studienteilnehmer gesucht!
Train4Positivity – Smartphone-basiertes Training zum Positivity bias

Worum geht es?

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Ziel der Studie ist die Untersuchung eines **Smartphone-basierten Trainings** zur Stärkung einer **Positivitätstendenz auf der Handlungsebene**.

Wer kann teilnehmen?

Wir suchen gesunde Erwachsene im Alter von **18 bis 30 Jahren**, die mit einer **großen Anzahl von stressreichen Momenten**, Unannehmlichkeiten und Ärgernissen des täglichen Lebens konfrontiert sind (z. B. Pendeln zur Arbeit, Prüfungen). Darüber hinaus sollten Teilnehmer **Studierende** an der Johannes Gutenberg-Universität Mainz sein.

Was wird gemacht?

Die Studienteilnahme umfasst ein **dreiwöchiges Training** mit einer Smartphone-App (täglich 6 Trainingseinheiten à 5 min.), das **Ausfüllen von Fragebögen** zu zwei Zeitpunkten und am Ende jeder Trainingswoche (sonntags) sowie die zweimalige Bearbeitung eines **Laborparadigmas**. Die Teilnahme besteht aus drei Terminen. Termin 1 und 2 können nach Möglichkeit kombiniert werden.

Was habe ich davon?

- Kostenfreie Teilnahme an verschiedenen Interventionsangeboten zur Förderung von Resilienz und der psychischen Gesundheit (z. B. 1-tägiger Resilienz-Workshop)
- **Aufwandsentschädigung i. H. v. 50€ im Falle einer dreiwöchigen Studienteilnahme**

Kontakt:

Deutsches Resilienz Zentrum (DRZ) gGmbH Mainz, AG Lieb, Dipl.-Psych. Angela Kunzler
E-Mail: Train4Positivity@uni-mainz.de

E-Mail: Train4Positivity@uni-mainz.de
E-Mail: Train4Positivity@uni-mainz.de
E-Mail: Train4Positivity@uni-mainz.de
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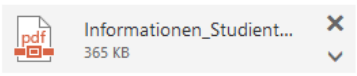
Appendix F2 Recruitment Email Including Information Brochure



Von: Kunzler, Angela Mareike
Gesendet: Dienstag, 11. Juni 2019 13:45
An: psych@lists.uni-mainz.de
Cc: Train4Positivity
Betreff: Studienteilnehmer gesucht! (Aufwandsentschädigung 50€) - Smartphone-Training zum Positivity bias

Liebe Studierende,

für die Studie **Train4Positivity - Evaluation eines Smartphone-basierten Trainings zum Positivity bias** suchen wir weitere Studienteilnehmerinnen und -teilnehmer.

Teilnahmevoraussetzungen sind insbesondere:

- Alter: 18-30 Jahre
- Große Anzahl von Stressoren und kleineren Unannehmlichkeiten (daily hassles) im täglichen Leben (z. B. Pendeln zur Arbeit, Prüfungen)
- Psychische Gesundheit.

Als **Aufwandsentschädigung** erhalten Sie **50€** im Falle einer dreiwöchigen Studienteilnahme. Außerdem gibt es die Möglichkeit, an einem eintägigen Resilienz-Workshop oder weiteren Präventionsangeboten der Leuphana Universität Lüneburg teilzunehmen.
Weitere Informationen zur Studie sowie zur Kontaktaufnahme bei Interesse befinden sich im Anhang.

Viele Grüße
Angela Kunzler

Dipl.-Psych. Angela Kunzler

Deutsches Resilienz Zentrum (DRZ) gGmbH
Untere Zahlbacher Str. 8
55131 Mainz
Tel. +49 (0)6131-17 6110
E-Mail: Angela.Kunzler@drz-mainz.de
Web: https://www.drz-mainz.de

Studienteilnehmer gesucht! Train4Positivity – Smartphone-basiertes Training zum Positivity bias

Für die Studie **TRAIN4Positivity** der Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz in Kooperation mit der Leuphana Universität Lüneburg suchen wir Studienteilnehmerinnen und -teilnehmer.

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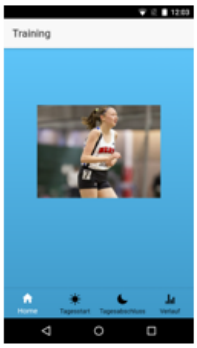
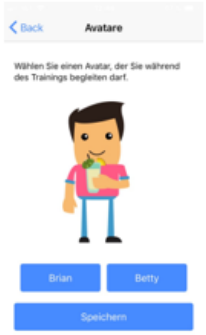
Ziel der TRAIN4Positivity-Studie ist die Untersuchung eines **Smartphone-basierten Trainings** zur Stärkung einer **Positivitätstendenz („positivity bias") auf der Handlungsebene**.

Wer kann teilnehmen?

Wir suchen **gesunde** Erwachsene im Alter von **18 bis 30 Jahren**, die in ihrem Leben mit einer **großen Anzahl von stressreichen Momenten**, Unannehmlichkeiten und Ärgernissen des täglichen Lebens konfrontiert sind (z. B. Pendeln zur Arbeit, Prüfungen).





Was wird gemacht?

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Was haben Sie davon?

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Appendix F3 Facebook Posting

Studienteilnehmer gesucht! Train4Positivity – Smartphone-basiertes Training zum Positivity bias

Für die Studie **TRAIN4Positivity** der Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz in Kooperation mit der Leuphana Universität Lüneburg suchen wir Studienteilnehmerinnen und -teilnehmer.

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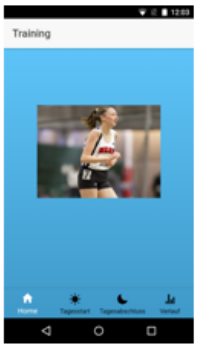
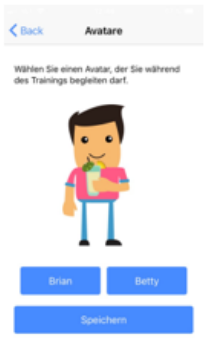
Ziel der TRAIN4Positivity-Studie ist die Untersuchung eines **Smartphone-basierten Trainings** zur Stärkung einer **Positivitätstendenz („positivity bias") auf der Handlungsebene**.

Wer kann teilnehmen?

Wir suchen **gesunde** Erwachsene im Alter von **18 bis 30 Jahren**, die in ihrem Leben mit einer **großen Anzahl von stressreichen Momenten**, Unannehmlichkeiten und Ärgernissen des täglichen Lebens konfrontiert sind (z. B. Pendeln zur Arbeit, Prüfungen).





Was wird gemacht?

Die Studienteilnahme umfasst ein **dreiwöchiges Training** mit einer Smartphone-App (täglich 6 Trainingseinheiten à 3 min.), das **Ausfüllen von Fragebögen** zu zwei Zeitpunkten und am Ende jeder Trainingswoche (sonntags) sowie die zweimalige Bearbeitung eines **Laborparadigmas**. Die Teilnahme besteht aus drei Terminen, Termin 1 und 2 können nach Möglichkeit kombiniert werden.

Was haben Sie davon?

- Kostenfreie Teilnahme an verschiedenen Interventionsangeboten zur Förderung von Resilienz und der psychischen Gesundheit (z. B. 1-tägiger Resilienz-Workshop)
- **Aufwandsentschädigung i. H. v. 50€ im Falle einer dreiwöchigen Studienteilnahme**

Wenn Sie die Voraussetzungen erfüllen und an einer Studienteilnahme interessiert sind, kontaktieren Sie bitte unser Studienteam unter:

Train4Positivity@uni-mainz.de

Bei Rückfragen zur Studie wenden Sie sich bitte an die Versuchsleitung, Dipl.-Psych. Angela Kunzler, Deutsches Resilienz Zentrum (DRZ) gGmbH Mainz, E-Mail: angela.kunzler@drz-mainz.de

Appendix F4 Study Information Brochure

Studienleitung:

Prof. Dr. phil. Michèle Wessa
Abteilung für Klinische Psychologie und Neuropsychologie
Psychologisches Institut
Johannes Gutenberg-Universität Mainz
Wallstr. 3 R. 06-105
55122 Mainz
Email: wessa@uni-mainz.de
Tel.: +49 (0)6131 39 39259

Prof. Dr. med. Klaus Lieb
Klinik und Poliklinik für Psychiatrie und Psychotherapie
Universitätsmedizin der Johannes Gutenberg-Universität Mainz
Untere Zahlbacher Str. 8
55131 Mainz
Email: klaus.lieb@unimedizin-mainz.de
Tel.: +49 (0)6131 17 7336

Studienaufklärungsbroschüre

„TRAIN4Positivity-Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias“

Sehr geehrter Studienteilnehmer, sehr geehrte Studienteilnehmerin,

vielen Dank für Ihr Interesse an unserer Studie teilzunehmen. Bitte lesen Sie sich diese Studienaufklärungsbroschüre sorgfältig durch. Ihre Teilnahme ist freiwillig, Wenn Sie sich gegen eine Teilnahme entscheiden, hat dies keinerlei Nachteile für Sie.

Hintergrund und Ziel der Studie

Tagtäglich trifft eine wahre „Flut“ von Umweltreizen auf uns ein. Viele Menschen richten ihre Aufmerksamkeit dabei spontan auf unangenehme Eindrücke in ihrer Umgebung (z. B. Zigarettenstummel auf dem Gehweg, genervte Gesichter anderer Kunden in der Supermarkt-Schlange). Sie können sich nur schwer von solchen negativen Reizen distanzieren, erinnern sich im Nachhinein öfter an negative Ereignisse oder interpretieren mehrdeutige Situationen auf negativere Art. Eine solche Verzerrung unserer Wahrnehmung („Bias“) in Richtung des Negativen kann unter Umständen nützlich sein, z. B. zum Schutz vor potentiellen Bedrohungen. Sie stellt jedoch auch einen Risikofaktor für unser emotionales Wohlbefinden dar.

Umgekehrt wirkt sich eine Tendenz in Richtung positiver Reize, eine sogenannte *Positivitätstendenz*, förderlich auf unsere Gesundheit aus. Eine Tendenz zum Positiven heißt keinesfalls, negative Reize völlig auszublenden oder zu vermeiden. Vielmehr geht es darum, sich von negativen Dingen besser distanzieren zu können, gleichzeitig aber das Positive stärker wahrzunehmen und nahe an sich heranzulassen.

Mit der Studie TRAIN4Positivity verfolgen wir das Ziel, mit Hilfe einer neuentwickelten Smartphone-Applikation („Breezly“) gezielt eine Tendenz zum Positiven zu stärken.

Das Training richtet sich insbesondere an Menschen, die in ihrem Leben vielen Stressereignissen ausgesetzt sind. Sie sollen dabei unterstützt werden, die Balance in der Wahrnehmung von Positivem und Negativem wiederherzustellen.

Grundlage für das Training ist die sogenannte „Cognitive Bias Modification“ – eine Methode, die in der Wissenschaft schon seit Längerem eingesetzt wird, um kognitive Verzerrungen in verschiedenen Bereichen (z. B. Aufmerksamkeit) gezielt zu trainieren und eine Tendenz zum Positiven zu fördern.

Mit der Studie wollen wir Erkenntnisse über die Wirksamkeit des Smartphone-Trainings auf die Tendenz zum Positiven und verschiedene andere psychologische Maße gewinnen.

JOHANNES GUTENBERG UNIVERSITÄT MAINZ | JG|U | DRZ MAINZ | JG|U UNIVERSITÄTSmedizin. MAINZ

Studienablauf

Vor Beginn der Studie werden wir Ihnen in einem kurzen Telefonat allgemeine Informationen über die Studie geben und die Ein- und Ausschlusskriterien besprechen.	Telefonat
Der Aufklärungstermin findet in den Räumen der Klinik und Poliklinik für Psychiatrie und Psychotherapie statt. Hierbei erhalten Sie alle relevanten Informationen zum Studienablauf. Sie können jederzeit Fragen stellen, wenn Ihnen etwas unklar sein sollte. Sind Sie nach der Aufklärung an einer Studienteilnahme interessiert und haben uns Ihre schriftliche Einwilligung zur Studienteilnahme und zur Prüfung der Ein- und Ausschlusskriterien gegeben, bearbeiten Sie verschiedene Fragebögen, mit denen wir die Ein- und Ausschlusskriterien überprüfen. Abgefragt wird unter anderem, mit wie vielen stressreichen Ereignissen Sie im Alltag konfrontiert sind. Innerhalb einer Woche erhalten Sie eine Rückmeldung darüber, ob Sie an der Studie teilnehmen können. Ist dies der Fall und sind Sie weiter an einer Studienteilnahme interessiert, werden Sie zu einem weiteren persönlichen Einführungstermin eingeladen.	Aufklärungstermin & Screening Aufklärung über den Studienablauf & Fragebogenerhebung (30 Minuten)
Der Einführungstermin (Dauer ca. 1,5 Stunden) findet in den Räumen der Klinik und Poliklinik für Psychiatrie und Psychotherapie statt. Hierbei werden Sie in die Funktionsweise des Trainings und die Erhebungen im Trainingszeitraum eingeführt. Sie können jederzeit Fragen stellen, wenn Ihnen etwas unklar sein sollte. Mit Hilfe eines Interviews, Fragebögen (Erhebung psychometrischer Daten) sowie einem Reaktionszeittest am Computer erheben wir Daten in Bezug auf Ihre Einstellungen und verschiedene Gesundheitsmaße. Danach erhalten Sie ein Studien-Smartphone und werden in dessen Handhabung eingewiesen. Sie werden genauestens über die Funktionsweise der App „Breezly“ informiert, mit der Sie dann selbständig innerhalb der nächsten drei Wochen das Training zur Positivitätstendenz durchführen.	Einführungstermin (ca. 1,5 Stunden) einschl. Fragebogen-erhebung und Reaktionszeitaufgabe (60 Minuten)
Im Anschluss an den Einführungstermin beginnen Sie das Training mit der App „Breezly“, die auf dem Studien-Smartphone installiert ist und eine Tendenz zum Positiven trainiert. Die wichtigste Funktion der App ist ein Bildertraining. Sie erstellen zunächst ihr persönliches Trainingsprofil, indem Sie 170 vorausgewählte Bilder nach bestimmten Kriterien beurteilen. Für Ihr persönliches Profil werden davon dann 25 positive und 25 negative Bilder ausgewählt, mit denen Sie anschießend trainieren. Jede Trainingseinheit umfasst 10 Bilder.	21 Tage Training mit der Smartphone-App „Breezly“ per Studien-Smartphone im Alltag **Persönliche Profil-erstellung:** • Bewertung von 170 Bildern in der App

Das Training besteht darin, auf positive und negative Bildmotive jeweils sehr bewusst gegensätzliche motorische Reaktionen auszuführen. Positive Bilder (z. B. Sonnenuntergang) ziehen Sie durch eine Bewegung auf dem Display „zu sich heran", negative Bilder (z. B. schmerzverzerrtes Gesicht) wischen Sie entschlossen „von sich weg". Jede Trainingseinheit endet mit der Abfrage Ihres aktuellen Befindens. Beziehen Sie sich dabei bitte auf den **aktuellen Moment, d.h. so wie es Ihnen während der Befragung gerade geht.**

Um mit einem guten Gefühl in den Tag zu starten und zu lernen, Geschehnisse am Ende des Tages positiver zu bewerten, bitten wir Sie, jeweils **morgens (7-11 Uhr)** und **abends (19-24 Uhr) dreimal** mit „Breezly" zu trainieren. In Abhängigkeit der Regelmäßigkeit Ihres Trainings sammeln Sie Punkte. Die Anzahl Ihrer Punkte spiegelt sich im Erscheinungsbild eines (je nach Wahl männlichen/weiblichen) Avatars wider, mit dem Sie Ihren bisherigen Trainingsfortschritt verfolgen können.

Zusätzlich zum Bildertraining bitten wir Sie, mit „Breezly" jeden Tag (morgens und abends) ihr Wohlbefinden und andere Maße (Schlaf, Tatkraft, Fähigkeit, sich von Negativem zu distanzieren bzw. Positives nahe an sich heranzulassen) zu bewerten. Auch hier können Sie den Verlauf über verschiedene grafische Darstellungen verfolgen.

21 Tage Training mit der Smartphone-App „Breezly"

per Studien-Smartphone im Alltag

Morgens (7-11 Uhr):

- 3 Trainingseinheiten (Bildertraining + Frage zum aktuellen Befinden) (jeweils ca. 3 Min.)
- Fragen zu Schlaf, Wohlbefinden, Tatkraft beantworten (1x)

Abends (19-24 Uhr):

- 3 Trainingseinheiten (s.o.)
- Fragen zu Wohlbefinden, Distanzierung von Negativem, „Heranlassen" von Positivem beantworten (1x)

Um die Wirksamkeit von „Breezly" auf Alltagsstressoren erfassen zu können, interessiert uns, welche Mikrostressoren oder sogenannte „daily hassles" im Zeitraum des Trainings bei Ihnen auftreten. Hierbei handelt es sich um die kleineren Ärgernisse und Unannehmlichkeiten des täglichen Lebens, wie Zeitdruck, Verzögerungen im öffentlichen Nahverkehr oder kleinere Meinungsverschiedenheiten. Diese Mikrostressoren werden daher ebenfalls mit Hilfe des Studien-Smartphones über eine zweite Applikation erfasst.

Die Erfassung der Mikrostressoren findet jeweils **am Ende jeder Woche** des dreiwöchigen Trainings statt und erfolgt über die App „movisensXS".

Wir werden Sie bitten, folgende Angaben zu machen:

Mikrostressor-Liste: Bitte wählen Sie in der dargebotenen Liste alle in Ihrem Alltag aufgetretenen Mikrostressoren aus (Mehrfachangaben möglich). Bitte beziehen Sie sich dabei immer auf den **Zeitraum der vergangenen Woche**. Es ist wichtig, dass Sie jedes Ereignis, das Ihnen unangenehm war, Sie verärgert oder im Alltag gestört hat, im Smartphone registrieren. Bitte beachten Sie auch, dass es sich bei den Auswahlmöglichkeiten in der Liste um Oberkategorien handelt, und Sie die passende Kategorie für Ihr persönliches Ereignis auswählen.

Belastung: Darüber hinaus sind wir an Ihrer subjektiven Belastung durch die Mikrostressoren interessiert. Dafür werden Sie im Anschluss an die Mikrostressor-Liste gebeten, anzugeben, wie belastend Sie die ausgewählten Mikrostressoren erlebt haben.

Bitte tragen Sie in dem 21-tägigen Zeitraum das Studien-Smartphone bei sich, soweit es Ihnen möglich ist. Achten Sie bitte auch auf den Akkuzustand des

Wöchentliche Messung von Mikrostressoren und Belastung durch Stressoren

per Studien-Smartphone im Alltag

- Sonntags (ganztägig möglich)
- ca. 10 Minuten

Gerätes und laden Sie es auf, wenn es erforderlich ist, damit Sie keine Messung verpassen.

Nach der 21-tägigen Erhebungsphase findet ein Abschlusstermin statt, an dem eine zweite Fragebogenerhebung sowie die erneute Durchführung des Reaktionszeittests am Computer erfolgen. Sie geben das Studien-Smartphone zurück, können Fragen stellen und von Ihren Erfahrungen mit dem Training berichten. Zudem können Sie angeben, welche Aufwandsentschädigung Sie erhalten möchten (siehe unter *Aufwandsentschädigung*).

Abschlusstermin
(1.5 Stunden)
einschl. Fragebogen-erhebung (45 Minuten)

Was ist zu beachten?

Sie sollten während Ihrer Teilnahme an der Studie Ihrem Alltag ganz normal nachgehen. Wir bitten Sie lediglich, das Studien-Smartphone mit sich zu tragen und dieses nicht auszuschalten. Für die Durchführung des Trainings muss das Studien-Smartphone lediglich für die erste Anmeldung mit dem Internet verbunden sein, während des Trainingszeitraums ist eine Internetverbindung nicht erforderlich. Lediglich zum Abschluss des Trainings sollte das Smartphone erneut einmal mit dem Internet verbunden werden.

Bitte laden Sie den Akku Ihres Studien-Smartphones jeden Abend auf, sodass Sie es am nächsten Tag ohne Probleme weiter nutzen können. Bei technischen Problemen können Sie sich an die ausgehändigte Telefonnummer des Studienteams wenden, das Ihre Fragen beantworten kann. Nach Ablauf der 21 Tage bitten wir Sie, das Ihnen ausgehändigte Smartphone zu einem vereinbarten Abschlusstermin wieder mitzubringen. Dieser Termin dient zudem der Durchführung abschließender Messungen.

Freiwilligkeit

Die Teilnahme an der Studie ist freiwillig. Es ergeben sich in keinerlei Hinsicht Nachteile für Sie, sollten Sie sich gegen eine Teilnahme entschließen. Sie können die Studie jederzeit ohne Angabe von Gründen abbrechen oder verlangen, dass von Ihnen angegebene Daten gelöscht werden.

Datenschutz

Ihre persönlichen Daten dienen ausschließlich Forschungszwecken. Sie werden als Teilnehmer in die Studie erst nach Ihrer schriftlichen Einwilligung aufgenommen. Die studienbezogenen Daten werden streng vertraulich behandelt. Die Daten werden pseudonymisiert auf Datenträgern gespeichert und vom Studienteam auswertet. Pseudonymisiert bedeutet, dass wir jegliche Sie identifizierbare Daten, wie Namen, Geburtstag, Adresse oder Ähnliches, von den erhobenen Daten entfernen bzw. trennen. Jeder Person wird ein Studien-Code zugeordnet, unter dem die inhaltlichen Daten im Smartphone bzw. nach Abschluss der Erhebungsphase in der Studiendatenbank erfasst und gespeichert werden. Aus dem Studien-Code kann nicht auf Ihre Person rückgeschlossen werden. Die pseudonyme Datenhaltung ist erforderlich, da aus unterschiedlichen Quellen Daten erhoben und zugeordnet werden müssen.

Die Personendaten werden getrennt von den inhaltlichen Smartphone- und Fragebogendaten erfasst und gespeichert. Nur die Studienleitung sowie damit anvertraute Studienmitarbeiter haben auf beide Datenbanken Zugriff, soweit dies für die Studie erforderlich ist (z. B. bei auftretenden technischen Komplikationen im Rahmen der Smartphone-Erhebung). Eine Weitergabe von Personendaten an Dritte erfolgt nicht. Publikationen erfolgen ausschließlich in anonymisierter Form (d.h. die Zuordnung der Daten zu Personen ist niemandem mehr möglich, auch nicht dem Studienteam). Nach Ablauf der vorgeschriebenen Aufbewahrungsdauer für wissenschaftliche Studiendaten von 10 Jahren werden alle Daten gelöscht.

Mit der Smartphone App „Breezly“ werden verschiedene Daten erfasst (z. B. aktuelle Befindlichkeit nach einer Trainingseinheit, Fragen zu Schlaf, Wohlbefinden, Tatkraft, Distanzierung von Negativem, „Heranlassen“ von Positivem). Neben diesen Angaben werden sogenannte Metadaten ausgewertet, z. B. wie

häufig Sie die App nutzen oder wie häufig Sie Übungen machen. Alle Daten werden pseudonymisiert (d.h. codiert) erfasst und ausgewertet sowie verschlüsselt übertragen. Die Speicherung dieser Daten erfolgt pseudonymisiert auf dem Server der Leuphana Universität Lüneburg.

Die mit der der Smartphone App movisensXS gesammelten Daten werden pseudonymisiert (d.h. codiert) erhoben. Die erfassten Daten werden von movisens GmbH, Karlsruhe, Deutschland verschlüsselt und auf der Datenbank der Host Europe GmbH in Köln, Deutschland pseudonymisiert gesammelt und gespeichert bis die Löschung der Daten von den Studienleitenden nach Ende der Datenerhebungsphase (voraussichtlich Januar 2019) angefordert wird. Die Identität der Probanden ist movisens unbekannt.

Der vorliegenden Studie ist nicht gestattet, Daten von Probanden mit dem Smartphone zu erheben, die die Identität der Probanden aufdecken könnten.

Gemäß der Europäischen Datenschutz-Grundverordnung (DSGVO)[1] werden Sie im Folgenden über Ihre Rechte in Bezug auf die Verarbeitung Ihrer personenbezogenen Daten (Art. 12 ff. DSGVO) informiert.

Rechtsgrundlage
Die Rechtsgrundlage zur Verarbeitung der Sie betreffenden personenbezogenen Daten bilden bei Studien Ihre freiwillige schriftliche Einwilligung gemäß DSGVO (Art. 6 Abs. 1 lit. a und Art. 9 Abs. 2 lit.) und § 37 Abs. 1 des Landeskrankenhausgesetzes Rheinland-Pfalz. Zeitgleich mit der DSGVO tritt in Deutschland das überarbeitete Bundesdatenschutzgesetz (BDSG) in Kraft.

Bezüglich Ihrer Daten haben Sie folgende Rechte (Art. 13 ff. DSGVO, §§ 32 ff. BDSG), **sofern diese Rechte nicht durch § 27 Abs. 2 BDSG im Rahmen der Datenverarbeitung zu wissenschaftlichen Forschungszwecken eingeschränkt sind**:

- **Recht auf Auskunft** (Art. 15 DSGVO, §§34 und 57 BDSG): Das Recht auf Auskunft über die Sie betreffenden personenbezogenen Daten, die im Rahmen der Studie erhoben, verarbeitet oder ggf. an Dritte übermittelt werden. Das Recht auf Auskunft ist nach § 27 Abs. 2 BDSG im Rahmen der wissenschaftlichen Forschung ggf. eingeschränkt.
- **Recht auf Berichtigung** (Art. 16 und 19 DSGVO, § 58 BDSG): Das Recht, Sie betreffende, unrichtige, personenbezogene Daten berichtigen zu lassen sowie das Recht über eine Berichtigung Sie betreffender, personenbezogener Daten informiert zu werden. Das Recht auf Berichtigung ist nach § 27 Abs. 2 BDSG im Rahmen der wissenschaftlichen Forschung ggf. eingeschränkt.
- **Recht auf Löschung** (Art. 17 und 19 DSGVO, §§ 35 und 58 BDSG): Das Recht auf Löschung Sie betreffender, personenbezogener Daten, z. B. wenn diese Daten für den Zweck, für den sie erhoben wurden, nicht mehr notwendig sind, sowie das Recht, über die Löschung Sie betreffender, personenbezogener Daten informiert zu werden.
- **Recht auf Einschränkung der Verarbeitung** (Art. 18 und 19 DSGVO, § 58 BDSG): Das Recht, unter bestimmten Voraussetzungen, die Einschränkung der Verarbeitung zu verlangen, d.h. die Daten dürfen nur gespeichert, nicht verarbeitet werden, sowie das Recht, über die Einschränkung der Verarbeitung informiert zu werden. Dies müssen Sie beantragen. Wenden Sie sich hierzu bitte an ihren Studienleiter oder an die Datenschutzbeauftragten der beteiligten Prüfzentren. Das Recht auf Einschränkung der Verarbeitung ist nach § 27 Abs. 2 BDSG im Rahmen der wissenschaftlichen Forschung ggf. eingeschränkt.

[1] Verordnung (EU) 2016/679 des Europäischen Parlaments und des Rates vom 27. April 2016 zum Schutz natürlicher Personen bei der Verarbeitung personenbezogener Daten, zum freien Datenverkehr und zur Aufhebung der Richtlinie 95/46/EG (Datenschutz-Grundverordnung).

- **Recht auf Datenübertragbarkeit** (Art. 20 DSGVO): Das Recht, die Sie betreffenden personenbezogenen Daten, die sie dem Verantwortlichen für die Studie bereitgestellt haben, zu erhalten. Damit können Sie beantragen, dass diese Daten entweder Ihnen oder, soweit technisch möglich, einer anderen von Ihnen benannten Stelle übermittelt werden.
- **Widerspruchsrecht** (Art. 21 DSGVO, § 36 BDSG): Das Recht, jederzeit gegen konkrete Entscheidungen oder Maßnahmen zur Verarbeitung der Sie betreffenden, personenbezogenen Daten Widerspruch einzulegen. Eine solche Verarbeitung findet anschließend grundsätzlich nicht mehr statt. Das Recht auf Widerspruch ist nach § 27 Abs. 2 BDSG im Rahmen der wissenschaftlichen Forschung ggf. eingeschränkt.

Einwilligung zur Verarbeitung personenbezogener Daten und Recht auf Widerruf dieser Einwilligung

Die Verarbeitung Ihrer personenbezogenen Daten ist nur mit Ihrer Einwilligung rechtmäßig (Art. 6 und 9 DSGVO, § 51 BDSG).

Sie haben das Recht, Ihre Einwilligung zur Verarbeitung personenbezogener Daten jederzeit zu widerrufen.

Möchten Sie eines dieser Rechte in Anspruch nehmen, wenden Sie sich bitte an Ihren Studienleiter oder an die Datenschutzbeauftragten der beteiligten Prüfzentren (Mainz, Lüneburg). Außerdem haben Sie das **Recht, Beschwerde bei der/den Aufsichtsbehörde/n einzulegen,** wenn Sie der Ansicht sind, dass die Verarbeitung der Sie betreffenden personenbezogenen Daten gegen die DSGVO verstößt. Die jeweiligen Kontaktdaten finden Sie am Ende der Studienaufklärungsbroschüre.

Risiken

Die Verwendung von Smartphones birgt im Alltag einige Risiken und kann Sie von wichtigen Tätigkeiten ablenken und Ihren Alltag unterbrechen. Zu Ihrer eigenen Sicherheit bitten wir Sie daher, Ihr Smartphone nicht während kritischer Situationen, in denen Sie Ihre volle Aufmerksamkeit benötigen z. B. bei der Bedienung von schweren Maschinen oder innerhalb des Straßenverkehrs (während der Lenkung eines Kraftfahrzeugs oder beim Fahrradfahren) zu nutzen und in solchen Situationen keine Kopfhörer zu tragen. Sollten Sie mit der Studienteilnahme einverstanden sein, verpflichten Sie sich, sich an gesetzliche Regelungen in Bezug auf die Benutzung von Mobiltelefonen zu halten.

Aufwandsentschädigung

Für die Teilnahme an der 3-wöchigen Studie erhalten Sie eine Aufwandsentschädigung i. H. v. **50€**. Außerdem haben Sie die Möglichkeit, an einem von drei **Präventionsangeboten** der Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz bzw. der Leuphana Universität Lüneburg teilzunehmen.

Folgende Angebote stehen Ihnen zur Verfügung:

1) *Resilienz-Workshop* (Dauer: 1 Tag): Überblick über das Konzept Resilienz und wissenschaftlich belegte psychosoziale Schutzfaktoren. Anhand praktischer und alltagsnaher Übungen wird vermittelt, wie physische und psychische Überlastungssymptome erkannt und ausgewählte Schutzfaktoren genutzt werden können, um alltägliche und berufliche Belastungen besser zu bewältigen und trotz dieser Belastungen gesund zu bleiben.
   Seminarinhalte: Resilienz – Definition, Wie kann ich Resilienz für mich nutzen? Integration von Resilienz in meinen Alltag, *Anbieter: Klinik und Poliklinik für Psychiatrie und Psychotherapie*
2) *HOLIDAILY-App zur Förderung einer nachhaltigen Erholung von beruflicher Anspannung vor, während und nach dem Urlaub* (Dauer: abhängig von der Dauer des Urlaubs). Die App fordert Sie jeden Tag zu einem „Daily" auf, d.h. kleine Übungen und Herausforderungen, die Ihnen dabei helfen sollen, gute

Voraussetzungen für einen erholsamen Urlaub zu schaffen und die Vorfreude zu steigern, im Urlaub gute Erfahrungen zu sammeln, den Erholungseffekt von Urlaub zu verlängern und berufliche Probleme in der Freizeit besser aus dem Kopf zu bekommen. Weitere Informationen zur App finden Sie unter: https://www.geton-training.de/holidaily.php. *Anbieter: Leuphana Universität Lüneburg*

3) *Dankbarkeits-Online-Training* (GET.ON Dankbarkeit; Dauer: 5 Einheiten à 45-60 Minuten) zur Förderung einer bewussteren Wahrnehmung von positiven Ereignissen im Alltag. Zur Durchführung des Trainings kommt zusätzlich zu einem Online-Training eine Dankbarkeits-App zum Einsatz. Sie lernen, im Alltagsstress ihre Aufmerksamkeit für die (kleinen) positiven Erlebnisse zu schärfen, erfreuliche Ereignisse mittels Dankbarkeits-Tagebuch bzw. Fotos intensiver zu erleben sowie positive Gefühle (z. B. Zufriedenheit, Dankbarkeit, Freude) in gezielten Übungen zu vertiefen. Weitere Informationen zum Dankbarkeitstraining finden Sie unter: https://geton-training.de/dankbarkeit.php. *Anbieter: Leuphana Universität Lüneburg*

Kontakt

Bei Rückfragen zur Studie oder zur Studienteilnahme wenden Sie sich bitte an die Versuchsleitung, Frau Dipl.-Psych. Angela Kunzler. Bei technischen Problemen mit dem Smartphone oder anderen Komplikationen während der 21-tägigen Trainings- und Erhebungsphase steht Ihnen das Studienteam telefonisch zur Verfügung. Bei Ausgabe des Smartphones erhalten Sie alle erforderlichen Kontaktdaten.

Versuchsleitung und verantwortliche Stelle für Datenverarbeitung:
Dipl.-Psych. Angela Kunzler
Klinik und Poliklinik für Psychiatrie und Psychotherapie, Universitätsmedizin der Johannes Gutenberg-Universität Mainz, Untere Zahlbacher Straße 8, 55131 Mainz
Email: angela.kunzler@unimedizin-mainz.de
Tel.: +49 (0)6131 17 6110

Studienteam:
Annalena Krayer, B.Sc. Psychologie
Email: Train4Positivity@uni-mainz.de

Datenschutzbeauftragter Prüfzentrum Mainz:
Der betriebliche Datenschutzbeauftragte der Universitätsmedizin Mainz
Email: datenschutz@unimedizin-mainz.de

Datenschutzbeauftragter Prüfzentrum Lüneburg:
Der betriebliche Datenschutzbeauftragte der Leuphana Universität Lüneburg
Email: datenschutz@leuphana.de

Studienleitung:
Prof. Dr. phil. Michèle Wessa
Abteilung für Klinische Psychologie und Neuropsychologie Psychologisches Institut, Johannes Gutenberg-Universität Mainz, Wallstr. 3 R. 06-105 55122 Mainz,
Email: wessa@uni-mainz.de
Tel.: +49 (0)6131 39 39259

Prof. Dr. med. Klaus Lieb
Klinik und Poliklinik für Psychiatrie und Psychotherapie Universitätsmedizin der Johannes Gutenberg-Universität Mainz, Untere Zahlbacher Str. 8 55131 Mainz
Email: klaus.lieb@unimedizin-mainz.de
Tel.: +49 (0)6131 17 7336

Datenschutz-Aufsichtsbehörde:
Prof. Dr. Dieter Kugelmann
Hintere Bleiche 34
55116 Mainz
Email: poststelle@datenschutz.rlp.de
Tel.: +49 (0)6131 208-24 49

Bundesdatenschutzbeauftragte:
Die Bundesbeauftragte für den Datenschutz und die Informationsfreiheit
Husarenstr. 30
53117 Bonn
Email: poststelle@bfdi.bund.de
Tel.: +49 (0)228 997799 0

Appendix F5 Privacy Statement

The privacy statement was formulated based on the EU General Data Protection Regulation (GDPR) and the revised German Federal Data Protection Act (BDSG).

Datenschutzerklärung zur TRAIN4Positivity-Studie

Der Schutz Ihrer Daten und Ihrer Privatsphäre ist für unser Studienteam und die Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz ein besonderes Anliegen.

Mit dieser Datenschutzerklärung beschreiben wir, welche personenbezogenen Daten wir von Ihnen erheben und verarbeiten. Im Folgenden finden Sie eine Beschreibung der entsprechenden Verfahren zur Verarbeitung und Behandlung Ihrer personenbezogenen Daten. Wir empfehlen, diese Datenschutzerklärung sorgfältig zu lesen.

Diese Datenschutzerklärung gilt für die Nutzung der Breezly App und der Fragebogen-App movisensXS auf Ihrem Studiensmartphone innerhalb der Studie. Darüber hinaus gilt diese für die Erfassung von personenbezogenen Daten im Rahmen des:

- Screenings
- Einführungstermins
- Abschlusstermins
- E-Mail- und Telefonkontakts während der Studienlaufzeit

Die Studie wird im Rahmen der Kooperation zwischen der Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz und der Leuphana Universität Lüneburg durchgeführt. Datenschutzrechtlich verantwortlich für die Verarbeitung Ihrer Daten ist die Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz.

Hier finden Sie die Kontaktdaten der zuständigen Datenschutzbeauftragten:

1. Der betriebliche Datenschutzbeauftragte der Universitätsmedizin Mainz:

   Langenbeckstr. 1, Gebäude 301
   55131 Mainz
   E-Mail: datenschutz@unimedizin-mainz.de

2. Der betriebliche Datenschutzbeauftragte der Leuphana Universität Lüneburg:

   Universitätsallee 1
   21335 Lüneburg
   E-Mail: datenschutz@leuphana.de

1.) Daten, die wir von Ihnen erheben

Wir erheben folgende personenbezogenen Daten von Ihnen:

1a.) Von Ihnen gemachte Angaben (eingereichte Informationen):

Dabei handelt es sich um Daten, die Sie selbst in den Fragebögen beim Screening sowie zum Einführungs- und Abschlusstermin angeben. Außerdem gehören dazu Daten, die Sie selbst in den Fragebögen der App movisensXS und in der Breezly App im Rahmen der täglichen Übungen auf Ihrem Studiensmartphone während der Studie angeben. Die von Ihnen gemachten Informationen umfassen bei oben Genanntem die Eingabe eines individuellen Pseudonymisierungscodes und Ihre Fragebogenantworten. Darüber hinaus schließen die von Ihnen gemachten Angaben auch die Daten mit ein, die Sie in der E-Mail- und Telefonkommunikation mit uns angeben (z. B. bei Problemen mit den Smartphone-Apps Breezly oder movisensXS, Studienadministration, weiteren Fragen in Bezug auf die Studienteilnahme). Wenn Sie uns kontaktieren, speichern wir eine Kopie der E-Mail-Korrespondenz bzw. führen ein Protokoll zum Telefongespräch.

1b.) Daten, die wir über Sie und die Nutzung der Apps erheben:

Folgende Kontaktdaten zu Ihnen liegen uns aus der „Gutenberg Brain Study (GBS)“ bereits vor und wurden zur Rekrutierung und Kontaktaufnahme verwendet:

- Vor- und Familienname
- Anschrift
- Telefonnummer
- E-Mail-Adresse

Aufgrund der Vergabe von Pseudonymisierungscodes werden die Kontaktdaten der Studienteilnehmer nicht weiterverarbeitet und entkoppelt von anderen erhobenen Daten (z. B. wissenschaftliche Fragebögen) aufbewahrt.

Wir erfassen Details zur Nutzung der Breezly App (u.a. Häufigkeit der Nutzung, Dauer der Nutzung, Zeitpunkt der Nutzung). Ausschließlich im Fehlerfall werden von der App movisensXS Gerätedaten zu Gerätetyp und Betriebssystem erhoben und an die Software Crashlytics gesendet.

Die Angabe der Daten ist grundsätzlich freiwillig. Beachten Sie aber, dass Sie die zur Verfügung gestellten Dienste in Form der beiden Apps dann ggf. nicht nutzen, nur eingeschränkten Support im Fall von technischen Problemen erhalten oder nicht an der Studie teilnehmen können.

2.) Zu welchem Zweck wir diese Informationen verwenden

Die in Nr. 1a.) und 1b.) genannten Daten dienen einzig der wissenschaftlichen Arbeit der beteiligten Universitäten bzw. Institute. Diese umfassen die Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz, die Johannes Gutenberg-Universität Mainz (Abteilung Klinische Psychologie und Neuropsychologie des Psychologischen Instituts) sowie die Leuphana Universität Lüneburg (Abteilung für Gesundheitspsychologie und Angewandte Biologische Psychologie des Psychologischen Instituts).

zu 1a.) Von Ihnen gemachte Angaben (eingereichte Informationen) werden zur Durchführung der Studie und der Ermöglichung Ihrer Teilnahme auf Basis der Teilnahmebedingungen benötigt. Die Datenverarbeitung liegt zudem in unserem berechtigten Interesse (Art. 6 Abs. 1 S. 1 lit. f DSGVO) und wird für wissenschaftliche Zwecke benötigt (Art. 9 Abs. 2 lit. J DSGVO; § 31 Abs. 1 LDSG Rheinland-Pfalz). Die Daten werden zu folgenden Zwecken verwendet:

- Pseudonymisierungscodes in Apps und Fragebögen werden zur Erstellung eines Kontos für die Apps und zur Bereitstellung der in der App angebotenen Funktionen sowie zur Bearbeitung der Fragebögen benötigt.
- Pseudonymisierungscodes in Apps und Fragebögen werden verwendet, um Fragebogendaten zu mehreren Messzeitpunkten den Teilnehmern zuordnen zu können.
- Die Fragebogendaten aus Apps und Fragebögen dienen der Überprüfung der Durchführbarkeit und Wirksamkeit der Smartphone-Applikation Breezly.
- Inhalte aus der E-Mail- und Telefonkommunikation mit Ihnen (z. B. in Bezug auf die Studienadministration, technischen Support und weitere Fragen in Bezug auf die Studienteilnahme) werden für die Organisation der Studie und zur Verbesserung der Studie benötigt.

zu 1b.) Daten, die wir über Sie und die Nutzung der Apps erheben, werden zur Durchführung der Studie und der Ermöglichung Ihrer Teilnahme auf Basis der Teilnahmebedingungen durchgeführt und liegen zudem in unserem berechtigten Interesse (Art. 6 Abs. 1 S. 1 lit. f DSGVO). Die Daten werden zu folgenden Zwecken verwendet:

- Sämtliche Details, die mit der Nutzung der Breezly App und der App movisensXS in Zusammenhang stehen, werden erhoben, um die Wirksamkeit und Durchführbarkeit der Breezly App wissenschaftlich zu überprüfen.

3.) Übermittlung an Dritte

Die hier beschriebenen personenbezogenen Daten geben wir nur weiter, soweit dies zur Durchführung der Studie und zur Bereitstellung unserer Dienste erforderlich ist. Im Rahmen der hier genannten Zwecke werden personenbezogene Daten an Dienstleister weitergeleitet, die für uns tätig sind und uns insbesondere bei der Leistungserbringung unterstützen. Diese Dienstleister sind neben ihrer gesetzlichen Verpflichtung zur Einhaltung aller Datenschutzbestimmungen durch uns an weitere vertragliche Vorgaben zum Datenschutz

gebunden. Regelmäßig umfasst dies eine Verpflichtung als Auftragsverarbeiter nach Art. 28 Absatz 3 DSGVO.

Es erfolgt keine Weitergabe personenbezogener Daten an Dritte, wie z. B. eine Krankenkasse oder ein Unternehmen. Eine Weitergabe der Daten zwischen den beteiligten Universitäten bzw. Instituten erfolgt ausschließlich zu Studienzwecken und nur in pseudonymisierter Form. Die Weitergabe von Gruppendaten erfolgt ausschließlich zu Publikationszwecken. Die Daten können im Falle unerwünschter Ereignisse (z. B. unerwünschte Nebenwirkungen durch das Training) pseudonymisiert an die zuständige Ethikkommission der Landesärztekammer Rheinland-Pfalz weitergegeben werden. Die Daten können bei der genannten Stelle für die Zeit der Bearbeitung gespeichert werden. Im Übrigen übermitteln wir personenbezogenen Daten an Dritte nur, sofern hierfür eine gesetzliche Erlaubnis besteht oder Sie zuvor eingewilligt haben (siehe Art. 6 Absatz 1 Buchstaben a DSGVO). Eine ggf. erteilte Einwilligung können Sie jederzeit mit Wirkung für die Zukunft widerrufen.

4.) Übermittlung in Staaten außerhalb der EU

Eine Datenübermittlung von personenbezogenen Daten in Drittländer findet nicht statt und ist auch nicht geplant.

5.) Löschung

Im Einklang mit den „Guidelines for Good Clinical Practice" werden Ihre personenbezogenen Daten 10 Jahre lang in den Räumen des Studienzentrums für psychische Erkrankungen (SPE) der Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz nach DCP-Vorgaben aufbewahrt und anschließend vernichtet. Erfasste personenbezogene Daten aus der E-Mail und Telefonkommunikation werden 3 Monate nach Studienabschluss vernichtet. Während der Studienlaufzeit haben die Studienteilnehmer jederzeit die Möglichkeit, eine Löschung Ihrer E-Mail- und Telefonkommunikation mit dem Studienteam zu beantragen. Sie haben jederzeit das Recht, die auf den Studiensmartphones installierten Apps zu löschen. Eine Löschung der bis dahin von Ihnen erfassten personenbezogenen Daten müssen Sie separat schriftlich bei Dipl.-Psych. Angela Kunzler (Angela.Kunzler@unimedizin-mainz.de) beantragen.

