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Investigating the Specificity of Mindfulness and Self-Compassion: Effects, Mechanisms, and Implications for Mental Health Promotion

presented by
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May this dissertation serve those who seek to understand and alleviate suffering,
whether through research, practice, or the simple yet profound act
of bringing mindful awareness and compassion to each moment of experience.

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On to the next chapter.

Abstract

Mindfulness and self-compassion have played a central role in recent mental health promotion efforts, with interventions demonstrating beneficial effects, both by reducing psychological ill-being and enhancing psychological well-being, across diverse populations and settings. However, these constructs face conceptual ambiguities, and fundamental questions about outcome specificity and underlying change mechanisms remain. This dissertation will address important research gaps pertaining to the examination of mindfulness and self-compassion across different conceptualizations—as practices, interventions, predictors, and mechanisms of change—through three empirical studies with diverse samples and methodologies. This work aims to contribute to a clearer understanding of when these approaches yield specific versus broad effects and through which processes change occurs in non-clinical populations.

To this end, in Chapters 2 and 3 of this dissertation, I present findings of two complementary randomized controlled trials that examine specific and broad effects of two mental health promotion interventions designed for healthy adults of different durations and delivery modes. More specifically, Chapter 2 evaluates a brief online, self-guided self-compassion training compared to an active control in a German convenience sample ($N = 200$; 85.5% female; $M_{age} = 30$ years; range 18-69 years) with a 4-week follow-up. Findings indicate similar improvements in self-compassion, self-criticism, perfectionism, and psychological well-being across both conditions, with self-compassion-specific training effects emerging only for highly self-critical individuals.

Chapter 3 examines an 8-week in-person, trainer-led socioemotional competence training targeting stress management and social competences in younger and older German adults ($N = 166$; 75.2% female; $M_{age} = 46.26$ years; range 19-39 and 50-78 years). Compared to a waitlist control group, specific effects only emerged on directly trained outcomes (i.e., mindfulness, perceived stress). They did, however, not spill over to broader domains, such as self-compassion and other indicators of emotional and social functioning. Still, most training effects persisted across 3- and 12-month follow-up.

Chapters 4 and 5 focus on potential mechanisms of mindfulness and self-compassion, examining state-trait relationships and self-referential processes more closely. Building on the same dataset as the study presented in Chapter 3, results of Chapter 4 revealed that despite significant improvements in state and trait indicators, state mindfulness changes did not predict trait-level changes in mindfulness, self-compassion, and perceived stress.

Chapter 5 presents data from a binational longitudinal study ($N_{T1} = 615$; $N_{T2} = 310$; 51.5% female; age range 18–84 years) on the relationship of mindfulness and self-compassion with self-evaluative processes. Results demonstrate that both mindfulness and self-compassion predicted reduced comparison frequency and more favorable comparison outcomes but not perceived comparison utility. These findings point to potential differences in self-referential processes, that is, differences in how more mindful and self-compassionate people perceive and relate to self-relevant information.

This systematic investigation underlines that mindfulness and self-compassion interventions yield both specific and broad effects and that intervention framing and design may affect the outcome specificity. Furthermore, the dissertation highlights the importance of distinguishing between actual state or trait change and changes in self-referential processes. Collectively, these findings underscore the need for precision-oriented approaches that consider individual characteristics, intervention framing, and the complex interplay between behavioral change and self-perception in future mental health promotion efforts.

Chapter 1: General Introduction

The past three decades have witnessed an extraordinary surge in scientific interest in mindfulness and self-compassion. What began as niche research on ancient contemplative practices, primarily derived from Buddhist origins, has evolved into a major field of psychological science (Shonin et al., 2014). Mindfulness refers to non-judgmental awareness of present-moment experience, while self-compassion entails responding to one's difficulties with kindness rather than self-criticism (Kabat-Zinn, 1994; Neff, 2003a). Beyond formal intervention programs, these practices have been systematically integrated across diverse settings relevant to everyday life—including healthcare, education, organizations, military training, and professional sports—with numerous studies documenting their benefits in mental health promotion (Crane et al., 2017; Neff, 2023; van Dam et al., 2018). Paralleling this rapid increase in scientific interest, in recent decades mindfulness and self-compassion have also become deeply ingrained in contemporary society and daily lives of many in Europe and the USA. This cultural adaptation is evident in widespread media coverage of mindfulness and self-compassion as self-optimization solutions, popular psychology books teaching these techniques for all age groups, and the availability of related products in mainstream stores (e.g., cushions, sound bowls, Buddha statues). Moreover, the two leading mindfulness and meditation apps, Headspace and Calm, dominate the mental health app market, accounting for about 70% of monthly active users (Wasil et al., 2022). This scientific and popular adoption reflects a remarkable development—practices once only found in religious contemplative traditions have become accessible, secularized exercises embraced by millions seeking stress reduction and psychological well-being (Goldberg & Davidson, 2024; Jiwani et al., 2022).