6.) Datensicherheit

Die Klinik und Poliklinik für Psychiatrie und Psychotherapie der Universitätsmedizin Mainz hat die notwendigen technischen und organisatorischen Maßnahmen ergriffen, um die von Ihnen zur Verfügung gestellten personenbezogenen Daten vor Verlust, Zerstörung, Manipulation und unberechtigtem Zugriff zu schützen. Unsere an der Studie beteiligten Mitarbeiter und alle an der Datenverarbeitung beteiligten Personen sind zur Einhaltung datenschutzrelevanter Gesetze und zum vertraulichen Umgang mit personenbezogenen Daten verpflichtet (§ 29 Abs. 2 LDSG Rheinland-Pfalz; § 31 Abs. 1 LDSG Rheinland-Pfalz).

7.) Ihre Rechte

Im entsprechenden rechtlichen Rahmen haben Sie folgende Rechte im Hinblick auf Ihre personenbezogenen Daten, sofern diese nicht durch § 27 Abs. 2 BDSG im Rahmen der Datenverarbeitung zu wissenschaftlichen Forschungszwecken eingeschränkt sind. Dazu gehören:

- Auskunft über die Verarbeitung Ihrer personenbezogenen Daten,
- Berichtigung Ihrer personenbezogenen Daten,
- Löschung Ihrer personenbezogenen Daten,
- Einschränkung der Verarbeitung Ihrer personenbezogenen Daten,
- Der Verarbeitung Ihrer personenbezogenen Daten zu widersprechen,
- Die Einwilligung zur Verarbeitung zu widerrufen,
- Personenbezogene Daten in einem strukturierten gängigen elektronischen Format zu erhalten oder an einen Dritten übermitteln zu lassen (Recht auf Datenübertragbarkeit) und
- Bei einer Datenschutzbehörde Beschwerde einzulegen oder sich an den betrieblichen Datenschutzbeauftragten der Universitätsmedizin Mainz oder der Leuphana Universität Lüneburg zu wenden, etwa per E-Mail an datenschutz@unimedizin-mainz.de bzw. datenschutz@leuphana.de.

8.) Keine automatisierte Einzelentscheidung

Eine automatisierte Einzelentscheidung i.S.d. Art. 22 DSGVO findet nicht statt.

Appendix F6 Written Informed Consent

Versuchsleitung & Datenverarbeitung:
Dipl.-Psych. Angela Kunzler

Klinik und Poliklinik für Psychiatrie und Psychotherapie
Universitätsmedizin der Johannes Gutenberg-Universität Mainz
Untere Zahlbacher Straße 8, 55131 Mainz

Email: angela.kunzler@unimedizin-mainz.de
Tel.: +49 (0)6131 17 6110

Einwilligungserklärung

„TRAIN4Positivity-Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias"

Name des Probanden: _____

Ich erkläre mich bereit, an der Studie freiwillig teilzunehmen. Ich bin durch das Studienteam ausführlich und verständlich über Wesen, Bedeutung, Risiken und Tragweite der Studie aufgeklärt worden. Alle meine Fragen wurden zufriedenstellend beantwortet, ich kann jederzeit neue Fragen stellen. Ich habe darüber hinaus den Text der Studienaufklärungsbroschüre gelesen und verstanden. Ich hatte ausreichend Zeit, mich zu entscheiden. Mir ist bekannt, dass ich jederzeit und ohne Angabe von Gründen meine Einwilligung zur Teilnahme an der Studie zurückziehen kann (mündlich oder schriftlich), ohne dass mir daraus Nachteile entstehen.
Ich weiß, dass ein Versicherungsschutz im Rahmen einer Betriebshaftpflichtversicherung der Universitätsmedizin Mainz besteht und dass dieser sich nicht auf Wegeunfälle bezieht.

Ich habe verstanden und bin damit einverstanden, dass meine studienbezogenen Daten aus den Fragebögen und der Erhebung über das Smartphone pseudonymisiert (d.h. kodiert ohne Angabe von Namen, Anschrift, Initialen oder Ähnlichem) erhoben, auf gesicherten Datenträgern gespeichert und vom Studienteam ausgewertet werden.

Ich habe verstanden und bin damit einverstanden, dass die Informationen, die über die Smartphone App „Breezly" erfasst werden (z. B. aktuelle Befindlichkeit nach jeder Trainingseinheit), und verschiedene Metadaten (z. B. Häufigkeit der Nutzung der App) pseudonymisiert erfasst und ausgewertet werden. Ich bin einverstanden, dass diese Daten über die App verschlüsselt an die Leuphana Universität Lüneburg übertragen und auf dem dortigen Server pseudonymisiert gespeichert werden.

Ich wurde darüber informiert und bin einverstanden, dass die Informationen, die über die Smartphone App „movisensXS" erfasst werden (wöchentliche Erhebung von Alltagsstressoren), pseudonymisiert erfasst und ausgewertet werden. Ich habe verstanden, dass es movisens GmbH, Karlsruhe, Deutschland gestattet ist, die mit der Smartphone App „movisensXS" erhobenen Informationen zu verarbeiten und auf der Datenbank der Host Europe GmbH in Köln, Deutschland zu speichern bis die Löschung der Daten von der Studienleitung angefordert wird. Die Daten werden pseudonymisiert gesammelt und gespeichert. Die Identität der ProbandInnen ist movisens unbekannt. Der vorliegenden Studie ist nicht gestattet, Daten von ProbandInnen mit dem Smartphone zu erheben, die die Identität der ProbandInnen aufdecken könnten.

Die Weitergabe von studienbezogenen Daten an Dritte einschließlich Publikation erfolgt ausschließlich in anonymisierter Form, d.h. kann nicht mehr meiner Person zugeordnet werden. Ich weiß, dass die Daten im Falle unerwünschter Ereignisse pseudonymisiert an die Ethikkommission der Landesärztekammer Rheinland-Pfalz weitergegeben werden können. Die Daten können bei der genannten Stelle für die Zeit der Bearbeitung gespeichert werden.

Ich habe verstanden, dass meine Personendaten getrennt von den studienbezogenen Daten gespeichert werden und darauf nur von der Studienleitung und einem engen wissenschaftlichen Mitarbeiterkreis kontrolliert zugegriffen werden kann. Die Datenhaltung und -verarbeitung erfolgt durch die Versuchsleitung (Kontaktdaten siehe oben). Ich weiß, dass die wissenschaftlichen Studiendaten für eine vorgeschriebene Dauer von 10 Jahren aufbewahrt werden.

Ich wurde über meine Rechte in Bezug auf die mich betreffenden, personenbezogenen Daten (Recht auf Auskunft, Recht auf Berichtigung, Recht auf Einschränkung der Verarbeitung, Recht auf Widerspruch, Recht auf Löschung, Recht auf Datenübertragbarkeit) informiert. Die Verarbeitung meiner personenbezogenen Daten ist nur mit meiner Einwilligung rechtmäßig. Ich habe das Recht, meine Einwilligung zur Verarbeitung personenbezogener Daten jederzeit zu widerrufen.

Wenn ich eines der obigen Rechte in Anspruch nehmen möchte, kann ich mich an den Studienleiter oder an die Datenschutzbeauftragten der beteiligten Prüfzentren in Mainz und Lüneburg wenden. Außerdem habe ich das Recht, Beschwerde bei der/den Aufsichtsbehörde/n einzulegen, wenn ich der Ansicht bin, dass die Verarbeitung der mich betreffenden personenbezogenen Daten gegen die DSGVO verstößt. Die jeweiligen Kontaktdaten finde ich am Ende der Studienaufklärungsbroschüre.

Ein Exemplar der Probandeninformation und ein Exemplar der Einwilligungserklärung habe ich erhalten, gelesen und verstanden.

Ich erkläre mich mit der Durchführung der vorgenannten Studie einverstanden.

Mainz, den

Unterschrift des Teilnehmers Name in Druckbuchstaben

Unterschrift des Studienmitarbeiters Name in Druckbuchstaben

Ich wurde über die Risiken über die Nutzung eines Smartphones im Alltag aufgeklärt und erkläre mich dazu bereit, das Gerät nicht in kritischen Situationen, die meine volle Aufmerksamkeit erfordern (z. B. im Straßenverkehr oder bei der Bedienung von schweren Maschinen) zu benutzen.

Unterschrift des Teilnehmers

Appendix F7 Generalization of Pseudonymization Codes

Several pseudonymization codes were generated for the different parts of the study (screening and pre-/postassessment, mobile-based training using "Breezly", EMA assessment using "movisensXS"). The codes for the *screening and the pre- and posttest* (fictional example: TP001HAF) were generated using the following principle: 1–2: abbreviation TP for TRAIN$_4$Positivity, 3–5: consecutive number (e.g., 001, 002, 003, …0XX), 6–7: first and second letter of first name in capitals (e.g., Hanna: HA), 8: gender (female: F, male: M). The access code to *"Breezly"* (fictional example: BRE001ML) was composed as follows: 1–3: abbreviation BRE for "Breezly", 4–6: consecutive number (e.g., 001, 002, …0XX), 7–8: ML (for Mainz/Lüneburg). The access code to "*movisensXS"* was generated automatically by the movisens software when scanning a QR code before the study smartphone was handed out to the participants.

Appendix F8 Acknowledgement of Receipt for Study Smartphone

„TRAIN4Positivity-Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias“

Empfangsbestätigung

Folgendes Gerät wurde mir für die 21-tägige Nutzung im Rahmen der Studienteilnahme ausgehändigt:

Gerät	Inventarnummer	Zubehör
Motorola Moto G		USB-Kabel, Hülle

Daten des Studienteilnehmers

Name, Vorname:
Straße & Nr.:
PLZ & Wohnort:
Telefon:
E-Mail:

Empfangsbestätigung mit Unterschrift

Ich verpflichte mich, das Gerät nach Ablauf der Trainings- und Erhebungsphase zurückzugeben.

Vor- und Zuname	Datum	Unterschrift

Ausgehändigt durch:

Vor- und Zuname	Datum	Unterschrift

voraussichtliches Rückgabedatum:

Rückgabe des Geräts

Datum der Rückgabe:

In Empfang genommen durch:

Unterschrift der Empfangsperson:

Appendix F9 Additional Information Concerning Operating Principles of "Breezly" App

JOHANNES GUTENBERG UNIVERSITÄT MAINZ JGU DRZ MAINZ JGU UNIVERSITÄTSmedizin. MAINZ

Weitere Erläuterungen zur Breezly-App & movisensXS

Funktionsweise der Handys allgemein:

- Motorola Moto G
- Die beiden für die Studienteilnahme erforderlichen Applikationen sind bereits installiert

„Breezly"

movisensXS

- Andere Apps auf dem Studiensmartphone sollten nach Möglichkeit **nicht** verwendet werden
- Das Studiensmartphone enthält **keine** SIM-Karte und ist nicht durch eine Antiviren-Software geschützt
- **Internetverbindung:**
  1. Für die Nutzung der App „movisensXS":
     - ist prinzipiell keine WLAN-Verbindung erforderlich
     - **Achtung:** Das Handy sollte jedoch am besten **regelmäßig mit einem WLAN-Netzwerk** verbunden werden
     - **Achtung:** Bitte auch Verbindung mit dem **Google PlayStore** regelmäßig prüfen (falls das Studiensmartphone automatisch abgemeldet wurde, lauten die Zugangsdaten: train4positivity@gmail.com; Passwort: Train4Positivity#ML
     - Am **Ende des dreiwöchigen Trainingszeitraums** sollte das Handy noch einmal mit einem WLAN-Netzwerk verbunden werden, um alle Daten zu übertragen
  2. Für die Nutzung der App „Breezly":
     - wird insbesondere für die **erste Anmeldung und anschließend in regelmäßigen Abständen** während des Trainingszeitraums eine WLAN-Verbindung empfohlen
     - Sobald die Bildbewertung begonnen wurde, kann diese auch offline stattfinden und jederzeit unterbrochen werden
     - Auch die Trainingseinheiten können offline stattfinden, sollten jedoch nicht unterbrochen werden
     - Am **Ende des dreiwöchigen Trainingszeitraums** sollte das Handy noch einmal mit einem WLAN-Netzwerk verbunden werden, um alle Daten zu übertragen
- Das Studiensmartphone sollte während des dreiwöchigen Erhebungszeitraums soweit möglich immer mitgeführt werden; hierbei sollte der Akkuzustand regelmäßig überprüft und das Gerät geladen werden, um ein kontinuierliches Training zu gewährleisten und um keine Messung auszulassen
- Im Falle technischer Probleme: Telefonnummer oder Mailadresse des Studienteams (s. Studienaufklärungsbroschüre)

Funktionsweise der App „Breezly“

1. Persönlicher Zugangscode

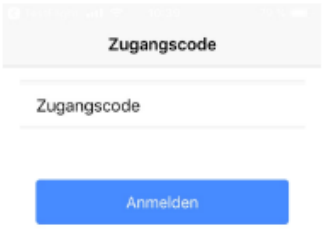


Abb. 1. Eingabe des persönlichen Zugangscodes

- Nutzung der App nur durch Zugangscode möglich
- Der Ihnen zugewiesene Zugangscode wurde bereits eingegeben
- Falls eine erneute Eingabe des Codes bei der ersten Nutzung oder zukünftigen Anmeldungen bei „Breezly“ erforderlich sein sollte, können Sie den Ihnen zugewiesenen Code der Verpackung des Smartphones entnehmen
- **Achtung:** Während der Eingabe des Zugangscodes muss das Smartphone **mit einem WLAN-Netzwerk verbunden sein**

2. Startbildschirm & Erstellung des persönlichen Profils

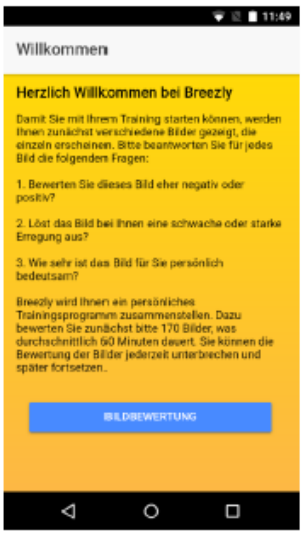


Abb. 2. Startbildschirm bei der erstmaligen Nutzung der App

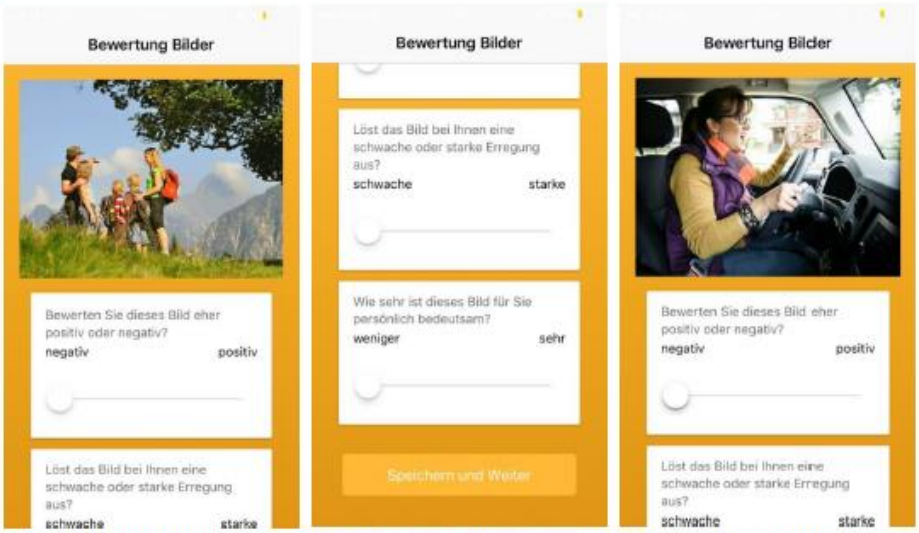


Abb. 3. Individuelle Bewertung jedes Bildes anhand von drei Merkmalen auf einer 9-stufigen Skala

1. Bewerte ich das Bild eher positiv oder negativ? (1: negativ, 9: positiv)
2. Löst das Bild bei mir eine schwache oder starke Erregung aus? (z. B. körperlich, emotional) (1: schwache Erregung, 9: starke Erregung)
3. Wie sehr ist dieses Bild für mich persönlich bedeutsam? (1: weniger bedeutsam, 9: sehr bedeutsam)

➢ Auswahl von **50 persönlichen Trainingsbildern** (25 positive, 25 negative)

3. Auswahl des Avatars („Betty" oder „Brian") zur Verfolgung des persönlichen Trainingsfortschritts

Abb. 4. Auswahl des persönlichen Avatars für den dreiwöchigen Trainingszeitraum

4. Ablauf einer Trainingseinheit

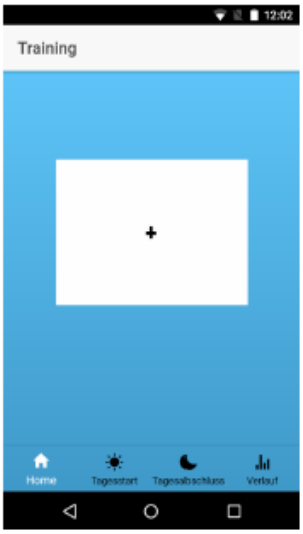


Abb. 5. Fixationskreuz vor dem Erscheinen jedes Bildes

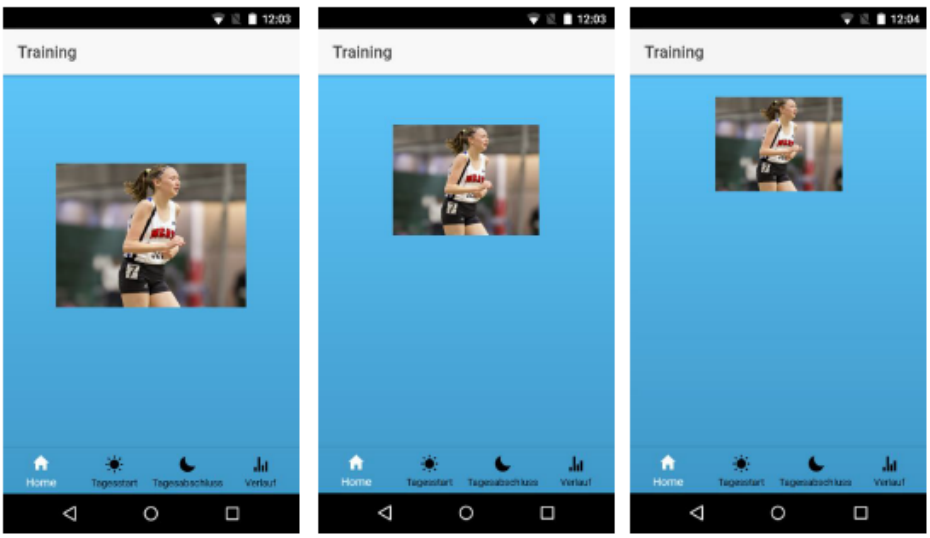


Abb. 6. Erscheinen eines negativen Bildes in einer Trainingseinheit und Verkleinerung des Motivs durch „Wegwischen" auf dem Display

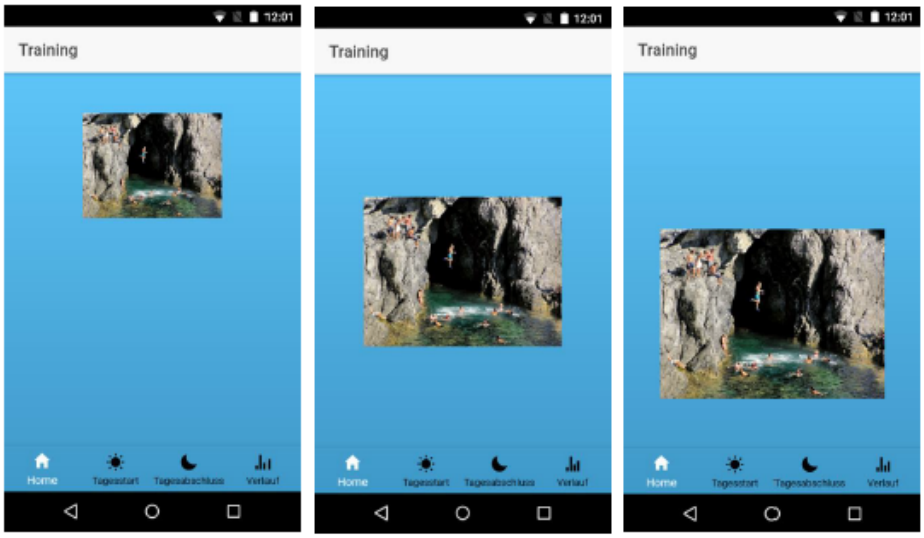


Abb. 7. Erscheinen eines positiven Bildes in einer Trainingseinheit und Vergrößerung des Motivs durch Heranziehen auf dem Display

- Insgesamt **10 Bilder** pro Trainingseinheit

Abb. 8. Erfassung der aktuellen Befindlichkeit am Ende jeder Trainingseinheit

5. Trainingsempfehlung

- Morgens (7-11 Uhr): 3 Trainingseinheiten
- Abends (19-24 Uhr): 3 Trainingseinheiten

6. Punktesystem durch Avatar („Betty“ oder „Brian“)

Abb. 9. Verschiedene mögliche Stufen des Avatars „Betty“ im Verlauf des Trainings

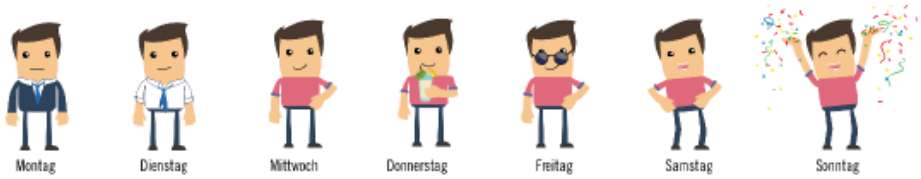


Abb. 10. Verschiedene mögliche Stufen des Avatars „Brian “ im Verlauf des Trainings

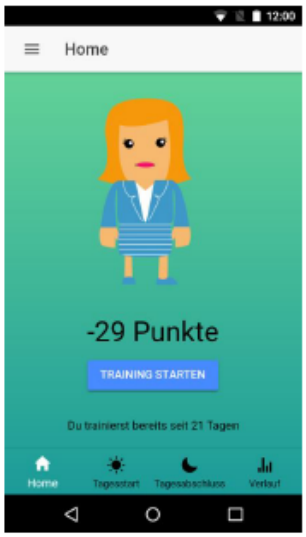


Abb. 11. Home Screen von „Breezly“ mit Avatar und Punkteanzeige sowie Darstellung der Anzahl von Trainingstagen

<u>Regeln des Punktesystems:</u>

- Die Übungen (1 Trainingseinheit = 1x Bildertraining + aktuelle Befindlichkeitsabfrage) sollten **möglichst täglich** gemacht werden
- Der Avatar startet bei Stufe 2
- Für jede Trainingseinheit innerhalb der **empfohlenen Zeiten (3x von 7-11 Uhr, 3x von 19-24 Uhr)** erhalten Sie jeweils **5 Punkte**

- Zusätzliche Trainingseinheiten (> 3) in diesen Zeiten ergeben nur noch **2 Punkte** pro Durchgang
- Trainingseinheiten außerhalb der empfohlenen Zeiten ergeben nur **1 Punkt** pro Einheit
- Trainieren Sie **3 Tage hintereinander nach Empfehlung**, erhalten Sie **Zusatzpunkte**
- 30 Punkte sind nötig, damit der Avatar um eine Stufe steigt (Zusatzpunkte haben keinen Einfluss auf die Stufenveränderung)
- Sie erhalten **mehr** Punkte, wenn Sie nach Empfehlung trainieren, als wenn Sie zu anderen Zeiten tagsüber trainieren
- **7 Tage Training nach Empfehlung** sind nötig, um den Avatar auf die höchste Stufe zu bringen. Wird weiterhin nach Empfehlung trainiert, bleibt diese Stufe erhalten

- Falls Trainingseinheiten ausgelassen werden, kommt es zu **Minuspunkten**
  - Kein Training morgens (7-11 Uhr) oder abends (19-24 Uhr) ergibt **6 Punkte Verlust** pro ausgelassener Trainingseinheit
  - 30 Minuspunkte sind nötig, damit der Avatar um eine Stufe fällt
  - Diese Minuspunkte können Sie **„abfedern" (10 Pluspunkte),** indem Sie zuvor regelmäßig trainiert haben (vgl. Fitnessstudio: Trainieren Sie drei Tage lang kontinuierlich und machen dann einen Tag nichts, baut ein Muskel nicht so stark ab als hätten Sie zuvor überhaupt nicht trainiert)

7. Erfassung weiterer psychologischer Maße zu Tagesstart und Tagesabschluss

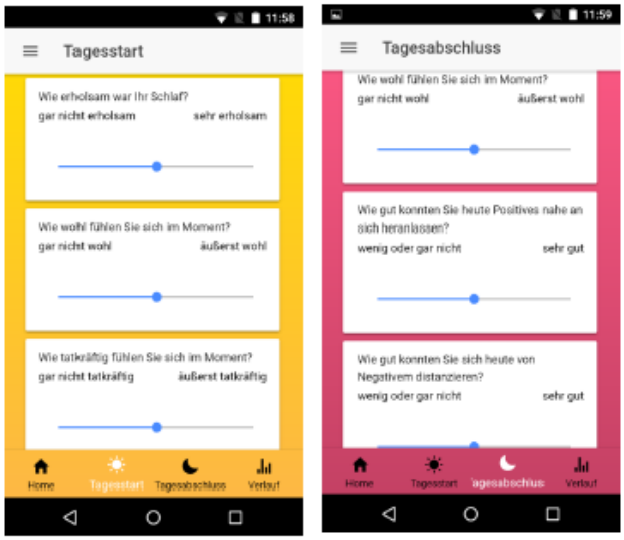


Abb. 12. Abfrage psychologischer Maße zu Tagesstart und Tagesabschluss

- Tagesstart: aktuelles Wohlbefinden; Schlaferholsamkeit; Tatkraft
- Tagesabschluss: aktuelles Wohlbefinden; Fähigkeit, Positives nahe an sich heranzulassen; Fähigkeit, sich von Negativem zu distanzieren

8. Erinnerungsfunktion

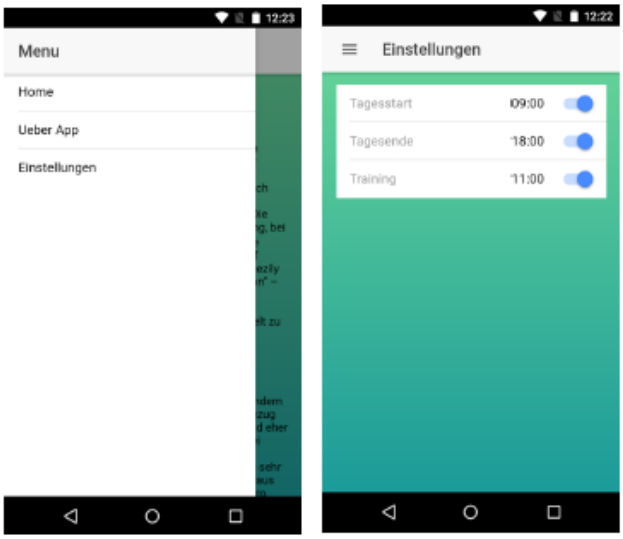


Abb. 13. Einstellung der Erinnerungsfunktion an Training und Tagesstart/ Tagesabschluss

9. Verlaufskurven für erfasste psychologische Maße

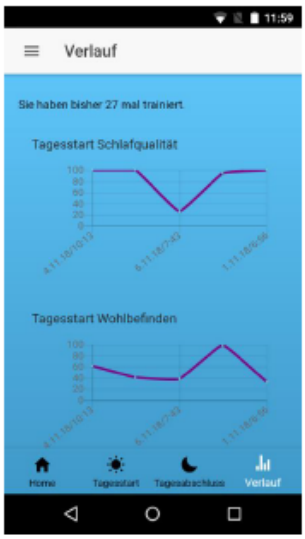


Abb. 14. Verlaufskurven für psychologische Maße (Tagesstart/Tagesabschluss)

10. Weitere Infos über die App

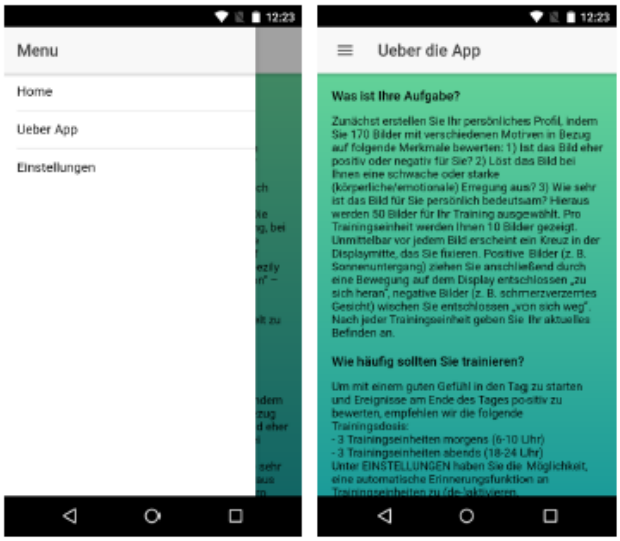


Abb. 15. Zusätzliche Informationen über Hintergrund und Funktionsweise von „Breezly" („Über die App")

11. Trainingsdauer

- Drei Wochen ab Ausgabe des Studiensmartphones
- **Achtung:** Beginnen Sie die Bildbewertung nach Möglichkeit bitte am **selben Tag oder spätestens am Folgetag** der Aufklärung und zwar **so, dass Sie am entsprechenden Tag zumindest noch drei der sechs empfohlenen Trainingseinheiten (morgens oder abends) absolvieren können**
- Nur so wird gewährleistet, dass Sie möglichst drei ganze Wochen trainieren und nicht gleich zu Beginn schon Minuspunkte haben

Funktionsweise der App „movisensXS"

- Diese App dient zur Erfassung von Alltagsstressoren („daily hassles")
- z. B. Zeitdruck, Verzögerungen im öffentlichen Nahverkehr
- Die Messung erfolgt immer am **Ende jeder Woche (jeden Sonntag)** des dreiwöchigen Trainingszeitraums
- Abgefragt wird jeweils die Art und die Anzahl der Alltagsstressoren sowie die hierdurch ausgelöste subjektive Belastung
  1. Mikrostressor-Liste (Mehrfachnennungen möglich): Oberkategorien von Stressoren, aus denen Sie die passende Kategorie für Ihr persönliches Erlebnis auswählen
  2. Subjektive Belastung für jeden von Ihnen angegebenen Stressor

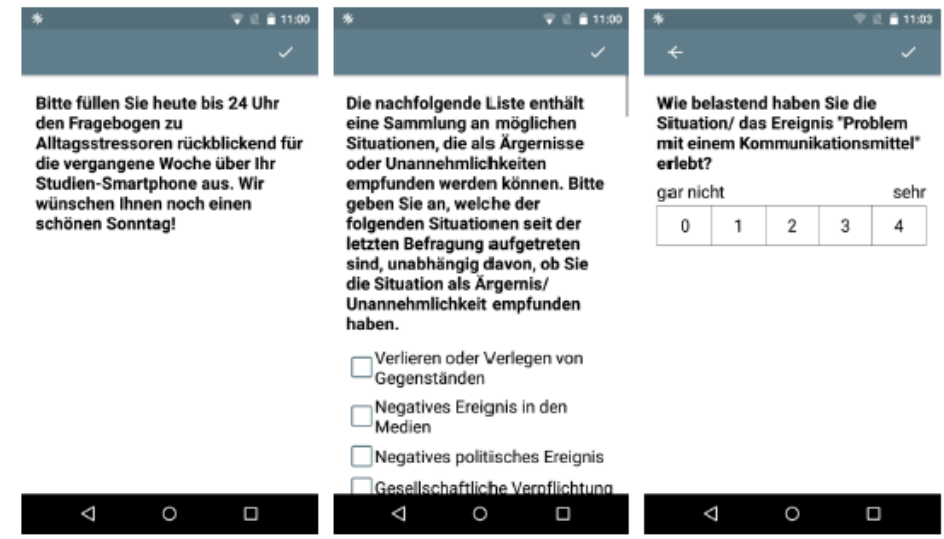


Abb. 16. Abfrage Mikrostressoren in „movisensXS“

1. Start der Abfrage

- Bis zur ersten Stressorabfrage am Sonntag der ersten Trainingswoche müssen Sie die App „movisensXS“ im Grunde gar nicht benutzen
- „movisensXS“ erinnert Sie automatisch jeden Sonntag um **11.00 Uhr** und um **14.00 Uhr** an die Bearbeitung des Fragebogens
- **Achtung:** Um die Stressorabfrage durchführen zu können, müssen Sie den Alarm der movisens-App **bitte annehmen**

  Falls Sie den Alarm um 11.00 Uhr abgewiesen haben, ist lediglich wieder die Stressorabfrage um 14.00 Uhr möglich! Ein eigenständiges Ausfüllen der Stressorabfrage über die App „movisensXS“ ist leider nicht möglich

- **Achtung:** Falls Sie bereits auf die Erinnerung um 11.00 Uhr reagiert haben, können Sie den zweiten Alarm um 14.00 Uhr einfach ignorieren bzw. abweisen
- Bei Fragen oder technischen Problemen haben Sie die Möglichkeit, innerhalb der App „movisensXS“ über die Chatfunktion eine Mitteilung an das Studienteam zu senden

Appendix F10 Study Reimbursement in TRAIN$_4$Positivity

All participants received a financial compensation of 50,00 € for the three-week study participation (mobile-based training, completion of pre-/posttest assessment and of weekly stressor monitoring). In addition, they were given the opportunity to choose between the participation in three prevention programs.

On the one hand, subjects could participate in a resilience workshop provided by the author of this thesis (Dipl.-Psych. Angela Kunzler) at the Leibniz Institute for Resilience Research (LIR; previously: Deutsches Resilienz Zentrum (DRZ) gGmbH). Since the sponsor of the TRAIN$_4$Positivity study changed during the study process, the workshop was indicated as being organized by the Department of Psychiatry and Psychotherapy of the UMC Mainz in the study brochure. The one-day workshop provides an overview of the concept of psychological resilience and proven resilience factors (e.g., optimism, social support). Using practical exercises, the participants learn how to recognize physical and mental symptoms of overload and stress, how to use selected resilience factors to better cope with daily stressors (e.g., private and/or professional life), and how to stay healthy despite these stressors.

On the other hand, the participants could choose between two mobile- or online-based health programs of the Leuphana University Lüneburg (HOLIDAILY, GET.ON Dankbarkeit). The smartphone app “HOLIDAILY” fosters the sustained recovery from professional strain before, during, and after holiday (https://geton-training.de/holidaily/). The online-based gratitude intervention, which additionally uses a smartphone application, contains regular practices of gratitude to strengthen the attention for positive events and to improve an individual’s emotional well-being. The study reimbursement of TRAIN$_4$Positivity (financial and workshops) was independent of the participants’ compliance with the intervention (e.g., number of training sessions).

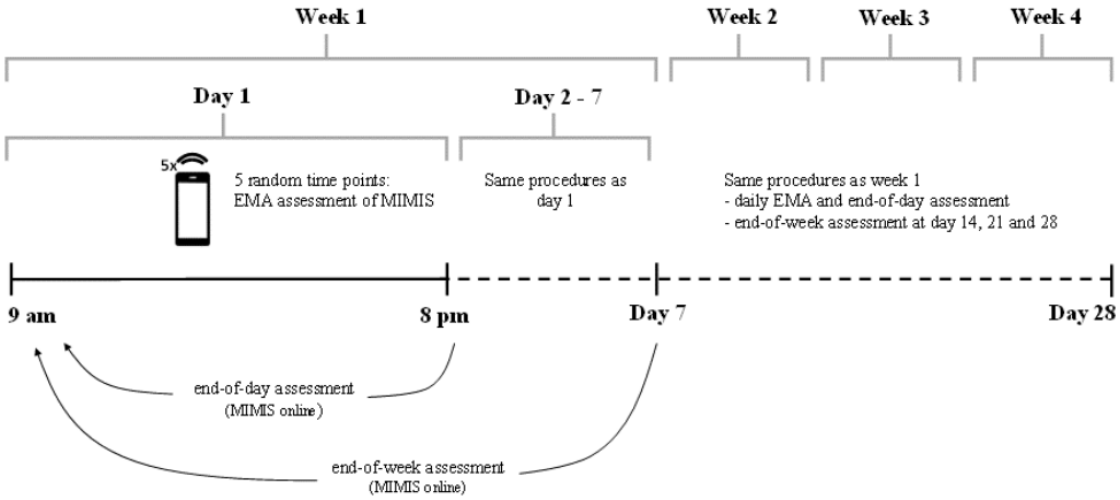
Appendix F11 Study Course and Variables Assessed in LifeStress (Historical Control Group)

The study procedures of LifeStress as longitudinal EMA study are described in detail in Chmitorz, Kurth, et al. (2020; study 3). At the beginning of the 4-week study (28 days), the baseline assessment included sociodemographic variables (sex, age, employment status, nationality, education), lifestyle (e.g., smoking behavior, internet consumption), and mental health-related variables (mental dysfunction and well-being) as well as chronic stress. The participants received a study smartphone before the ambulatory assessment. In a final session during the week after the 28-day assessment period, participants returned the study mobile phones and provided feedback in a semistructured interview. For the mobile-based ambulatory assessment (signal-contingent approach) using the app "movisensXS", the revised 58-item version of the MIMIS was used. At five random time points between 9 a.m. and 8 p.m., an acoustic and visual signal ("please answer the questions below"; p. 4) notified the participants to indicate the occurrence ("Please indicate which of the following situations occurred since the last alarm, independent from whether they were perceived as a hassle or not."; p. 4-5) and the perceived severity of microstressors. Participants who were not able to enter their data by the time the signal occurred, were reminded every 30 minutes to complete the questionnaire for the next 90 minutes. In addition, participants could manually activate the data entry during the subsequent 90 minutes after they had ignored the initial signal. Consistent with the EMA microstressor monitoring in TRAIN$_4$Positivity (using "movisensXS" each Sunday), the participants could choose the possibly occurred stressors from a list of microstressors and were asked to rate the severity of the stressor on a 5-point Likert scale from 0 (*not at all severe*) to 4 (*very severe*). They were also asked to indicate their current mood using an EMA-validated short scale (consistent with current mood assessment in TRAIN$_4$Positivity). In addition to the mobile phone-based ambulatory assessment throughout the day, participants of LifeStress also completed an online-delivered (SoSci Survey) end-of-day assessment on each of the 28 days as well as an end-of-week assessment at the end of each of the 4 weeks (end-of-week assessment at week 3 is relevant for the comparison between TRAIN$_4$Positivity and LifeStress in this study; see RQ1, exploratory RQ 1.2). Comparable with

TRAIN4Positivity, the participants were reminded to complete these assessments, respectively (by email). The study design is presented in Figure F11.1.

Figure F11.1

Study Design of LifeStress (Taken From Chmitorz, Kurth, et al., 2020)



As in TRAIN4Positivity, the participants of LifeStress received a monetary compensation at the end of the study (see Chmitorz, Kurth, et al., 2020). The variables assessed in LifeStress are presented in Table F11.1.

Table F11.1

Study Measures and Scales of the LifeStress Longitudinal Study

Variable	Time point
Baseline assessment	
Sociodemographic variables, lifestile, and pre-existing conditions (anamnesis questionnaire of the GBS)[a]	Baseline
General health including mental health (GHQ-28; PHQ-D)	
Cognitive emotion regulation (CERQ)	
Optimism and pessimism (SOP2)	
Impulsive behavior (I-8)	
Internal and external locus of control (IE-4)	
Interpersonal trust (KUSIV3)	
Social desirability (KSE-G)	
Self-efficacy (ASKU)	
Personality (BFI-10)	
Anxiety (STAI-Y2)	
Physical activity (IPAQ)	
Chronic stress (STICS-R)	

Variable	Time point
Macrostressors (life events; LE Checklist)	
Well-being (WHO-5)	
Social support (OSS-3)	
Coping (Brief COPE)	
Ability to recover from stress (BRS)	
Positive appraisal style (gPASQ)	
Study outcomes	
Microstressors (daily hassles; MIMIS)	Baseline; EMA during 28 subsequent days; end-of-day (online survey); end-of-week (online survey)
Mood (MDMQ)	EMA during 28 subsequent days

Note. GBS = Gutenberg Brain Study; GHQ-28 = General Health Questionnaire-28 (Goldberg & Hillier, 1979; Klaiberg et al., 2004); PHQ-D = Patient Health Questionnaire (Löwe et al., 2002; Spitzer et al., 1999); CERQ = Cognitive Emotion Regulation Questionnaire (Garnefski et al., 2002; Loch et al., 2011); SOP2 = Scale Optimism-Pessimism-2 (Kemper, Beierlein, Kovaleva, et al., 2012); I-8 = Impulsive Behavior Scale (Kovaleva et al., 2012); IE-4 = Short Scale for the Assessment of Locus of Control (Kovaleva, 2012); KUSIV3 = Interpersonal Trust Short Scale (Beierlein et al., 2012b); KSE-G = Social Desirability–Gamma Short Scale (Kemper, Beierlein, Bensch, et al., 2012); ASKU = Short Scale for Measuring General Self-efficacy Beliefs (Beierlein et al., 2012a); BFI-10 = Big-Five-Inventory-10 (Rammstedt et al., 2012); STAI-Y2 = State-Trait Anxiety Inventory-Y2 (Laux et al., 1981; Spielberger et al., 1970); IPAQ = International Physical Activity Questionnaire (Craig et al., 2003; Helmerhorst et al., 2012); STICS-R = Short Trier Inventory for Chronic Stress (Schulz et al., 2004); LE Checklist = Life Events Checklist (Chmitorz, Neumann, et al., 2020); WHO-5 = Well-being Index (Bech, 2004; Brähler et al., 2007); OSS-3 = Oslo Social Support Scale (Dalgard et al., 1995); Brief COPE = Brief Coping Orientation to Problems Experienced (Carver, 1997; Knoll et al., 2005); BRS = Brief Resilience Scale (Chmitorz, Wenzel, et al., 2018; Smith et al., 2008); gPASQ = general Positive Appraisal Style Questionnaire (self-developed); MIMIS = Mainz Inventory of Microstressors (Chmitorz, Kurth, et al., 2020); MDMQ = Multidimensional Mood Questionnaire (Wilhelm & Schoebi, 2007).

[a] mostly based on Beckman et al. (2016).

Appendix F12 Further Ethical Aspects

In general, no serious burden or adverse events were assumed for the participants. The mobile-based intervention was non-invasive. In addition, comparable studies to foster resilience or mental health in a group with stressor exposure (e.g., Joyce et al., 2018; Leppin et al., 2014; Macedo et al., 2014; Robertson et al., 2015; Vanhove et al., 2016) generally indicated high levels of acceptability. Participants were instructed to perform training sessions with "Breezly" especially in the morning and evening in the attempt to prevent an intrusion of the subjects' privacy and a disruption of their daily life as far as possible. During the intervention with the smartphone app, participants were confronted with positive as well as aversive stimuli (e.g., picture of crying child). However, by pre-selecting the training pictures and by excluding inadequate motives (e.g., pictures with elevated arousal or those showing physical injuries), the potential negative impact of these pictures was minimized. Potential adverse events through the intervention were assessed at posttest (qualitative feedback) und were compensated by the study team.

With respect to *EMA*, there could have been a certain risk or burden for participants in case they experienced this survey method as intrusive due to the repeated assessments in the morning, in the evening, after each training session, and at each weekend as well as due to the study period of three weeks, where they had to carry the smartphones along. However, experiences from previous studies indicate high acceptance for this survey method (Kubiak et al., 2008; Kubiak et al., 2014). Besides, in the current study, an intrusion of the participants' privacy was minimized as far as possible by dispensing methods of automatic data collection, such as GPS tracking. An unauthorized tapping of the smartphone microphone was excluded.

Through the *mobile-based implementation* of the positivity training and the EMA assessments, distractions during an individual's daily life or an interruption of important activities may have occurred. To reduce these risks, participants were informed about possible risks of using a smartphone while driving or operating heavy machinery. When giving their informed consent for study participation, the participants undertook not to use the device in situations that required their

full attention. Participants were requested to mute the study smartphones in these situations. Moreover, due to the age range of participants in the present study (inclusion criterion: 18–30 years), it could be assumed that the subjects were largely familiar with using smartphones in daily life. Therefore, no additional risks by the study smartphones were expected.

The *number of questionnaires and the computer paradigm AAT*, that had to be answered or performed at pre- and posttest, could have been experienced as burdensome by some participants. However, the extent of questionnaires in the current study seemed necessary to assess baseline characteristics and potential effects of the MB-PBT. The selection of questionnaires had been established in previous studies of the LIR with ethical approval (e.g., Gutenberg Brain Study [GBS], no.: 837.539.12 [8640-F]; LifeStress, no.: 837.183.16 [10502]; Longitudinal Resilience Assessment [LORA], no.: 837.105.16 [10424]) and was based on theory. In addition, the time for performing the AAT was relatively low (10 minutes). As, to date, there is no questionnaire-based measurement of positivity bias, the use of this computer paradigm was required to assess one of the secondary outcomes of this project (positivity bias at the level of action tendencies).

Overall, the potential risks and burden for participants in the current study were minimized as far as possible. Due to the information provided to the participants at individual meetings with the study team (e.g., objectives of study, functioning of “Breezly”, potential risks, and burden of the study), participants were qualified to decide about their participation. During the entire study period, there was the opportunity to be in close contact to the study team.

Appendix F13 Ethical Approval

Landesärztekammer Rheinland-Pfalz
Körperschaft des Öffentlichen Rechts
Postfach 29 26 · 55019 Mainz
Deutschhausplatz 3 · 55116 Mainz
Telefon (06131) 28822-63 /-64 /-65 /-67
Telefax (06131) 28822-66

EINGANG
25. Jan. 2018
- Chefsekretariat -

- Ethik-Kommission -

Landesärztekammer Rhld.-Pf. · Postfach 29 26 · 55019 Mainz

Universitätsmedizin Mainz, Klinik für Psychiatrie und Psychotherapie
Prof. Dr. Klaus Lieb
Untere Zahlbacher Straße 8
55131 Mainz

Ansprechpartner:
Frau Pierzina, Frau Lermen,
Frau Rademacher, Frau Schleith

Telefon:
-63 (Pie), -64 (Ler), -65 (Rd), -67 (GS)

E-Mail: ethik-kommission@laek-rlp.de

Bitte bei jedem Schriftwechsel die Bearbeitungsnummer angeben!

Mainz, den 23.01.2018 / We

Antragstitel: TRAIN4Positivity - Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias
Antragsteller: Prof. Dr. Klaus Lieb
Antrags-Nummer: 2018-13043-KliFo erstberatend

Sehr geehrte Damen und Herren,

die Ethik-Kommission der Landesärztekammer Rheinland-Pfalz bestätigt den Eingang des o. g. Antrags auf berufsrechtliche Beratung gemäß § 15 Berufsordnung vom 18.01.2018.

Die Unterlagen sind unvollständig.

Bitte reichen Sie zu folgenden Punkten ergänzende Angaben/Unterlagen nach:

Bitte reichen Sie die Unterschriften der Leiter der beteiligten Einrichtungen nach.

Bitte reichen Sie die Unterlagen zeitnah, möglichst innerhalb von 14 Tagen nach. Die weitere Bearbeitung kann erst erfolgen, wenn die nachgeforderten Unterlagen vollständig vorliegen.

Mit freundlichen Grüßen
i.V.
Prof. Dr. Ignaz Wessler
Geschäftsstelle

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Landesärztekammer Rheinland-Pfalz
Körperschaft des Öffentlichen Rechts
Postfach 29 26 · 55019 Mainz
Deutschhausplatz 3 · 55116 Mainz
Telefon (06131) 28822-63 /-64 /-65 /-67
Telefax (06131) 28822-66

EINGANG
26. Feb. 2018
- Chefsekretariat -

- Ethik-Kommission -

→ F. Kunstler, Britta Rademacher
Ke

Landesärztekammer Rhld.-Pf. · Postfach 29 26 · 55019 Mainz

Universitätsmedizin Mainz
Klinik für Psychiatrie und Psychotherapie
Herrn Prof. Dr. Klaus Lieb
Untere Zahlbacher Straße 8
55131 Mainz

Ansprechpartner:
Frau Pierzina, Frau Lermen,
Frau Rademacher, Frau Schleith

Telefon:
-63 (Pie), -64 (Ler), -65 (Rd), -67 (GS)

E-Mail: ethik-kommission@laek-rlp.de

Bitte bei jedem Schriftwechsel die Bearbeitungsnummer angeben!

Mainz, den 23.02.2018 / Pie

Antragstitel: TRAIN4Positivity - Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias
Antragsteller: Prof. Dr. Klaus Lieb
Antrags-Nummer: 2018-13043-KliFo erstberatend

Sehr geehrter Herr Professor Lieb,

die Ethik-Kommission hat in der Sitzung am 14.02.2018 das o. g. Forschungsvorhaben beraten.

Prinzipielle Bedenken gegen die Durchführung der Studie haben sich dabei nicht ergeben.

Vor Erteilen eines zustimmenden Votums sind die nachfolgenden Punkte zu klären / folgende Unterlagen nachzureichen:

1. Die Ethik-Kommission bittet um Nachreichung der Stellungnahme des Datenschutzbeauftragten der Universitätsmedizin.

2. Die Informationsschrift sollte in folgenden Punkten überarbeitet werden:
   a. Der Studienablauf ist zu überarbeiten. Die Screening-Untersuchung ist bereits Bestandteil der Studie und kann erst erfolgen, wenn die betroffene Person nach Aufklärung in die Studienteilnahme eingewilligt hat.
   Alle relevanten Informationen zum Studienablauf müssen im Rahmen der Aufklärung (nicht erst beim Einführungstermin) gegeben werden, damit die betroffene Person eine informierte Einwilligung erteilen kann.
   b. Bei den Angaben zum Datenschutz wird zunächst angegeben, dass alle Daten anonym erfasst und ausgewertet werden. Anschließend wird jedoch von der verschlüsselten Speicherung der Daten gesprochen. Vermutlich handelt es sich um pseudonymisierte (d.h. codierte) Daten.

3. In der Einwilligungserklärung sind die Angaben zum Datenschutz zu vervollständigen (z.B. Angaben zur Weitergabe und Speicherung der Daten an die Universität Lüneburg sowie die Firma movisens). Unklar ist für diese Studie die Angabe, dass Daten an die zuständige Bundesoberbehörde weitergegeben werden.

Bei Ihrer Rückantwort nehmen Sie bitte im Begleitschreiben zu jedem Punkt Stellung und geben auch an, wo ggf. entsprechende Änderungen in den nachgereichten Unterlagen gemacht wurden. Die Änderungen in den nachgereichten Unterlagen sind drucktechnisch hervorzuheben. Ferner sind die überarbeiteten Dokumente mit einer aktuellen Versionsnummer zu versehen, um eine eindeutige Zuordnung sicherzustellen.

Mit freundlichen Grüßen

Prof. Dr. Letzel
Vorsitzender

Landesärztekammer Rheinland-Pfalz
Körperschaft des Öffentlichen Rechts
Postfach 29 26 · 55019 Mainz
Deutschhausplatz 3 · 55116 Mainz
Telefon (06131) 28822-63 /-64 /-65 /-67
Telefax (06131) 28822-66

EINGANG
20. Aug. 2018
- Chefsekretariat -

- Ethik-Kommission -

Landesärztekammer Rhld.-Pf. · Postfach 29 26 · 55019 Mainz

→ Fr. Kunkle

Universitätsmedizin der
Johannes-Gutenberg-Universität Mainz
Klinik für Psychiatrie und Psychotherapie
Herrn Prof. Dr. Klaus Lieb
Untere Zahlbacher Straße 8
55131 Mainz

Ansprechpartner:
Frau Pierzina, Frau Lermen,
Frau Rademacher, Frau Schleith

Telefon:
-63 (Pie), -64 (Ler), -65 (Rd), -67 (GS)

E-Mail: ethik-kommission@laek-rlp.de

Bitte bei jedem Schriftwechsel die Bearbeitungsnummer angeben!

Mainz, den 16.08.2018 / Pie

Antragstitel: TRAIN4Positivity - Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias
Antragsnummer: 2018-13043-Klinische Forschung / erstberatend

Sehr geehrter Herr Professor Lieb,

die Ethik-Kommission bei der Landesärztekammer Rheinland-Pfalz hat in ihrer Sitzung am 14.02.2018 über Ihren Antrag beraten.

Auf der Grundlage der vorgelegten Unterlagen bestehen nach dem gegenwärtigen Stand keine berufsethischen und berufsrechtlichen Bedenken gegen die Durchführung der geplanten Studie. Sie erhalten damit ein zustimmendes Votum.

Ferner gibt die Ethik-Kommission folgende allgemeine Hinweise:

Die Verantwortlichkeit des Studienarztes bleibt in vollem Umfang bestehen und wird durch diese Entscheidung nicht berührt. Die Entscheidung ergeht unter dem Vorbehalt gleichbleibender Gegebenheiten.

Der Ethik-Kommission sind alle schwerwiegenden Komplikationen in beurteilbarer Form unverzüglich mitzuteilen. Die Ethik-Kommission bittet darum, dass ihr das Ergebnis der Studie zur Kenntnis gebracht wird.

Datenschutzrechtliche Aspekte von Forschungsvorhaben werden durch die Ethik-Kommission grundsätzlich nur kursorisch überprüft. Dieses Votum/diese Bewertung ersetzt mithin nicht die Konsultation des zuständigen betrieblichen oder behördlichen Datenschutzbeauftragten.

Mit freundlichen Grüßen
i.V.
Prof. Dr. Letzel
Vorsitzender

Landesärztekammer Rheinland-Pfalz

Folgende Unterlagen haben zur Beratung vorgelegen:

Antragsformular - Studienprotokoll_Positivity bias_Version_2.pdf (hinzugefügt 13.08.2018)
Prüfplan - Zusammenfassung_Studienprotokoll_Version_2.pdf (hinzugefügt 13.08.2018)
Fragebogen - Anlage 5_TRAIN4Positivity_Prätest_Fragebogen_Version_2.pdf (hinzugefügt 13.08.2018)
Fragebogen - Anlage 6_TRAIN4Positivity_Posttest_Fragebogen_Version_2.pdf (hinzugefügt 13.08.2018)
sonstiges Studienmaterial - Anlage 10_Screening-Checkliste_Version_2.pdf (hinzugefügt 13.08.2018)
Anlage 13_Datenschutzerklärung Train4Positivity_Version_1.pdf (hinzugefügt 13.08.2018)
Rekrutierungsmaterial - Anlage 1_Rekrutierungsemail_Version_2.pdf (hinzugefügt 13.08.2018)
Patienteninformation - Anlage 2_Studienaufklärungsbroschüre_Version_2.pdf (hinzugefügt 13.08.2018)
Informationsschrift/Einwilligung Erw - Anlage 3_Einwilligungserklärung_Version_2.pdf (hinzugefügt 13.08.2018)
Fragebogen - Anlage 4_TRAIN4Positivity_Screening_Fragebogen_Version_2.pdf (hinzugefügt 13.08.2018)
13043 E-Mail vom 27.04.2018.pdf (hinzugefügt 03.05.2018)
Einverständnis Klinikleiter - SKopi-Ethik18020614240.pdf (hinzugefügt 06.02.2018)
Fragebogen - Anlage 4_TRAIN4Positivity_Screening_Fragebogen_Version_1.pdf (hinzugefügt 22.01.2018)
Patienteninformation - Anlage 2_Studienaufklärungsbroschüre_Version_1.pdf (hinzugefügt 22.01.2018)
Patienteninformation - Anlage 3_Einwilligungserklärung_Version_1.pdf (hinzugefügt 22.01.2018)
Prüfplan - Studienprotokoll_Version_1.pdf (hinzugefügt 22.01.2018)
Antragsformular - Zusammenfassung_Studienprotokoll_Version_1.pdf (hinzugefügt 22.01.2018)
Rekrutierungsmaterial - Anlage 1_Rekrutierungsemail_Version_1.pdf (hinzugefügt 22.01.2018)
Lebenslauf - Anlage 11_CV Michèle Wessa.pdf (hinzugefügt 22.01.2018)
Lebenslauf - Anlage 12_CV Klaus Lieb.pdf (hinzugefügt 22.01.2018)
Anschreiben/Inhaltsverzeichnis - Anschreiben.pdf (hinzugefügt 22.01.2018)
Fragebogen - Anlage 8_TRAIN4Positivity_Ambulantes Monitoring (Stressormonitoring) über movisens_Version_1.pdf (hinzugefügt 22.01.2018)
sonstiges Patientenmaterial - Anlage 9_Smartphone Empfangsbestätigung_Version_1.pdf (hinzugefügt 22.01.2018)
sonstiges Studienmaterial - Anlage 10_Screening-Checkliste_Version_1.pdf (hinzugefügt 22.01.2018)
Fragebogen - Anlage 6_TRAIN4Positivity_Posttest_Fragebogen_Version_1.pdf (hinzugefügt 22.01.2018)
Fragebogen - Anlage 7_TRAIN4Positivity_Ambulantes Monitoring (EMA) innerhalb der „Breezily"-Applikation_Version_1.pdf (hinzugefügt 22.01.2018)
Fragebogen - Anlage 5_TRAIN4Positivity_Prätest_Fragebogen_Version_1.pdf (hinzugefügt 22.01.2018)

Das Votum ist gültig für folgendes Studienzentrum:

Prof. Dr. Klaus Lieb
Universitätsmedizin der Johannes Gutenberg-Universität Mainz
Klinik für Psychiatrie und Psychotherapie
Untere Zahlbacher Straße 8
55131 Mainz

Appendix G Screening and Data Collection

Appendix G1 Inclusion and Exclusion Criteria

Table G1.1

Detailed Inclusion and Exclusion Criteria of TRAIN4*Positivity*

Inclusion criteria	Exclusion criteria
• Age: 18–30 years • Written consent to participate in the study • Sufficient written and spoken language skills in German • Increased stressor exposure (sum score ≥ 30 stressors in the past week in the MIMIS) • Knowledge/skills about how to use a mobile phone • No physical or cognitive impairments which prevent the use of a mobile phone (e.g., uncorrected or non-correctable visual impairments, motor skills limitations) • The average alcohol consumption is less than 15 standard drinks per week (one standard drink contains 10-12 grams of pure alcohol; e.g., 0.1 litre wine, 0.25 litre beer). • No consumption of illegal drugs	• Current mental disorders (according to GHQ-28 screening and eventually structured clinical interview for mental disorders [SCID-I])[a] • Psychotherapeutic and/or psychiatric treatments in the anamnesis (according to self-report) • Neurological diseases in the anamnesis (according to self-report) • Other serious health problems or current physical burden (according to self-report) • Previous participation in an intervention to foster resilience or a prevention program to promote mental health during the last 3 months • The participant spends more than four days abroad during the study/data collection period (in order to meet technical requirements). • Limited ability to give an informed consent to the study participation

Note. GHQ-28 = General Health Questionnaire-28; SCID-I = Structured Clinical Interview for DSM-IV Axis I Disorders; MIMIS = Mainz Inventory of Microstressors.

[a] At pre- and posttest, the GHQ-28 (see 3.2.7.3) as reliable screening instrument of mental health was used, with the results being analyzed by a trained member of the study team. In case of increased values in the GHQ-28 above the cut-off (≥ 23; Sterling, 2011) that indicated mental health problems (including suicidal tendencies), it had been planned to perform a structured clinical interview for DSM-V (SCID-I), to test for the clinical relevance of mental symptoms. In addition, respective participants would have been contacted immediately by phone by a physician (Prof. Dr. Oliver Tüscher) or a clinical psychologist (Dr. Dipl.-Psych. Andrea Chmitorz), to ask them for their need for psychological support and to refer them, if appropriate, to the psychiatric outpatient department (PIA) at the Department of Psychiatry and Psychotherapy of the UMC Mainz. During the recruitment and the screening of eligibility criteria, none of the participants showed increased values in the suicidality items of the GHQ-28. For *n* = 4, increased GHQ-28 total scores above the cut-off were identified; however, all of these participants had already been diagnozed with a mental disorder at an earlier time point in their life (e.g., childhood), negated the need of psychological support, and/or already received a psychological or psychotherapeutic treatment. Therefore, no SCID-I was conducted and no contact with a physician or clinical psychologist was required.

Table G1.2

Detailed Inclusion and Exclusion Criteria of LifeStress (Control Group)

Inclusion criteria	Exclusion criteria
• Age: 18–30 years • Written consent to participate in the study • Sufficient written and spoken language skills in German • Knowledge/skills about how to use a mobile phone • No physical or cognitive impairments which prevent the use of a mobile phone (e.g., hearing impairment, uncorrected or non-correctable visual impairments, motor skills limitations) • The average alcohol consumption is less than 15 standard drinks per week (one standard drink contains 10-12 grams of pure alcohol; e.g., 0.1 litre wine, 0.25 litre beer). • No severe mental disorder (e.g., schizophrenia) and good mental health (screening questionnaire: GHQ-28 total score < 24) • No consumption of illegal drugs	• Mental or neurological diseases in the anamnesis (according to self-report of potential participants) • Participants in current psychiatric or psychotherapeutic treatment • The participant spends more than four days abroad during the study/data collection period (in order to meet technical requirements). • Users of illegal drugs • Individuals reporting high levels of alcohol consumption (average consumption of standard glasses of alcohol per week > 15) • Limited ability to give an informed consent to the study participation

Note. LifeStress as longitudinal EMA study also described in Chmitorz, Kurth, et al. (2020; study 3); GHQ-28 = General Health Questionnaire-28.

Appendix G2 Screening Checklist

JOHANNES GUTENBERG UNIVERSITÄT MAINZ JG|U VDRZ MAINZ JG|U UNIVERSITÄTSmedizin. MAINZ

Screening-Checkliste

„TRAIN$_4$Positivity-Studie zur Evaluation eines Smartphone-basierten Trainings zum Positivity Bias“

Name:

Geb.:

Einschlusskriterien		Ja	Nein
1.	Alter: 18 – 30 Jahre		
2.	Einwilligung in die Studienteilnahme		
3.	Ausreichende Deutschkenntnisse in Wort und Schrift		
4.	Hohe Stressorexposition (Wert ≥ 30 Stressoren in Fragebogen zur Erfassung des Auftretens und der Art von Mikrostressoren (daily hassles) MIMIS)		
5.	Kenntnisse über die Bedienung eines Smartphones		
6.	Keine körperlichen oder geistigen Einschränkungen, die einer Bedienung des Smartphones entgegenstehen (z. B. unkorrigierte oder nicht korrigierbare visuelle Beeinträchtigungen, feinmotorische Einschränkungen)		
7.	Der durchschnittliche Alkoholkonsum beträgt weniger als 15 Standardgläser pro Woche (ein Standardglas enthält 10-12 Gramm reinen Alkohol; z. B. 0,1 Liter Wein, 0,25 Liter Bier)		
8.	Es werden keine illegalen Drogen konsumiert		

Ausschlusskriterien		Ja	Nein
1.	Aktuelle psychische Erkrankungen (laut Screening-Instrument GHQ-28 und ggf. strukturiertem klinischen Interview für DSM-IV)		
2.	Psychotherapeutische oder psychiatrische Behandlungen in der Anamnese (laut Selbstbericht der potentiellen Probanden)		
3.	Neurologische Erkrankungen in der Anamnese (laut Selbstbericht der potentiellen Probanden)		
4.	Andere schwerwiegende gesundheitliche Probleme oder gegenwärtige starke körperliche Belastungen		
5.	Vorherige Teilnahme (letzte drei Monate) an einer Intervention zur Resilienzförderung oder einem Präventionsprogramm zur Förderung der psychischen Gesundheit		
6.	Der Proband hält sich im Erhebungszeitraum länger als 4 Tage im Ausland auf		
7.	Mangelnde Einwilligungsfähigkeit		

	Ja	Nein
Studienteilnahme		

Ort, Datum

Unterschrift des Verantwortlichen aus dem Studienteam

Version 2 1

➢ **For self-report questionnaires at screening and pretest, see Appendix K2.1 and Appendix K2.2**

Appendix G3 Sociodemographic, Clinical, and Lifestyle Variables at Screening and Pretest

The majority of items used to assess the sociodemographic, clinical, and lifestyle characteristics was based on questionnaires partly developed by the GESIS Leibniz Institute for the Social Sciences (Beckman et al., 2016). In addition, the baseline characterization of TRAIN$_4$Positivity was based on the measures used in the LORA study (ethical approval number: 837.105.16 [10424]).

Table G3.1

Sociodemographic, Clinical, and Lifestyle Data Assessed at Screening/Pretest

Screening
gender
age
country of birth
German native speaker or not
subjective health status compared to peers
medical diagnoses of physical (e.g., diabetes, cancer) or mental disorders (e.g., depression, schizophrenia)
previous/current psychiatric or psychotherapeutic treatment
regular intake of prescription drugs
diseases of the mother/father
limited professional, social, or everyday activities due to health problems
physical pain
consultations of a general practitioner in the past 12 months
inpatient admissions in the past 12 months
visual impairments and visual aids
motoric disabilities
average use of computer/mobile phone/internet per week and per day, respectively
frequency of alcohol intake and average quantity per day
additional questions concerning alcohol intake (e.g., alcohol-related harming of another person)
smoking and smoking behavior
frequency of physical exercise
consumption of illegal drugs
previous participation in a training/workshop/coaching to foster resilience or to promote mental health
Pretest
size of the town where the participants grew up
number of years of childhood/adolescence that were spent in the respective town
current marital status
children yes/no
siblings yes/no
highest educational degree
current employment status and main professional activity
employed or self-employed
number of employment relationships (i.e., dependent employment)
staff responsibility yes/no
average number of working hours per week
shift work yes/no
income (e.g., number of individuals living in the household, number of persons that contribute to the income, average monthly net income)

Appendix G4 Self-Report Questionnaires Posttest

The posttest assessment (see Appendix K2.3) was identical to the pretest assessment except for the following changes. The GHQ-28 and the MIMIS from the screening were included. Compared to the pretest, the SDS-17 and SPF were omitted. The LE Checklist was modified compared to the pretest by not including the option to indicate different ages for the occurrence of a certain macrostressor anymore. In addition, the posttest comprised quantitative and qualitative questions to evaluate the training.

Appendix G5 Study Measures and Characteristics (Psychometric Quality, Example Items, Recoding of Items, Calculation of Total Scores)

Table G5.1

LOT-R

Life Orientation Test-Revised (LOT-R; Glaesmer et al., 2012; Scheier et al., 1994)	
Variable assessed	Dispositonal optimism
Number of items	10 items, filler items 2, 5, 6, 8
Scale	5-point Likert scale from 0 = *strongly disagree* to 4 = *strongly agree*
Subscales	2 subscales (2-dimensional): 1. optimism (items 1, 4, 10) 2. pessimism (items 3, 7, 9)
Recoding of inverse items	Recoding of three pessimism items (3, 7, 9) to aggregate pessimism score with optimism score 0 = 4 1 = 3 2 = 2 3 = 1 4 = 0
Calculation of total score(s)	- Total score: sum score of six items (1, 3, 4, 7, 9, 10) without filler items - Optimism subscale: sum score of items 1, 4, 10 (for current study, only total score and optimism subscale analyzed)
Range	Total score: 0–24 Subscale optimism: 0–12
Interpretation	Higher scores indicate higher optimism
Psychometric quality	For the German LOT-R: - satisfactory internal consistencies (optimism: α = .70; pessimism: α = .74; total score: α = .68) - significant correlations with depression (*r* = –.32), anxiety (*r* = –.22), and life satisfaction (*r* = .45), which provides evidence for the construct validity of the scale (Glaesmer et al., 2012)
Other	Population-based norms of the German LOT-R available (Glaesmer et al., 2012)

Table G5.2

SPF/IRI-S-D

Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie (SPF/IRI-S D; Paulus, 2009, 2016)	
Variable assessed	Empathy, since the participants' RT to positive and negative stimuli in the MB-PBT (e.g., crying child) could be influenced by their level of empathy; items describe (generalized) human characteristics or reactions associated to emotions
Example item	„Ich empfinde warmherzige Gefühle für Leute, denen es weniger gut geht als mir."
Number of items	16 items
Scale	5-point Likert scale from 1 = *never* to 5 = *always*; participants indicate to what extent the respective statement applies to them or not
Subscales	4 subscales: 1. empathy (items 1, 5, 9, 11) 2. fantasy (items 2, 7, 12, 15)

Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie (SPF/IRI-S D; Paulus, 2009, 2016)	
	3. perspective taking (items 4, 10, 14, 16) 4. distress (items 3, 6, 8, 13)
Recoding of inverse items	Not required (no inversed items)
Calculation of total score(s)	Total score: sum score of all items/all 4 subscales (for current study, only total score analyzed)
Range	16–80
Interpretation	Higher scores indicate higher empathy
Psychometric quality	Acceptable reliabilities (α = .75–.79) for four subscales (Paulus, 2017); validity confirmed by positive correlations of the subscales empathy (*r* = .58), fantasy (*r* = .78), and distress (*r* = .31) with respective scales of other empathy scales (Schmitt, 1982; Silbereisen & Schulz, 1977)

Table G5.3

SDS-17

Social Desirability Scale-17 (SDS-17; Stöber, 1999)	
Variable assessed	Social desirability
Example item	"I always admit my mistakes openly and face the potential negative consequences."
Number of items	17 items
Scale	Dichotomous scale: 0 = *false*, 1 = *true*; participants indicate if they agree or disagree with 17 statements
Subscales	No subscales
Recoding of inverse items	Recoding of 7 items (1, 4, 6, 7, 11, 15, 17) 0 = 1 1 = 0
Calculation of total score(s)	Total score: mean score of all items
Range	0–1
Interpretation	Higher scores indicate higher social desirability
Psychometric quality	Satisfactory internal consistency (α = .72–.75) and test-retest reliability (r_{tt} = .82 over four weeks); validity: SDS-17 shown to be associated with other measures of social desirability (e.g., Marlowe-Crowne Scale; *r* = .52–.85; Stöber, 2001)

Table G5.4

ASF-E

Attributionsstilfragebogen für Erwachsene (ASF-E; Poppe et al., 2005); German adaptation of the Attributional Style Questionnaire (Peterson et al., 1982)	
Variable assessed	Inter-individual differences in the AS for positive and negative events
Example item	Participants are asked to indicate the major cause they feel about a situation as if it happened to themselves using an open-ended format (e.g., positive: "You apply for a position that you want very badly (e.g., important job, graduate school admission) and you get it."; negative: "You meet a friend who acts hostilely toward you."). Subsequently, they are asked to rate this major cause along three dimensions (internal–external, stable–variable, and global–specific) based on 6 items
Number of items	96 items (16 situations; eight positive, eight negative); 6 items for each situation, respectively Positive situations: 1, 2, 5, 8, 10, 12, 14, 16

Attributionsstilfragebogen für Erwachsene (ASF-E; Poppe et al., 2005); German adaptation of the Attributional Style Questionnaire (Peterson et al., 1982)	
	Negative situations: 3, 4, 6, 7, 9, 11, 13, 15
Scale	7-point bipolar scale with varying anchors - internality–externality: 1 = *is totally due to other people or circumstances* versus 7 = *is totally due to me* - stability–variability: 1 = *will change over time* versus 7 = *will be stable over time* - globality–specificity: 1 = *only affects the current situation* versus 7 = *affects many other situations concerning me in a positive/negative way*
Subscales	Each subscale and the total score are calculated separately for positive and negative situations - internality, stability, globality, generality (stability + globality)
Recoding of inverse items	Not required (no inversed items)
Calculation of total score(s)	- Positive situations: total score as sum score across all positive situations for the dimensions internality, stability, globality - Negative situations: total score as sum score across all negative situations for the dimensions internality, stability, globality (for current study, only total scores ASF-E-P and ASF-E-N analyzed)
Range	ASF-E-P: 48–336 ASF-E-N: 48–336
Interpretation	- ASF-E-P: Higher scores indicate internal, stable, and global attributions for positive situations - ASF-E-N: Higher scores indicate internal, stable, and global attributions for negative situations - Based on two total scores, a positive AS is indicated by a high total score for positive events and a low total score for negative events
Psychometric quality	Poppe et al. (2005) found acceptable to high internal consistency for the total scores (positive: α = .88; negative: α = .89) and for the three dimensions (internality: α = .73–.74; stability: α = .76–.80; globality: α = .84–.91); significant associations between the AS total scores and depression (positive: *r* = –.15; negative: *r* = .22)

Table G5.5

LE Checklist

Life Events Checklist (LE Checklist; Chmitorz, Neumann, et al., 2020)	
Variable assessed	Stressor exposure macrostressors/life events that can be positive (e.g., marriage) or negative (e.g., job loss)
Number of items	27 items (based on life history calendar of Caspi et al. (1996) and standard life events checklist from Canli et al. (2006), the items of this scale were modified and extended at the Leibniz Institute for Resilience Research)
Scale	1. Number of occurred macrostressors (life events) o *Pretest*: lifespan: maximum of five age indications (age 1, age 2, age 3, age 4, age 5) at which a macrostressor can have occurred; for each stressor: never (0), 1 age indicated (1), 2 ages indicated (2), 3 ages indicated (3), 4 ages indicated (4), 5 ages indicated (5); "no statement" possible if participants refuse to give information about a stressor o *Posttest:* past three weeks (no possibility of multiple answers); 0 = *did not occur*, 1 = *did occur*; "no statement" possible if participants refuse to give information about a stressor 2. Perceived severity of occurred macrostressors[a]: 5-point Likert scale from 0 = *not at all severe* to 4 = *very severe*; not relevant for this

Life Events Checklist (LE Checklist; Chmitorz, Neumann, et al., 2020)	
	study
Subscales	1. Number of occurred macrostressors 2. Perceived severity of macrostressors
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	Number of occurred macrostressors: sum score (pretest: based on age indications) Perceived severity of macrostressors: sum score (for current study, only total number of macrostressors analyzed)
Range	*Pretest:* - Number of occurred macrostressors: 0–135 - [Perceived severity of macrostressors: 0–540] *Posttest* (modified scale to assess only macrostressors occurred during three-week training period): - Number of occurred macrostressors: 0–27 - [Perceived severity of macrostressors: 0–108]
Interpretation	- Number of macrostressors: Higher values indicate a larger number of occurred macrostressors - [Perceived severity of macrostressors: Higher values indicate a higher perceived severity of occurred macrostressors]

Note. [a] For example, an individual may have perceived a job loss at the age of 25 as not burdensome, whereas the same LE at the age of 40 years might have been rated as very stressful.

Table G5.6

MIMIS

Mainz Inventory of Microstressors (MIMIS) & EMA[a] version of MIMIS (Chmitorz, Kurth, et al., 2020)	
Variable assessed	- Number of microstressors at pre- and posttest as component of the participants' stressor exposure - Perceived microstressor severity
Example items	e.g., noise, traffic jam, disagreements
Number of items	61 items: - 58 fixed stressors (daily hassles) - Item 59 to ask for potential additional stressors occurred - Possibility to indicate up to five additional stressors (items 60a-e) in open questions - Item 61a to ask for special burden/stressors in the past seven days - Item 61b to ask whether the past seven days were extraordinary (e.g., especially happy/burdensome) (for the analyses in this study only 58 fixed items/stressors relevant) - <u>*EMA version:*</u> participants can select between 58 stressors in "movisensXS" (multiple answers are possible)
Scale	- Number of occurred microstressors (daily hassles) in past seven days: o <u>*Paper-pencil questionnaire:*</u> 7-point scale from 1 = *1 day* to 7 = *7 days* o <u>*EMA version:*</u> 0 = *did not occur;* 1 = *did occur* - Perceived severity of occurred microstressors: 5-point scale from 0 = *not at all severe* to 4 = *very severe*
Subscales	- Number of occurred microstressors during past seven days - Perceived severity of occurred microstressors
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	- Number of occurred microstressors: sum score of number of days each stressor occurred across items/stressors 1–58 (*Paper-pencil*

Mainz Inventory of Microstressors (MIMIS) & EMA[a] version of MIMIS (Chmitorz, Kurth, et al., 2020)	
	questionnaire); EMA version: sum score of occurrences - Perceived severity of microstressors: sum score across items/stressors 1–58
Range	- Number of occurred microstressors: ○ *Paper-pencil questionnaire:* 0–406 ○ *EMA version:* 0–58 - Perceived severity by microstressors: 0–232[b]
Interpretation	- Number of microstressors: Higher values indicate a higher number of occurred microstressors - Perceived severity of microstressors: Higher values indicate a higher perceived severity of occurred microstressors

Note. [a] administered by mobile app "movisensXS"; every Sunday at 11 a.m. and 2 p.m., participants were reminded by "movisensXS" to complete this end-of week assessment. [b] Daily hassles, that were assessed as non-stressing (0) or did not occur at all, were not considered for the sum variable "perceived microstressor severity". On the other hand, higher severity ratings for single stressors and a higher count of microstressors resulted in higher values of this score, which could range between 0 (no stressor occurred or was rated as severe) and a maximum of 232 (each of the 58 stressors occurred and was rated as 4 = *very severe*).

Table G5.7

PSS-10

Perceived Stress Scale-10 (PSS-10; Cohen et al., 1983; Klein et al., 2016)	
Variable assessed	Subjective perception of stress, that is, the extent to which situations in the participants' life were perceived as stressful in the past month
Example item	"In the past month, how often have you felt that you were unable to control the important things in your life?"
Number of items	10 items
Scale	5-point Likert scale from 0 = *never* to 4 = *very often*
Subscales	2 subscales: 1. perceived helplessness 2. perceived self-efficacy
Recoding of inversed items	Recoding of positively worded items (4, 5, 7, 8) 4 = 0 3 = 1 2 = 2 1 = 3 0 = 4
Calculation of total score(s)	Total score: sum score of all items (for current study, only total score analyzed)
Range	0–40
Interpretation	Higher values indicate higher perceived stress
Psychometric quality	For the German adaptation of the PSS-10 (Klein et al., 2016), a good reliability (α = .84) and significant correlations with depression (r = .59), anxiety (r = .59), and life satisfaction (r = –.47) were shown, all indicating good construct validity

Table G5.8

GHQ-28

General Health Questionnaire-28 (GHQ-28; Goldberg & Hillier, 1979; Klaiberg et al., 2004)	
Variable assessed	Mental health (self-report screening); based on an outcome definition of resilience (i.e., change in mental health controlled for individual stressor exposure), the GHQ-28 served to operationalize resilience in this study; for this purpose, the stressor data assessed with the LE Checklist (pre-/posttest), the MIMIS (pre-/posttest), and the end-of-week assessments using the MIMIS EMA version were considered when analyzing the GHQ-28
Number of items	28 items
Scale	4-point Likert scale with varying anchors (e.g., 0 = *not at all* to 3 = *much more than usual*)
Subscales	4 subscales (dimensions): 1. somatic symptoms 2. anxiety and insomnia 3. social dysfunction 4. severe depression
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	Total score: sum score of all items (for current study, only total score analyzed)
Range	0–84 (cut-off for clinical caseness: 23; Sterling, 2011)
Interpretation	Lower values indicate better mental health (i.e., lower psychological distress)
Psychometric quality	For the German adaptation (Klaiberg et al., 2004), a very good reliability for the total scale (α = .92) and satisfactory to good internal consistencies for the subscales (α = .79–.86) found

Table G5.9

BRS

Brief Resilience Scale (BRS; Chmitorz, Wenzel, et al., 2018; Smith et al., 2008)	
Variable assessed	Ability to recover from stress
Example item	"I tend to bounce back quickly after hard times."
Number of items	6 items
Scale	5-point Likert scale from 1 = *strongly disagree* to 5 = *strongly agree*
Subscales	No subscales
Recoding of inversed items	Recoding of three negatively phrased items (2, 4, 6) 1 = 5 2 = 4 3 = 3 4 = 2 5 = 1
Calculation of total score(s)	Total score: mean score of all items
Range	1–5
Interpretation	Higher scores indicate a higher ability to recover from stress
Psychometric quality	The psychometric properties of the German translation have been demonstrated (Chmitorz, Wenzel, et al., 2018; Kunzler, Chmitorz, et al., 2018), including fair to good convergent and discriminant validity concerning perceived stress (r = –.53) and resilience factors (e.g., optimism and self-efficacy: r = .51) as well as good internal consistency (α = .85)
Othere	Population-based norms for the German BRS available (Kunzler, Chmitorz, et al., 2018)

Table G5.10

PANAS

Positive and Negative Affect Schedule (PANAS; Krohne et al., 1996; Watson et al., 1988)	
Variable assessed	Positive and negative affect (mood)
Example item	Positive affect: e.g., active, interested Negative affect: e.g., nervous, afraid For each adjective, participants indicate the extent to which they experienced the respective mood state.
Number of items	20 items (20 adjectives)
Scale	"How do you feel at the moment" instruction (pre- and posttest) with 5-point scale from 1 = *not at all* to 5 = *very much*
Subscales	2 subscales: 1. positive affect (PA) 2. negative affect (NA)
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	- Positive affect: sum score of 10 items - Negative affect: sum score of 10 items
Range	10–50 (for each subscale)
Interpretation	- Positive affect: Higher values indicate higher positive affect - Negative affect: Higher values indicate higher negative affect
Psychometric quality	In the German adaptation (Krohne et al., 1996), both scales were shown to be internally consistent (PA: α = .85; NA: α = .86), and relatively stable (PA/NA: r_{tt} = .19; one week)

Table G5.11

WHO-5

Well-being Index (WHO-5; Bech, 2004)	
Variable assessed	Well-being over past two weeks
Example item	"I have felt active and vigorous."
Number of items	5 (positively phrased) items
Scale	6-point Likert scale from 0 = *at no time* to 5 = *all of the time*
Subscales	No subscales
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	Total score: mean score of all items
Range	1–100 (after linearly transformed index scores)
Interpretation	Higher scores indicate higher well-being
Psychometric quality	Validated with respect to both clinical and psychometric validity (Bech, 2012); for the German version (Brähler et al., 2007), excellent psychometric quality demonstrated, such as very good reliability (α = .92) and validity (e.g., *r* = .60 with generic quality of life; *r* = .56–.65 with other scales for subjective well-being; *r* = –.53 with physical or psychosocial symptoms)
Other	Brähler et al. (2007) provided gender and age-specific norm values for Germany

Table G5.12

STAXI

State-Trait Anger Expression Inventory (STAXI; Schwenkmezger et al., 1992; Spielberger, 1988)	
Variable assessed	State anger and different forms (three dispositions) of anger expression (anger in: feelings of anger are suppressed and held in; anger out: feelings of anger are expressed toward people/objects by attacking them physically or verbally; anger control: individual tries to control the anger and to react in a socially desirable way)
Number of items	44 items
Scale	4-point Likert scale from 1 = *not at all* to 4 = *very much*
Subscales	5 subscales: 1. state anger (10 items) 2. trait anger (10 items; not relevant for this study) 3. anger expression – anger in (8 items) 4. anger expression – anger out (8 items) 5. anger expression – anger control (8 items)
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	Separate scores for each subscale: sum scores of respective items (for current study, only state anger, anger in, anger out, anger control analyzed)
Range	- state anger: 10–40 - anger in: 8–32 - anger out: 8–32 - anger control: 8–32
Interpretation	Higher scores on each subscale indicate higher state anger, anger in, anger out and anger control
Psychometric quality	Internal consistencies of the German STAXI were in an acceptable to very high range (α = .71–.95). The factorial validity of the STAXI was demonstrated (Schwenkmezger et al., 1992).
Other	Norm values available (Schwenkmezger et al., 1992)

Table G5.13

ERQ

Emotion Regulation Questionnaire (ERQ) – subscale reappraisal (Abler & Kessler, 2009; Gross & John, 2003)	
Variable assessed	ER strategy reappraisal
Example item	"I control my emotions by changing the way I think about the situation I'm in."
Number of items	6 items of the subscale reappraisal used
Scale	7-point Likert scale from 1 = *strongly disagree*, 4 = *neutral* to 7 = *strongly agree*
Subscales	Only subscale reappraisal used in this study (subscale suppression not distributed to participants)
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	Total score for subscale reappraisal: mean score of all items
Range	1–7
Interpretation	Higher values indicate higher reappraisal
Psychometric quality	Acceptable internal consistencies were found for the German ERQ (α = .74).

Table G5.14

CERQ

Cognitive Emotion Regulation Questionnaire (CERQ; German and abbreviated version; Garnefski et al., 2002; Loch et al., 2011)	
Variable assessed	Use of cognitive ER
Number of items	27 items + 2 additional items (not analyzed) Additional items: - „Ich versuche, die Situation aus einer losgelösten Perspektive, wie von außen, zu betrachten." - „Ich versuche, mich von der Situation und meinen Gefühlen zu distanzieren."
Scale	5-point Likert scale from 1 = *(almost) never* to 5 = *(almost) always*
Subscales	9 subscales (each 3 items) for 9 dimensions of emotion regulation (ER): - adaptive ER: positive reappraisal, positive refocusing, putting into perspective, refocusing on planning, acceptance - maladaptive ER: catastrophizing, self-blame, rumination, blaming others
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	Total scores for each subscale: sum score of 3 corresponding items → positive reappraisal: items 4, 13, 22 (for current study, only total score for positive reappraisal analyzed)
Range	3–15 (positive reappraisal)
Interpretation	Higher scores indicate higher positive reappraisal
Psychometric quality	An acceptable reliability (α = .78) for this subscale was demonstrated (Loch et al., 2011). Positive reappraisal was also moderately correlated with depressive symptoms (r = –.34) and anxiety sensitivity (r = –.26– –.33), supporting the construct validity.

Table G5.15

EMA Psychological Measures ("Breezly")

EMA psychological measures ("Breezly" evening rating)	
Variable assessed	Well-being, ability to get easily affected by positive stimuli, ability to distance from negative stimuli (EMA measures in the morning [sleep restfulness, vigor, well-being] not relevant for this study)
Number of items	3 (self-developed) items
Scale	Bipolar scale (0–100) with varying anchors - well-being: 0 = *not well at all* to 100 = *extremely well* - ability to get affected by positive stimuli: 0 = *little or not at all* to 100 = *very well* - ability to distance from negative stimuli: 0 = *little or not at all* to 100 = *very well*
Subscales (single items)	1. Well-being 2. Ability to get affected by positive stimuli 3. Ability to distance from negative stimuli
Recoding of inversed items	Not required (no inversed items)
Calculation of total score(s)	No total scores calculated; single item analysis
Range	0–100
Interpretation	Higher values indicate higher well-being, higher ability to get affected by positive stimuli, and higher ability to distance from negative things, respectively

Table G5.16

EMA Psychological Measures (Current Mood) Integrated Into "Breezly"

EMA psychological measures (current mood) with EMA version (Wilhelm & Schoebi, 2007) of Multidimensional Mood Questionnaire (MDMQ; Steyer et al., 1997)	
Variable assessed	Current mood with three dimensions positive valence (V), energetic arousal (E), and calmness (C)
Example item	"At the moment I feel…"
Number of items	6 items
Scale	Bipolar items (modified from original scale to 0–100) - tired/awake (E+) - content/discontent (V-) - agitated/calm (C+) - full of energy/without energy (E-) - unwell/well (V+) - relaxed/tense (C-)
Subscales	3 subscales (dimensions; two items each): 1. calmness (agitated/calm; relaxed/tense) 2. valence (content/discontent; unwell/well) 3. energetic arousal (tired/awake; full of energy/without energy)
Recoding of inversed items	Recoding of three negatively phrased items (V-, E-, C-) 0 = 100 1 = 99 … 100 = 0
Calculation of total score(s)	Mean scores for positive valence, energetic arousal, and calmness (Forster et al., 2020)
Range	0–100 (modified from original version with endpoints 0–6)
Interpretation	Higher scores indicate higher values in the respective mood dimension (i.e., higher positive valence, higher energetic arousal, higher calmness)
Psychometric quality	A high (average) internal consistency across observations in the EMA version was found for all dimensions (α = .90–.92) at the between-person level. Within-person reliabilities were also sufficient (α = .70–.77)

Table G5.17

AAT

Modified Approach Avoidance Task (AAT; Becker et al., 2016; Wiers et al., 2011)	
Variable assessed	Implicit action tendencies to affective stimuli
Laboratory paradigm	Software millisecond Inquisit 5 Lab (Borchert, 2012) on study netbook with USB mouse
Variables	Reaction times for 52 trials: - 13 trials: pull positive pictures - 13 trials: push positive pictures - 13 trials: pull negative pictures - 13 trials: push negative pictures

Modified Approach Avoidance Task (AAT; Becker et al., 2016; Wiers et al., 2011)	
Analysis	Based on Becker et al. (2016) and Becker et al. (2019): - Exlusion of RTs of incorrect reactions (i.e., pulling of landscape format, pushing of portrait format in contrast to AAT instructions) - Exlusion of 1% slowest and 1% fastest RTs - Calculation of median RTs: o median RT of 13 trials pull-positive o median RT of 13 trials push-positive o median RT of 13 trials pull-negative o median RT of 13 trials push-negative - Calculation of median RTs for compatible and incompatible trials: - Compatible trials: median RT 13 trials pull-positive + median RT push-negative - Incompatible trials: median RT 13 trials push-positive + median RT pull-negative - Calculation of Compatibility Score (AAT-CS): median RT incompatible trials – median RT compatible trials
Interpretation	Compatibility Score > 0: positivity bias Compatibility Score = 0: no bias Compatibility Score < 0: negativity bias

Note. For details, see also Appendix G10.