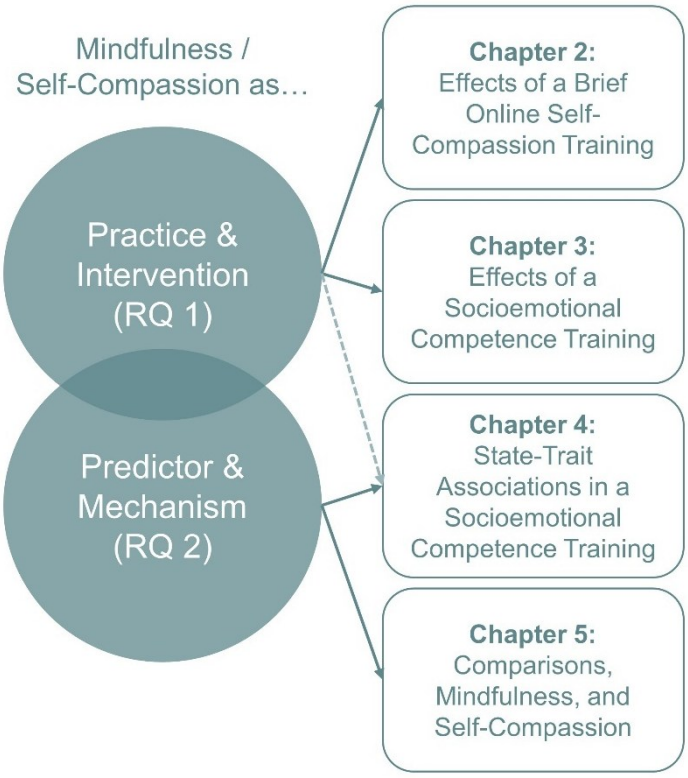
However, mindfulness and self-compassion interventions face similar challenges to those found in psychotherapy and personality intervention research, with fundamental open questions about how, when, and for whom psychological interventions actually work (Cha et al., 2023; Krämer et al., 2025; van Dam et al., 2018; Zilcha-Mano, 2021). Important research gaps pertaining to the specificity of mindfulness and self-compassion intervention effects, as well as their mechanisms, remain (Cha et al., 2023; Finlay-Jones, 2023; Goldberg, 2022; Goldberg & Davidson, 2024; Goldberg et al., 2017, 2022). Both concepts have also been criticized for their conceptual and psychometric ambiguity, likely resulting from the inherent challenge of translating ancient complex ideas into unitary psychological constructs (Baer, 2019; Cha et al., 2023; Chiesa, 2013; Pfattheicher et al., 2017; Strauss et al., 2016; van Dam et al., 2018). For instance, mindfulness and self-compassion have been conceptualized as predictors, mechanisms, and outcomes. Within these conceptualizations, they have been

measured as transient states, stable traits, and learnable skills which can be developed through practice (Baer, 2019; Neff, 2023; Neff et al., 2021; Warren et al., 2023). Given this multiplicity, researchers have suggested that their multifaceted nature is too broad to be described, explained, and predicted by a single theoretical model (Cha et al., 2023; van Dam et al., 2018). Consequently, there have been calls for a more granular analysis of these concepts, zooming in on specific aspects of mindfulness and self-compassion as practices and supposed mechanisms to isolate active therapeutic ingredients and tailor interventions more precisely (Cha et al., 2023; van Dam et al., 2018).

The goal of this dissertation is to contribute to understanding the specificity and boundary conditions of mindfulness and self-compassion by systematically examining both constructs across multiple conceptualizations—as practices in and outcomes of interventions (Chapters 2 and 3) and as predictors and mechanisms of change (Chapters 4 and 5). More specifically, this dissertation focuses on two central research questions: (RQ 1) What are the specific and broad effects of mindfulness exercises and self-compassion-based interventions with different delivery formats in non-clinical samples? And (RQ 2): How do mindfulness and self-compassion predict trait change and self-referential processes? Figure 1.1 depicts an overview of how these questions will be covered in the following chapters of this dissertation.