Appendix G6

Reliabilities (Internal Consistencies Cronbach's α) for Scales at Pre- and Posttest

Scale	Reliabilities (α)			
	Pretest	Judgement	Posttest	Judgement
Baseline variables & covariates				
LOT-R total	.78	acceptable	.76	acceptable
LOT-R optimism	.81	good	.74	acceptable
SPF	.82	good	/	/
SDS-17	.76	acceptable	/	/
ASF-E-P	.90	excellent	.92	excellent
ASF-E-N	.90	excellent	.94	excellent
MIMIS	/		/	
MIMIS EMA	/		/	
LE Checklist	/		/	
Outcomes				
PSS-10	.80	good	.71	acceptable
GHQ-28 total	.67	questionable	.75	acceptable
BRS	.75	acceptable	.66	questionable
WHO-5	.75	acceptable	.82	good
ERQ (reappraisal)	.83	good	.79	acceptable
CERQ (positive reappraisal)	.74	acceptable	.80	good
PANAS positive	.85	good	.88	good
PANAS negative	.76	acceptable	.67	questionable
STAXI state anger	.73	acceptable	.74	acceptable
STAXI anger in	.84	good	.78	acceptable
STAXI anger out	.11	unacceptable	.78	acceptable
STAXI anger control	.70	acceptable	.58	poor

Note. No reliability measures for MIMIS, MIMIS EMA and LE Checklist available; judgement based on conventions to interpret Cronbach's α (George & Mallery, 2003).

LOT-R = Life Orientation Test-Revised; SPF = Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie; SDS-17 = Social Desirability Scale-17; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; MIMIS = Mainz Inventory of Microstressors; MIMIS EMA = EMA assessment using MIMIS; LE Checklist = Life Events Checklist; PSS-10 = Perceived Stress Scale-10; GHQ-28 = General Health Questionnaire-28; BRS = Brief Resilience Scale; WHO-5 = Well-being Index; ERQ = Emotion Regulation Questionnaire; CERQ = Cognitive Emotion Regulation Questionnaire; PANAS = Positive and Negative Affect Schedule; STAXI = State-Trait Anger Expression Inventory.

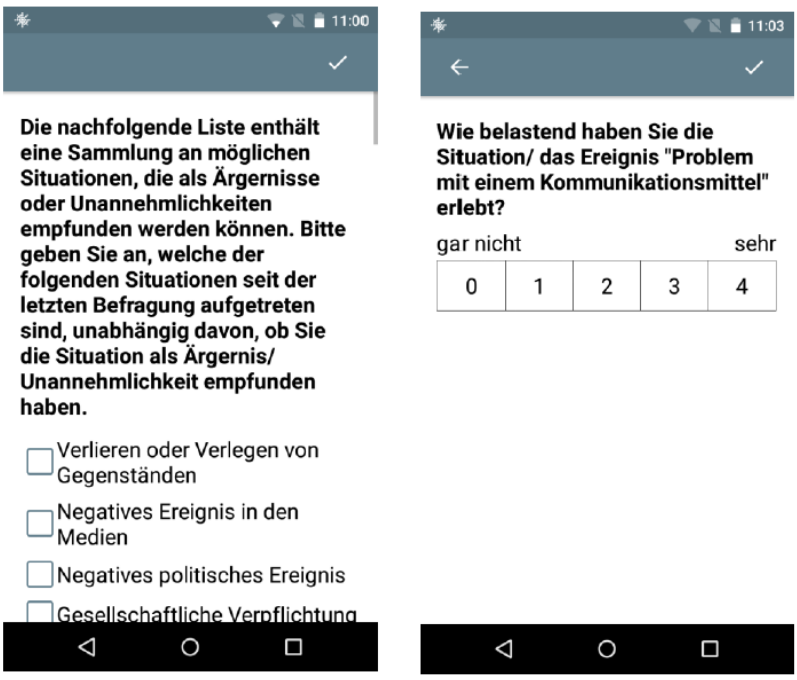
Appendix G7 EMA Stressor Monitoring Using Smartphone App “movisensXS”

Figure G7.1

Alarm of “movisens XS” to Complete the Assessment

Figure G7.2

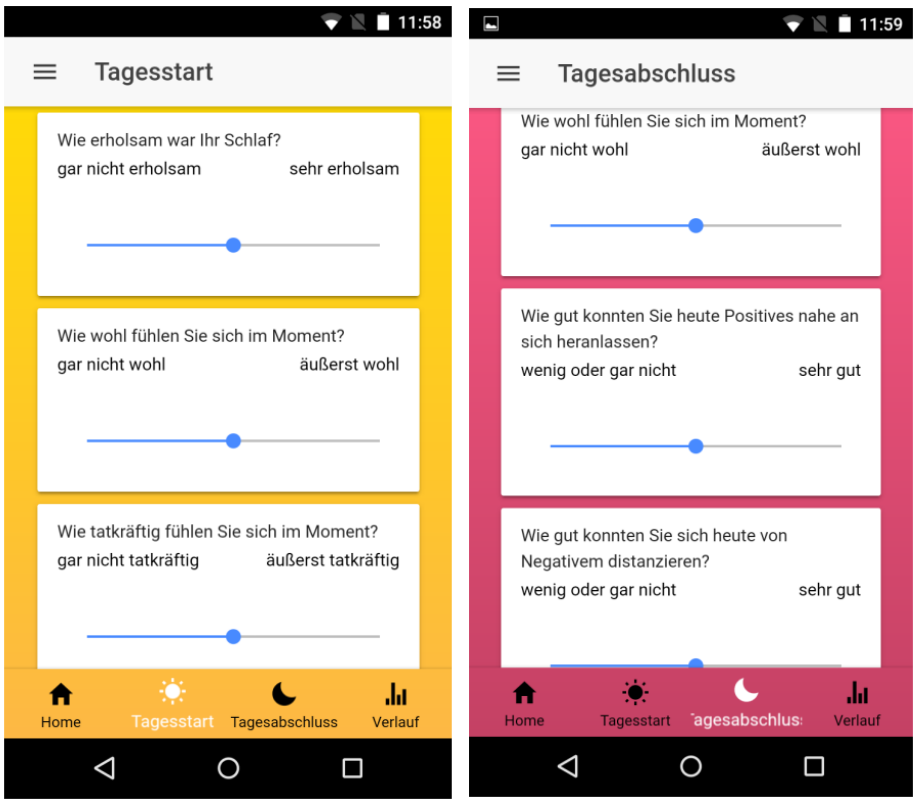
Stressor Monitoring in “movisensXS” Using MIMIS EMA Version



Appendix G8 EMA of Psychological Measures in “Breezly”

Figure G8.1

EMA Assessment (Start and End of Day) in “Breezly” (Scale 0–100)



Note. left: assessment of sleep restfulness, well-being, and vigor for the upcoming day (“Tagesstart”); right: assessment of well-being, the ability to get affected by positive stimuli, and the ability to distance from negative stimuli (“Tagesabschluss”).

Appendix G9 Picture Selection for Assessment Version of Approach Avoidance Task (AAT)

For the outcome assessment with the AAT, two categories of images, each with 13 pictures, were used: pictures with positive and those with negative valence (*N* = 26 in total). The two picture sets were extracted from three (validated) sources: 1) the database International Affective Picture System (IAPS; Lang et al., 2008), 2) the Emotional Picture Set (EmoPicS; Wessa et al., 2010), and 3) a database generated for a fMRI study (pictures collected from Flickr) that was used to investigate implicit approach and avoidance tendencies in healthy individuals at the JGU Mainz (Ascheid et al., 2019). The pictures for the assessment were compiled based on norm values that had been found for the three databases concerning valence and arousal using the 9-level Self-Assessment Manikin (SAM) (IAPS: *N* = 1000; EmoPics: *N* = 90; fMRT study: *N* = 100–107 as the database consists of four picture sets that were validated using four different samples). Positive pictures had received a valence rating ≥ 6 in the norm samples, while negative pictures were identified based on valence ratings of ≤ 4. For the selection of the *N* = 26 pictures, the following criteria were used: 1) pictures that elicited an arousal level ≤ 8 in the SAM in the respective norm sample, and 2) pictures for which no significant gender differences in valence and arousal were found. Potential gender differences were tested using R (R Core Team, 2019) based on the SAM values in the respective norm sample ($\alpha = .01$). In addition, pictures whose motives (despite arousal ≤ 8) were judged to be inadequate by two independent raters (A. Kunzler, A. Krayer), such as erotic contents or physical injuries, were excluded.

As the IAPS database had already been generated in 2005, some additional pictures were excluded if they were assessed as not up to date (e.g., outdated clothing or technology) or if the picture quality was assessed as insufficient (e.g., pixelated) by two independent raters (A. Kunzler, A. Krayer). Pictures of EmoPicS and of the database of fMRT pictures that had been used in the MB-PBT "Breezly", were *not* used for the assessment. As for the training stimuli (see Appendix H1), pairs of suitable pictures were matched concerning arousal (i.e., difference in arousal ≤ 2 points on the SAM) resulting in 13 pairs of pictures that consisted of a positive and a negative image, respectively.

Appendix G10 Details on Calculation of AAT Compatibility Score (AAT-CS) at Pre- and Posttest

- Entry of raw data (generated by Inquisit 5 Lab) for all participants (*N* = 41) and (simultaneously) testing whether participants showed correct reactions to pictures of different formats (i.e., pull portrait, push landscape)
- Exclusion of two participants (TP003JEF, TP021SOF) as instructions were misunderstood (i.e., after correct practice trials, participants started to push portraits and to pull landscape formats) → *n* = 39
- Identification (performed manually) of 1% slowest and 1% fastest RTs for the *n* = 39 participants, and exclusion of respective values
- Calculation of median RTs in the four possible conditions (1. pull positive, 2. push positive, 3. pull negative, 4. push negative)
- Calculation of median RTs (sum) for compatible and incompatible trials
- Calculation of the AAT-CS (new variable) based on the difference between median RTs in incompatible and compatible trials

The methods to calculate the AAT-CS were based on Becker et al. (2016) and Becker et al. (2019). A separate SPSS data set was created including 52 columns (13x pull-positive: 8/p, 13x push-positive: 8/l, 13x pull-negative: 7/p, 13x push-negative: 7/l) for individual RTs (values.completeRT). In the original Inquisit data set, “values.targetcategory” indicates whether a positive (8) or negative picture (7) was shown. The score “values.targetformat” indicates whether a portrait (p) or landscape (l) format was presented (i.e., whether participants had to push or pull). The order of trials in the AAT varies (i.e., in trial 1, participant 1 eventually has to pull a positive picture, while participant 2 has to pull a negative one). Therefore, when entering the data in SPSS, the first trial, when participants had to pull a positive picture, was entered first, followed by the second trial, where a positive picture had to be pulled, and so on.

In the AAT studies by Becker et al. (2016) and Becker et al. (2019) using the joystick version, a picture did not disappear if participants moved the joystick into the incorrect direction (i.e., 100%

response accuracy for all trials). As this was not the case in the paradigm of this study, the RTs of false reactions to the two picture formats were excluded. To decide about correct versus incorrect reactions, several indicators are available in the AAT. The score “values.correct” was used, which indicates whether an individual responded correctly to a picture from the beginning or started to move the mouse (mouse version in TRAIN$_4$Positivity) in the wrong direction first (1: correct response from the beginning, 0: mouse is initially moved in the wrong direction, then the reaction is corrected). However, to exclude all RTs with a value of 0 in this variable would be rather conservative. Therefore, due to the pilot character of this study, the score “values.finalresponse” was used which indicates whether the *final* movement is consistent to the picture format, as instructed. RTs of incorrect reactions/movements were excluded.

For each of the 52 columns (i.e., RTs in each of the 52 trials), the slowest 1% and fastest 1% were identified. Subsequently, four new variables for the median RTs were calculated for each participant:

a. median RT in 13 trials positive-pull
b. median RT in 13 trials positive-push
c. median RT in 13 trials negative-pull
d. median RT in 13 trials negative-push

Based on these four scores, two new variables for compatible (median RT positive-pull + median RT negative-push) and incompatible trials (median RT positive-push + negative-pull) were calculated. Using these two variables, the AAT-CS was calculated (median RT incompatible trials – median RT compatible trials).

Appendix H Mobile-Based Positivity Training ("Breezly")

Appendix H1 Picture Selection for "Breezly"

To not only use disorder-specific stimuli – as opposed to many previous CBM studies – the (preselected) set of *N* = 170 pictures, that had to be assessed by each participant at the start of the positivity training, was compiled of different semantic categories (e.g., social situations, animals, plants). This preselection was extracted from two picture databases for psychological and neuropsychological research (see also Appendix G9), the EmoPicS (Wessa et al., 2010) and a database generated for a fMRI study (pictures collected from Flickr) that was used to investigate implicit approach and avoidance tendencies in healthy individuals (Ascheid et al., 2019). The pictures were also compiled based on the SAM values for valence and arousal that had been identified for each stimulus in the norm samples for the two databases (EmoPicS: *N* = 90; fMRI study: *N* = 101–107; see Appendix G9). Positive pictures had received a valence rating ≥ 6 in the norm samples, while negative pictures were identified based on valence ratings of ≤ 4.

For the selection of the *N* = 170 pictures, the same criteria as for the assessment stimuli were used (compare Appendix G9): 1) pictures that elicited an arousal level ≤ 8 in the SAM in the respective norm sample, and 2) pictures for which no significant gender differences in valence and arousal were found. Again, potential gender differences were tested using R (R Core Team, 2019) based on the SAM values in the respective norm sample ($\alpha = .01$). In addition, pictures that (despite arousal ≤ 8) were assessed as inadequate by two independent raters (A. Kunzler, A. Krayer), such as erotic contents or physical injuries, were excluded. These criteria resulted in *n* = 85 positive (valence ≥ 6) and *n* = 85 negative pictures (valence ≤ 4) that were matched with respect to arousal (i.e., difference in arousal ≤ 2 points on the SAM between the positive and negative picture), respectively.

Appendix H2 “Breezly”– First Use and Picture Rating

Access to the training was only possible by *an individual access code* that was communicated to the subjects at the laboratory appointment. “Breezly” was developed in cooperation with Prof. Dr. Dirk Lehr of the Leuphana University Lüneburg. The content-related and technical conception of the application were performed by Dipl.-Psych. Angela Kunzler, Ing. Stefanie Grimm, M. Sc., and Prof. Dr. Dirk Lehr.

To individualize the intervention, study participants had to create their personal profile in “Breezly” before being able to start a training session. After starting the app by entering the access code (Figure H2.1), participants were required to assess the preselection of *N* = 170 pictures (Figures H2.2 and H2.3).

Figure H2.1

Entering Access Code in “Breezly”

Figure H2.2

Start Screen When Using "Breezly" for the First Time

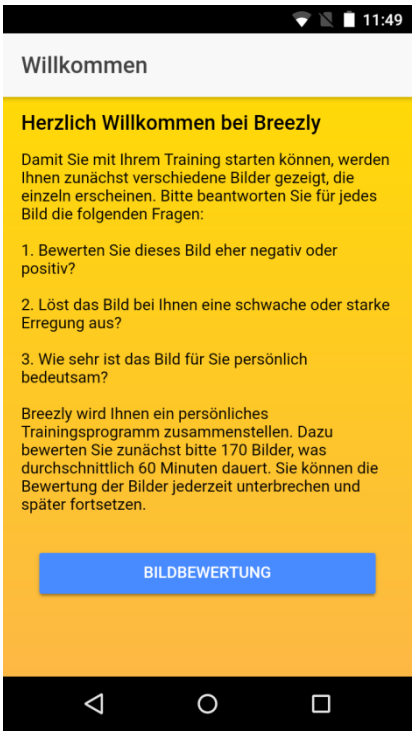
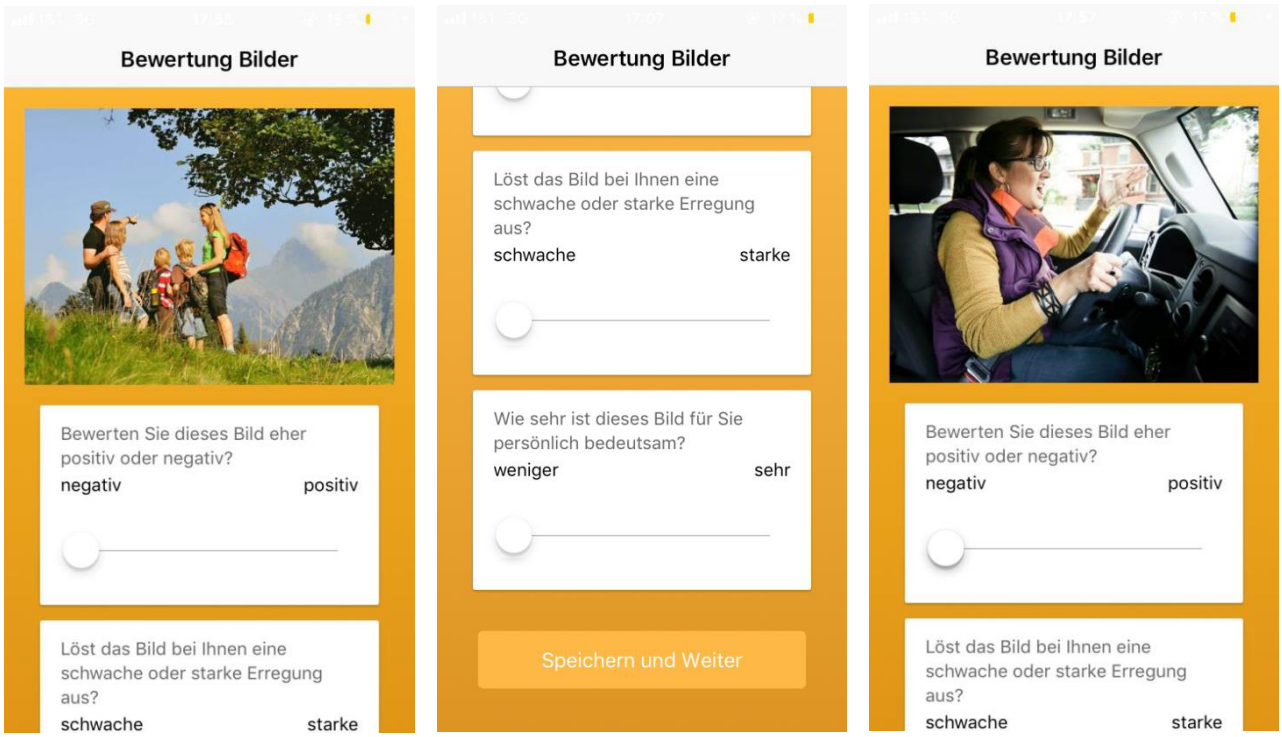


Figure H2.3

Individual Rating of Each Picture Based on Three Characteristics (Valence, Arousal, Personal Relevance) on a 9-Level Scale

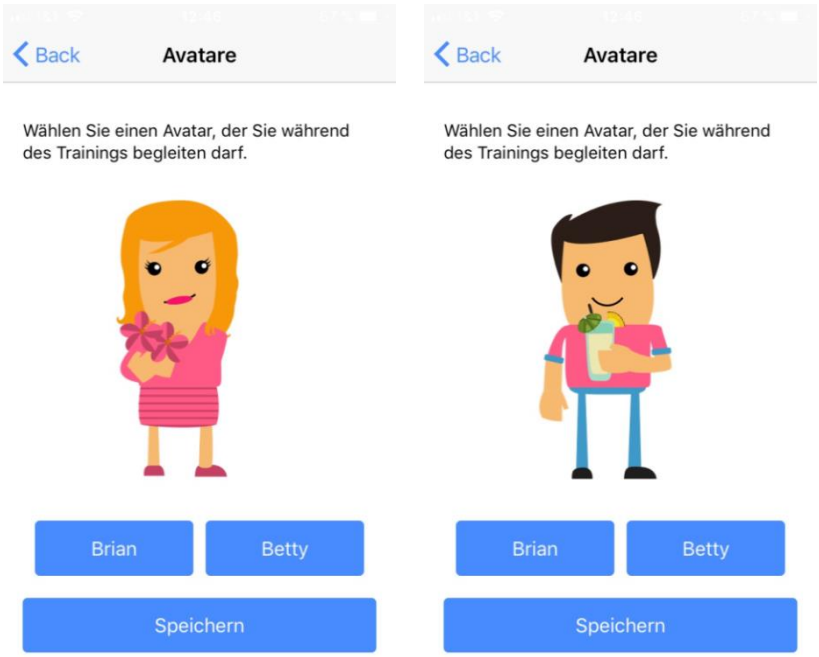


Appendix H3 "Breezly" – Selection of Individual Avatar

After the picture rating and the automatic selection of an individual training set generated by the "Breezly" algorithm, participants were invited to choose their personal male ("Brian") or female ("Betty") avatar that led them through the training (Figure H3.1). This avatar presented the frequency of completed training sessions and the number of points collected to track one's personal training progress.

Figure H3.1

Selection of Personal Avatar for the 3-Week Training

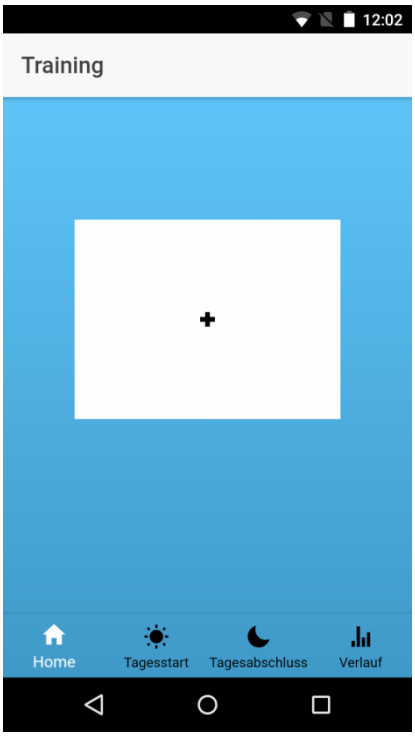


Appendix H4 "Breezly" – Training Session Including EMA Assessment of Current Mood

The course of one training session in "Breezly" (each session approx. 3 minutes) was based on the principles of AAT (Becker et al., 2016; Becker et al., 2019; Wiers et al., 2011). First, subjects were instructed to focus their attention on a fixation cross (Figure H4.1) in the center of the screen.

Figure H4.1

Fixation Cross Before the Presentation of Each Picture



Following the fixation cross, each picture was presented in an identical format in the middle of the screen and participants had to perform swipe movements depending on the valence of the respective picture (swipe or pull closer positive pictures; Figure H4.2; swipe away or push negative pictures, Figure H4.3), resulting in the typical zooming effect. At the end of each training session, the participants' current mood was measured using the EMA version of the MDMQ (Figure H4.4).

Figure H4.2

Presentation of a Positive Picture (i.e., Positive for the Respective Participant) in a Training Session and Increase of Picture Size by Swiping/Pulling Closer the Picture on the Display

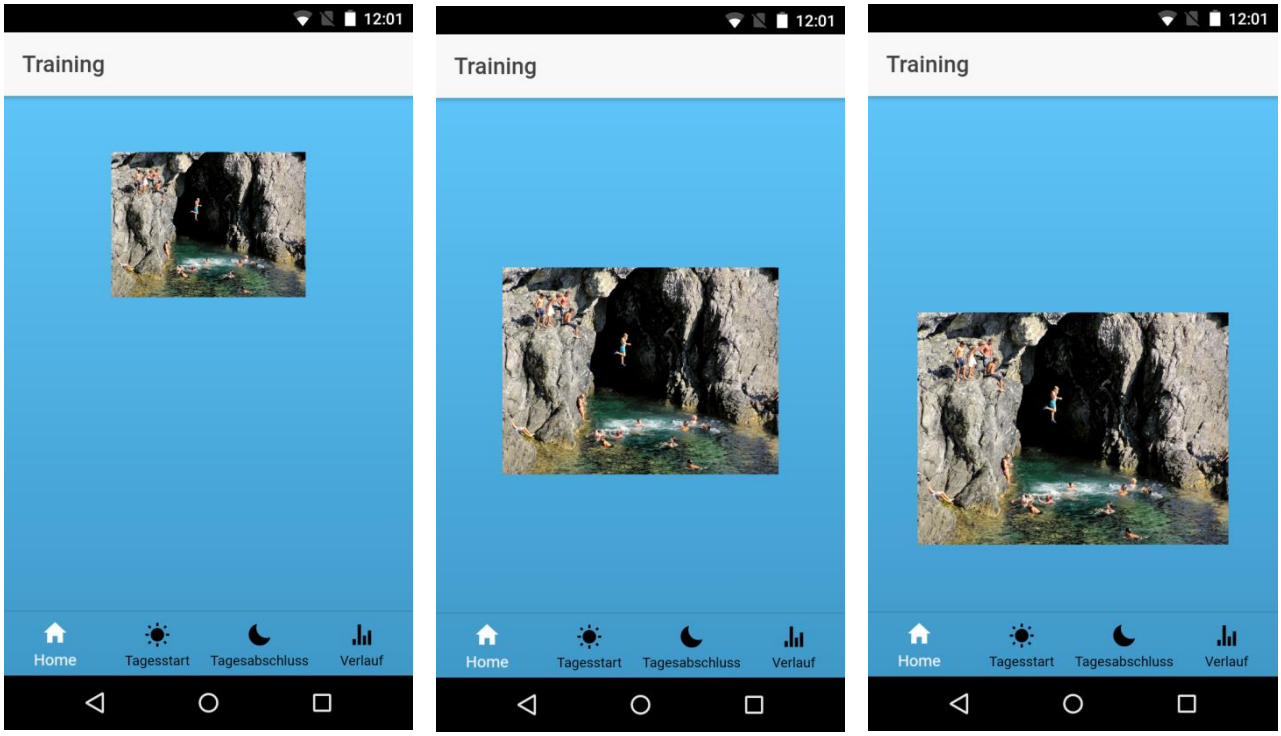


Figure H4.3

Presentation of a Negative Picture (i.e., Negative for the Respective Participant) in a Training Session and Reduction of Picture Size by Swiping/Pushing Away the Picture on the Display

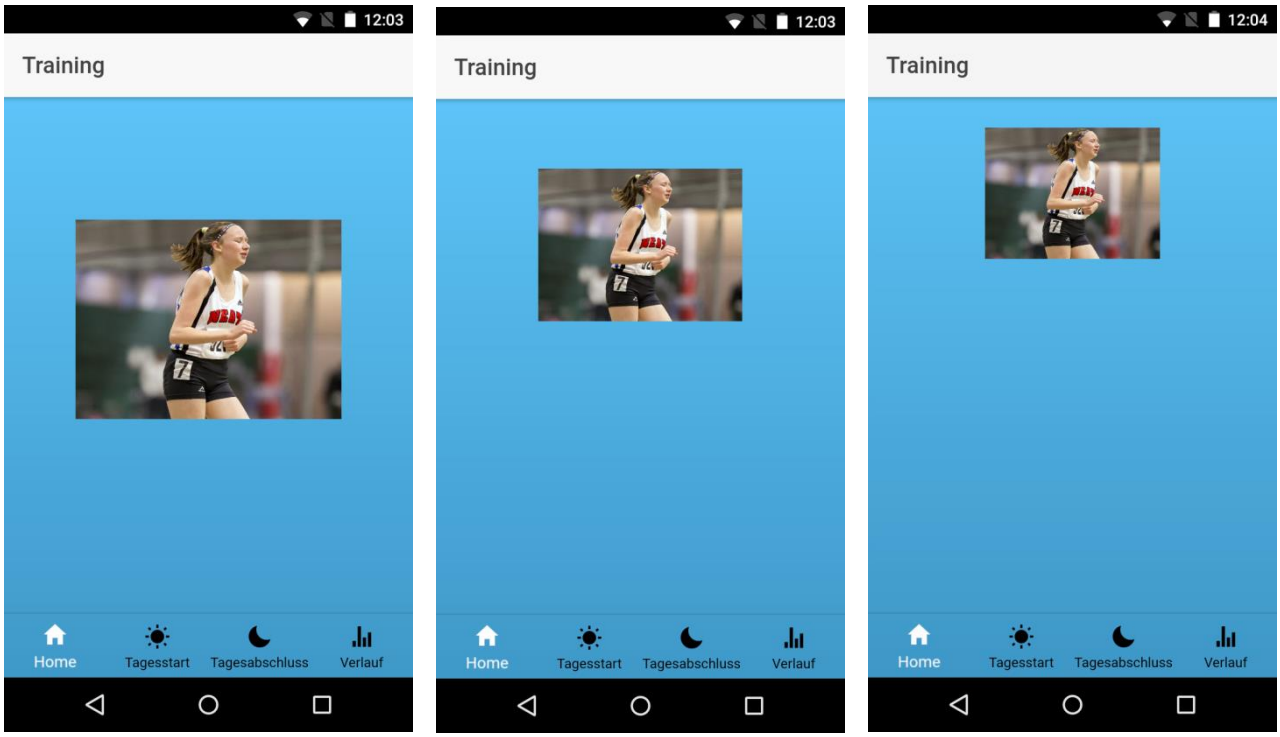
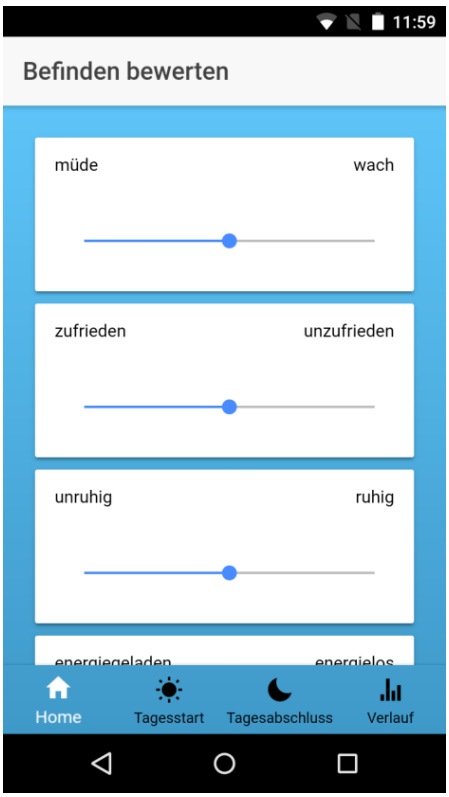


Figure H4.4

Assessment of Current Mood at the End of Each Training Session



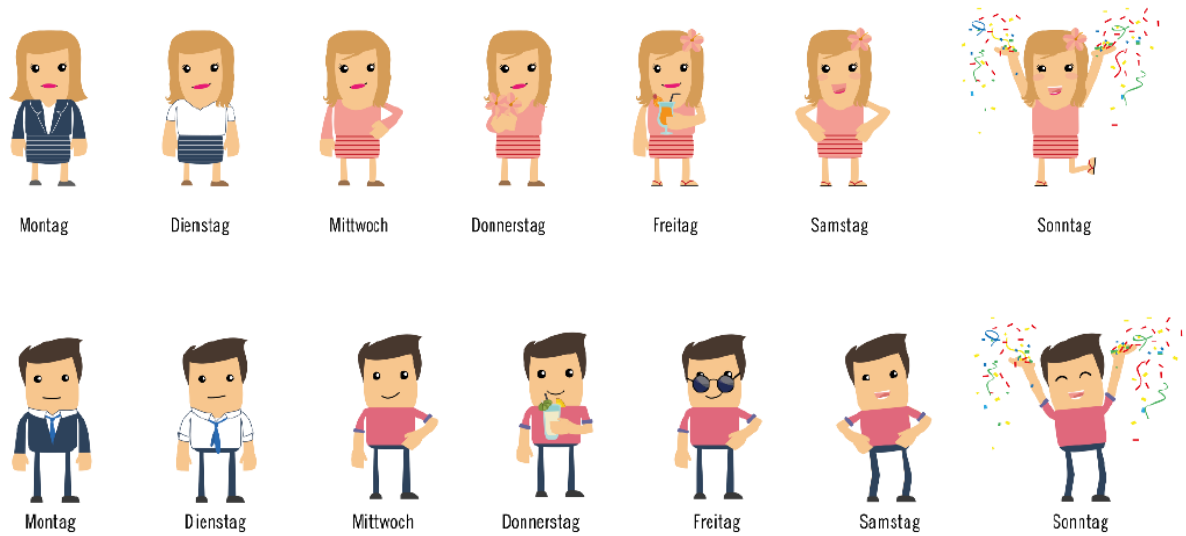
Appendix H5 "Breezly" – Rewarding System

Depending on the frequency and the regular performance of training sessions, the participants collected points that were reported in the form of a changing appearance of the chosen avatar (e.g., changing clothes or mood). In total, seven levels of the avatar were possible by regular training sessions (Figure H5.1). To foster an individual's compliance to the recommended training times, participants could achieve more points for training sessions performed during the respective time slots (7–11 a.m., 7–12 p.m.). By training according to the recommendations, the avatar could increase one level per day.

Based on the COR theory (Hobfoll et al., 2018), the rewarding system of "Breezly" was designed to make the gain of points (i.e., resources) more difficult than the loss of points. Therefore, participants received minus points for omitted training sessions. However, if they had trained continuously before, these minus points could be mitigated. The rules of the rewarding system were also handed over to the participants when introducing them into the training (see Appendix F9 Additional Information Concerning Operating Principles of "Breezly" App).

Figure H5.1

Different Possible Levels of the Avatars Betty and Brian During Training



Rules of the rewarding system:

- Ideally, training sessions (1 session = 1 picture training + current mood assessment) should be performed *daily.*
- The avatar started at level 2.
- For each training session within the recommended periods (3x 7–11 a.m., 3x 7–12 p.m.), participants received five points, respectively.
- Additional training sessions (> 3) in these periods only resulted in two points per session.
- Subjects received more points if they trained according to the recommendation than if they performed training sessions during other times of the day. Training sessions outside of recommended times yielded only one point per session.
- If participants trained three consecutive days according to the recommendation, they received additional points.
- 30 points were necessary for the avatar to increase one level (additional points had no impact on the level change).
- Seven days training following the recommendation were required for the avatar to achieve the highest level. If participants continued to train as recommended, this level was maintained.
- If training sessions were omitted, subjects received minus points: No training in the morning (7–11 a.m.) or evening (7–12 p.m.) resulted in a loss of six points per omitted session.
- Consistent with gaining points, the avatar decreased one level in case of 30 minus points.
- Minus points could be mitigated if participants had regularly trained before (→ 10 plus points) (storyline for participants: “Compare Breezly with a training in the gym: If you trained continuously for a period of three days and take a one-day break, your muscle tissue is broken down more slowly as if you had not trained at all before.”).

Appendix H6 “Breezly” – Main Menu/Home Screen and Additional Information About the App

Figure H6.1 presents the main menu/home screen of “Breezly”. By selecting the menu item “Über die App”, additional information about “Breezly”, its principles, and the (scientific) background for the training were provided (Figure H6.2).

Figure H6.1

Home Screen of “Breezly” With the Avatar and Current Points as well as Number of Training Days

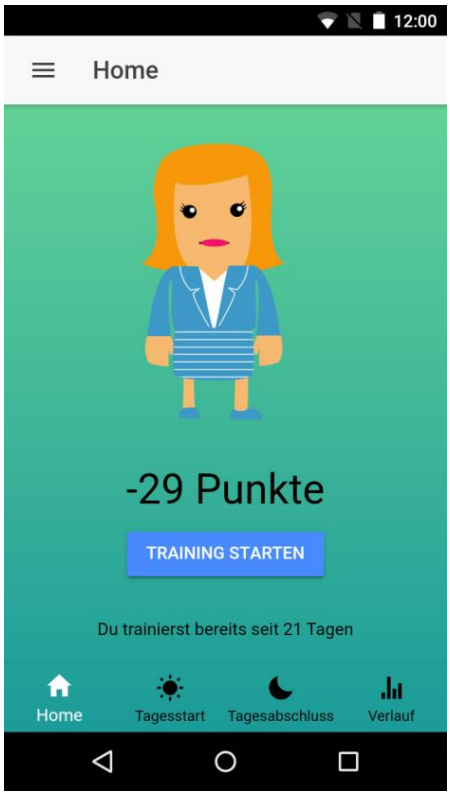
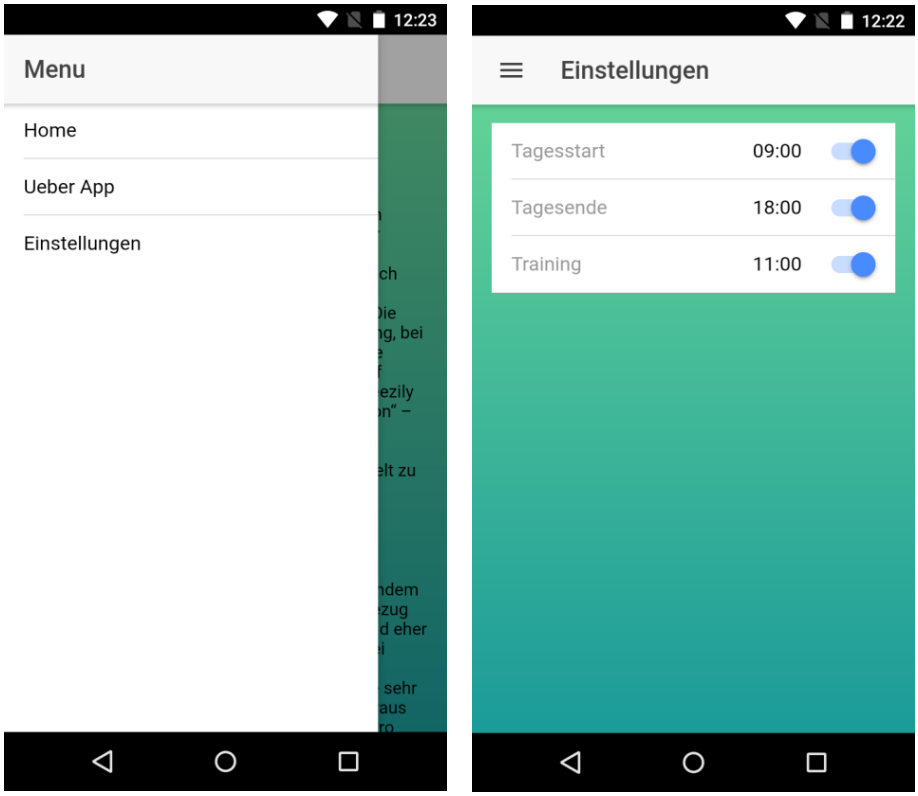


Figure H6.2

Additional Information about Background and Functioning of "Breezly"



Appendix H7 "Breezly" – Day Start/End Assessment and History Graphs

At the start of each training day, participants were invited to give a subjective rating of their current well-being, the restfulness of sleep in the previous night, and their vigor for the upcoming day ("Tagesstart"; Figure H7.1). Similarly, each evening during the three-week training period, they were requested to answer questions concerning their current well-being, their ability to get affected by positive stimuli, and to distance from negative stimuli ("Tagesabschluss", Figure H7.1). By using the menu item "Verlauf", participants also had the possibility to keep track of the history of these EMA measures in the morning and the evening (Figure H7.2). The training history also indicated the number of training sessions performed.

Figure H7.1

Inquiry of Psychological Measures at Day Start/End Assessment

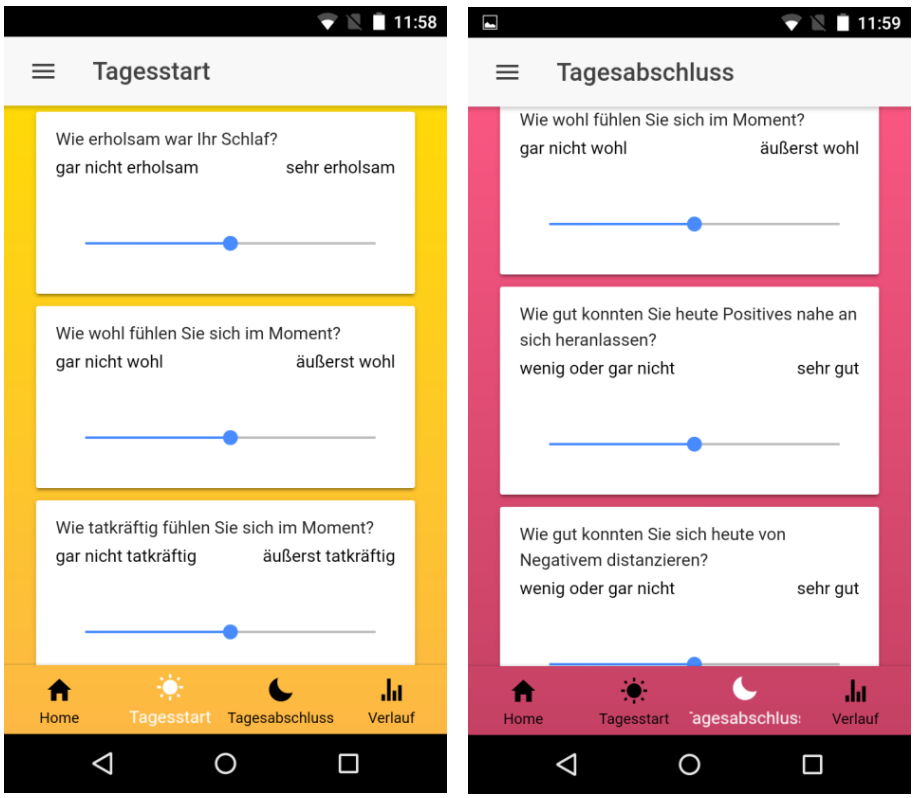


Figure H7.2

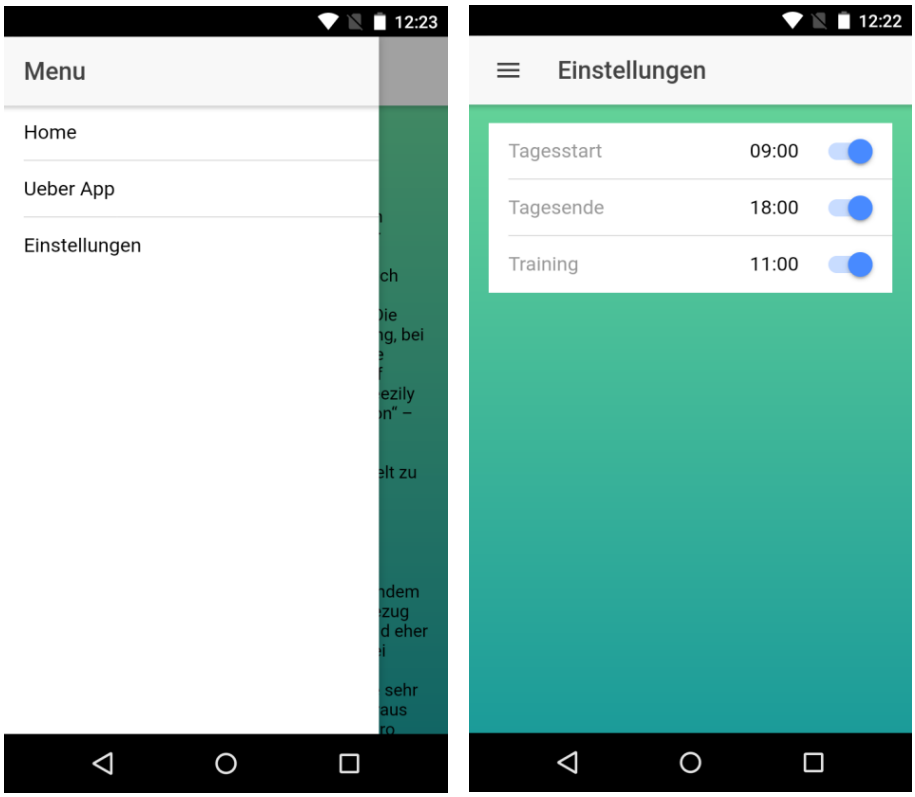
History Graphs for Different Psychological Measures Measured in Day Start/End Assessment

Appendix H8 “Breezly” – Reminder

Within “Breezly”, participants had the possibility to activate or deactivate a reminder function (Figure H8.1). Upon request, participants were reminded automatically to conduct training sessions in the morning and the evening, respectively, as well as to fill out the regular surveys on psychological measures (e.g., well-being).

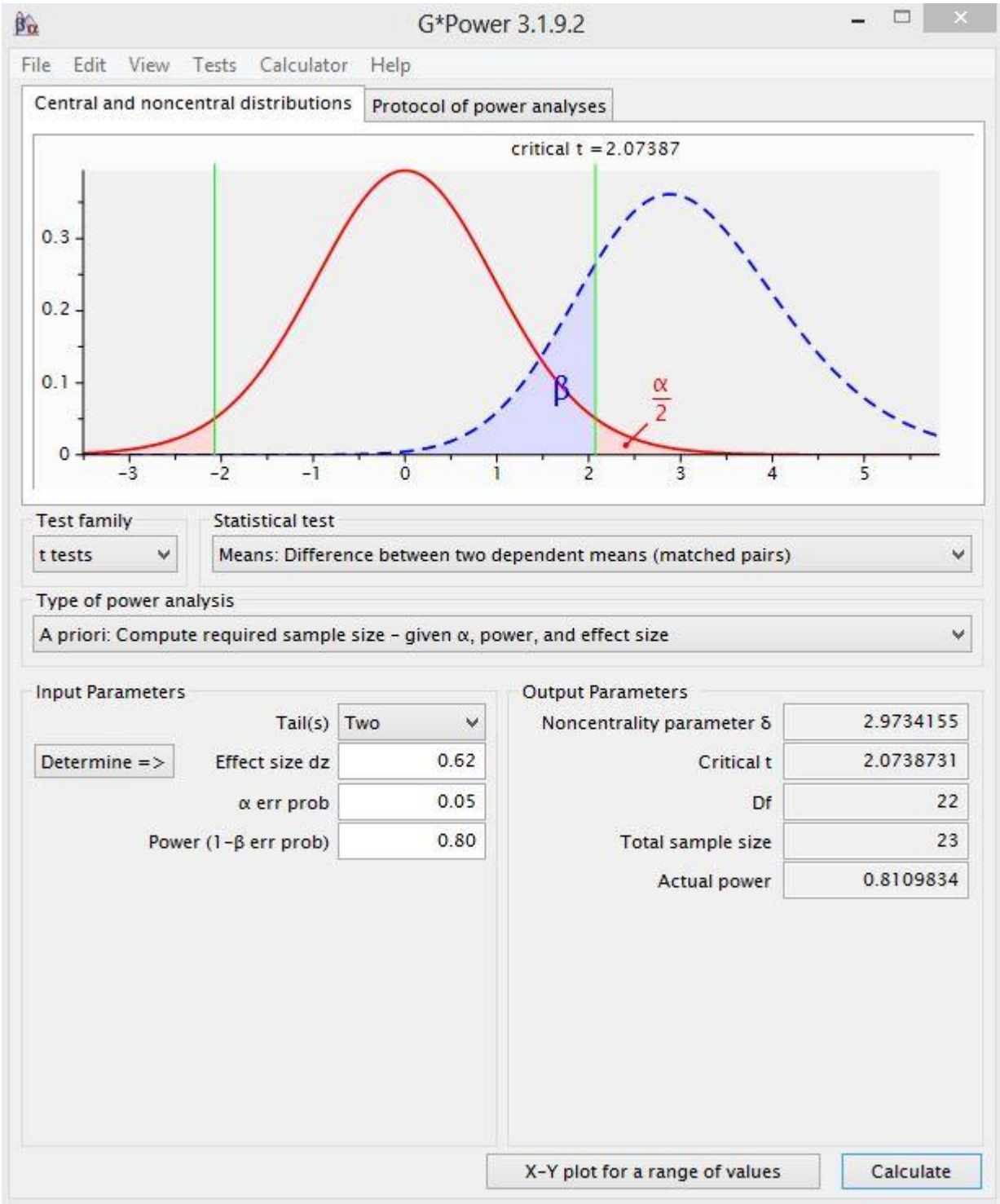
Figure H8.1

Setting of Reminder Function for Training and Day Start/End Assessments



Appendix I Statistical Analyses

Appendix I1 Sample Size Calculation Using G*Power



Appendix I2 Statistical Analyses and Their Assumptions (and Testing)

Table I2.1

Statistical Analyses and Their Assumptions – Within-Group Analyses (TRAIN$_4$Positivity)

Assumptions	Testing of assumptions
Paired *t* tests (RQ1/hypothesis 1.1, RQ2/hypothesis 2.1, RQ3, RQ5)	
1. normally distributed difference scores of observed pairs	Shapiro-Wilk test (Shapiro & Wilk, 1965)
2. independence of observations within a sample (i.e., time point)	not testable
(Bortz & Schuster, 2010; Eid et al., 2017)	
Multiple linear regression analyses (RQ1/hypothesis 1.1, RQ2/hypothesis 2.1, RQ3, RQ5)	
1. linearity	Partial regression plots
2. reliability of predictors	Internal consistency values found in this study (see Appendix G6)
3. no outliers or influential data points	- all data tested for outliers and extreme values (see 3.2.8.1) - outliers in predictors[a]: calculation of centered leverage scores (Eid et al., 2017), with values > 3*k/n (i.e., k = number of predictors; n = sample size; parsimonious model: .29, comprehensive model: .44) being further inspected (Urban & Mayerl, 2006) - outliers in the dependent variable: identified using the studentized deleted residuals with absolute value > 3 being considered as critical (Eid et al., 2017) - influential datapoints: (standardized) DfBETAS values identified (Eid et al., 2017); for small to medium samples, DfBETAS > |1| were suggested to indicate outliers (Cohen et al., 2003), while other authors only assumed this for values > |2| (Stevens, 2009)
4. no multicollinearity	Tolerance (TOL > 0.2) and Variance Inflation Factor (VIF < 10) indicate no multicollinearity (Eid et al., 2017)
5. homoscedasticity	inspection of residual plots; homoscedasticity indicated by scores randomly scattering about the horizontal line (i.e., zero; Field, 2015)
6. normality of residuals	Probability-probability plots (P-P-plots; Field, 2015); normality indicated by points in the plot lying (close) to a straight line
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	Durbin Watson test; Durbin Watson statistic can have values from 0–4; no auto-correlation is indicated by values around 2; the more a value deviates from 2, the more likely is auto-correlation (Field, 2015)
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	
Three-way (2 x 2 x 2) repeated measures analyses of variance (RQ2/hypothesis 2.2)	
1. continuous dependent variable	in this study: AAT-CS
2. independent observations within a time point	ensured by the design of TRAIN$_4$Positivity
3. normality of dependent variable for each combination of factor levels	Shapiro-Wilk test to examine the normality assumption, with repeated-measures ANOVA being relatively robust against violations (Bortz & Schuster, 2010); assumption is supported based on non-significant findings (Eid et al., 2017; Field, 2015)

Assumptions	Testing of assumptions
4. homogeneous variances and homogeneous variance-covariance matrices between the levels of the non-repeated factor(s)	- homogeneous variances: Levene's test - homogeneous variance-covariance matrices: Box test both assumptions supported based on non-significant findings (Eid et al., 2017; Field, 2015)
5. equal variance of differences between the groups (sphericity) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015; Göllner et al., 2010)	due to only two levels of the repeated factor, sphericity can be viewed as fulfilled (Eid et al., 2017; Field, 2015)
Multilevel analyses (RQ4)[b,c]	
1. for 2-level models, not aiming at analyzing cross-level interactions, at least *n* = 30 units on the highest level required	in this study: *N* = 41
2. linear change of the outcomes over time for all subjects	scatter plots between time as level-1 predictor and the respective outcome for each level-2 unit
3. normal distribution of residuals at all levels (level-1 residuals, residuals for random slopes, residuals for random intercepts)	visual analysis of standardized residuals and histograms; however, parameter estimates not seriously affected by non-normal residuals at level 2 of the model (Maas & Hox, 2004)
4. homogeneity of residual variances (homoscedasticity; i.e., here: equal variances of level-1 residuals at each level of level-1 predictor) (Bell et al., 2019; Göllner et al., 2010; Nezlek et al., 2006; Singer & Willett, 2003)	scatter plots of (standardized) residuals and the predicted values (StataCorp, 2019)
Bivariate Pearson correlations	
1. linearity	scatterplots (Field, 2015)
2. continuous variables	given in this study for AAT-CS, LOT-R, ASF-E-P, ASF-E-N, BRS, PSS-10
3. bivariate normal distribution	inspection of univariate normal distribution of the single variables using the Shapiro Wilk test (Bortz & Schuster, 2010), that is suggested for small samples (n < 50; Ghasemi & Zahediasl, 2012)

Note. AAT-CS = Compatibility Score of Approach Avoidance Task; LOT-R = Life Orientation Test-Revised; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; BRS = Brief Resilience Scale; PSS-10 = Perceived Stress Scale-10.

[a] Predictors whose distance from the mean of the variable is large can have a greater impact on the determination of regression weights (Eid et al., 2017). [b] Multilevel modeling tolerant of missing data; that is, each subject does not have to be observed at each time point; time points do not have to be at the same time for all subjects; multilevel modeling can be used in case of missing data (Göllner et al., 2010; Richter & Naumann, 2002). [c] The multilevel structure in the current study manifested in repeated daily observations (i.e., repeated assessments of, for example, current mood; level 1), that were nested or clustered within participants (level 2). Regression slopes represent the change in the criterion over time.

Table I2.2

Statistical Analyses and Their Assumptions – Between-Group Analyses (TRAIN$_4$Positivity vs. LifeStress)

Assumptions	Testing of assumptions
Propensity score matching (PS matching) and analysis with independent *t* test for matched data (RQ1/exploratory RQ 1.2)	
PS matching	
1. Conditional Independence Assumption (CIA; i.e., outcome variable is independent of treatment conditional on the PS)	- Selection of covariates for the PS model was based on observable characteristics (age, gender, education, mental health, stressor exposure) - All variables that influence treatment assignment and potential outcomes were simultaneously observed by the researcher (all covariates assessed at pretest in both studies)
2. common support or overlap between IG and CG (i.e., individuals with the same covariate values have a positive probability of being both participants and non-participants)	- Minima and Maxima comparison ("common" command; i.e., all observations whose PS is smaller than the minimum and larger than the maximum in the opposite group are deleted) and the trimming method implemented for the first matching algorithm tested (Caliendo & Kopeinig, 2005) - visual analysis to determine the region of common support (i.e., range where PS for IG and CG participants has similar density)
3. correct specification of PS model and balanced distribution of relevant variables in two groups before comparison (Caliendo & Kopeinig, 2005; Heckman et al., 1999; Kuss et al., 2016)	- PS histogram by treatment status - PS density plot - determination coefficient pseudo R^2 (value near zero indicates equal distribution of covariates) - LR χ^2 test (non-significant test indicates equal distribution of covariates)
Independent sample *t* test (with matched data)	
1. continuous outcome	perceived severity of microstressor as continuous variable (interval level)
2. independent variable with two categorical, independent groups	treatment status with two independent conditions (TRAIN$_4$Positivity condition vs. LifeStress)
3. independence of observations (i.e., no relationship between observations within each group or between groups themselves)	- ensured by LifeStress being a historical control group - different participants within each group with no participant being in more than one group
4. no significant outliers	Boxplot
5. normal distribution of the dependent variable (i.e., perceived severity of microstressors) for each group of the independent variable (i.e., within each treatment condition)	- independent *t* test robust against violations of normality if sample size is $n \geq 30$ in both groups - Shapiro-Wilk test (Shapiro & Wilk, 1965)
6. homogeneity of variances (Bortz & Schuster, 2010; Field, 2015)	Levene's test

Note. PS = propensity score; IG = intervention group; CG = control group.

Appendix I3

Different Data Sets Created for Statistical Analyses

RQ/hypotheses	Data set
RQ1, hypothesis 1.1	*Data set 1* (only TRAIN$_4$Positivity; without "Breezly" EMA measures, but including the weekly MIMIS EMA assessments on Sundays); in wide format
RQ1, exploratory RQ 1.2	*Data set 2* (TRAIN$_4$Positvity and LifeStress data); initially unmatched data in this data set are matched using PS matching; in wide format (includes *n* = 41 participants of TRAIN$_4$Positvity and *n* = 46 participants of LifeStress who meet the inclusion criteria of TRAIN$_4$Positvity) → new data set with matched participants is created
RQ2, hypothesis 2.1 and hypothesis 2.2	*Data set 1* (only TRAIN$_4$Positivity; without "Breezly" EMA measures, but including the weekly MIMIS EMA assessments on Sundays); in wide format
RQ3	*Data set 1* (only TRAIN$_4$Positivity; without "Breezly" EMA measures, but including the weekly MIMIS EMA assessments on Sundays); in wide format
RQ4	*Data set 3* (only TRAIN$_4$Positivity) including "Breezly" EMA measures (current mood: positive valence, emotional arousal, and calmness); in long format *Data set 4* (only TRAIN$_4$Positivity) including "Breezly" EMA measures (well-being, ability to get affected by positive stimuli, ability to distance from negative stimuli); in long format
RQ5	*Data set 1* (only TRAIN $_4$Positivity; without "Breezly" EMA measures, but including the weekly MIMIS EMA assessments on Sundays); in wide format
RQ6	*Data set 1* (only TRAIN$_4$Positivity; without "Breezly" EMA measures, but including the weekly MIMIS EMA assessments on Sundays); in wide format
Two data sets (iqdat format) for each participant (not used for hypothesis testing): original data of AAT assessment generated by millisecond Inquisit at pre- and posttest in TRAIN$_4$Positivity *Data set 7 and 8 (.sav format; not used for hypothesis testing):* created to calculate the AAT-CS for the pre- and posttest in TRAIN$_4$Positivity, which was then included as variable in *data set 1* on TRAIN$_4$Positivity	

Note. EMA = Ecological Momentary Assessment; MIMIS = Mainz Inventory of Microstressors.

Appendix I4

Summary Single Analyses per RQ to Determine the Bonferroni-Corrected Significance Level

RQ/hypotheses	Single analyses
RQ1, hypothesis 1.1	1. *t* test for the pre-post difference in perceived stress (PSS-10)
	2. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in PSS-10
RQ1, exploratory RQ 1.2	3. *t* test for the three-week comparison between TRAIN$_4$Positivity (IG) and LifeStress (CG) in the perceived severity of microstressors after PS matching
RQ2, hypothesis 2.1	4. *t* test for the pre-post difference in implicit action tendencies (AAT-CS)
	5. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in the AAT-CS
RQ2, hypothesis 2.2	6. Three-way Repeated-Measures ANOVA to examine the impact of pretest AS (ASF-E-P, ASF-E-N) on the pre-post change in the AAT-CS
RQ3	7. Wilcoxon signed-rank test for the pre-post difference in mental health (GHQ-28)
	8. Multiple regression analysis to control for individual stressor exposure (microstressors: MIMIS and MIMIS EMA; macrostressors: LE Checklist) concerning the pre-post change in GHQ-28 (proxy measure of resilience)
	9. Multiple regression analysis for the predictive impact of covariates (microstressors: MIMIS and MIMIS EMA; macrostressors: LE Checklist; ASF-E-P, ASF-E-N) on the pre-post change in GHQ-28
	10. *t* test for the pre-post difference in the ability to recover from stress (BRS)
	11. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in BRS
	12. *t* test for the pre-post difference in subjective well-being (WHO-5)
	13. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in WHO-5
RQ4[b]	14. Multilevel modeling (random-coefficients model with unstructured covariance matrix; available-case analysis) for the ability to get affected by positive stimuli
	15. Multilevel modeling (random-coefficients model with unstructured covariance matrix; imputed data) for the ability to get affected by positive stimuli
	16. Multilevel modeling (random intercept model; available-case analysis) for the ability to distance from negative stimuli
	17. Multilevel modeling (random intercept model; imputed data) for the ability to distance from negative stimuli
	18. Multilevel modeling (random-coefficients model with unstructured covariance matrix; available-case analysis) for well-being
	19. Multilevel modeling (random-coefficients model with unstructured covariance matrix; imputed data) for well-being
	20. Multilevel modeling (random-coefficients model with unstructured covariance matrix; available-case analysis) for current mood – valence
	21. Multilevel modeling (random-coefficients model with unstructured covariance matrix; imputed data) for current mood – valence
	22. Multilevel modeling (random-coefficients model with unstructured covariance matrix; available-case analysis) for current mood – energetic arousal
	23. Multilevel modeling (random-coefficients model with unstructured covariance matrix; imputed data) for current mood – energetic arousal
	24. Multilevel modeling (random-coefficients model with unstructured covariance matrix; available-case analysis) for current mood – calmness
	25. Multilevel modeling (random-coefficients model with unstructured

RQ/hypotheses	Single analyses
	covariance matrix; imputed data) for current mood – calmness
RQ5	26. *t* test for the pre-post difference in reappraisal (ERQ)
	27. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in ERQ
	28. *t* test for the pre-post difference in reappraisal (positive reappraisal subscale CERQ)
	29. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in CERQ–positive reappraisal
	30. Wilcoxon signed-rank test for the pre-post difference in positive affect (PANAS subscale)
	31. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in PANAS-positive affect
	32. *t* test for the pre-post difference in negative affect (PANAS subscale)
	33. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in PANAS-negative affect
	34. Wilcoxon signed-rank test for the pre-post difference in state anger (STAXI subscale)
	35. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in STAXI state anger
	36. *t* test for the pre-post difference in anger in (STAXI subscale)
	37. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in STAXI anger in
	38. Wilcoxon signed-rank test for the pre-post difference in anger out (STAXI subscale)
	39. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in STAXI anger out
	40. *t* test for the pre-post difference in anger control (STAXI subscale)
	41. Multiple regression analysis for the predictive impact of covariates[a] on the pre-post change in STAXI anger control
RQ6	42. Correlation between LOT-R total score and AAT-CS
	43. Correlation between LOT-R optimism score and AAT-CS
	44. Correlation between ASF-E-P and AAT-CS
	45. Correlation between ASF-E-N and AAT-CS
	46. Correlation between BRS and AAT-CS
	47. Correlation between PSS-10 and AAT-CS
Calculation of Bonferroni-corrected significance level based on the above 47 single analyses (α_{corr} = .001)	

Note. PSS-10 = Perceived Stress Scale-10; IG = intervention group; CG = control group; AAT = Approach Avoidance Task; AAT-CS = AAT Compatibility Score; ASF-E = Attributionsstilfragebogen für Erwachsene; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; GHQ-28 = General Health Questionnaire-28; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment; LE Checklist = Life events Checklist; BRS = Brief Resilience Scale; WHO-5 = Well-being Index; ERQ = Emotion Regulation Questionnaire; CERQ = Cognitive Emotion Regulation Questionnaire; PANAS = Positive and Negative Affect Schedule; STAXI = State-Trait Anger Expression Inventory; LOT-R = Life Orientation Test-Revised.

[a] covariates: microstressors: MIMIS and MIMIS EMA; macrostressors: LE Checklist; attributional style: ASF-E-P; ASF-E-N.

[b] For multilevel modeling, only model with best model fit (available-case analysis and imputed data) relevant for hypothesis testing and considered for Bonferroni correction.

Appendix I5 Reasons for Considering SPF and SDS-17 as Covariates

Empathy may affect the reactions to affective stimuli in the MB-PBT (and the AAT assessment) and was therefore planned to be included as predictor in the multiple regression analyses. For example, when being exposed to the picture of a crying child (i.e., negative valence; e.g., training instruction is to push this picture away), (highly) empathetic participants might have the underlying tendency to comfort the child in a similar real-life situation. They could respond to the stimulus by swiping it closer instead of swiping it away (i.e., wrong reaction) or might respond with delay. Although high-arousal and inappropriate pictures were excluded from the training pictures, "Breezly" could still include sad pictures (e.g., crying child) that might trigger the above reaction. Consequently, the effects of the intervention on different outcomes might be affected, which would also bias conclusions about the efficacy of the MB-PBT. Particularly with respect to the AAT assessment (i.e., assessment of implicit action tendencies), the participants' empathy might also affect their performance/outcome (AAT-CS) as the task partly included sad pictures potentially causing compassion and an empathetic reaction. Although participants are instructed to only respond to the picture format, they might have contrary movement tendencies when being confronted with a sad picture, resulting in wrong or delayed reactions (i.e., longer RTs) in the AAT.

Similarly, a tendency for social desirability might affect the participants' reactions during the training and thus the intervention effects. For example – based on the assumption that such a behavior is generally rated as brave by the society – participants might assess a picture of skydivers as positive (i.e., swiped closer in training session), although they personally feel nervous or anxious when seeing the picture.

Appendix I6 Details of Propensity Score (PS) Matching

Only CG participants meeting the inclusion criteria for TRAIN$_4$Positivity were considered for the PS analysis that included seven steps. First, based on the non-matched data (*N* = 87; *data set 2*), the groups were compared concerning potential covariates measured at pretest in both groups, using independent *t* tests and χ^2 tests (age, gender, education, mental health [GHQ-28], microstressors [MIMIS], macrostressors [LE Checklist]). Although the CG included a 4-week assessment, the MIMIS assessment at the end of week 3 was used for compatibility reasons with the IG.

The PS matching included the following seven steps: First (1), a *logistic regression* (outcome: allocated group) with a limited number of (pretest) covariates (age, gender, dummy-coded education) was calculated to determine the PS[31] for each participant. Due to the binary study design (IG vs. CG), the logit model was used for the PS estimation, which is recommended for small samples (Caliendo & Kopeinig, 2005). The variable choice for the PS model was based on the Conditional Independence Assumption (CIA)[32]. Besides, the study used the method of *statistical significance* (Caliendo & Kopeinig, 2005; Heckman et al., 1998) to select the predictors for the PS model. Beginning with a model with few predictors (age, gender, education), for whom different matching algorithms were tested (see below), mental health and the stressor load (micro-, macrostressors), were iteratively added to the model that used the algorithm with the best matching quality. Only variables that were significant at the α = .05 level were kept. In general, only pretest variables that simultaneously affected the participation decision and the outcome, but were unaffected by the participation (or the anticipation of it), were selected (Caliendo & Kopeinig, 2005; Kuss et al., 2016). In addition, the same data sources had been used for participants and non-participants to measure the covariates (Heckman et al., 1999). For the PS estimation, different matching algorithms were performed, which is suggested for small samples (Caliendo & Kopeinig, 2005). Based on the literature (Caliendo & Kopeinig, 2005; Müller, 2012), the following algorithms were tested using “psmatch2” in Stata: 1a) 1-to-1 matching without replacement (RE), 1b) 1-to-1 matching with RE, 2) 2-nearest neighbour (nn) matching, 3) 3-nn

[31] probability to receive the intervention to be tested

[32] outcome variable is independent of treatment conditional on the PS, that is, the choice whether a subject receives the intervention or not, is not correlated to the outcome

matching, 4) 4-nn matching, 5a) Caliper matching[33] (caliper default 0.2) without RE, 5b) Caliper matching (0.2) with RE, 6a) Caliper matching (0.3) without RE, 6b) Caliper matching (0.3) with RE, 7a) Radius caliper matching[34] (0.2) without RE, 7b) Radius caliper matching (0.2) with RE, and 8) Kernel matching. PS matching requires the implementation of the common support or overlap between the IG and CG (Caliendo & Kopeinig, 2005).

To determine the *region of common support* (2), that is, the range where the PS for IG and CG participants has similar density, a visual analysis was performed. Both the Minima and Maxima comparison ("common") and the trimming method (Caliendo & Kopeinig, 2005) were implemented for the first algorithm, with better matching quality resulting for the Minima and Maxima comparison[35], which was maintained for the following algorithms.

To assess the *matching quality* of each algorithm[36] and to select the "best" algorithm (3), several indicators were used. The distribution of the PS (PS histogram by condition), the PS density plot, and statistical tests were inspected to check for a similar PS distribution across groups and to determine the similarity between groups in the covariates used. Using the command "pstest", the determination coefficient pseudo R^2 and the Likelihood Ratio (LR) χ^2 test were investigated. Equal distribution of covariates is indicated by a Pseudo R^2 near zero and a non-significant LR test (Müller, 2012).

Fourth, for the chosen algorithm, it was decided whether to also include mental health and the stressor exposure in the model based on statistical significance (4). Again, the overlap assumption and the matching quality were tested. For the final algorithm and model, a sensitivity analysis (5) using Rosenbaum bounds was performed to investigate the sensitivity of the estimated treatment effect concerning unobserved covariates (Caliendo & Kopeinig, 2005). The Stata package "teffects psmatch"

[33] Caliper matching: For each IG subject, a CG matching partner is identified who has a PS within a maximum tolerated range around the PS of the treated individual (caliper width or "propensity range"; Caliendo & Kopeinig, 2005).

[34] Radius caliper matching: For each IG subject, not only one matching partner with the closest PS is used, but all available untreated subjects within the caliper.

[35] all observations whose PS is smaller than minimum and larger than maximum in the opposite group are deleted

[36] correct specification of PS model and balanced distribution of variables in two groups (Caliendo & Kopeinig, 2005)

offers the advantage of considering that PS are *estimated* when calculating standard errors of the treatment effect. Therefore, the final matching algorithm was also examined using this package (6) and the overlap assumption was checked graphically. Finally, for the matched data, the perceived microstressor severity at posttest was compared (7) between groups using an independent *t* test.

Appendix I7 Different Models Tested in Multilevel Modeling

First, as recommended (Nezlek, 2001), a null model or unconditional model (1) without any predictors (i.e., only intercept) was calculated for each outcome to determine if there was enough variation to warrant multilevel modeling and to calculate intra-class correlation coefficients (ICCs; Göllner et al., 2010; Hox, 2010; Nezlek, 2001). Scatterplots were inspected to explore the data structure over time. Based on the assumption that the outcomes change in a linear fashion and given the observed data structure (see Appendix J5), this study used linear growth models (Göllner et al., 2010; Kwok et al., 2008; Richter & Naumann, 2002) with time as level-1 predictor. Second, a random-intercept model (2; i.e., fixed level-1 predictor time with random intercepts) was calculated for each outcome. As the current study used an uncontrolled design for RQ4, it was assumed that individuals would differ concerning their pretest values in the outcomes (i.e., variance of random intercepts > 0). Third, a random-coefficients model was tested, which extended the previous model by also allowing random slopes for time. Random slopes (i.e., different growth rates over the training) were also expected due to the possible variability in training intensity between participants of TRAIN$_4$Positivity in this feasibility study. Initially, no covariance structure was specified for the random effects (3). Subsequently, the random-coefficients model was tested with a parsimonious exchangeable (compound symmetry; CS) covariance structure (4), which assumes that all measurements on the same individual are equally correlated (Liu, 2016). Finally, the random-coefficients model was performed with an unstructured covariance structure (5), which allows for all (co-)variances to be distinct (StataCorp, 2019) and offers the most flexibility (Núnez-Antón & Zimmerman, 2013).

Appendix J Results

Appendix J1

Analysis of Outliers and Extreme Values

Variable	Number of outliers[a]	Number of extreme values[b]
Baseline variables and covariates		
Optimism (LOT-R total score), pretest	/	/
Optimism (LOT-R total score), posttest	1	/
Empathy (SPF), only pretest	/	/
Social desirability (SDS-17), only pretest	/	/
Attributional style (ASF-E-P), pretest	/	/
Attributional style (ASF-E-P), posttest	1	/
Attributional style (ASF-E-N), pretest	2	/
Attributional style (ASF-E-N), posttest	2	/
Macrostressors (LE Checklist), pretest	/	/
Macrostressors (LE Checklist), posttest	/	/
Macrostressors (LE Checklist), total number	1	/
Microstressors (MIMIS), pretest	1	/
Microstressors (MIMIS), posttest	1	/
Primary and secondary outcomes		
Perceived stress (PSS-10), pretest	/	/
Perceived stress (PSS-10), posttest	1	/
Perceived stress (PSS-10), difference score	5	/
AAT-CS, pretest	1	/
AAT-CS, posttest	2	1
AAT-CS, difference score	/	/
Mental health (GHQ-28), pretest	1	/
Mental health (GHQ-28), posttest	1	/
Mental health (GHQ-28), difference score	4	1
Ability to recover from stress (BRS), pretest	2	/
Ability to recover from stress (BRS), posttest	2	/
Ability to recover from stress (BRS), difference score	/	/
Well-being (WHO-5), pretest	1	/
Well-being (WHO-5), posttest	/	/
Well-being (WHO-5), difference score	1	/
Reappaisal (ERQ), pretest	1	/
Reappaisal (ERQ), posttest	1	/
Reappaisal (ERQ), difference score	1	/
Positive reappraisal (CERQ), pretest	/	/
Positive reappraisal (CERQ), posttest	/	/
Positive reappraisal (CERQ), difference score	1	/
Positive affect (PANAS-PA), pretest	2	/
Positive affect (PANAS-PA), posttest	/	/
Positive affect (PANAS-PA), difference score	1	/
Negative affect (PANAS-NA), pretest	6	1
Negative affect (PANAS-NA), posttest	1	/
Negative affect (PANAS-NA), difference score	5	/
State anger (STAXI), pretest	2	2
State anger (STAXI), posttest	3	1
State anger (STAXI), difference score	4	1
Anger in (STAXI), pretest	1	/
Anger in (STAXI), posttest	1	/
Anger in (STAXI), difference score	3	/
Anger out (STAXI), pretest	/	1

Variable	Number of outliers[a]	Number of extreme values[b]
Anger out (STAXI), posttest	1	/
Anger out (STAXI), difference score	1	1
Anger control (STAXI), pretest	/	/
Anger control (STAXI), posttest	1	/
Anger control (STAXI), difference score	3	/

Note. LOT-R = Life Orientation Test-Revised; SPF = Saarbrücker Persönlichkeitsfragebogen zur Messung von Empathie; SDS-17 = Social Desirability Scale-17; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; LE Checklist = Life Events Checklist; MIMIS = Mainz Inventory of Microstressors; PSS-10 = Perceived Stress Scale-10; AAT-CS = Approach Avoidance Task Compatibility Score; GHQ-28 = General Health Questionnaire-28; BRS = Brief Resilience Scale; WHO-5 = Well-being Index; ERQ = Emotion Regulation Questionnaire; CERQ = Cognitive Emotion Regulation Questionnaire; PANAS = Positive and Negative Affect Schedule (NA = negative affect; PA = positive affect); STAXI = State-Trait Anger Expression Inventory.

[a] outliers: values > 1.5·interquartile range (IRQ) below the first or above the third quartile (Eid et al., 2017).

[b] extreme values: values > 3·interquartile range (IRQ) below the first or above the third quartile (Eid et al., 2017).

For the respective Box-whisker diagrams, please see Appendix K4.

Appendix J2 Descriptive Analysis of Sociodemographic, Lifestyle, and Clinical Characteristics in TRAIN$_4$Positivity and Sample Description of the LifeStress Study

Table J2.1

Sociodemographic Characteristics of Participants at Pretest

Sociodemographic variable	*n*	Participants
Gender *n* (%)	41	
Female		33 (80.5)
Male		8 (19.5)
Age	41	
M (SD)		25.0 (3.0)
range (years)		20–30
Education (highest level) *n* (%)	41	
No formal degree		0
9 years (“Hauptschule”)		0
10 years (“Realschule”)		0
12 or 13 years (A level)		14 (34.1)
Completed vocational training		2 (4.9)
University or University of Applied Sciences degree		25 (61.0)
Marital status *n* (%)	41	
Unmarried		39 (95.1)
Married		2 (4.9)
Children *n* (%)	41	
None		41 (100)
1 child		0
2 or 3 children		0
More		0
Siblings *n* (%)	41	
None		4 (9.8)
1 sibling		21 (51.2)
2 or 3 siblings		16 (39.0)
More		0
Country of birth *n* (%)	41	
Germany		41 (100)
German native speaker *n* (%)	41	
Yes		41 (100)
No		0
Size of town where participants grew up *n* (%)	41	
< 10.000 residents		19 (46.3)
10.000–100.000 residents		13 (31.7)
> 100.000 residents		9 (22.0)
Number of years of childhood/adolescents spent in respective town	41	
M (SD)		19.4 (4.0)
range (years)		1–25
Study or mainly professional activity (filter question) *n* (%)	39	
Study		26 (66.7)
Professional activity		13 (33.3)
Student job *n* (%)[a]	38	
Yes		11 (28.9)
No		14 (36.8)
Filter not fulfilled		13 (34.2)

Sociodemographic variable	*n*	Participants
Employment status *n* (%)	39	
Full-time		10 (25.6)
Part-time (health reasons)		0
Part-time (NO health reasons)		17 (43.6)
Retired (health reasons)		0
Retired (NO health reasons)		0
Unemployed (health reasons)		0
Unemployed (NOT retired for health reasons/sick leave)		0
Filter not fulfilled		12 (30.8)
Employed or self-employed *n* (%)	37	
Employed		24 (64.9)
Self-employed		1 (2.7)
Filter not fulfilled		12 (32.4)
Number of employment relationships (e.g., dependent employment) *n* (%)[a]	39	
1		22 (56.4)
2		4 (10.3)
More than 2		1 (2.6)
Filter not fulfilled		12 (30.8)
Staff responsibility *n* (%)[a]	39	
Yes		1 (2.6)
No		26 (66.7)
Filter not fulfilled		12 (30.8)
Average number of working hours per week	27	
M (SD)		25.1 (16.0)
range (hours)		2–55
Filter not fulfilled *n* (%)		12 (29.3)
Shift work *n* (%)	39	
Yes		3 (7.7)
No		24 (61.5)
Filter not fulfilled		12 (30.8)
Income – number of individuals living in the household	41	
M (SD)		2.3 (1.1)
range (persons)		1–6
Income – number of persons that contribute to income	41	
M (SD)		2.0 (0.9)
range (persons)		1–4
Average monthly net income (household) *n* (%)	41	
Information denied		13 (31.7)
< 800 €		8 (19.5)
801–1500 €		6 (14.6)
1501–2000 €		5 (12.2)
2001–3000 €		4 (9.8)
3001–5000 €		4 (9.8)
> 5000 €		1 (2.4)
Average monthly net income (own household) *n* (%)	41	
Information denied		8 (19.5)
< 800 €		20 (48.8)
801–1500 €		7 (17.1)
1501–2000 €		3 (7.3)
2001–3000 €		3 (7.3)
3001–5000 €		0
> 5000 €		0

Note. M = mean; *SD* = standard deviation; *n* = sample size.

[a] does not add up to 100% due to rounded numbers.

Table J2.2

Lifestyle Variables of Participants at Pretest

Lifestyle variables	*n*	Participants
Average use of computer (days/week)	41	
M (SD)		6.2 (0.9)
range (days)		4–7
Average use of computer per day (workdays) *n* (%)[a]	41	
< 1 h		2 (4.9)
1–2 h		7 (17.1)
2–3 h		5 (12.2)
3–4 h		4 (9.8)
4–6 h		11 (26.8)
6–8 h		4 (9.8)
> 8 h		8 (19.5)
Average use of computer per day (weekend) *n* (%)	41	
< 1 h		8 (19.5)
1–2 h		19 (46.3)
2–3 h		6 (14.6)
3–4 h		4 (9.8)
4–6 h		2 (4.9)
6–8 h		2 (4.9)
> 8 h		0
Average use of mobile phone (days/week)	41	
M (SD)		7 (0)
range (days)		7–7
Average use of mobile phone per day (workdays) *n* (%)	40	
< 1 h		1 (2.5)
1–2 h		14 (35.0)
2–3 h		11 (27.5)
3–4 h		6 (15.0)
4–6 h		6 (15.0)
6–8 h		1 (2.5)
> 8 h		1 (2.5)
Average use of mobile phone per day (weekend) *n* (%)	40	
< 1 h		5 (12.5)
1–2 h		7 (17.5)
2–3 h		15 (37.5)
3–4 h		3 (7.5)
4–6 h		7 (17.5)
6–8 h		2 (5)
> 8 h		1 (2.5)
Average use of internet (days/week)	41	
M (SD)		7.0 (0.2)
range (days)		6–7
Average use of internet per day (workdays) *n* (%)[a]	39	
< 1 h		1 (2.6)
1–2 h		5 (12.8)
2–3 h		9 (23.1)
3–4 h		8 (20.5)
4–6 h		8 (20.5)
6–8 h		4 (10.3)
> 8 h		4 (10.3)

Lifestyle variables	*n*	Participants
Average use of internet per day (weekend) *n* (%)	40	
< 1 h		2 (5.0)
1–2 h		5 (12.5)
2–3 h		13 (32.5)
3–4 h		6 (15.0)
4–6 h		10 (25.0)
6–8 h		3 (7.5)
> 8 h		1 (2.5)
Frequency of alcohol intake (filter question) *n* (%)	41	
Never		2 (4.9)
Approx. 1 time/month or less		8 (19.5)
2-4 times/month		25 (61.0)
2-4 times/week		5 (12.2)
4 times or more/week		1 (2.4)
Typical quantity of alcohol per day (number of drinks/per day) *n* (%)	41	
1–2		28 (68.3)
3–4		10 (24.4)
5–6		1 (2.4)
7–9		0
10 or more		0
Filter not fulfilled		2 (4.9)
Previous consumption of substances for cognitive enhancement (filter question) *n* (%)	39	
Yes		1 (2.6)
No		38 (97.4)
Substances used for cognitive enhancement *n* (%)[a]	41	
Energy drink		1 (2.4)
Coffee, food supplement (vitamins, etc.)		1 (2.4)
Filter not fulfilled		39 (95.1)
Smoking (filter question) n (%)	41	
Yes		3 (7.3)
No		38 (92.7)
Regular physical exercise (filter question) *n* (%)	41	
Yes		33 (80.5)
No		8 (19.5)
Frequency of physical exercise (average) *n* (%)[a]	41	
1 day		2 (4.9)
2 days		9 (22.0)
3 days		11 (26.8)
4 days		3 (7.3)
5 days		6 (14.6)
6 days		1 (2.4)
7 days		1 (2.4)
Filter not fulfilled		8 (19.5)
Consumption of illegal drugs *n* (%)	41	
Yes		0
No		41 (100)
Previous consumption of illegal drugs (filter question) *n* (%)	41	
Yes		14 (34.1)
No		27 (65.9)
Substance no. 1 *n* (%)[a]	41	
Cannabis		9 (22.0)
Weed		3 (7.3)
Marihuana		2 (4.9)

Lifestyle variables	*n*	Participants
Filter not fulfilled		27 (65.9)
Current consumption of substance no. 1 *n* (%)	41	
Yes		0
Within the last 3 months		2 (4.9)
Within the last 6 months		1 (2.4)
Within the last year		1 (2.4)
Not anymore since at least 1 year		10 (24.4)
Filter not fulfilled		27 (65.9)
Number of consumptions of substance no. 1 *n* (%)	38	
Sporadically		11 (28.9)
Regularly		0
Daily		0
Filter not fulfilled		27 (71.1)
Substance no. 2 *n* (%)	41	
Weed		1 (2.4)
Filter not fulfilled		40 (97.6)
Current consumption of substance no. 2 *n* (%)	41	
Yes		0
Within the last 3 months		0
Within the last 6 months		0
Within the last year		0
Not anymore since at least 1 year		1 (2.4)
Filter not fulfilled		40 (97.6)
Number of consumptions of substance no. 2 *n* (%)	41	
Sporadically		1 (2.4)
Regularly		0
Daily		0
Filter not fulfilled		40 (97.6)
Previous participation in training to foster resilience (filter question) *n* (%)	41	
Yes		3 (7.3)
No		38 (92.7)
Name of training *n* (%)[a]	41	
"Auf Kurs bleiben"		3 (7.3)
Training of cognitive emotion regulation		1 (2.4)
Filter not fulfilled		37 (90.2)
Previous participation in training to promote mental health (filter question) *n* (%)	41	
Yes		2 (4.9)
No		39 (95.1)
Name of training *n* (%)	41	
Workshop/coaching on procrastination and internet consumption		1 (2.4)
Filter not fulfilled		40 (97.6)

Note. *M* = mean; *SD* = standard deviation; *n* = sample size; h = hour.

[a] does not add up to 100% due to rounded numbers; [b] not presented here: items on dynamics of consumption of illegal drugs at the time of consumption.

Table J2.3

Clinical Variables of Participants at Pretest

Clinical variables[b]	*n*	Participants
Subjective health status compared to peers *n* (%)	41	
Less healthy/sick more frequently		2 (4.9)
Equally healthy/sick		18 (43.9)
Healthier/less sick		11 (26.8)
Much healthier/much less sick		8 (19.5)
I'm never sick		2 (4.9)
Medical diagnoses of physical or mental disorders *n* (%)	41	
1. Diabetes		0
2. Hypertension		0
3. Increased cholesterol		0
4. Myocardical infarction		0
5. Liver disease (hepatitis, cirrhosis)		0
6. Renal insufficiency		0
7. Cancer disease		0
8. Brain tumor		0
9. Traumatic brain injury (e.g., brain concussion)		
Yes		1 (2.4)
No		40 (97.6)
10. Severe brain injuries		0
11. Cerebral hemorrhage (stroke with hemorrhage)		0
12. Cerebral infarction (stroke with ischemia)		0
13. Inflammatory brain diesease (e.g., meningitis, multiple sclerosis)		
Yes		2 (4.9)
No		39 (95.1)
14. Neurodegenerative disease (e.g., Parkinson)		0
15. Bipolar disorder		0
16. Organic mental disorder (e.g., dementia)		0
17. Intellectual disability (mental retardation)		0
18. Epilepsia		0
19. Depression		0
20. Schizophrenia		0
21. Anxiety disorder (phobia, panic disorder)		0
22. Obsessive-compulsive disorder (OCD)		0
23. Eating disorder		0
24. Posttraumatic stress disorder (PTSD)		0
25. ADHD		0
26. Personality disorder		0
27. Substance use disorder (alcohol)		0
28. Substance use disorder (prescription drugs)		0
29. Substance use disorder (drugs)		0
Previous or current psychotherapeutic treatment n (%)	41	
Previous		1 (2.4)
Current		0
Does not apply		40 (97.6)
Previous or current psychiatric treatment *n* (%)	41	
Previous		0
Current		0
Does not apply		41 (100)
Regular intake of prescription drugs *n* (%)	41	
Yes		12 (29.3)
No		29 (70.7)

Clinical variables[b]	*n*	Participants
Limited professional activities due to health problems (past 4 weeks) *n* (%)	41	
Not at all		25 (61.0)
A little		14 (34.1)
Moderate		2 (4.9)
Quite a lot		0
Extremely		0
Limited social activities due to health problems (past 4 weeks) *n* (%)	41	
Not at all		31 (75.6)
A little		8 (19.5)
Moderate		0
Quite a lot		2 (4.9)
Extremely		0
Limited everyday activities due to health problems (past 4 weeks) *n* (%)	41	
Not at all		22 (53.7)
A little		15 (36.6)
Moderate		3 (7.3)
Quite a lot		1 (2.4)
Extremely		0
Physical pain (past 4 weeks) *n* (%)[a]	41	
Not at all		19 (46.3)
A little		19 (46.3)
Moderate		3 (7.3)
Quite a lot		0
Extremely		0
Consultations of general practitioner	41	
M (SD)		2.2 (4.0)
range (number of consultations)		0–25
Impatient admissions	41	
M (SD)		0.1 (0.6)
range (number of admissions)		0–4
Visual impairment *n* (%)	41	
Yes		17 (41.5)
No		24 (58.5)
Visual aids *n* (%)	41	
Yes		15 (36.6)
No		26 (63.4)
Motor skills impairments *n* (%)		
Yes	41	0
No		41 (100)

Note. n = sample size*; M* = mean; *SD* = standard deviation.

[a] does not add up to 100% due to rounded numbers. [b] not presented here: diseases of the participants' parents in the anamnesis.

Figure J2.1

Study Flow Diagram for LifeStress (Taken From Chmitorz, Kurth, et al., 2020)

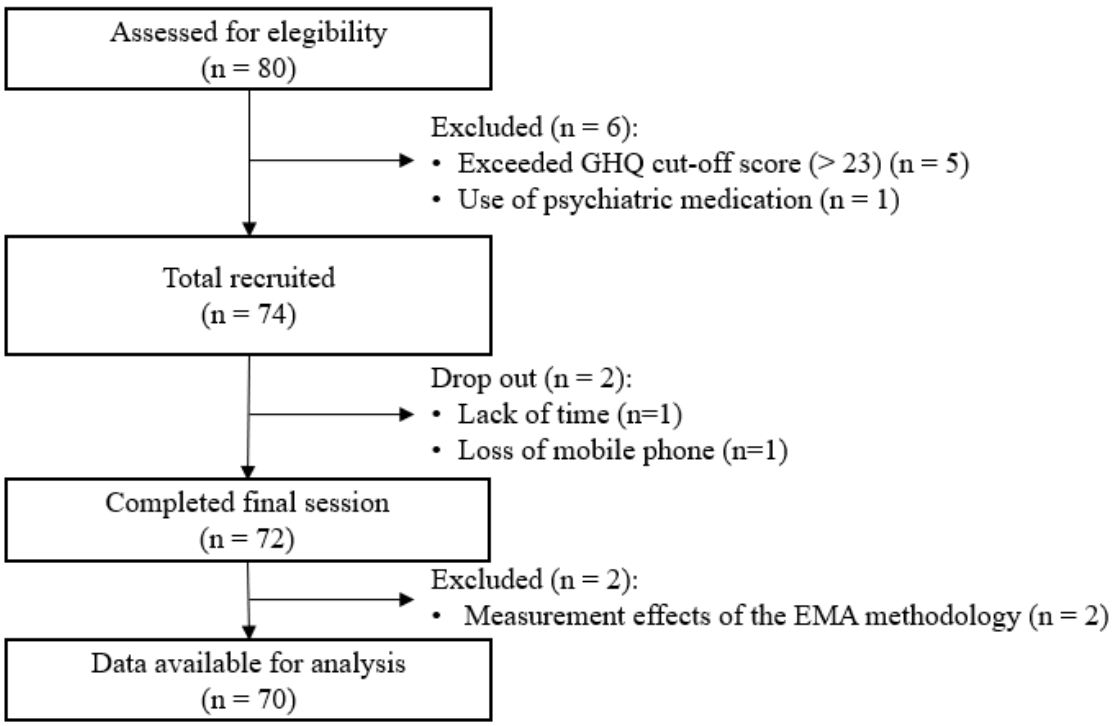


Table J2.4

Sample Description (Short) for LifeStress (N = 70; Taken from Chmitorz, Kurth, et al., 2020)[a]

Variable	Participants
Gender *n* (%)	
Female	41 (59)
Male	29 (41)
Age *M (SD)*	23.9 (3.2)
Nationality *n* (%)	
German	66 (94)
Others	4 (6)
Employment status *n* (%)	
Full-time	5 (7)
Part-time[b]	15 (21)
Others[c]	32 (46)
Not employed	18 (26)

Note. n = sample size*; M* = mean; *SD* = standard deviation.

[a] All participants had a high school diploma (≥ 12 years of formal education) or equivalent. [b] 18–20 hours per week.

[c] occasional jobs, jobs with less than 18 hours per week.

Appendix J3 Descriptive Analysis of Baseline Variables and Covariates (With Comparative/Norm Values)

The descriptive statistics of all baseline variables and covariates were tested for plausibility and are presented in Table J3.1. To control for the macrostressors in the testing of the RQs, a sum score based on the LE Checklist at pre- and posttest was created. Similarly, to determine the total number of microstressors (i.e., before and during the intervention) as covariate, the pre- and posttest values (MIMIS paper-pencil version) and the sum scores of the MIMIS EMA assessment were considered. Concerning optimism (LOT-R), most participants showed average scores at pretest, the same applied to the AS for positive and negative events (ASF-E-P, ASF-E-N; see Table J3.2). Compared to other (representative) German samples, this sample reported higher values of optimism (LOT-R) and empathy (SPF) at pretest, while the number of micro- and macrostressors (MIMIS, LE Checklist) did not differ at baseline (see Table J3.2).

Table J3.1

Descriptive Statistics of Baseline Variables and Covariates

Variables	Pretest		Intervention phase		Posttest	
	M (SD)	range	*M (SD)*	range	*M (SD)*	range
LOT-R (total) range: 0–24	17.8 (3.7)	9–23	/	/	18.6 (3.5)	9–24
SPF range: 16–80	52.4 (7.6)	33–68	/	/	/	/
SDS-17 range: 0–1	0.6 (0.2)	0–0.88	/	/	/	/
ASF-E-P range: 48–336	229.4 (31.9)	160–305	/	/	223.9 (36.8)	134–293
ASF-E-N range: 48–336	178.4 (32.9)	93–248	/	/	178.1 (39.4)	100–289
MIMIS range:0–406	66.2 (22.3)	34–147	/	/	53.5 (22.8)	12–137
MIMIS EMA 1 range: 0–58	/	/	16.4 (7.5)	2–34	/	/
MIMIS EMA 2 range: 0–58	/	/	15.3 (9.8)	0–35	/	/
MIMIS EMA 3 range: 0–58	/	/	15.7 (7.0)	1–37	/	/
LE Checklist range: pre: 0–135 range: post: 0–27	11.4 (5.0)	2–22	/	/	1.05 (1.09)	0–4

Note. M = mean; *SD* = standard deviation; LOT-R = Life Orientation Test-Revised; SPF = Saarbrücker Persönlichkeits-fragebogen zur Messung von Empathie (pretest); SDS-17 = Social Desirability Scale-17 (pretest); ASF-E-P = ASF-E total score

for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; MIMIS = Mainz Inventory of Microstressors; MIMIS EMA = MIMIS EMA assessment; LE Checklist = Life Events Checklist; *N* = 41.

Table J3.2

Descriptive Statistics of Baseline Variables/Covariates With Comparative/Norm Values

LOT-R total score	Pretest	Posttest	Glaesmer et al. (2012) Total sample	Glaesmer et al. (2012)[a] 18–30 y. in total sample
M (SD)	17.8** (3.7)	18.6** (3.5)	15.2 (3.8)	15.8 (3.9)
range	9–23	9–24		
n	41	41	2372	389
T values[b] *n* (%)				
below average (*T* < 40)	2 (4.9%)	2 (4.9%)		
average (*T* = 40–60)	24 (58.5%)	23 (56.1%)		
above average (*T* > 60)	15 (36.6%)	16 (39.0%)		
SPF	Pretest	Posttest	Paulus (2016)[c]	
M (SD)	52.4 (7.6)**		43.3 (7.5)	
range	33–68	/		
n	41		7097	
ASF-E-P total score	Pretest	Posttest		
M (SD)	229.4 (31.9)	223.9 (36.8)		
range	160–305	134–293		
N	41	41		
T values[b,d] *n* (%)				
below average (*T* < 40)	7 (17.1%)	9 (22.0%)		
average (*T* = 40–60)	27 (65.9%)	25 (61.0%)		
above average (*T* > 60)	7 (17.1%)	7 (17.1%)		
ASF-E-N total score	Pretest	Posttest		
M (SD)	178.4 (32.9)	178.1 (39.4)		
range	93–248	100–289		
N	41	41		
T values[b,d] *n* (%)				
below average (*T* < 40)	7 (17.1%)	11 (26.8%)		
average (*T* = 40–60)	30 (73.2%)	25 (61.0%)		
above average (*T* > 60)	4 (9.8%)	5 (12.2%)		
LE Checklist – number of macrostressors	Pretest	Posttest	Chmitorz, Neumann, et al. (2020)[e]	
M (SD)	11.4 (5.0)	1.1 (1.1)**	11.8 (7.1)	
range	2–22	0–4	0–39	
n	41	41	1188	
MIMIS – number of microstressors	Pretest	Posttest	Chmitorz, Neumann, et al. (2020)[e]	
M (SD)	66.2 (22.3)	53.5 (22.8)**	63.7 (27.1)	
range	34–147	12–137	0–175	
n	41	41	1149	

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$. ** $p \leq .01$. *M* = mean; *SD* = standard deviation; *n* = sample size; LOT-R = Life Orientation Test-Revised; SPF = Saarbrücker

Persönlichkeitsfragebogen zur Messung von Empathie; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; LE Checklist = Life Events Checklist.

[a] Glaesmer et al. (2012): representative population survey in Germany (*N* = 2372); independent *t* test between LOT-R total score in TRAIN$_4$Positivity and values of age group 18–30 years.

[b] classification of *T* values based on usual conventions (*M* = 50, *SD* = 10).

[c] Paulus (2016): norm values (*T* values/z values) for different age groups also available, however, no adequate age group to compare with current sample; thus, comparison with descriptive statistics.

[d] norm values published in Poppe et al. (2005).

[e] Chmitorz, Neumann, et al. (2020): descriptive statistics regarding micro- and macrostressors from a German sample of healthy participants (*N* = 1191); data available for *n* = 1188 (macrostressors) and *n* = 1149 (microstressors).

Appendix J4 Descriptive Analysis of Outcome Variables (With Comparative/Norm Values)

The descriptive statistics of the primary and secondary outcomes were also tested for plausibility and are included in Table J4.1. The data of TRAIN$_4$Positiviy were compared to and classified with reference to published norm data or descriptive statistics from other (representative) German samples, if available (see Table J4.2 to Table J4.9). At pretest, the current sample showed higher values of perceived stress (PSS-10 total score), the ability to recover from stress (BRS), and cognitive reappraisal (ERQ: females, CERQ), while lower values were found for negative affect (PANAS), well-being (WHO-5), state anger (STAXI: females) than in other (representative) German samples. Compared to norm values, most participants had average baseline values in the ability to recover from stress (BRS), the subscales of anger expresson (STAXI) and a percentile rank of 11–50 in well-being (WHO-5).

Table J4.1

Descriptive Statistics of Outcome Variables

Variables	Pretest		Posttest	
	M (SD)	range	*M (SD)*	range
PSS-10 total score range: 0–40	15.3 (5.0)	4–26	13.7 (4.2)	5–25
MIMIS perceived severity range: 0–232	44.2 (24.2)	9–130	27.7 (16.9)	5–85
GHQ-28 total score range: 0–84	14.7 (4.0)	2–23	15.9 (5.3)	6–30
BRS range: 1–5	3.7 (0.6)	2.2–4.8	3.7 (0.6)	2.2–4.7
PANAS-PA range: 10–50	29.2 (6.4)	18–45	29.4 (7.0)	19–49
PANAS-NA range: 10–50	12.2 (3.0)	10–25	12.1 (2.6)	10–22
WHO-5 range: 0–100	61.9 (15.2)	20–84	58.2 (16.3)	28–80
STAXI state anger range: 10–40	10.8 (1.7)	10–19	10.9 (1.6)	10–19
STAXI anger in range: 8–32	15.1 (4.2)	8–25	15.1 (3.5)	8–24
STAXI anger out range: 8 – 32	12.6 (6.8)	8–51	11.0 (2.4)	8–17
STAXI anger control range: 8–32	20.8 (3.6)	13–28	21.4 (3.2)	16–31
ERQ reappraisal range: 1–7	4.8 (1.1)	1.5–6.7	4.8 (0.9)	2.2–6.3
CERQ positive reappraisal range: 3–15	10.0 (2.9)	5–15	10.1 (2.9)	4–15

Variables	Pretest		Posttest	
	M (SD)	range	*M (SD)*	range
AAT-CS (*n* = 39)	0.5 (66.1)	-242–142	42.4 (129.1)	-459–327

Note. *M* = mean; *SD* = standard deviation; PSS-10 = Perceived Stress Scale-10; MIMIS = Mainz Inventory of Microstressors; GHQ-28 = General Health Questionnaire-28; BRS = Brief Resilience Scale; PANAS = Positive and Negative Affect Schedule (NA = negative affect; PA = positive affect); WHO-5 = Well-being Index; STAXI = State-Trait Anger Expression Inventory; ERQ = Emotion Regulation Questionnaire; CERQ = Cognitive Emotion Regulation Questionnaire; AAT-CS = AAT Compatibility Score; EMA-assessed outcomes (see RQ4) not presented due to large number of assessments; *N* = 41.

Table J4.2

Descriptive Statistics of PSS-10 With Comparative/Norm Values

PSS-10	Pretest			Posttest			Klein et al. (2016)[a]		
	pse	ph	t	pse	ph	t	pse	ph	t
M	4.7**	10.7**	15.3**	4.6**	9.2**	13.7	7.6	5.8	12.7
(SD)	(2.0)	(3.7)	(5.0)	(1.9)	(3.1)	(4.2)	(4.7)	(3.7	(6.7)
range	0–8	3–19	4–26	0–9	2–18	5–25			
n	41	41	41	41	41	41	298	298	2463

	Pretest		Posttest		Klein et al. (2016)[a] 20–29 y. (*n* = 298)	
	pse	ph	pse	ph	pse	ph
percentiles						
5%	1	4.2	1.1	3.2	0	1
25%	4	8	3.5	7	3	4
50%	5	10	5	10	5	7
75%	6	12	5.5	11	8	11
95%	7	18	8.9	13	13	16

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$. PSS-10 = Perceived Stress Scale-10; *M* = mean; *SD* = standard deviation; *n* = sample size; pse = PSS-10 subscale perceived self-efficacy; ph = PSS-10 subscale perceived helplessness; t = PSS-10 total score.

[a] Klein et al. (2016): representative, randomly selected German sample (*N* = 2463); ph and pse values from Klein et al. (2016) refer to the age group 20–29 years in the total sample (12.1% of the total sample; *n* = 298); total score from Klein et al. (2016) refers to the age group 20–39 years in the total sample.

Table J4.3

Descriptive Statistics (Detailed With Subscales) of GHQ-28

GHQ-28	Pretest	Posttest
Total score		
M (SD)	14.7 (4.3)	15.9 (5.3)
range	2–23	6–30
n	41	41
Somatic symptoms		
M (SD)	4.4 (1.9)	4.5 (2.3)
range	1–9	1–10
n	41	41
Anxiety symptoms and insomnia		
M (SD)	3.4 (2.5)	4.3 (2.6)
range	0–10	0–10
n	41	41
Social dysfunction		
M (SD)	6.6 (1.6)	6.6 (1.8)
range	1–9	2–11
n	41	41
Severe depression		
M (SD)	0.2 (0.5)	0.5 (0.9)
range	0–2	0–3
n	41	41

Note. GHQ-28 = General Health Questionnaire-28; *M* = mean; *SD* = standard deviation; *n* = sample size; all participants below cut-off of 23 due to inclusion criteria of TRAIN$_4$Positivity.

Table J4.4

Descriptive Statistics of BRS With Comparative/Norm Values

BRS	Pretest	Posttest	Kunzler, Chmitorz, et al. (2018)[a]
M (SD)	3.7** (0.6)	3.7** (0.6)	3.4 (1.0)
range	2.2–4.8	2.2–4.7	
n	41	41	1128
Stanine values[b] *n* (%)			
below average (Stanine < 3)	/	/	
average (Stanine = 3–7)	37 (90.2%)	37 (90.2%)	
above average (Stanine > 7)	4 (9.8%)	4 (9.8%)	

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$. BRS = Brief Resilience Scale; *M* = mean; *SD* = standard deviation; *n* = sample size.

[a] Kunzler, Chmitorz, et al. (2018): representative German sample; age-specific norm values available but not for age group in this sample (range: 20–30 years); therefore, comparison with total sample.

[b] classification of Stanine values according to usual conventions (*M* = 5, *SD* = 2).

Table J4.5

Descriptive Statistics of PANAS With Comparative Values

PANAS-PA positive affect	Pretest	Posttest	Krohne et al. (1996)[a]
M (SD)	29.2 (6.4)	29.4 (7.0)	27.4 (6.4)
range	18–45	19–49	
n	41	41	349
PANAS-NA negative affect	Pretest	Posttest	Krohne et al. (1996)[a]
M (SD)	12.2** (3.0)	12.1** (2.6)	14.7 (5.2)
range	10–25	10–22	
n	41	41	349

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$.

PANAS-PA = Positive and Negative Affect Schedule, positive affect subscale; *M* = mean; *SD* = standard deviation; *n* = sample size; PANAS-NA = Positive and Negative Affect Schedule, negative affect subscale.

[a] Krohne et al. (1996): examination of the German PANAS in sample of students (*N* = 349) since values in representative German sample not available for instruction "in the moment", as used in this study.

Table J4.6

Descriptive Statistics of WHO-5 With Comparative/Norm Values

WHO-5 (linear transformation; 0–100)	Pretest	Posttest	Brähler et al. (2007)[a] ≤ 40 years
M (SD)	61.9** (15.2)	58.2** (16.3)	73.4 (19.2)
range	20–84	28–80	
n	41	41	929
WHO-5 (0–25) *PR*[b]	Pretest	Posttest	
≤ 10	6	12	
11–50	31	24	
> 50	4	5	

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$.

WHO-5 = Well-being Index; *M* = mean; *SD* = standard deviation; *n* = sample size; *PR* = percentile rank.

[a] Brähler et al. (2007): representative German sample (*N* = 2473); comparison with age group ≤ 40 years (mean and SD of linearly transformed index value; 0–100).

[b] percentile ranks identified from Brähler et al. (2007) for the age group ≤ 40 years (raw scores, 0–25).

Table J4.7

Descriptive Statistics of STAXI With Comparative/Norm Values

STAXI – state anger	Pretest		Posttest		Schwenkmezger et al. (1992); norm sample of students[a]	
	m	f	m	f	m	f
M (SD)	11.4 (1.9)	10.6 (1.7)**	11.5 (3.1)	10.7 (1.0)**	12.4 (3.9)	12.2 (3.8)
range	10–15	10–19	10–19	10–13		
n	8	33	8	33	249	202

STAXI – anger in	Pretest	Posttest
M (SD)	15.1 (4.2)	15.1 (3.5)
range	8–25	8–24
n	41	41
Stanine values[b] *n* (%)		
below average (Stanine < 3)	3 (7.3%)	5 (12.2%)
average (Stanine = 3–7)	33 (80.5%)	34 (82.9%)
above average (Stanine > 7)	5 (12.2%)	2 (4.9%)

STAXI – anger out	Pretest	Posttest
M (SD)	12.6 (6.8)	11.0 (2.4)
range	8–51	8–17
n	41	41
Stanine values[b] *n* (%)		
below average (Stanine < 3)	8 (19.5%)	5 (12.2%)
average (Stanine = 3–7)	32 (78.0%)	36 (87.8%)
above average (Stanine > 7)	1 (2.4%)	/

STAXI – anger control	Pretest	Posttest
M (SD)	20.76 (3.62)	21.44 (3.20)
range	13–28	16–31
n	41	41
Stanine values[b] *n* (%)		
below average (Stanine < 3)	4 (9.8%)	1 (2.4%)
average (Stanine = 3–7)	37 (90.2%)	39 (95.1%)
above average (Stanine > 7)	/	1 (2.4%)

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$.

STAXI = State-Trait Anger Expression Inventory; *M* = mean; *SD* = standard deviation; *n* = sample size; m= male; f = female; trait anger not presented as not relevant for this study.

[a] Schwenkmezger et al. (1992): means and SDs for male and female participants in a norm sample of students (*N* = 451); norm values (Stanine values) only available for the subscales trait anger (not relevant here), anger in, anger out, and anger control.

[b] classification of Stanine values according to usual conventions (*M* = 5, *SD* = 2).

Table J4.8

Descriptive Statistics of ERQ With Comparative Values

ERQ	Pretest		Posttest		Abler and Kessler (2009)[a]	
	m	f	m	f	m	f
M (SD)	4.3 (1.4)	4.9** (0.9)	4.7* (0.7)	4.9** (0.9)	4.1 (1.1)	4.2 (0.9)
range	1.5–6	2.7–6.7	3.7–5.7	2.2–6.3		
n	8	33	8	33	61	113

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$.

ERQ = Emotion Regulation Questionnaire; *M* = mean; *SD* = standard deviation; *n* = sample size; m = male; f = female.

[a] Abler and Kessler (2009): values in a German sample of students (*N* = 174); values in a representative German sample exist, but not available (conference abstract).

Table J4.9

Descriptive Statistics of CERQ With Comparative Values

CERQ	Pretest	Posttest	Loch et al. (2011)[a]
M (SD)	10.0** (2.9)	10.1** (2.9)	7.2 (3.0)
range	5–15	4–15	
n	41	41	414

Note. Independent *t* tests between TRAIN$_4$Positivity and respective comparative values (two-tailed): * $p \leq .05$; ** $p \leq .01$.

CERQ = Cognitive Emotion Regulation Questionnaire; *M* = mean; *SD* = standard deviation; *n* = sample size.

[a] Loch et al. (2011): randomly selected participants in the German validation study of the CERQ (*N* = 414).

Appendix J5 Testing of Assumptions of Statistical Analyses

Despite some violations of the assumptions of statistical analyses (e.g., outlier in predictor variables for multiple linear regression analyses), the prerequisites were largely viewed as fulfilled and the respective analyses were nevertheless performed. This decision was also taken due to the pilot character of this (feasibility) study and the small sample size.

Table J5.1

Results of the Testing of Assumptions of Statistical Analyses – RQ1 (Hypothesis 1.1)

RQ1/hypothesis 1.1	Results of the testing of assumptions
Paired *t* test (PSS-10)	
1. normally distributed difference scores of observed pairs	Normality assumption for the difference score of PSS-10 of observed pairs was fulfilled based on the Shapiro-Wilk test (Shapiro & Wilk, 1965; *p* = .25).
2. independence of observations within a sample (i.e., time point) (Bortz & Schuster, 2010; Eid et al., 2017)	not testable, but could be assumed based on single assessments for each participant at each time point
Multiple linear regression analysis (four predictors)	
1. linearity	see Appendix K5, Table K5.1
2. reliability of predictors	- Concerning the reliability of the two predictors of stressor exposure (MIMIS, LE Checklist), it must be considered for the current and all following regression analyses that there is no reliability measure available, as the calculation of internal consistency for these two measures does not seem adequate. This prevented the testing of the reliability prerequisite for these two predictors in this study. - for ASF-E-P and ASF-E-N: see Appendix K5, Table K5.1
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - influential datapoints: A single standardized DfBETAS value (MIMIS: |1.12|) slightly exceeded the suggested threshold of |1| (Cohen et al., 2003). However, because of different suggestions in the literature (critical DfBETAS values > |2|; Stevens, 2009) and the pilot character of the study as well as since the remaining assumptions were fully given, the regression analysis was conducted.
4. no multicollinearity	see Appendix K5, Table K5.1
5. homoscedasticity	see Appendix K5, Table K5.1
6. normality of residuals	see Appendix K5, Table K5.1
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.1; Durbin-Watson test statistic: 2.45; slightly above the critical value of 2; as the study served as feasibility study and given the small sample size, the regression analysis was nevertheless conducted

Note. *p* = *p* value; PSS-10 = Perceived Stress Scale-10; MIMIS = Mainz Inventory of Microstressors; LE Checklist = Life Events

Checklist; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events.

Table J5.2

Results of the Testing of Assumptions of Statistical Analyses – RQ1 (Exploratory RQ 1.2)

RQ1 (exploratory RQ 1.2)	
Independent sample *t* test (with matched data)	
1. continuous outcome	perceived severity of microstressors as continuous variable (interval level)
2. independent variable with two categorical, independent groups	treatment status with two independent conditions (TRAIN$_4$Positivity condition vs. LifeStress)
3. independence of observations (i.e., no relationship between observations within each group or between groups themselves)	- ensured by LifeStress being a historical control group - different participants within each group with no participant being in more than one group
4. no significant outliers	Box-whisker plots for dependent variable (perceived severity of microstressors) and metric covariates (age, GHQ-28, MIMIS, LE Checklist) indicate outliers for perceived severity of microstressors (1 outlier), GHQ-28 (1 outlier), MIMIS (1 outlier) and LE Checklist (3 outliers); however, relatively few outliers and those did not result from data entry errors; due to the pilot character of this study, the outliers were remained in the analysis
5. normal distribution of the dependent variable (i.e., perceived severity of microstressors) for each group of the independent variable (i.e., within each treatment condition)	Normality assumption for perceived severity of microstressors is fulfilled in the LifeStress (CG) group based on the Shapiro-Wilk test (Shapiro & Wilk, 1965; p = .36); no normality in the TRAIN$_4$Positivity (IG) group (p = .005); however, independent *t* test is robust against violations of normality if the sample size is ≥ 30 in both groups which was the case in this study (IG: n = 30; CG: n = 31)
6. homogeneity of variances	Levene's test: the null hypothesis (variances are equal) cannot be rejected (p = .18), that is, homogeneous variances in the two groups
(Bortz & Schuster, 2010; Field, 2015)	

Note. GHQ-28 = General Health Questionnaire-28; MIMIS = Mainz Inventory of Microstressors; LE Checklist = Life Events Checklist; p = p value; IG = intervention group; CG = control group.

Table J5.3

Results of the Testing of Assumptions of Statistical Analyses – RQ2 (Hypothesis 2.1)

RQ2/hypothesis 2.1	Results of the testing of assumptions
Paired *t* test (AAT-CS)	
1. normally distributed difference scores of observed pairs	Normality assumption for the difference of pre- and posttest scores in the AAT-CS was fulfilled based on the Shapiro-Wilk test (Shapiro & Wilk, 1965; p = .95).
2. independence of observations within a sample (i.e., time point)	not testable, but could be assumed based on single assessments for each participant at each time point
(Bortz & Schuster, 2010; Eid et al., 2017)	
Multiple linear regression analysis (predictors)	

RQ2/hypothesis 2.1	Results of the testing of assumptions
1. linearity	see Appendix K5, Table K5.2
2. reliability of predictors	No reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N: see Appendix K5, Table K5.2
3. no outliers or influential data points	- outliers in predictors: centered leverage score .40, that is, above threshold of .29 in parsimonious model with four predictors - influential datapoints: standardized DfBETAS value for the ASF-E-N at pretest (105) was >|1| (ASF-E-N: |1.42|) and could be considered as critical (Cohen et al., 2003). However, as the participant also showed an increased value at posttest (111) and the value seemed plausible as well as due to different recommendations in the literature (critical DfBETAS values > |2|; Stevens, 2009), the participant was nevertheless considered in the analysis. Overall, based on the pilot character of this study, the assumptions of multiple linear regression were largely considered as fulfilled and the analysis was performed.
4. no multicollinearity	see Appendix K5, Table K5.2
5. homoscedasticity	see Appendix K5, Table K5.2
6. normality of residuals	see Appendix K5, Table K5.2
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.2; Durbin-Watson test statistic: 1.50 (no auto-correlation is indicated by values around 2)

Note. p = p value; AAT-CS = Approach Avoidance Task Compatibility Score; MIMIS = Mainz Inventory of Microstressors; LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events.

Table J5.4

Results of the Testing of Assumptions of Statistical Analyses – RQ2 (Hypothesis 2.2)

RQ2/hypothesis 2.2	Results of the testing of assumptions
Three-way (2 x 2 x 2) repeated-measures (RM) analysis of variance	
1. continuous dependent variable	AAT-CS as metric variable
2. independent observations within a time point	ensured by the design of TRAIN$_4$Positivity
3. normality of dependent variable for each combination of factor levels	- normality assumption for the dependent variable tested using the Shapiro-Wilk test for each combination of factor levels (i.e., in all groups at both time points) - pretest: AAT-CS normally distributed for the group with high values of ASF-E-N (p = .70) - posttest: AAT-CS normally distributed in the group with high ASF-E-N (p = .69) and ASF-E-P scores (p = .11) - for other group-time combinations, normality assumption not fulfilled (see Appendix K8) - However, based on the central limit theorem, normality can be assumed for a sample of at least 30 participants (here: N = 41) and the RM-ANOVA

RQ2/hypothesis 2.2	Results of the testing of assumptions
	is relatively robust against violations (Bortz & Schuster, 2010)
4. homogeneous variances and homogeneous variance-covariance matrices between the levels of the non-repeated factor(s)	- homogeneous variances: given based on Levene's test (AAT-CS pretest: p = .22; AAT-CS posttest: p = .62) - homogeneous variance-covariance matrices: Box test ○ Box test showed a significant result (p = .05), which indicates heterogeneous covariances (Eid et al., 2017). ○ However, the Box test is very sensitive for violations of normality, resulting in significant findings, even in case of homogeneous covariance matrices (Eid et al., 2017). ○ Thus, the violation of the normality assumption for several cases in this study might also have led to the significant Box test. ○ Based on recommendations to test the homogeneity of covariances using a significance level of p = .025 (Mertler & Vannatta, 2005), the homogeneity assumption was also fulfilled in the current study.
5. equal variance of differences between the groups (sphericity) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	due to only two levels of the repeated factor, sphericity can be viewed as fulfilled and testing of sphericity not relevant (Eid et al., 2017; Field, 2015)

Note. AAT-CS = Approach Avoidance Task Compatibility Score; p = p value; ASF-E-N = ASF-E total score for attributions of negative events; ASF-E-P = ASF-E total score for attributions of positive events; RM-ANOVA = repeated-measures analysis of variance; n = sample size.

Table J5.5

Results of the Testing of Assumptions of Statistical Analyses – RQ3

RQ3	Results of the testing of assumptions
Paired t tests (GHQ-28, BRS, WHO-5)	
1. normally distributed difference scores of observed pairs	- normality assumption for the difference scores (post-pre) for the BRS (p = .85) and the WHO-5 (p = .32) was fulfilled based on the Shapiro-Wilk test (Shapiro & Wilk, 1965) - not fulfilled for the GHQ-28 (p = .001); therefore, Wilcoxon signed-rank test (non-parametric test) for this outcome
2. independence of observations within a sample (i.e., time point) (Bortz & Schuster, 2010; Eid et al., 2017)	not testable, but could be assumed based on single assessments for each participant at each time point
Multiple linear regression analysis (GHQ-28; only two predictors for stressor exposure)	
1. linearity	see Appendix K5, Table K5.3
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist
3. no outliers or influential data points	- outliers in predictors: centered leverage score .34, that is, above threshold of .15 in a parsimonious

RQ3	Results of the testing of assumptions
	model with two predictors - outliers in the dependent variable: studentized deleted residual indicated a single outlier in the dependent variable (|3.23|), which slightly exceeded the recommended value of |3| (Eid et al., 2017) - influential datapoints: A single standardized DfBETAS value (MIMIS: |1.54|) exceeded the suggested threshold of 1 (Cohen et al., 2003). However, because of different suggestions in the literature (critical DfBETAS values > |2|; Stevens, 2009) and the pilot character of the study with a small sample size, the respective participant was included and the regression analysis was computed.
4. no multicollinearity	see Appendix K5, Table K5.3
5. homoscedasticity	see Appendix K5, Table K5.3
6. normality of residuals	see Appendix K5, Table K5.3
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.3; Durbin-Watson test statistic: 2.24 (no auto-correlation is indicated by values around 2)
Multiple linear regression analysis (GHQ-28; four predictors)	
1. linearity	see Appendix K5, Table K5.4
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N: see Appendix K5, Table K5.4
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - outliers in the dependent variable: studentized deleted residual indicated a single outlier in the dependent variable (|3.24|), which slightly exceeded the recommended value of |3| (Eid et al., 2017) - influential datapoints: Three standardized DfBETAS values (MIMIS: |1.26|, LE Checklist: |1.11|, ASF-E-N: |1.82|) exceeded the suggested threshold of |1| (Cohen et al., 2003). However, because of different suggestions in the literature (critical DfBETAS values > |2|; Stevens, 2009) and the pilot character of the study with a small sample size, the respective participant was included and the regression analysis was computed.
4. no multicollinearity	see Appendix K5, Table K5.4
5. homoscedasticity	see Appendix K5, Table K5.4
6. normality of residuals	see Appendix K5, Table K5.4
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.4; Durbin-Watson test statistic: 2.20 (no auto-correlation is indicated by values around 2)
Multiple linear regression analysis (BRS; three predictors, without microstressors)	
1. linearity	see Appendix K5, Table K5.5; partial regression plot indicated a non-linear association between the number of microstressors (MIMIS) and the BRS difference score, therefore, this predictor was deleted from the analysis

RQ3	Results of the testing of assumptions
2. reliability of predictors	no reliability measure available for LE Checklist; for ASF-E-P and ASF-E-N see Appendix K5, Table K5.5
3. no outliers or influential data points	- outliers in predictors: centered leverage score .25, that is, above threshold of .22 in a parsimonious model with three predictors - influential datapoints: A single standardized DfBETAS value (ASF-E-P: |1.07|) slightly exceeded the suggested threshold of |1| (Cohen et al., 2003). However, because of different suggestions in the literature (critical DfBETAS values > |2|; Stevens, 2009), the pilot character of the study with a small sample size, and in consistence with the procedure chosen for other outcomes (e.g., PSS-10; Table J5.1), the respective participant was included and the regression analysis was computed.
4. no multicollinearity	see Appendix K5, Table K5.5
5. homoscedasticity	see Appendix K5, Table K5.5
6. normality of residuals	see Appendix K5, Table K5.5
7. independence of residuals (i.e., statistical insignificance of auto-correlation (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.5; Durbin Watson test statistic: 1.85 (no auto-correlation is indicated by values around 2)
Multiple linear regression analysis (WHO-5; four predictors)	
1. linearity	see Appendix K5, Table K5.6
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N: see Appendix K5, Table K5.6
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors
4. no multicollinearity	see Appendix K5, Table K5.6
5. homoscedasticity	see Appendix K5, Table K5.6
6. normality of residuals	see Appendix K5, Table K5.6
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.6; Durbin-Watson test statistic: 1.60 (no auto-correlation is indicated by values around 2)

Note. GHQ-28 = General Health Questionnaire-28; BRS = Brief Resilience Scale; WHO-5 = Well-being Index; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events.

Table J5.6

Results of the Testing of Assumptions of Statistical Analyses – RQ4

Multilevel analyses (RQ4), current mood subscales (for each of the outcomes, *n* = 6 participants with less than three observations excluded; i.e., *n* = 35)	
1. for 2-level models, not aiming at analyzing cross-level interactions, at least *n* = 30 units on the highest level required	in this study: *N* = 41
2. linear change of the outcomes over time for all subjects	scatter plots indicate linear change of positive valence, energetic arousal, and calmness over time (see Appendix K7.2)
3. normal distribution of residuals at all levels	normality assumption fulfilled based on graphical analysis of standardized residuals and histograms regarding the distribution of residuals at all levels (level-1 residuals, residuals for random slopes, and for random intercepts; see Appendix K7.3)
4. homogeneity of residual variances (homoscedasticity; i.e., here: equal variances of level-1 residuals at each level of level-1 predictor)	scatter plots of (standardized) residuals and predicted/fitted values indicated homoscedasticity, respectively (see Appendix K7.4)
(Bell et al., 2019; Göllner et al., 2010; Nezlek et al., 2006; Singer & Willett, 2003)	
Multilevel analyses (RQ4), end-of-day measures for each of the outcomes, *n* = 8 participants with less than three observations excluded; i.e., *n* = 33)	
1. for 2-level models, not aiming at analyzing cross-level interactions, at least *n* = 30 units on the highest level required	in this study: *N* = 41
2. linear change of the outcomes over time for all subjects	scatter plots indicate linear change of well-being, the ability to distance from negative stimuli, and the ability to get affected by positive stimuli over time (see Appendix K7.2)
3. normal distribution of residuals at all levels	normality assumption fulfilled based on visual analysis of standardized residuals and histograms regarding the distribution of residuals at all levels (level-1 residuals, residuals for random slopes, and for random intercepts; see Appendix K7.3)
4. homogeneity of residual variances (homoscedasticity; i.e., here: equal variances of level-1 residuals at each level of level-1 predictor)	scatter plots of (standardized) residuals and predicted/fitted values indicated homoscedasticity, respectively (see Appendix K7.4)
(Bell et al., 2019; Göllner et al., 2010; Nezlek et al., 2006; Singer & Willett, 2003)	

Note. n = sample size.

Table J5.7

Results of the Testing of Assumptions of Statistical Analyses – RQ5

RQ5	Results of the testing of assumptions
Paired *t* tests (ERQ, CERQ, PANAS-PA, PANAS-NA, STAXI-state anger, STAXI-anger in, STAXI-anger out, STAXI-anger control)	
1. normally distributed difference scores of observed pairs	normality assumption for the difference scores (post-pre) fulfilled (i.e., *p*s > .05) based on the Shapiro-Wilk test (Shapiro & Wilk, 1965) for all but

RQ5	Results of the testing of assumptions
	three outcomes (positive affect: $p = .01$; state anger: $p < .001$; anger out: $p < .001$); therefore, Wilcoxon signed-rank test (non-parametric test) for these outcomes
2. independence of observations within a sample (i.e., time point)	not testable, but could be assumed based on single assessments for each participant at each time point
(Bortz & Schuster, 2010; Eid et al., 2017)	
Multiple linear regression analysis (ERQ reappraisal; four predictors)	
1. linearity	see Appendix K5, Table K5.7
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.7
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - outliers in the dependent variable: maximum score of the studentized deleted residual (|3.67|) indicated a single outlier in the dependent variable (above recommended value of |3| (Eid et al., 2017), with the respective participant having a smaller ERQ value at pretest compared to other individuals
4. no multicollinearity	see Appendix K5, Table K5.7
5. homoscedasticity	see Appendix K5, Table K5.7
6. normality of residuals	see Appendix K5, Table K5.7
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	see Appendix K5, Table K5.7; Durbin Watson test statistic: 2.04 (no auto-correlation is indicated by values around 2)
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	
Multiple linear regression analysis (CERQ positive reappraisal; four predictors)	
1. linearity	see Appendix K5, Table K5.8
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.8
3. no outliers or influential data points	outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors
4. no multicollinearity	see Appendix K5, Table K5.8
5. homoscedasticity	see Appendix K5, Table K5.8
6. normality of residuals	see Appendix K5, Table K5.8
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	see Appendix K5, Table K5.8; Durbin-Watson test statistic: 2.09 (no auto-correlation is indicated by values around 2)
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	
Multiple linear regression analysis (PANAS-PA; four predictors)	
1. linearity	see Appendix K5, Table K5.9
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.9
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - outliers in the dependent variable: studentized deleted residual (|5.19| above suggested threshold of |3| (Eid et al., 2017); however, due to the pilot character of this study, the

RQ5	Results of the testing of assumptions
	small sample size, and the meeting of the other assumptions of multiple regression, the respective participants were still considered, and the regression analysis was conducted.
4. no multicollinearity	see Appendix K5, Table K5.9
5. homoscedasticity	see Appendix K5, Table K5.9
6. normality of residuals	see Appendix K5, Table K5.9
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	see Appendix K5, Table K5.9; Durbin-Watson test statistic: 1.59 (no auto-correlation is indicated by values around 2)
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	
Multiple linear regression analysis (PANAS-NA; four predictors)	
1. linearity	see Appendix K5, Table K5.10
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.10
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - influential datapoints: A single standardized DfBETAS value (LE Checklist: |1.21|) was larger than the recommended threshold of |1| (Cohen et al., 2003); however, based on the small sample size, the exploratory design of this study, different suggestions in the literature for DfBETAS (critical DfBETAS values > |2|; Stevens, 2009) as well as consistent with the methods chosen for other outcomes (e.g., PSS-10; Table J5.1), this value was still considered in the analysis.
4. no multicollinearity	see Appendix K5, Table K5.10
5. homoscedasticity	see Appendix K5, Table K5.10
6. normality of residuals	see Appendix K5, Table K5.10
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	see Appendix K5, Table K5.10; Durbin Watson test statistic: 1.61 (no auto-correlation is indicated by values around 2)
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	
Multiple linear regression analysis (STAXI state anger; four predictors)	
1. linearity	see Appendix K5, Table K5.11
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.11
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - outliers in dependent variable: studentized deleted residual indicated one outlier (|5.43|) above suggested threshold of |3| (Eid et al., 2017); however, due to the pilot character of this study and as the remaining assumptions were fulfilled, the regression analysis was performed including this outlier
4. no multicollinearity	see Appendix K5, Table K5.11
5. homoscedasticity	see Appendix K5, Table K5.11
6. normality of residuals	see Appendix K5, Table K5.11
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	see Appendix K5, Table K5.11; Durbin-Watson test statistic: 2.49; slightly above the critical value of 2; as the study served as feasibility study and given the small sample size, the regression analysis was

RQ5	Results of the testing of assumptions
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	nevertheless conducted
Multiple linear regression analysis (STAXI anger in; four predictors)	
1. linearity	see Appendix K5, Table K5.12
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.12
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - outliers in dependent variable: studentized deleted residual indicated one outlier (|3.25) above suggested threshold of |3| (Eid et al., 2017)
4. no multicollinearity	see Appendix K5, Table K5.12
5. homoscedasticity	see Appendix K5, Table K5.12
6. normality of residuals	see Appendix K5, Table K5.12
7. independence of residuals (i.e., statistical insignificance of auto-correlation)	see Appendix K5, Table K5.12; Durbin Watson test statistic: 1.71 (no auto-correlation is indicated by values around 2)
(Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	
Multiple linear regression analysis (STAXI anger out; four predictors; *n* = 40)	➢ several assumptions violated due to one participant (see Appendix K5, Table K5.13); studentized deleted residual (|19.38|), which indicates outlier in the dependent variable, and the DfBETAS scores for the four predictors for this participant clearly exceeded the suggested critical values of |3| or |2|, respectively. ➢ To meet the assumptions of the multiple linear regression, the respective participant was excluded from the analysis
1. linearity	see Appendix K5, Table K5.14
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.14
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors - outliers in dependent variable: studentized deleted residual below threshold (|2.91|) - influential datapoints: DfBETAS scores below |2| for all predictors
4. no multicollinearity	see Appendix K5, Table K5.14
5. homoscedasticity	see Appendix K5, Table K5.14
6. normality of residuals	see Appendix K5, Table K5.14
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.14; Durbin-Watson test statistic (2.31) slightly above the critical value of 2; as the study served as feasibility study and given the small sample size, the regression analysis was nevertheless conducted
Multiple linear regression analysis (STAXI anger control; four predictors)	
1. linearity	see Appendix K5, Table K5.15
2. reliability of predictors	no reliability measures available for MIMIS and LE Checklist; for ASF-E-P and ASF-E-N, see Appendix K5, Table K5.15
3. no outliers or influential data points	- outliers in predictors: centered leverage score .37, that is, above threshold of .29 in parsimonious model with four predictors
4. no multicollinearity	see Appendix K5, Table K5.15

RQ5	Results of the testing of assumptions
5. homoscedasticity	see Appendix K5, Table K5.15
6. normality of residuals	see Appendix K5, Table K5.15
7. independence of residuals (i.e., statistical insignificance of auto-correlation) (Bortz & Schuster, 2010; Eid et al., 2017; Field, 2015)	see Appendix K5, Table K5.15; Durbin-Watson test statistic: 2.16 (no auto-correlation is indicated by values around 2

Note. ERQ = Emotion Regulation Questionnaire; CERQ = Cognitive Emotion Regulation Questionnaire; PANAS = Positive and Negative Affect Schedule (PA = positive affect; NA = negative affect); STAXI = State-Trait Anger Expression Inventory; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events.

Table J5.8

Results of the Testing of Assumptions of Statistical Analyses – RQ6

RQ6	Results of the testing of assumptions
Bivariate Pearson correlations	
1. linearity	scatter plots (Field, 2015) for the bivariate associations (see Appendix J10) indicate linearity
2. continuous variables	metric scale levels fulfilled for all variables (LOT-R total score, LOT-R optimism score, ASF-E-P, ASF-E-N, BRS, PSS-10, AAT-CS)
3. bivariate normal distribution	inspection of univariate normal distribution of the single variables using the Shapiro Wilk test (Bortz & Schuster, 2010): - significant results in the Shapiro Wilk test for the LOT-R optimism score (*p* = .002) and the AAT-CS (*p* = .01) at pretest pointed to the lack of univariate normal distribution; however, according to the central limit theorem, the normality of data can be also assumed based on a sample size of above 30 participants (here: *n* = 39 for all correlations with AAT-CS) - For the remaining variables (LOT-R total score, ASF-E-P, ASF-E-N, BRS, PSS-10), the normality assumption was supported (all *p*s > .05).

Note. LOT-R = Life Orientation Test-Revised; ASF-E-P = ASF-E total score for attributions of positive events; ASF-E-N = ASF-E total score for attributions of negative events; BRS = Brief Resilience Scale; PSS-10 = Perceived Stress Scale-10; AAT-CS = Approach Avoidance Task Compatibility Score; *p* = *p* value.

Appendix J6 Detailed Results of Paired *t* Tests and Wilcoxon Signed-Rank Tests

Table J6.1

Results of Paired t Tests for Perceived Stress (PSS-10) for RQ1 (Hypothesis 1.1)

	M_{pre}	SD_{pre}	M_{post}	SD_{post}	*t(40)*	*p*	Cohen's *d*
Perceived stress (PSS-10)	15.3	5.0	13.7	4.2	–2.28	.03	–0.36

Note. PSS-10 = Perceived Stress Scale-10; *M* = mean; *SD* = standard deviation; *t* = *t* value (degrees of freedom in parentheses); *p* = *p* value; Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed); *d* = Cohen's delta (effect size); *N* = 41.

Table J6.2

Results of Paired t Tests for the AAT-CS for RQ2 (Hypothesis 2.1)

	M_{pre}	SD_{pre}	M_{post}	SD_{post}	*t(38)*	*p*	Cohen's *d*
AAT Compatibility Score (AAT mouse version)	0.5	66.1	42.4	129.1	2.31	.03	0.37

Note. AAT = Approach Avoidance Task; *M* = mean; *SD* = standard deviation; *t* = *t* value (degrees of freedom in parentheses); *p* = *p* value; Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed); *d* = Cohen's delta (effect size); *n* = 39.

Table J6.3

Results of Paired t Tests for the Ability to Recover From Stress (BRS), and Well-Being (WHO-5) for RQ3

	M_{pre}	SD_{pre}	M_{post}	SD_{post}	*t(40)*	*p*	Cohen's *d*
Ability to recover from stress (BRS)	3.7	0.6	3.7	0.6	–0.15	.88	–.02
Well-being (WHO-5)	61.9	15.2	58.2	16.3	–1.40	.17	–0.22

Note. BRS = Brief Resilience Scale; WHO-5 = Well-being Index; *M* = mean; *SD* = standard deviation; *t* = *t* value (degrees of freedom in parentheses); *p* = *p* value; Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed); *d* = Cohen's delta (effect size); *N* = 41.

Table J6.4

Results of Wilcoxon Signed-Rank Test (Non-Parametric Test) for Mental Health (GHQ-28) for RQ3

	M_{pre}	SD_{pre}	M_{post}	SD_{post}	*Z*	*p*	*r*	Cohen's *d*[b]
Mental health[a] (GHQ-28)	14.7	4.3	15.9	5.3	–0.96	.34	–0.15	–0.30

Note. GHQ-28 = General Health Questionnaire-28; *M* = mean; *SD* = standard deviation; *Z* = Z value; *p* = *p* value; *r* = effect size (correlation), z/√(N); (Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed); *N* = 41.

[a] GHQ-28 measures psychological distress (i.e., higher value indicates higher psychological distress and lower mental health).

[b] Based on formula, negative value of Cohen's *d*; however, as GHQ-28 refers to psychological distress, there was a descriptive increase of psychological distress in this study with an effect size of Cohen's *d* = 0.30.

Table J6.5

Results of Paired t Tests for Reappraisal (ERQ, CERQ) and Emotional Experience (PANAS-NA, STAXI-Anger in, STAXI-Anger control) for RQ5

	M_{pre}	SD_{pre}	M_{post}	SD_{post}	*t(40)*	*p*	Cohen's *d*
Reappraisal (ERQ)	4.8	1.1	4.8	0.9	0.39	.70	0.06
Reappraisal (CERQ)	10.0	2.9	10.1	2.9	0.43	.67	0.07
Negative affect (PANAS-NA)	12.2	3.0	12.1	2.6	–0.49	.63	–0.08
Anger in (STAXI)	15.1	4.2	15.1	3.5	–0.06	.95	–0.01
Anger control (STAXI)	20.8	3.6	21.4	3.2	1.49	.14	0.23

Note. ERQ = Emotion Regulation Questionnaire; CERQ = Cognitive Emotion Regulation Questionnaire; PANAS = Positive and Negative Affect Schedule (NA = negative affect); STAXI = State-Trait Anger Expression Inventory; *M* = mean; *SD* = standard deviation; *t* = *t* value (degrees of freedom in parentheses); *p* = *p* value; Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed); *d* = Cohen's delta (effect size); *N* = 41.

Table J6.6

Results of Wilcoxon Signed-Rank Tests for Emotional Experience (PANAS-PA, STAXI-State Anger, STAXI-Anger Out) for RQ5

	M_{pre}	SD_{pre}	M_{post}	SD_{post}	*Z*	*p*	*r*	Cohen's *d*
Positive affect (PANAS-PA)	29.2	6.4	29.4	7.0	–0.46	.65	–0.07	–0.14[a]
State anger (STAXI)	10.8	1.7	10.9	1.6	–0.64	.52	–0.10	–0.20[a]
Anger out (STAXI)	12.6	6.8	11.0	2.4	–2.30	.02	–0.36	–0.77

Note. PANAS = Positive and Negative Affect Schedule (PA = positive affect); STAXI = State-Trait Anger Expression Inventory; *M* = mean; *SD* = standard deviation; *Z* = *Z* value; *p* = *p* value; *r* = effect size (correlation), z/√(N); (Bonferroni correction: for 47

single analyses in total: α_{corr} = .001 (two-tailed); *N* = 41.

[a] Based on formula, negative value of Cohen's *d*; however, there was a descriptive increase of positive affect and state anger in this study with effect sizes of Cohen's *d* = 0.14 and 0.20, respectively.

Appendix J7 Detailed Results of Multiple Linear Regression Analyses (Model Summaries and Regression Coefficients)

Table J7.1

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of PSS-10 for RQ1 (Hypothesis 1.1)

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.21	.04	–.06	.04	4.66	0.40	4	36	.81

Note. Dependent variable: change in perceived stress (PSS-10) between pre- and posttest; $N = 41$; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.2

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of AAT-CS for RQ2 (Hypothesis 2.1)

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.25	.06	–.05	.06	116.02	0.57	4	34	.69

Note. Dependent variable: change in AAT-CS between pre- and posttest; $n = 39$; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the

training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.3

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of AAT-CS (Linear Regression; Enter method) for RQ2 (Hypothesis 2.1)

Variable (z-standardized)	*B*	*SE*	*p*
Constant	41.95	18.63	.03
Microstressors (MIMIS, MIMIS EMA version)	–35.29	25.10	.17
Macrostressors (LE Checklist)	21.37	21.79	.33
ASF-E-P	–1.27	18.90	.95
ASF-E-N	6.86	21.69	.75

Note. Dependent variable: change in AAT-CS between pre- and posttest; *n* = 39; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.4

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of GHQ-28 for RQ3

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist)	.19	.04	–.02	.04	5.96	0.70	2	38	.51

Note. Dependent variable: change in GHQ-28 between pre- and posttest; *N* = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.5

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of GHQ-28 (Linear Regression; Enter Method) for RQ3

Variable (z-standardized)	*B*	*SE*	*p*
Constant	1.26	0.93	.18
Microstressors (MIMIS, MIMIS EMA version)	1.22	1.04	.25
Macrostressors (LE Checklist)	–0.57	1.04	.58

Note. Dependent variable: change in GHQ-28 between pre- and posttest; $N = 41$; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.6

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of GHQ-28 for RQ3

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.22	.05	–.06	.05	6.07	0.47	4	36	.76

Note. Dependent variable: change in GHQ-28 between pre- and posttest; $N = 41$; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.7

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of GHQ-28 (Linear Regression; Enter Method) for RQ3

Variable (z-standardized)	*B*	*SE*	*p*
Constant	1.26	0.95	.19
Microstressors (MIMIS, MIMIS EMA version)	1.60	1.21	.19
Macrostressors (LE Checklist)	–0.81	1.11	.47
ASF-E-P	0.01	0.99	.99
ASF-E-N	–0.79	1.07	.47

Note. Dependent variable: change in GHQ-28 between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.8

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of BRS for RQ3

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.25	.06	–.01	.06	0.52	0.81	3	37	.49

Note. Dependent variable: change in BRS between pre- and posttest; *N* = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.9

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of BRS (Linear Regression; Enter Method) for RQ3

Variable (z-standardized)	*B*	*SE*	*p*
Constant	–0.01	0.08	.88
Macrostressors (LE Checklist)	0.11	0.08	.21
ASF-E-P	0.07	0.08	.44
ASF-E-N	0.04	0.08	.61

Note. Dependent variable: change in BRS between pre- and posttest; $N = 41$; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.10

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of WHO-5 for RQ3

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.35	.12	.03	.14	16.72	1.26	4	36	.31

Note. Dependent variable: change in WHO-5 between pre- and posttest; $N = 41$; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.11

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of WHO-5 (Linear Regression; Enter Method) for RQ3

Variable (z-standardized)	*B*	*SE*	*p*
Constant	–3.71	2.61	.16
Microstressors (MIMIS, MIMIS EMA version)	1.96	3.32	.56
Macrostressors (LE Checklist)	–3.49	3.06	.26
ASF-E-P	5.02	2.72	.07
ASF-E-N	1.43	2.95	.63

Note. Dependent variable: change in WHO-5 between pre- and posttest; $N = 41$; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.12

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Reappraisal Subscale of ERQ for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.53	.28	.20	.39	0.78	3.46	4	36	.02

Note. Dependent variable: change in reappraisal subscale of ERQ between pre- and posttest; $N = 41$; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.13

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Reappraisal Subscale of ERQ (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	0.05	0.12	.67
Microstressors (MIMIS, MIMIS EMA version)	0.21	0.16	.19
Macrostressors (LE Checklist)	–0.45	0.14	.004
ASF-E-P	0.10	0.13	.46
ASF-E-N	0.14	0.14	.34

Note. Dependent variable: change in reappraisal subscale of ERQ between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.14

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Reappraisal Subscale of CERQ for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.12	.02	–.10	.02	2.28	0.13	4	36	.97

Note. Dependent variable: change in positive reappraisal subscale of CERQ between pre- and posttest; *N* = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.15

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Reappraisal Subscale of CERQ (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	0.15	0.36	.68
Microstressors (MIMIS, MIMIS EMA version)	–0.15	0.45	.75
Macrostressors (LE Checklist)	0.05	0.42	.91
ASF-E-P	0.04	0.37	.91
ASF-E-N	–0.16	0.40	.69

Note. Dependent variable: change in positive reappraisal subscale of CERQ between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.16

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Positive Affect Subscale of PANAS for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.40	.16	.07	.19	5.89	1.73	4	36	.17

Note. Dependent variable: change in positive affect subscale of PANAS between pre- and posttest; *N* = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.17

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Positive Affect Subscale of PANAS (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	0.15	0.92	.87
Microstressors (MIMIS, MIMIS EMA version)	2.62	1.17	.03
Macrostressors (LE Checklist)	–2.03	1.08	.07
ASF-E-P	1.41	0.96	.15
ASF-E-N	–1.38	1.04	.19

Note. Dependent variable: change in positive affect subscale of PANAS between pre- and posttest; $N = 41$; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.18

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Negative Affect Subscale of PANAS for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.46	.21	.13	.27	2.38	2.44	4	36	.07

Note. Dependent variable: change in negative affect subscale of PANAS between pre- and posttest; $N = 41$; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: $\alpha_{corr} = .001$ (two-tailed).

Table J7.19

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Negative Affect Subscale of PANAS (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	–0.20	0.37	.60
Microstressors (MIMIS, MIMIS EMA version)	1.20	0.47	.02
Macrostressors (LE Checklist)	0.10	0.44	.82
ASF-E-P	0.38	0.39	.33
ASF-E-N	–0.54	0.42	.21

Note. Dependent variable: change in negative affect subscale of PANAS between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed)

Table J7.20

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of State Anger Subscale of STAXI for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.34	.12	.02	.14	1.51	1.17	4	36	.34

Note. Dependent variable: change in state anger subscale of STAXI between pre- and posttest; *N* = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.21

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of State Anger Subscale of STAXI (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	0.07	0.24	.76
Microstressors (MIMIS, MIMIS EMA version)	0.26	0.30	.39
Macrostressors (LE Checklist)	0.10	0.28	.73
ASF-E-P	–0.11	0.25	.65
ASF-E-N	–0.48	0.27	.08

Note. Dependent variable: change in state anger subscale of STAXI between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.22

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Anger In Subscale of STAXI for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.25	.07	–.04	.08	2.52	0.62	4	36	.65

Note. Dependent variable: change in anger in subscale of STAXI between pre- and posttest; *N* = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.23

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Anger In Subscale of STAXI (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	–0.02	0.39	.95
Microstressors (MIMIS, MIMIS EMA version)	0.07	0.50	.90
Macrostressors (LE Checklist)	0.09	0.46	.84
ASF-E-P	–0.59	0.41	.16
ASF-E-N	–0.18	0.45	.69

Note. Dependent variable: change in anger in subscale of STAXI between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.24

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Anger Out Subscale of STAXI for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.52	.27	.19	.37	1.89	3.29	4	35	.02

Note. Dependent variable: change in anger out subscale of STAXI between pre- and posttest; *n* = 40 (*n* = 1 exclusion since assumptions of multiple linear regression analysis were better fulfilled); R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; *F* = *F* value; *df* = degrees of freedom; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.25

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Anger Out Subscale of STAXI (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	–0.75	0.30	.02
Microstressors (MIMIS, MIMIS EMA version)	–0.42	0.39	.28
Macrostressors (LE Checklist)	1.04	0.36	.01
ASF-E-P	0.27	0.31	.38
ASF-E-N	0.82	0.34	.02

Note. Dependent variable: change in anger out subscale of STAXI between pre- and posttest; n = 40 (n = 1 exclusion since assumptions of multiple linear regression analysis were better fulfilled); *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; p = p value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.26

Model Summary Multiple Linear Regression Analysis on Difference Score (Post-pre) of Anger Control Subscale of STAXI for RQ5

Model	*R*	R^2	corr. R^2	f^2	*SE*	*F*	df_1	df_2	*p*
Microstressors (MIMIS, MIMIS EMA version), macrostressors (LE Checklist), ASF-E-P, ASF-E-N	.12	.01	–.10	.01	3.07	0.13	4	36	.97

Note. Dependent variable: change in anger control subscale of STAXI between pre- and posttest; N = 41; R^2 = determination coefficient; corr. R^2 = corrected determination coefficient; f^2 = effect size ($R^2/(1-R^2)$); *SE* = standard error; F = F value; df = degrees of freedom; p = p value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Table J7.27

Regression Coefficients of Predictors to Predict Difference Score (Post-pre) of Anger Control Subscale of STAXI (Linear Regression; Enter Method) for RQ5

Variable (z-standardized)	*B*	*SE*	*p*
Constant	0.68	0.48	.16
Microstressors (MIMIS, MIMIS EMA version)	0.32	0.61	.61
Macrostressors (LE Checklist)	–0.35	0.56	.54
ASF-E-P	0.04	0.50	.93
ASF-E-N	–0.03	0.54	.95

Note. Dependent variable: change in anger control subscale of STAXI between pre- and posttest; *N* = 41; *B* = standardized regression coefficient (due to z-standardized variables); *SE* = standard error; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors; EMA = Ecological Momentary Assessment, LE Checklist = Life Events Checklist; ASF-E-P = ASF-E total score for attributions of positive events at pretest; ASF-E-N = ASF-E total score for attributions of negative events at pretest; microstressors = sum score of microstressors at pre- and posttest (MIMIS) and during the training (MIMIS EMA version); macrostressors = sum score of macrostressors at pre- and posttest (LE Checklist); Bonferroni correction: for 47 single analyses in total: α_{corr} = .001 (two-tailed).

Appendix J8 PS Matching (RQ1, Exploratory RQ 1.2)

Appendix J8.1 Summary of Results of PS Matching

With respect to the exploratory RQ 1.2 if the PBT decreases the participants' perceived microstressor severity compared to a CG, the pre-analyses (two-sample *t* tests; Appendix J8.2) for the PS matching showed significant pretest differences between the unmatched groups in age ($p = .01$), and education ($p < .01$), while there were no significant between-group differences in gender ($p = .07$), mental health (GHQ-28; $p = .43$), the exposure to micro- ($p = .91$), and macrostressors (LE Checklist; $p = .09$). Based on the first matching algorithm tested (*1-to-1 matching without RE*) including the covariates age, gender, and education (three dummy variables), the best matching quality was received for identifying the region of common support by the Minima and Maxima comparison (pseudo $R^2 = .15$; LR $\chi^2 = 12.46$, $p = .01$; balanced covariates; all *p*s > .23; Appendix J8.3). Thus, the following 12 matching algorithms, tested using "psmatch 2"[37] (see Appendix I6), included this method (Stata command "common"), respectively. For each algorithm, the PS distribution (on and off support) in both groups and the PS density before and after matching were inspected graphically (for Figures see Appendix K6.2). In addition, statistical tests were performed to examine the balancing of covariates after matching and to determine Pseudo R^2 and the *p* value of the LR χ^2 test (Appendix J8.4). Based on these indicators, the best matching quality resulted for the *2-nn matching with RE* (Pseudo $R^2 = .005$, LR $\chi^{2.} = .39$, $p = .94$), which considered $n = 61$ of 87 participants in the region of common support (PS score between .1 and .9). To potentially select further covariates for the PS model, a stepwise procedure was used for this algorithm. Mental health (GHQ-28), the number of micro- (MIMIS) and macrostressors (LE Checklist) at pretest were iteratively included in the model. The regression coefficients of logistic regression and the matching quality using the above indicators were inspected (Appendix J8.5). Based on statistical significance ($\alpha = .05$), none of the three variables significantly predicted the group assignment of participants (IG vs. CG) and, thus, were omitted from

[37] 1a. 1-to-1 matching without RE, 1b. 1-to-1 matching with RE, 2. 2nn-matching, 3. 3nn-matching, 4. 4nn-matching, 5a. Caliper matching (0.2) without RE, 5b. Caliper matching (0.2) with RE, 6a. Caliper matching (0.3) without RE, 6b. Caliper matching (0.3) with RE, 7a. Radius caliper matching (0.2) without RE, 7b. Radius caliper matching (0.2) with RE, 8. Kernel matching

the model specification. Therefore, the final matching algorithm and model included 2-nn matching with RE and three covariates (n = 61 in region of common support) with balanced covariates (relative reduction of differences by matching from 60.5%–100%) and acceptable matching quality (see above; Pseudo R^2 = .005, LR $\chi^{2.}$ = 0.39, p = .94; for PS plots see Appendix J8.4). The dummy variable 3 of education was omitted in logistic regression due to multicollinearity. A sensitivity analysis (Rosenbaum bounds) for this algorithm and model (Appendix J8.6) showed no change of the significance level (sig+) depending on gamma values. When running the same algorithm under "teffects psmatch" (i.e., standard error takes account that PS is estimated), an average treatment effect on the treated (ATET) of 1.80 (*SE* 3.53) was found. The testing of the overlap assumption using "teffects psmatch" using a density plot ("teffects overlap") indicated a likely violation of the overlap assumption (Appendix J8.7). However, it has to be considered that there is no possibility to include the common support condition (Minima and Maxima comparison) in "teffects psmatch". Due to the feasibility character of this study, the matching algorithm and PS model were maintained.

Appendix J8.2 Pre-Analyses and Between-Group Comparison in Pretest Covariates

Table J8.2.1

Results of Independent t Tests for Age, Mental Health (GHQ-28), Exposure to Microstressors (MIMIS), and to Macrostressors (LE Checklist)

	TRAIN$_4$Positivity (IG)		LifeStress (CG)				
	M_{IG}	SD_{IG}	M_{CG}	SD_{CG}	*t*	*df*	*p*
Age	25	3.0	26.8	3.3	2.57	85	.01
Mental health (GHQ-28)[a]	14.7	4.3	15.4	4.3	0.79	85	.43
Microstressors (MIMIS)	58.6	22.7	59.1	18.4	0.12	85	.91
Macrostressors (LE Checklist)	16.9	22.0	10.9	5.3	–1.72	44.13[b]	.09

Note. Based on descriptive analysis of *SD*s in the two groups for each covariate, independent *t* tests were conducted with equal variances for age, mental health, microstressors, but with unequal variances for macrostressors; TRAIN$_4$Positivity: N = 41; LifeStress: N = 46; GHQ-28 = General Health Questionnaire-28; MIMIS = Mainz Inventory of Microstressors; LE Checklist = Life Events Checklist; IG = intervention group; *M* = mean; *SD* = standard deviation; CG = control group; *t* = *t* value; *df* = degrees of freedom; *p* = *p* value.

[a] GHQ-28 measures psychological distress (i.e., higher value indicates higher psychological distress and lower mental health).

[b] Satterthwaite's degrees of freedom.

Tabelle J8.2.2

Results of χ^2 Test for Gender

Gender	TRAIN$_4$Positivity (IG)	LifeStress (CG)			
	n_{IG}	n_{CG}	χ^2	*df*	*p*
Male	8	17			
Female	33	29	3.22	1	.07
Total	41	46			

Note. IG = intervention group; CG = control group; *n* = sample size; χ^2 = χ^2 value (test statistic); *df* = degrees of freedom; *p* = *p* value.

Table J8.2.3

Results of χ^2 Test for Education

Education	TRAIN$_4$Positivity (IG)	LifeStress (CG)			
	n_{IG}	n_{CG}	χ^2	*df*	*p*
General (A level) or subject-related entrance qualification	14	26	15.13	2	.001

Education	TRAIN$_4$Positivity (IG)	LifeStress (CG)			
	n_{IG}	n_{CG}	χ^2	*df*	*p*
Completed vocational training	2	10			
Completed degree (university or University of Applied Sciences)	25	10			
Total	41	46			

Note. IG = intervention group; CG = control group; *n* = sample size; χ^2 = χ^2 value (test statistic); *df* = degrees of freedom; *p* = *p* value.

Appendix J8.3 PS Matching – Common Support Assumption (Minima Maxima Comparison vs. Trimming Method)

Table J8.3.1

Statistical Indicators of Matching Quality for the Different Algorithms

Algorithm	Indicators of matching quality			
		LR χ^2 test		
	Pseudo R^2	LR χ^2	*p*	All covariates balanced after matching (yes/no)
1-to-1-matching, norepl, nocommon	.48	51.09	< .001	
1-to-1-matching, norepl, common	.15	12.46	.01	✓
1-to-1-matching, norepl, trimming 1%	.48	51.09	< .001	
1-to-1-matching, norepl, trimming 5%	.42	46.16	< .001	
1-to-1-matching, norepl, trimming 25%	.26	21.69	< .001	✓

Note. All analyses performed using Stata command "psmatch2". norepl = no replacement; nocommon = no common command in Stata, that is, Minima and Maxima comparison is not applied; Pseudo R^2 = pseudo determination coefficient; higher matching quality is indicated by a R^2 near zero; LR = Likelihood Ratio; LR χ^2 test: higher matching quality is indicated by a non-significant LR χ^2 test (i.e., $p > .05$); p = p value.

Appendix J8.4 PS Matching – Different Matching Algorithms (Parsimonious Model With Three Covariates)

Figure J8.4.1

PS Density Plot for Both Groups Before Matching

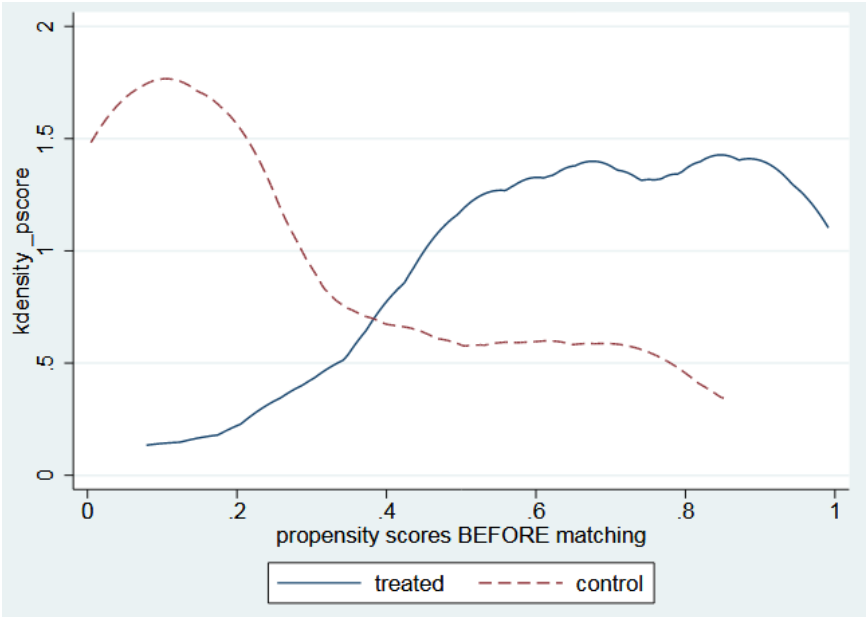


Table J8.4.1

Statistical Indicators of Matching Quality for the Different Algorithms Including Three Covariates (Age, Gender, Dummy Variables for Education)

Algorithm	Indicators of matching quality				
			LR χ^2 test		
	n in region of common support	Pseudo R^2	LR χ^2	*p*	All covariates balanced after matching (yes/no)
1-to-1-matching, norepl, common	60	.152	12.46	.01	✓
1-to-1-matching, withrepl, common	61	.006	.52	.92	✓
2-nn-matching[a], common	61	.005	.39	.94	✓
3-nn matching[a], common	61	.021	1.75	.78	✓
4-nn matching[a], common	61	.021	1.78	.78	✓
Caliper (0.2) matching[b], norepl, common	40	.041	1.87	.60	✓

Algorithm	Indicators of matching quality				
			LR χ^2 test		
	n in region of common support	Pseudo R^2	LR χ^2	*p*	All covariates balanced after matching (yes/no)
Caliper (0.2) matching[b], withrepl, common	61	.006	.52	.92	✓
Caliper (0.3) matching[b], norepl, common	43	.039	1.82	.61	✓
Caliper (0.3) matching[b], withrepl, common	61	.006	.52	.92	✓
Radius caliper (0.2) matching, norepl, common	61	.01	.80	.94	✓
Radius caliper (0.2) matching, withrepl, common	61	.01	.80	.94	✓
Kernel matching, common	61	.014	1.15	.89	✓

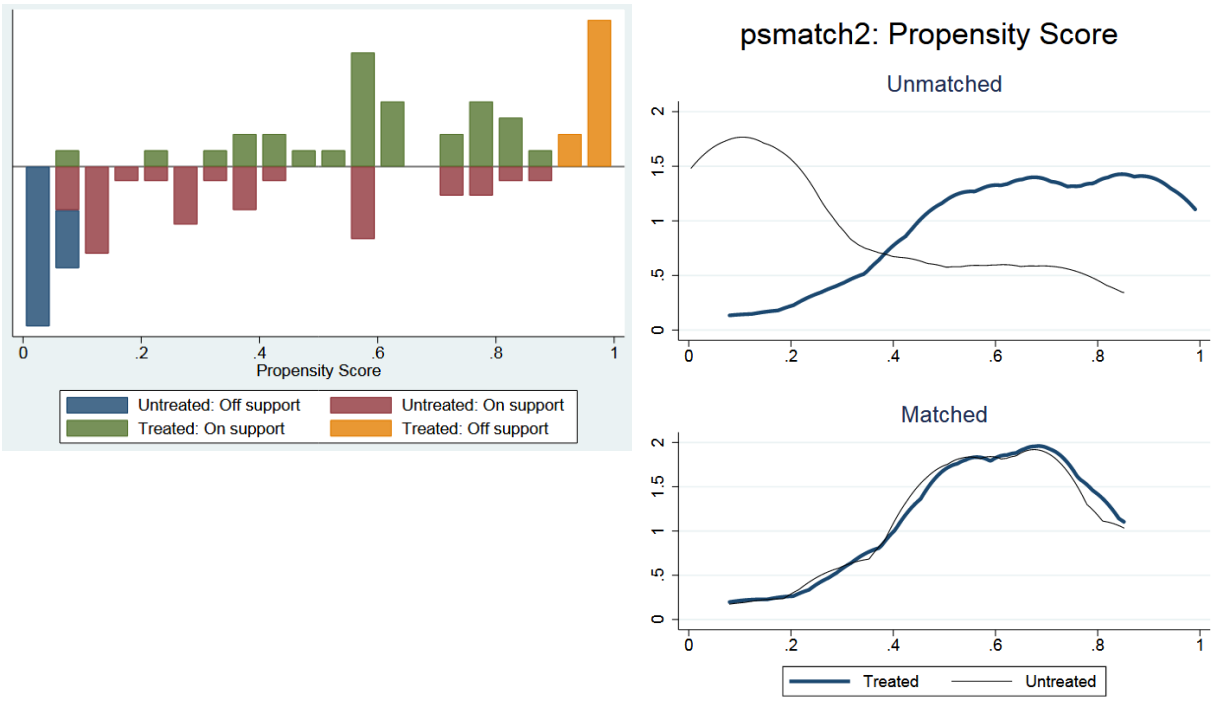
Note. nn = nearest neighbor; norepl = no replacement; withrepl = with replacement; common = common command in Stata, that is, Minima and Maxima comparison is applied for all algorithms; *n* = sample size; Pseudo R^2 = pseudo determination coefficient; higher matching quality is indicated by a R^2 near zero; LR = Likelihood Ratio; LR χ^2 test: higher matching quality is indicated by a non-significant LR χ^2 test (i.e., $p > .05$); *p* = *p* value; all analyses performed using Stata command "psmatch2".

[a] only possible with replacement.

[b] default for caliper is 0.2.

Figure J8.4.2

PS Distribution (left) and PS Density Plot (right) for 2nn-Matching (Only With Repl), Common (Matching Algorithm With Best Matching Quality Among 12 Tested Algorithms)



Appendix J8.5 PS Matching – Selection of Covariates for Final Algorithm (2nn-Matching With Replacement, Common)

Table J8.5.1

Statistical Indicators of Matching Quality for the Different Algorithms Including Three Initial Covariates (Age, Gender, Dummy Variables for Education) + Additional Coviates (GHQ-28, MIMIS, LE Checklist)

Algorithm	Indicators of matching quality				
			LR χ^2 test		
	n in region of common support	Pseudo R^2	LR χ^2	*p*	All covariates balanced after matching (yes/no)
2-nn-matching[a], common 3 covariates	61	.005	.39	.94	✓
2-nn-matching[a], common 3 covariates + GHQ-18	58	.009	.69	.95	✓
2-nn-matching[a], common 3 covariates + MIMIS	58	.007	.57	.97	✓
2-nn-matching[a], common 3 covariates + LE Checklist	40	.045	3.0	.70	✓

Note. nn = nearest neighbor; common = common command in Stata, that is, Minima and Maxima comparison is applied for all algorithms; *n* = sample size; Pseudo R^2 = pseudo determination coefficient; higher matching quality is indicated by a R^2 near zero; LR = Likelihood Ratio; LR χ^2 test: higher matching quality is indicated by a non-significant LR χ^2 test (i.e., $p > .05$); *p* = *p* value; all analyses performed using Stata command "psmatch2".

[a] only possible with replacement.

Table J8.5.2

Results of Logistic Regression Including Three Covariates (Age, Gender, Education) + Mental Health (GHQ-28) as Predictors in 2nn-Matching (With Replacement, Common)

	Coeff.	*SE*	*Z*	*p* > |z|
Constant	19.52	5.18	3.77	< .001
Age	–0.67	0.18	–3.76	<.001
Gender	0.92	0.66	1.40	.16
Education – dummy 1	–5.07	1.22	–4.15	<.001
Education – dummy 2	–2.36	1.04	–2.28	.02
Education – dummy 3	0	(omitted)		
Mental health (GHQ-28)	–0.02	0.06	–0.29	.77

Note. Coeff. = Regression coefficient; *SE* = standard error; Z = *Z* value; *p* = *p* value; GHQ-28 = General Health Questionnaire-28.

Table J8.5.3

Results of Logistic Regression Including Three Covariates (Age, Gender, Education) + Exposure to Microstressors (MIMIS) as Predictors in 2nn-Matching (With Replacement, Common)

	Coeff.	*SE*	*Z*	*p* > |z|
Constant	19.14	5.24	3.65	< .001
Age	–0.68	0.18	–3.76	<.001
Gender	0.89	0.65	1.38	.17
Education – dummy 1	–5.08	1.22	–4.18	<.001
Education – dummy 2	–2.36	1.03	–2.28	.02
Education – dummy 3	0	(omitted)		
Microstressors (MIMIS)	0.002	0.01	0.17	.86

Note. Coeff. = Regression coefficient; *SE* = standard error; Z = *Z* value; *p* = *p* value; MIMIS = Mainz Inventory of Microstressors.

Table J8.5.4

Results of Logistic Regression Including Three Covariates (Age, Gender, Education) + Exposure to Macrostressors (LE Checklist) as Predictors in 2nn-Matching (With Replacement, Common)

	Coeff.	*SE*	*Z*	*p* > |z|
Constant	21.80	5.85	3.72	< .001
Age	–0.79	0.21	–3.82	<.001
Gender	0.72	0.67	1.06	.29
Education – dummy 1	–5.62	1.38	–4.08	<.001

	Coeff.	*SE*	*Z*	*p* > |z|
Education – dummy 2	–2.16	1.08	–1.99	.05
Education – dummy 3	0	(omitted)		
Macrostressors (LE Checklist)	0.06	0.03	1.80	.07

Note. Coeff. = Regression coefficient; *SE* = standard error; Z = *Z* value; *p* = *p* value; LE Checklist = Life Events Checklist.

Appendix J8.6 PS Matching – Sensitivity Analysis for Final Algorithm (2nn-Matching With Replacement, Common) and Model (Covariates Age, Gender, Education)

Rosenbaum bounds for delta_Stressorbel (N = 30 matched pairs)

Gamma	sig+	sig-	t-hat+	t-hat-	CI+	CI-
1	.057775	.057775	5.25	5.25	–1.25	12.25
1.1	.088831	.035691	4.5	5.75	–2	13.3888
1.2	.126189	.021903	3.75	6.75	–2.75	14.25
1.3	.168676	.013374	3.25	7	–3.5	15
1.4	.214959	.008136	2.75	7.88883	–4.25	15.75
1.5	.263702	.004935	1.94441	8.25	–4.86117	16.75
1.6	.313676	.002986	1.5	9	–5.5	17.5
1.7	.36381	.001804	1.25	9.25	–6	18.5
1.8	.413222	.001088	.75	9.75	–6.5	19.3888
1.9	.461212	.000656	.25	10.25	–7	19.75
2	.507254	.000395	–3.7e-07	10.75	–7.5	20.5

Note. gamma = log odds of differential assignment due to unobserved factors; sig+ = upper bound significance level; sig- = lower bound significance level; t-hat+ = upper bound Hodges-Lehmann point estimate; t-hat- = lower bound Hodges-Lehmann point estimate; CI+ = upper bound confidence interval (a= .95); CI- = lower bound confidence interval (a= .95); Stressorbel = relevant outcome variable perceived severity by microstressors at posttest (3 weeks).

Appendix J8.7 PS Matching – PS Density Plot and Visual Testing of Overlap (Common Support) Assumption for Final Algorithm and Model Using "teffects psmatch"

Figure J8.7.1

PS Density Plot Before Matching (Left) and After 2nn-Matching Under "teffects psmatch" With Three Original Covariates (Right)

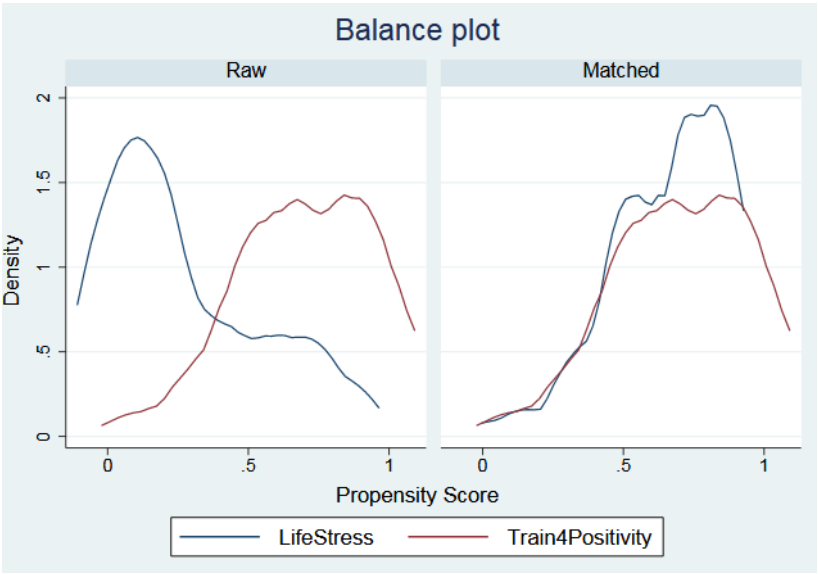
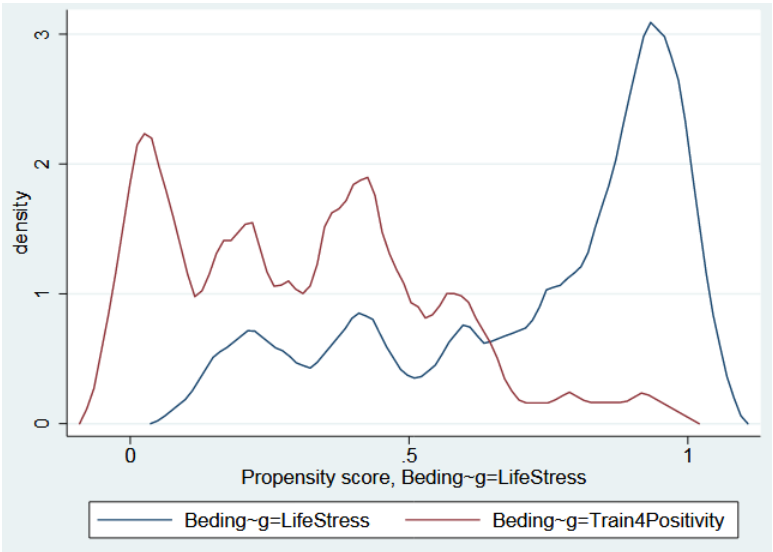


Figure J8.7.2

Visual Testing of Overlap Assumption Under "teffects psmatch" After Matching



Appendix J8.8 PS Matching – Final Posttest Comparison Between TRAIN$_4$Positivity (IG) and LifeStress (CG) in Perceived Severity of Microstressors

Tabelle J8.8.1

Results of Independent t Test for Perceived Severity of Microstressors

Independent *t* test, equal variances	TRAIN$_4$Positivity (IG)			LifeStress (CG)						
	M_{IG}	SD_{IG}	n_{IG}	M_{CG}	SD_{CG}	n_{CG}	*t*	*df*	*p*	*d*
Perceived severity by microstressors	26.7	17.5	30	23.7	12.1	31	–0.78	59	.44	0.20

Note. IG = intervention group; CG = control group; *M* = mean; *SD* = standard deviation; *n* = sample size; *t* = *t* value; *df* = degrees of freedom; *p* = *p* value; *d* = Cohen's delta (effect size); IG-CG.

Appendix J9 Multilevel Modeling (RQ4)

Table J9.1

Results of Multilevel Analysis (Available-Case Analysis and Imputed Data) for Current Mood – Positive Valence

	Available-case analysis[a]					Imputed data[b]
	Model					Model
Model	Null model (Step 1)	Random Intercept model (Step 2)	Random Coefficients (Step 3)	Random Coefficients, CS matrix (Step 4)	Random Coefficients, unstructured covariance matrix (Step 5)	Random Coefficients, unstructured covariance matrix (Step 5)
Fixed part						
Predictor	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)
intercept (γ_{00})	65.78** (2.22)	65.05** (2.24)	65.40** (2.30)	66.25** (0.52)	65.36** (2.33)	65.24** (2.29)
time (γ_{01})		0.02 (0.01)	0.004 (0.03)	–0.03 (0.04)	0.01 (0.03)	0.009 (0.02)
Random part						
Var(Residual), σ^2_e	199.24	199.04	171.45	218.58	171.41	185.26
Var(_cons), σ^2_{u0}	170.09	168.31	176.48	/	182.45	176.87
Var(time), σ^2_{u1}			0.03	/	0.03	0.01
Cov(time,_cons), σ^2_{u0u1}				0.06	–0.76	–0.45[c]
Var(time_cons)				0.06		
Additional information						
ICC	0.46					
AIC	26195.42	26193.77	25810.22	26492.86	25808.78	
BIC	26213.63	26218.06	25840.58	26523.22	25845.21	
Deviance D (–2 · log(L))	26189.42	26185.78	25800.22			
LR test (χ^2)		3.65	385.55**			
R^2		0.001	0.14		0.14	0.07

Note. Level 1: time points; level 2: participants; CS matrix = compound symmetry matrix; Fixed part (i.e., fixed parameters): γ_{00} = average intercept (i.e., average score of positive valence at time point 0); γ_{01} = average slope (i.e., average change in positive valence); Random part (i.e., random parameters): Var(Residual), σ^2_e = variance of level 1 residuals (i.e., differences between time points within participants (level-2 entities) concerning the deviation of observed and predicted values of positive valence); Var(_cons), σ^2_{u0} = variance of random intercepts (i.e., unexplained differences between the intercepts of participants [level-2 entities]); Var(time), σ^2_{u1} = variance of random slopes (i.e., unexplained differences between the slopes [i.e., change in positive valence

between two time points] of participants [level-2 entities]); Cov(time,_cons), σ^2_{u0u1} = pairwise association between random intercepts and random slopes; > 0 = the higher the predicted outcome value of a participant at time point 0, the larger is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); < 0 = the higher the predicted outcome value of a participant at time point 0, the smaller is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); Var(time_cons) = common variance for all random effects (random intercepts and random slopes) in model with exchangeable or compound symmetry covariance structure; ICC = intra-class correlation; AIC = Akaike Information Criteria; BIC = Bayesian Information Criteria; deviance: -2 · log likelihood of respective model; LR test = Likelihood-ratio test, compares the deviance of two models by subtracting the smaller deviance (model with more parameters) from the larger deviance (model with less parameters); no deviance for step 4 and step 5 since the previous model (step 3) is not nested within these models (i.e., identical models except for covariance matrix); R^2 = determination coefficient; * ≤ .05; ** ≤ .01; [a] 3202 observations; [b] 4410 observations; [c] calculated using $r_{xy} = cov(x,y)/(s_x s_y)$.

Table J9.2

Results of Multilevel Analysis (Available-Case Analysis and Imputed Data) for Current Mood – Energetic Arousal

	Available-case analysis[a]					Imputed data[b]
	Model					Model
Model	Null model (Step 1)	Random Intercept model (Step 2)	Random Coefficients (Step 3)	Random Coefficients, CS matrix (Step 4)	Random Coefficients, unstructured covariance matrix (Step 5)	Random Coefficients, unstructured covariance matrix (Step 5)
Fixed part						
Predictor	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)
intercept (γ_{00})	50.49** (1.75)	50.19** (1.82)	50.50** (1.91)	50.96** (0.68)	50.42** (2.02)	50.28** (1.92)
time (γ_{01})		0.006 (0.01)	–0.002 (0.02)	–0.02 (0.04)	0.002 (0.02)	0.001 (0.02)
Random part						
Var(Residual), σ^2_e	354.74	354.72	341.88	370.98	341.66	349.89
Var(_cons), σ^2_{u0}	102.81	102.30	112.57	/	128.03	116.85
Var(time), σ^2_{u1}			0.01	/	0.01	0.006
Cov(time,_cons), σ^2_{u0u1}				0.05	–0.58	–0.28[c]
Var(time_cons)				0.05		
Additional information						
ICC	0.23					
AIC	28005.83	28007.50	27942.88	28158.85	27940.47	
BIC	28024.04	28031.78	27973.24	28189.21	27976.90	
Deviance D (–2 · log(L))	27999.82	27999.50	27932.88			
LR test (χ^2)		0.33	66.61**			
R^2		0.00006	0.04		0.04	0.01

Note. Level 1: time points; level 2: participants; CS matrix = compound symmetry matrix; Fixed part (i.e., fixed parameters): γ_{00} = average intercept (i.e., average score of energetic arousal at time point 0); γ_{01} = average slope (i.e., average change in energetic arousal); Random part (i.e., random parameters): Var(Residual), σ^2_e = variance of level 1 residuals (i.e., differences between time points within participants [level-2 entities] concerning the deviation of observed and predicted values of energetic arousal); Var(_cons), σ^2_{u0} = variance of random intercepts (i.e., unexplained differences between the intercepts of participants [level-2 entities]); Var(time), σ^2_{u1} = variance of random slopes (i.e., unexplained differences between the slopes [i.e., change in energetic arousal between two time points] of participants [level-2 entities]); Cov(time,_cons), σ^2_{u0u1} = pairwise association between random intercepts and random slopes; > 0 = the higher the predicted outcome value of a participant at time point 0, the larger is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); < 0 = the higher the predicted

outcome value of a participant at time point 0, the smaller is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); Var(time_cons) = common variance for all random effects (random intercepts and random slopes) in model with exchangeable or compound symmetry covariance structure; ICC = intra-class correlation; AIC = Akaike Information Criteria; BIC = Bayesian Information Criteria; deviance: -2 · log likelihood of respective model; LR test = Likelihood-ratio test, compares the deviance of two models by subtracting the smaller deviance (model with more parameters) from the larger deviance (model with less parameters); no deviance for step 4 and step 5 since the previous model (step 3) is not nested within these models (i.e., identical models except for covariance matrix); R^2 = determination coefficient; * ≤ .05; ** ≤ .01; [a] 3202 observations; [b] 4410 observations; [c] calculated using $r_{xy} = cov(x,y)/(s_x s_y)$.

Table J9.3

Results of Multilevel Analysis (Available-Case Analysis and Imputed Data) for Current Mood – Calmness

	Available-case analysis[a]					Imputed data[b]
	Model					Model
Model	Null model (Step 1)	Random Intercept model (Step 2)	Random Coefficients (Step 3)	Random Coefficients, CS matrix (Step 4)	Random Coefficients, unstructured covariance matrix (Step 5)	Random Coefficients, unstructured covariance matrix (Step 5)
Fixed part						
Predictor	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)
intercept (γ_{00})	64.00** (2.13)	63.41** (2.17)	63.57** (2.46)	63.98** (0.58)	63.55** (2.53)	63.56** (2.43)
time (γ_{01})		0.01 (0.009)	0.01 (0.04)	–0.007 (0.04)	0.01 (0.04)	0.009 (0.03)
Random part						
Var(Residual), σ^2_e	258.21	258.07	214.63	271.72	214.53	222.22
Var(_cons), σ^2_{u0}	155.31	154.56	201.95	/	213.02	199.15
Var(time), σ^2_{u1}			0.04	/	0.04	0.02
Cov(time,_cons), σ^2_{u0u1}				0.05	–1.56	–0.97[c]
Var(time_cons)				0.05		
Additional information						
ICC	0.38					
AIC	27013.52	27013.62	26528.57	27176.44	26520.38	
BIC	27031.73	27037.90	26558.93	27206.80	26556.81	
Deviance D (–2 · log(L))	27007.52	27005.62	26518.58			
LR test (χ^2)		1.90	487.04**			
R^2		0.0005	0.17		0.17	0.14

Note. Level 1: time points; level 2: participants; CS matrix = compound symmetry matrix; Fixed part (i.e., fixed parameters): γ_{00} = average intercept (i.e., average score of calmness at time point 0); γ_{01} = average slope (i.e., average change in calmness); Random part (i.e., random parameters): Var(Residual), σ^2_e = variance of level 1 residuals (i.e., differences between time points within participants [level-2 entities] concerning the deviation of observed and predicted values of calmness); Var(_cons), σ^2_{u0} = variance of random intercepts (i.e., unexplained differences between the intercepts of participants [level-2 entities]); Var(time), σ^2_{u1} = variance of random slopes (i.e., unexplained differences between the slopes [i.e., change in calmness between two time points] of participants [level-2 entities]); Cov(time,_cons), σ^2_{u0u1} = pairwise association between random intercepts and random slopes; > 0 = the higher the predicted outcome value of a participant at time point 0, the larger is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); < 0 = the higher the predicted outcome value of a participant at time

point 0, the smaller is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); Var(time_cons) = common variance for all random effects (random intercepts and random slopes) in model with exchangeable or compound symmetry covariance structure; ICC = intra-class correlation; AIC = Akaike Information Criteria; BIC = Bayesian Information Criteria; deviance: -2 · log likelihood of respective model; LR test = Likelihood-ratio test, compares the deviance of two models by subtracting the smaller deviance (model with more parameters) from the larger deviance (model with less parameters); no deviance for step 4 and step 5 since the previous model (step 3) is not nested within these models (i.e., identical models except for covariance matrix); R^2 = determination coefficient; * ≤ .05; ** ≤ .01; [a] 3202 observations; [b] 4410 observations; [c] calculated using $r_{xy} = cov(x,y)/(s_x s_y)$.

Table J9.4

Results of Multilevel Analysis (Available-Case Analysis and Imputed Data) for End-of-Day Measures – Well-Being

	Available-case analysis[a]					Imputed data[b]
	Model					Model
Model	Null model (Step 1)	Random Intercept model (Step 2)	Random Coefficients (Step 3)	Random Coefficients, CS matrix (Step 4)	Random Coefficients, unstructured covariance matrix (Step 5)	Random Coefficients, unstructured covariance matrix (Step 5)
Fixed part						
Predictor	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)
intercept (γ_{00})	66.74** (1.98)	64.32** (2.38)	63.99** (2.38)	64.19** (1.76)	63.56** (2.78)	65.68** (2.49)
time (γ_{01})		0.34 (0.19)	0.40 (0.25)	0.36 (0.29)	0.50 (0.28)	0.04 (0.16)
Random part						
Var(Residual), σ^2_e	308.59	306.88	287.98	321.77	281.90	305.80
Var(_cons), σ^2_{u0}	103.12	99.48	98.32	/	166.11	117.85
Var(time), σ^2_{u1}			0.57	/	1.02	0.22
Cov(time,_cons), σ^2_{u0u1}				1.06	–8.17	–0.96[c]
Var(time_cons)				1.06		
Additional information						
ICC	0.25					
AIC	3710.46	3709.30	3702.16	3725.76	3699.04	
BIC	3722.62	3725.52	3722.43	3746.03	3723.36	
Deviance D (–2 · log(L))	3704.46	3701.30	3692.16			
LR test (χ^2)		3.16	9.14*			
R^2		0.006	0.07		0.09	0.01

Note. Level 1: time points; level 2: participants; CS matrix = compound symmetry matrix; Fixed part (i.e., fixed parameters): γ_{00} = average intercept (i.e., average score of well-being at time point 0); γ_{01} = average slope (i.e., average change in well-being); Random part (i.e., random parameters): Var(Residual), σ^2_e = variance of level 1 residuals (i.e., differences between time points within participants [level-2 entities] concerning the deviation of observed and predicted values of well-being); Var(_cons), σ^2_{u0} = variance of random intercepts (i.e., unexplained differences between the intercepts of participants [level-2 entities]); Var(time), σ^2_{u1} = variance of random slopes (i.e., unexplained differences between the slopes [i.e., change in well-being between two time points] of participants [level-2 entities]); Cov(time,_cons), σ^2_{u0u1} = pairwise association between random intercepts and random slopes; > 0 = the higher the predicted outcome value of a participant at time point 0, the larger is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); < 0 = the higher the predicted outcome value of a participant at time

point 0, the smaller is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); Var(time_cons) = common variance for all random effects (random intercepts and random slopes) in model with exchangeable or compound symmetry covariance structure; ICC = intra-class correlation; AIC = Akaike Information Criteria; BIC = Bayesian Information Criteria; deviance: -2 · log likelihood of respective model; LR test = Likelihood-ratio test, compares the deviance of two models by subtracting the smaller deviance (model with more parameters) from the larger deviance (model with less parameters); no deviance for step 4 and step 5 since the previous model (step 3) is not nested within these models (i.e., identical models except for covariance matrix); R^2 = determination coefficient; * ≤ .05; ** ≤ .01; [a] 426 observations; [b] 660 observations; [c] calculated using $r_{xy} = cov(x,y)/(s_x s_y)$.

Table J9.5

Results of Multilevel Analysis (Available-Case Analysis and Imputed Data) for End-of-Day Measures – Ability to Distance From Negative Stimuli

	Available-case analysis[a]					Imputed data[b]
	Model					Model
Model	Null model (Step 1)	Random Intercept model (Step 2)	Random Coefficients (Step 3)	Random Coefficients, CS matrix (Step 4)	Random Coefficients, unstructured covariance matrix (Step 5)	Random Intercept model (Step 2)
Fixed part						
Predictor	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)
intercept (γ_{00})	61.05** (2.20)	54.69** (2.60)	54.63** (2.59)	54.69** (1.98)	54.43** (2.82)	57.56** (2.99)
time (γ_{01})		0.89** (0.21)	0.90** (0.22)	0.88** (0.31)	0.95** (0.24)	0.25 (0.17)
Random part						
Var(Residual), σ^2_e	399.86	385.31	383.12	414.26	378.49	413.77
Var(_cons), σ^2_{u0}	124.44	113.56	110.41	/	151.74	158.37
Var(time), σ^2_{u1}			0.09	/	0.32	
Cov(time,_cons), σ^2_{u0u1}				1.05	–3.79	
Var(time_cons)				1.05		
Additional information						
ICC	0.24					
AIC	3818.98	3803.79	3805.59	3827.57	3806.66	
BIC	3831.14	3820.01	3825.86	3847.84	3830.98	
Deviance D (–2 · log(L))	3812.98	3795.79	3795.58			
LR test (χ^2)		17.19**	0.20			
R^2		0.04	0.04		0.05	

Note. Level 1: time points; level 2: participants; CS matrix = compound symmetry matrix; Fixed part (i.e., fixed parameters): γ_{00} = average intercept (i.e., average score of the ability to distance from negative stimuli at time point 0); γ_{01} = average slope (i.e., average change in the ability to distance from negative stimuli); Random part (i.e., random parameters): Var(Residual), σ^2_e = variance of level 1 residuals (i.e., differences between time points within participants [level-2 entities] concerning the deviation of observed and predicted values of the ability to distance from negative stimuli); Var(_cons), σ^2_{u0} = variance of random intercepts (i.e., unexplained differences between the intercepts of participants [level-2 entities]); Var(time), σ^2_{u1} = variance of random slopes (i.e., unexplained differences between the slopes [i.e., change in the ability to distance from negative stimuli between two time points] of participants [level-2 entities]); Cov(time,_cons), σ^2_{u0u1} = pairwise association between random intercepts and random slopes; > 0 = the higher the predicted outcome value of a participant at time point 0, the larger is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); < 0 = the higher the predicted outcome value of a participant at time point 0, the smaller is the increase in

the outcome if the level-1 predictor increases by one unit (i.e., between two time points); Var(time_cons) = common variance for all random effects (random intercepts and random slopes) in model with exchangeable or compound symmetry covariance structure; ICC = intra-class correlation; AIC = Akaike Information Criteria; BIC = Bayesian Information Criteria; deviance: -2 · log likelihood of respective model; LR test = Likelihood-ratio test, compares the deviance of two models by subtracting the smaller deviance (model with more parameters) from the larger deviance (model with less parameters); no deviance for step 4 and step 5 since the previous model (step 3) is not nested within these models (i.e., identical models except for covariance matrix); R^2 = determination coefficient; * ≤ .05; ** ≤ .01; [a] 426 observations; [b] 660 observations.

Table J9.6

Results of Multilevel Analysis (Available-Case Analysis and Imputed Data) for End-of-Day Measures – Ability to Get Affected by Positive Stimuli

	Available-case analysis[a]					Imputed data[b]
	Model					Model
Model	Null model (Step 1)	Random Intercept model (Step 2)	Random Coefficients (Step 3)	Random Coefficients, CS matrix (Step 4)	Random Coefficients, unstructured covariance matrix (Step 5)	Random Coefficients, unstructured covariance matrix (Step 5)
Fixed part						
Predictor	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)	coefficient (*SE*)
intercept (γ_{00})	69.49** (2.21)	66.23** (2.51)	66.20** (2.52)	66.55** (1.67)	66.06** (2.80)	67.92** (2.55)
time (γ_{01})		0.46** (0.18)	0.46 (0.24)	0.37 (0.32)	0.49 (0.27)	0.10 (0.16)
Random part						
Var(Residual), σ^2_e	266.88	263.03	248.72	282.36	244.83	257.62
Var(_cons), σ^2_{u0}	137.88	133.48	131.42	/	180.88	144.94
Var(time), σ^2_{u1}			0.59	/	0.94	0.12
Cov(time,_cons), σ^2_{u0u1}				1.74	–6.37	3.84[c]
Var(time_cons)				1.74		
Additional information						
ICC	0.34					
AIC	3660.47	3655.77	3652.33	3685.65	3651.78	
BIC	3672.63	3671.98	3672.61	3705.93	3676.10	
Deviance D (–2 · log(L))	3654.47	3647.77	3642.33			
LR test (χ^2)		6.70**	5.43*			
R^2		0.01	0.07		0.08	0.04

Note. Level 1: time points; level 2: participants; CS matrix = compound symmetry matrix; Fixed part (i.e., fixed parameters): γ_{00} = average intercept (i.e., average score of the ability to get affected by positive stimuli at time point 0); γ_{01} = average slope (i.e., average change in the ability to get affected by positive stimuli); Random part (i.e., random parameters): Var(Residual), σ^2_e = variance of level 1 residuals (i.e., differences between time points within participants [level-2 entities] concerning the deviation of observed and predicted values of the ability to get affected by positive stimuli); Var(_cons), σ^2_{u0} = variance of random intercepts (i.e., unexplained differences between the intercepts of participants [level-2 entities]); Var(time), σ^2_{u1} = variance of random slopes (i.e., unexplained differences between the slopes [i.e., change in the ability to get affected by positive stimuli between two time points] of participants [level-2 entities]); Cov(time,_cons), σ^2_{u0u1} = pairwise association between random intercepts and random slopes; > 0 = the higher the predicted outcome value of a participant at time point 0, the larger is the increase in the outcome if the

level-1 predictor increases by one unit (i.e., between two time points); < 0 = the higher the predicted outcome value of a participant at time point 0, the smaller is the increase in the outcome if the level-1 predictor increases by one unit (i.e., between two time points); Var(time_cons) = common variance for all random effects (random intercepts and random slopes) in model with exchangeable or compound symmetry covariance structure; ICC = intra-class correlation; AIC = Akaike Information Criteria; BIC = Bayesian Information Criteria; deviance: -2 · log likelihood of respective model; LR test = Likelihood-ratio test, compares the deviance of two models by subtracting the smaller deviance (model with more parameters) from the larger deviance (model with less parameters); no deviance for step 4 and step 5 since the previous model (step 3) is not nested within these models (i.e., identical models except for covariance matrix); R^2 = determination coefficient; * ≤ .05; ** ≤ .01; [a] 426 observations; [b] 660 observations; [c] calculated using $r_{xy} = cov(x,y)/(s_x s_y)$.

Figure J9.1

Fitting Plots for Individual Participants (n = 35) for Current Mood – Positive Valence

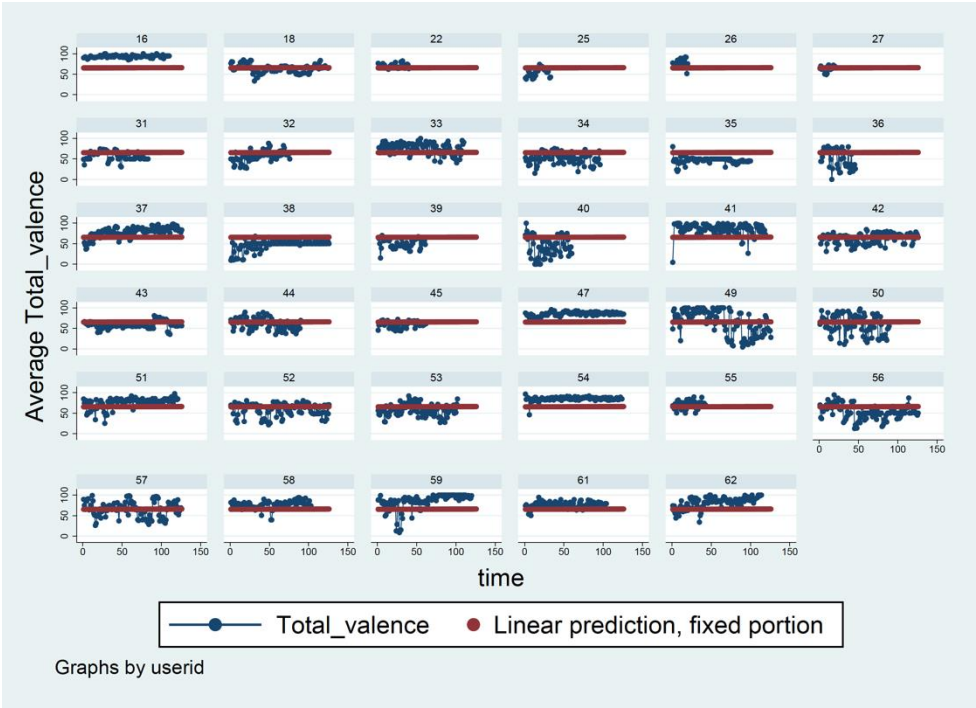


Figure J9.2

Fitting Plots for Individual Participants (n = 35) for Current Mood – Energetic Arousal

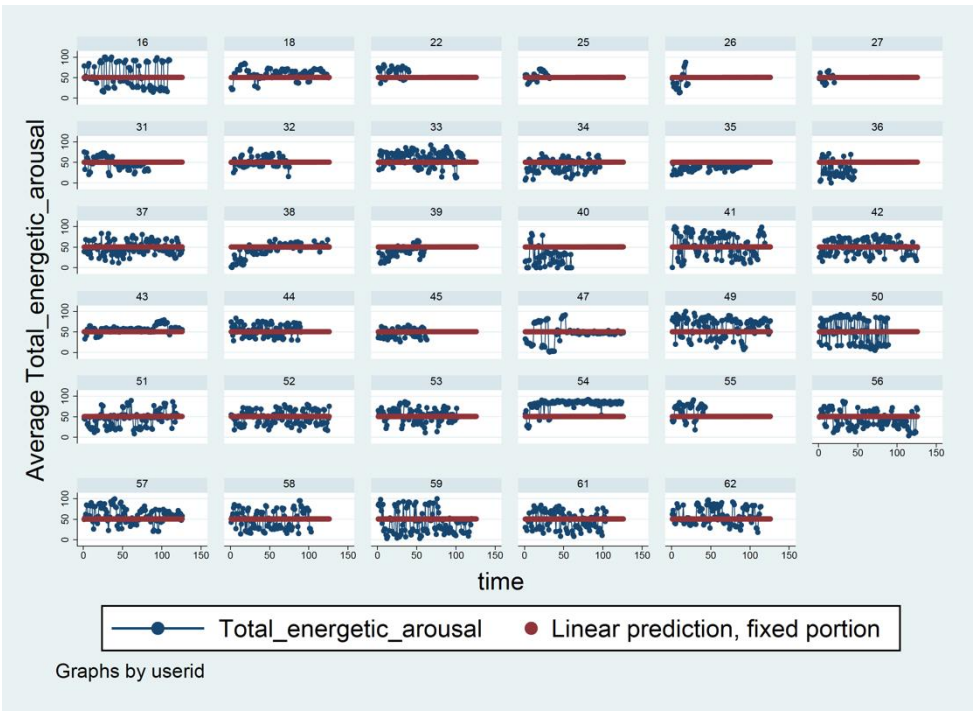


Figure J9.3

Fitting Plots for Individual Participants (n = 35) for Current Mood – Calmness

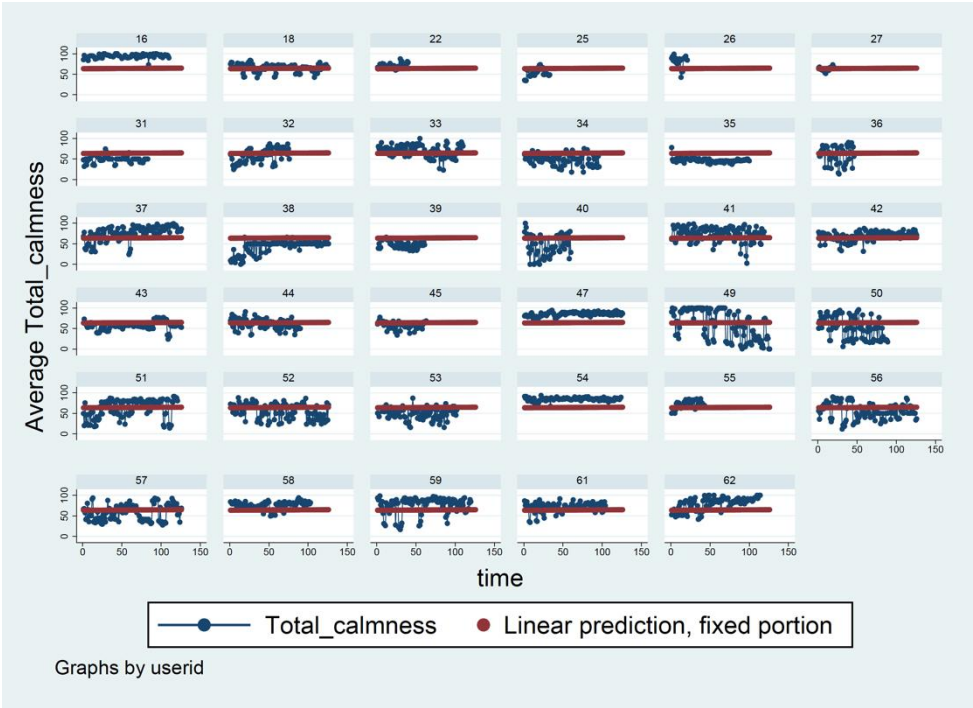


Figure J9.4

Fitting Plots for Individual Participants (n = 33) for End-of-Day Measures – Well-Being

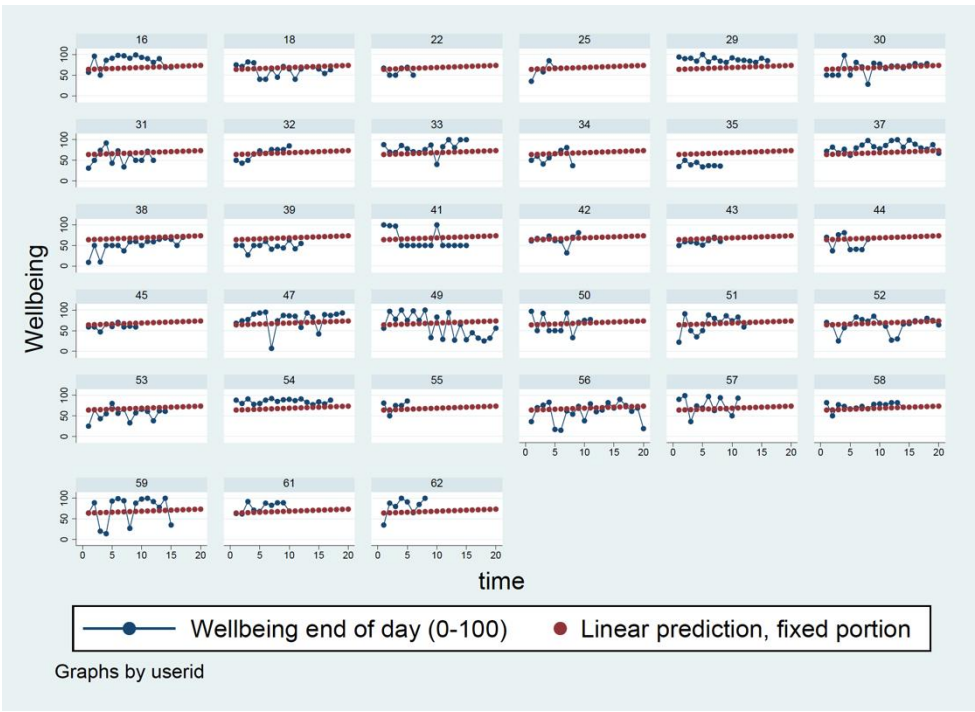


Figure J9.5

Fitting Plots for Individual Participants (n = 33) for End-of-Day Measure – Ability to Distance From Negative Stimuli

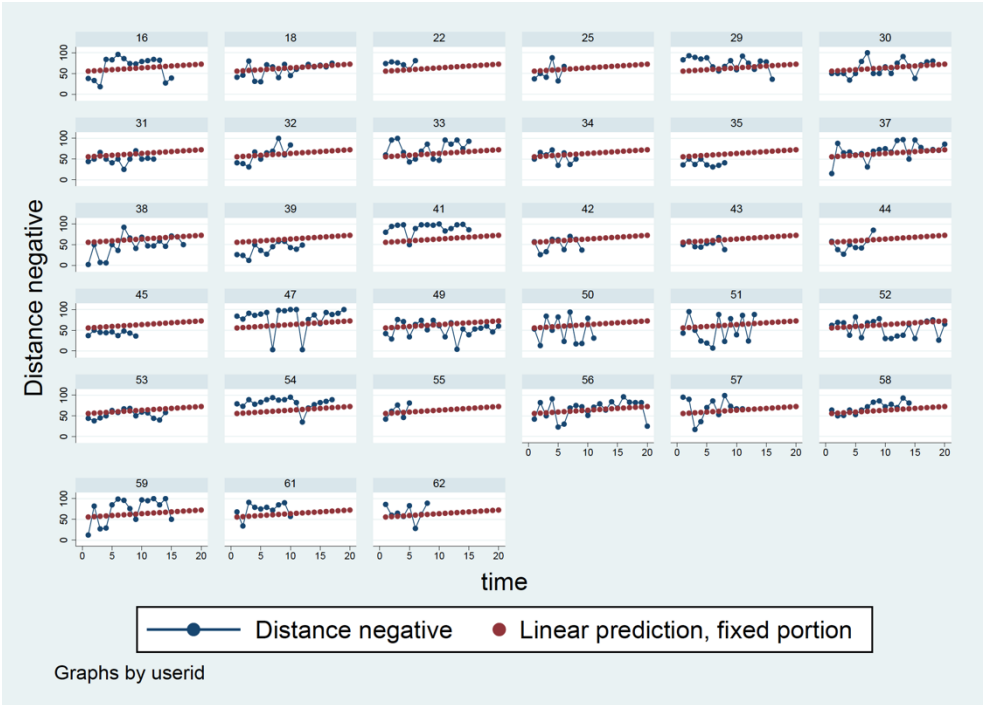
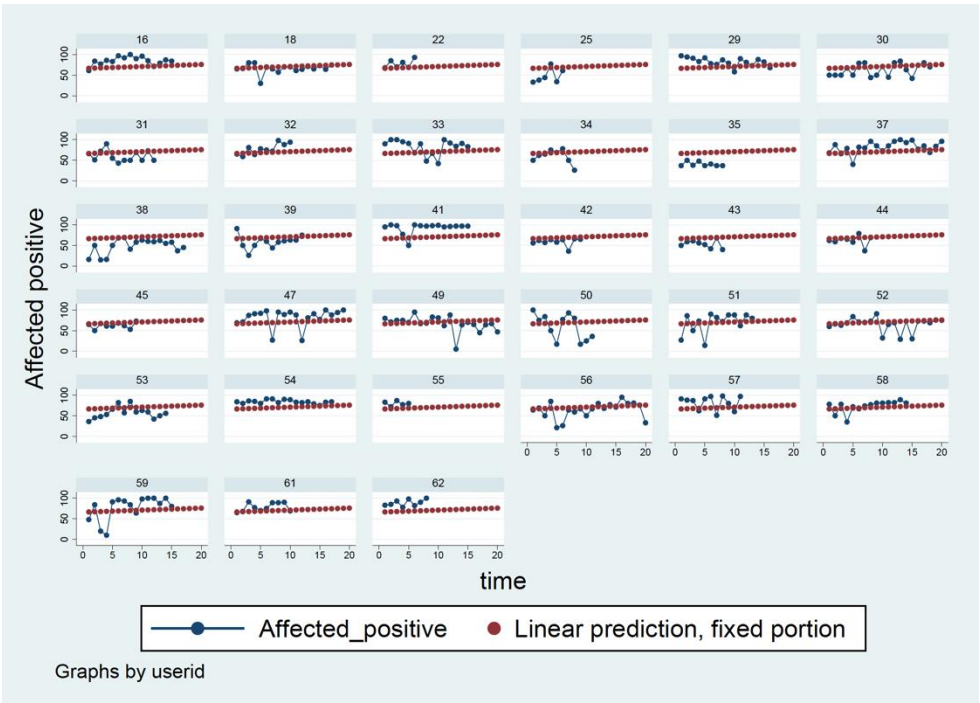


Figure J9.6

Fitting Plots for Individual Participants (n = 33) for End-of-Day Measure – Ability to Get Affected by Positive Stimuli



Appendix J10 Scatter Plots for Bivariate Correlations (RQ6)

Figure J10.1

Scatter Plot for the Pretest Correlation Between LOT-R (Total Score) and AAT-CS

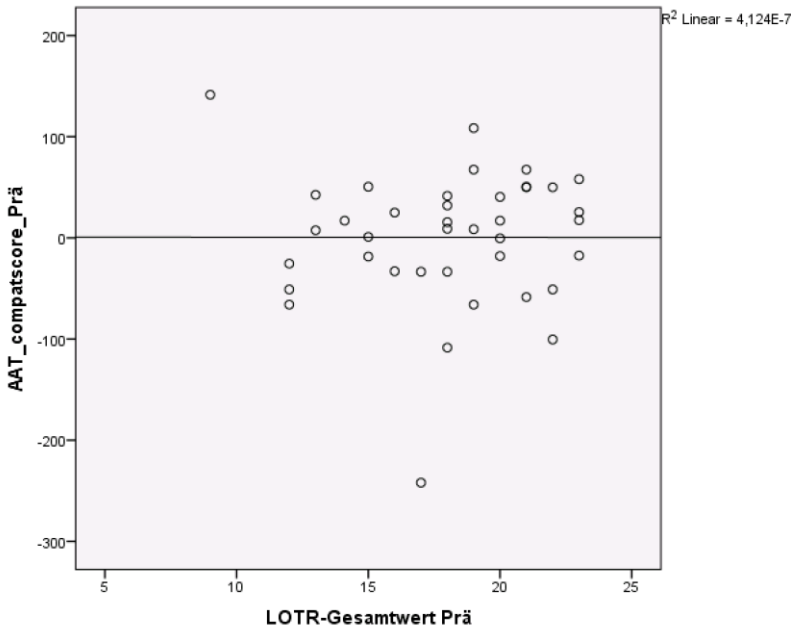


Figure J10.2

Scatter Plot for the Pretest Correlation Between LOT-R (Optimism Score) and AAT-CS

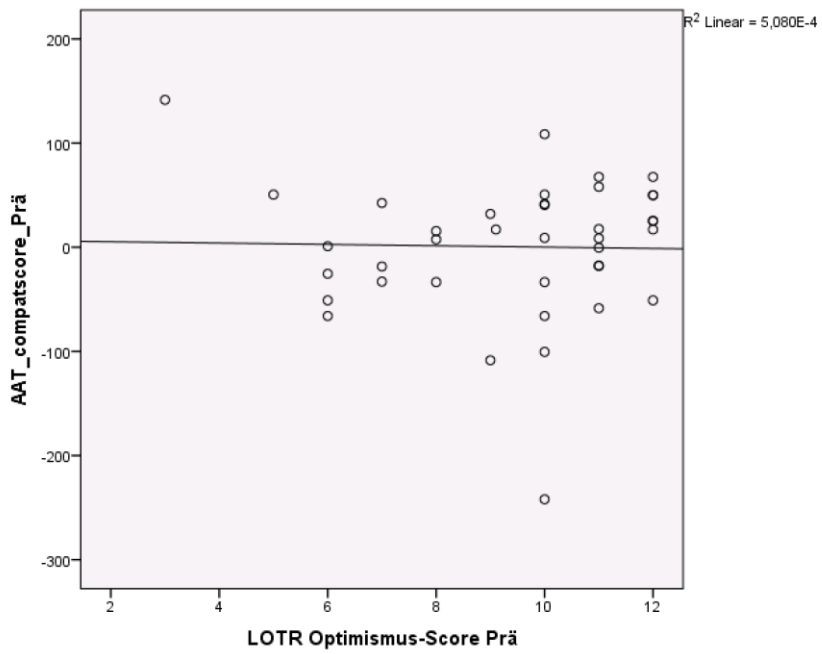


Figure J10.3

Scatter Plot for the Pretest Correlation Between ASF-E-P and AAT-CS

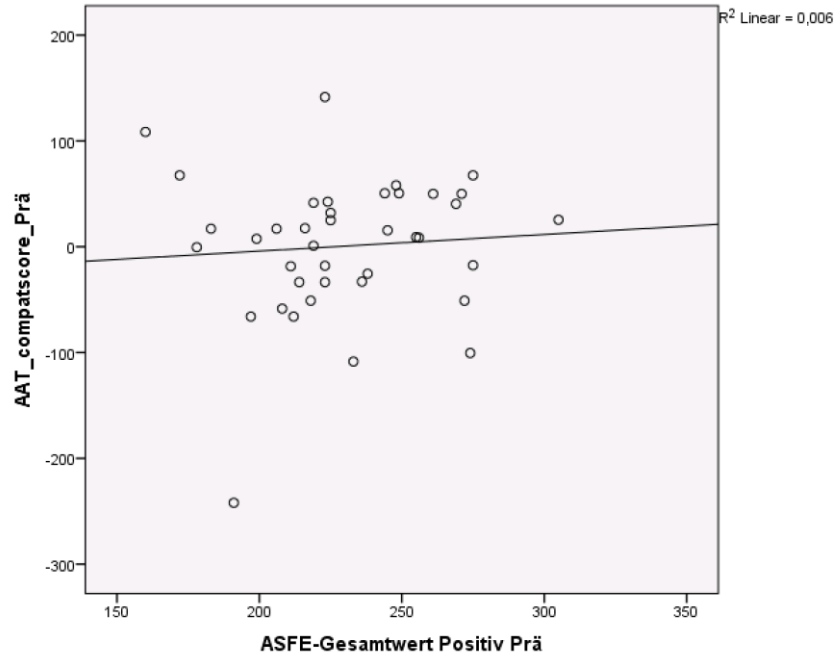


Figure J10.4

Scatter Plot for the Pretest Correlation Between ASF-E-N and AAT-CS

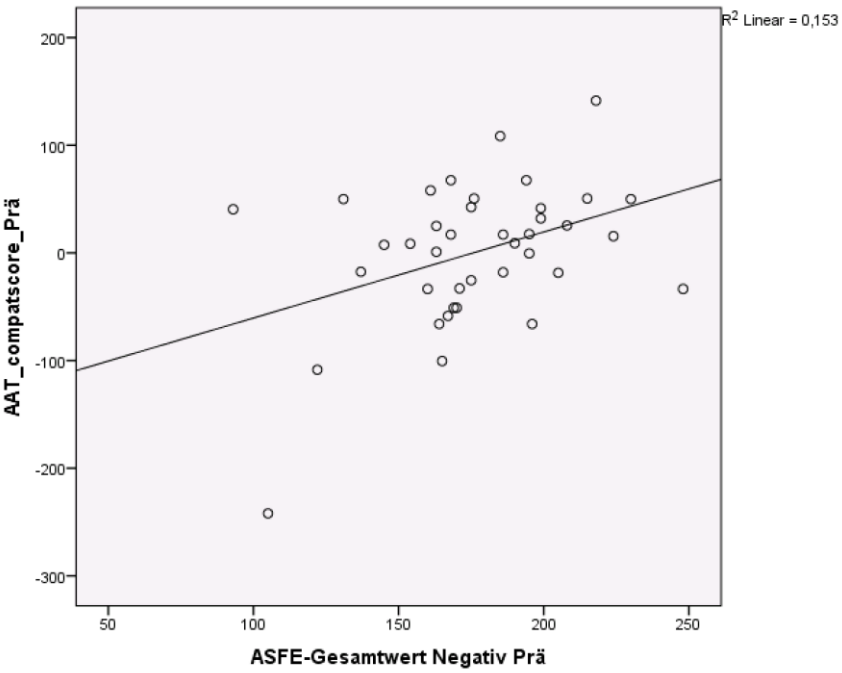


Figure J10.5

Scatter Plot for the Pretest Correlation Between BRS and AAT-CS

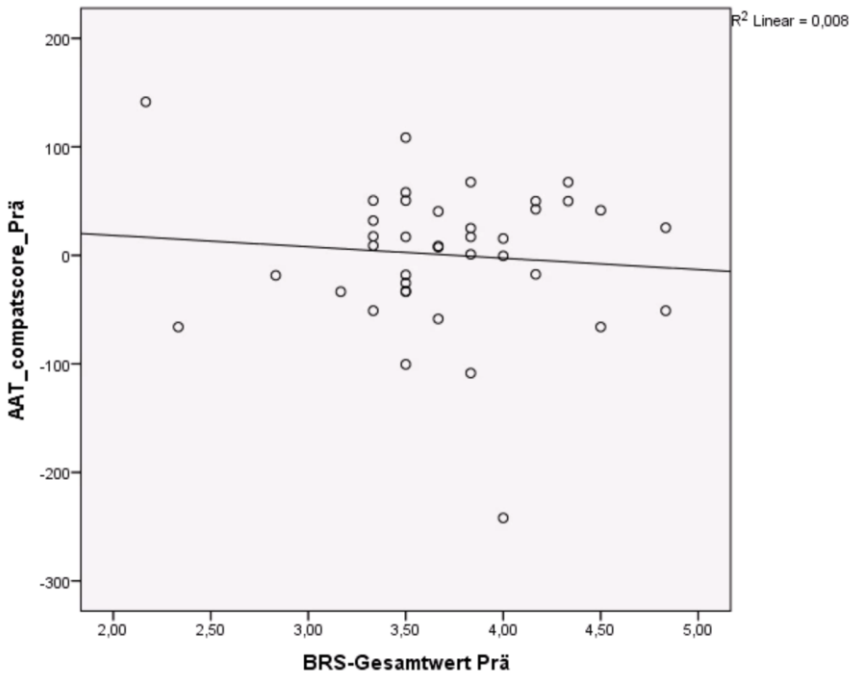
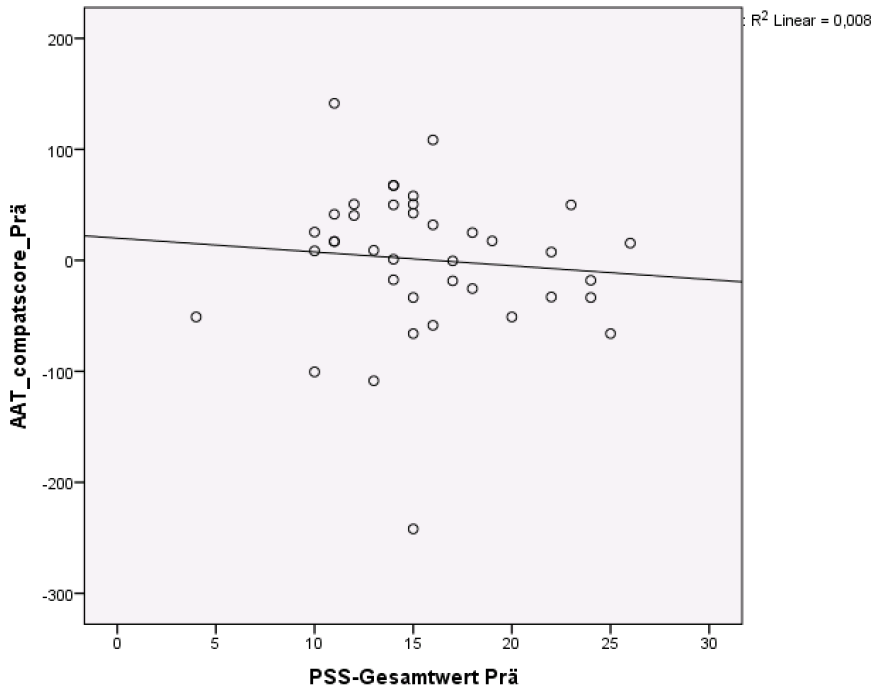


Figure J10.6

Scatter Plot for the Pretest Correlation Between PSS-10 and AAT-CS



Appendix J11 Evaluation of the Training and Qualitative Feedback From Participants

Table J11.1

Evaluation of the MB-PBT at Posttest (Self-Developed Items)

Item	*M(SD)*	*range*
1. Ich bin zufrieden mit dem Training.	2.3 (0.7)	1–3
2. Insgesamt wurden meine Erwartungen an das Training erfüllt.	2.3 (0.7)	1–4
3. Insgesamt wurden meine Erwartungen an das Training nicht erfüllt.	2.5 (0.8)	1–4
4. Das Erlernte ist nützlich für meinen privaten und/oder beruflichen Alltag.	1.9 (0.8)	1–3
5. Ich finde das Training gut anwendbar.	2.6 (0.9)	1–4
6. Das Training ist bedeutsam für meinen privaten/beruflichen Alltag.	1.5 (0.6)	1–3
7. Ich kann das Erlernte in meinem privaten/beruflichen Alltag anwenden.	1.8 (0.8)	1–4
8. Es fällt mir schwer, das Erlernte im privaten/beruflichen Alltag anzuwenden.	2.7 (1.0)	1–4
9. Das Training hat mir dabei geholfen, mich besser von Negativem zu distanzieren.	1.8 (0.8)	1–3
10. Das Training hat mir dabei geholfen, das Positive näher an mich heranzulassen.	1.9 (0.9)	1–4
11. Es fällt mir schwer, mich von Negativem zu distanzieren.	2.4 (0.8)	1–4
12. Es fällt mir schwer, das Positive nahe an mich heranzulassen.	1.9 (0.7)	1–4
13. Die Ausführung der „Wegwisch“-Bewegung auf dem Smartphone während des Trainings verbinde ich mit der Distanzierung von negativen Dingen (z. B. Ereignisse, Situationen, Personen etc.).	2.2 (1.0)	1–4
14. Die Ausführung der „Heranzieh“-Bewegung auf dem Smartphone im Training verbinde ich mit dem Heranlassen von positiven Dingen (z. B. Ereignisse, Situationen, Personen etc.).	2.2 (0.9)	1–4
15. Das „Wegwischen“ der negativen Bilder im Training hat dazu geführt, dass ich mich besser von Negativem distanzieren konnte.	1.7 (0.7)	1–3
16. Das „Heranziehen“ der positiven Bilder im Training hat dazu geführt, dass ich das Positive näher an mich heranlassen konnte.	1.7 (0.8)	1–3
17. Die Gestaltung der Trainings-App finde ansprechend.	2.8 (0.9)	1–4

Note. M = mean; *SD* = standard deviation; item format for the 17 items from 1 = *does not apply („trifft nicht zu“*) to 4 = *does fully apply (“trifft voll und ganz zu”)*; *N* = 41.

Table J11.2

Further Single Items (Self-Developed) Concerning Evaluation of MB-PBT

Item	*n* (%)
1. Das Training hatte negative Auswirkungen auf mich	
ja	10 (24.4%)
nein	31 (75.6%)
2. Würden Sie dieses Training Ihrer Familie/Freunden/Arbeitskollegen etc. weiterempfehlen	
yes	14 (34.1%)
no	22 (53.7%)

Item	*n* (%)
no indication	5 (12.2%)

Note. $N = 41$.

Table J11.3

Summary of Qualitative Feedback of Participants Regarding the MB-PBT

Item	Summary of answers in open-response format
„Falls das Training negative Auswirkungen hatte, bitte genauer beschreiben.“	• Training sessions and EMA: - in part, mood-depressing effects of the pictures in the morning - in part, training was perceived as a (additional) stressor (e.g., pressure due to training during specific times to avoid minus points) - seeing the “negative” pictures increased negative mood - subjective feeling of irritation if training session was forgotten • Rewarding system: - training experienced as frustrating/irritating, as it was possible to gain only a maximum of five points for one successful training session, but many minus points - minus points over night (“frustrating”; “reduced motivation”)
„Was hat Ihnen am Training gefallen?“	• General aspects and training conditions: - use of study smartphone - low time requirement, flexibility - training routine - training did distract from stress • Background of training: - underlying idea behind the training (e.g., differentiate between positive and negative pictures by different movements) - training initialized thoughts about the topic → positive stimuli should be approached versus distancing from negative stimuli → increased awareness of difficulties; more thinking about dealing with events and emotions - positive feeling to do something for self-care that can be performed easily; to get active • Design of training: - app design - gaming character - format of training - simple swiping movements - progress is visible • Training sessions and EMA: - easy and fast training sessions; easy to integrate in daily life (e.g., training could be performed everywhere, no Wi-Fi required) - current mood questionnaires after the sessions - EMA in the morning and evening (“Tagesstart”, “Tagesende”) • Pictures: - selection of pictures (e.g., interesting and positive pictures; presentation of daily situations that are known from personal life or movies/TV, like political aspects) - Aggressive-reducing effects of swiping negative pictures away

	• Rewarding system: - avatar and rewarding system (e.g., compliment for training after each completed session)
„Was hat Ihnen am Training nicht gefallen?“	• General aspects and training conditions: - use of study smartphone - app always had to be online - high time effort - technical problems (e.g., crashing; re-installation was necessary; app was instable; long app load time) - (limited) recommended training times (morning/evening) which was not always possible in daily life • Background of training: - no experience of conscious rethinking or positive attributions of daily situations in association with the training - unclear if training is really effective/personal impression that it is not effective • Training sessions and EMA: - swiping movement is not intense enough to integrate into daily life and use it - during training sessions (swiping movements), the pictures partly “stucked” - time period for training in the evening too short - current mood questionnaires after the sessions - accidental movements of selected answers when scrolling up and down - reminder function for start/end of day measures did not work - in part, no memory of how many training sessions had already been performed on a specific day • Pictures: - picture rating in the beginning (“mindless” rating without really focusing on the pictures) - selection of pictures o e.g., pictures not appealing enough o pictures partly indefinable; ambiguous pictures that were difficult to be assessed as positive/negative o too many pictures with little emotional relevance o “positive” pictures were not really more positive o 90% of pictures without personal relevance → difficult to assess them as positive or negative o no clear valence of certain pictures due to contradicting meaning and aesthetics - repeated pictures across 21 days of training • Rewarding system: - minus points (e.g., frustrating and demotivating effect if negative scores over night; feeling of pressure) - rewarding system is too strict - errors in scoring (e.g., avatar did not change despite training during recommended times) - no further “training successes” after 21 days - scoring and gaining/losing points for training sessions in general

"Verbesserungsvorschläge"	• General aspects and training conditions: - use of personal mobile phone - improved programming - improved internet connection by use of SIM card • Design of training: - more gamification (e.g., faster changes of avatar, animations of the avatar to foster positive emotions) • Training sessions and EMA: - 30 pictures in one training session instead of 3x10 in the morning and evening - possibility to indicate events that were positive/negative when swiping positive pictures closer and negative pictures away - item concerning the ability to distance from negative stimuli ("Tagesende"): participants should name an example to increase awareness - items of start/end of day assessments could be adapted individually to participants → time periods where points are gained could be based on individual schedule - more exercises beyond picture training with swiping movements - improved reminder function (daily reminder of training sessions; more reminders; times when training has to be performed should be indicated) - no specific recommended times but training over whole day (i.e., more flexible training times that are adapted to the individual daily rhythm) - longer training times in the evening (e.g., 7 p.m. to 1 a.m.) - less training sessions per day (e.g., two times) • Pictures: - larger selection of pictures - pictures with greater relevance for personal life/daily life (e.g., from holiday, family, friends, favorite band); e.g., pictures can be uploaded or only used for the app - more precise pictures with less ambiguity - use of pictures with more consistent aesthetic level • Rewarding system: - rewarding system without automatic minus points if training sessions are not performed - no rewarding system with gaining/losing points at all - no rewarding system with gaining/losing points, but more motivating feedback

Note. N = 41.

Appendix K Digital Appendix TRAIN$_4$Positivity