Figure 1.1

Overview of Mindfulness and Self-Compassion as Practices, Interventions, Predictors, and Mechanisms in the Chapters of this Dissertation.



1.1 From Reactive Treatment to Proactive Mental Health Promotion

Good mental health is essential for individuals to lead healthy, fulfilling, and productive lives. Conversely, poor mental health negatively impacts educational and professional achievement, physical health, and mortality (e.g., Cuijpers et al., 2014; M. Fernandes et al., 2023; Organization for Economic Cooperation and Development [OECD], 2018, 2023; Walsh et al., 2023). The impact of poor mental health extends beyond individual consequences to workplaces, healthcare systems, and economies at large. For instance, the number of psychological disorders in Germany doubled between 2013 and 2023 and they rank among the three most prevalent causes of sick leave (DAK, 2024). This trend is not unique to Germany. Globally, mental health issues generate substantial economic and societal costs, such as reduced productivity at work and increased health insurance costs (McGorry, 2013; Mulfinger et al., 2024; Trautmann et al., 2016).

The rising levels of psychological distress emphasize the relevance of appropriate treatment and prevention strategies. Historically, mental healthcare has focused on treating disorders with medication or psychotherapy. Although essential, the effectiveness of treatments in reducing the overall burden of mental health problems is constrained by accessibility issues such as long waiting lists and geographical barriers (Bennett et al., 2020). Additionally, treatment scope remains limited, with individuals below diagnostic thresholds remaining untreated despite elevated distress (Bennett et al., 2020; Bower & Gilbody, 2005). Therefore, scholars underscore the importance of innovative mental health promotion and prevention efforts (Cuijpers et al., 2021; Galante et al., 2021, 2023; Keyes, 2007; McGorry, 2013).

This focus aligns with the World Health Organization's (WHO) definition of health as holistic physical, psychological, and social well-being rather than just the absence of illness (WHO, 1946). Similarly, the two continua model suggests that mental health and illness are related but separate dimensions. Individuals can enjoy high well-being even with psychological symptoms or experience low well-being without meeting clinical diagnoses (Keyes, 2005, 2007). Consequently, it is important to address both psychological well-being and ill-being.

Along with the paradigm shift from reactive treatment to mental health promotion and prevention, psychological intervention approaches have diversified. Besides traditional psychodynamic and cognitive-behavioral psychotherapy (CBT), "third wave" approaches have emerged. While traditional approaches focused on symptom reduction by changing dysfunctional thoughts and behaviors, third wave approaches emphasize psychological well-being promotion by including how individuals relate to their experiences (Goldberg et al., 2023; Henriques & Gralha, 2024). To facilitate these processes, many third wave approaches

incorporate contemplative concepts and practices from Buddhism, such as meditation, mindfulness, and acceptance practices (Chiesa & Malinowski, 2011; Goldberg et al., 2023; Shonin et al., 2014). Prominent third wave approaches include Systemic Therapy (von Schlippe & Schweitzer, 2016), Acceptance and Commitment Therapy (Hayes et al., 1999), Dialectical Behavior Therapy (Linehan & Wilks, 2015), Compassion Focused Therapy (P. Gilbert, 2009), or Mindfulness-Based Cognitive Therapy (Segal et al., 2013). Concurrently, the positive psychology movement emerged and further shifted the focus from deficits to resources, resilience, and psychological well-being (Seligman & Csikszentmihalyi, 2000).

This historical development has not only significantly broadened the content, but also the scope of psychological interventions. Interventions have become increasingly suitable for non-clinical populations. Offering low-threshold mental health promotion interventions to this group is particularly valuable because such interventions can address psychological issues before they escalate to clinical levels (Cuijpers et al., 2021). Previous research suggests that increases in psychological well-being reduce the likelihood of developing a psychological disorder (Keyes et al., 2010). This points to the immense preventive potential of well-being promotion interventions. Crucially, meta-analyses on mental health promotion and preventive interventions show their effectiveness in reducing disorder incidence by up to 20%, while also improving and maintaining psychological well-being over the long term (Cuijpers et al., 2021; Galante et al., 2021, 2023; van Agteren et al., 2021).

Additionally, the demand for mental health promotion is high: Research indicates that up to 85% of healthy adults want to improve some aspects of themselves, especially their stress management and social skills (Baur et al., 2023; Grosse & Grawe, 2002; Hudson et al., 2019; Lücke et al., 2021; Stieger et al., 2021). Notably, changes are larger when interventions target participants' own identified areas for growth—that is, when participants initially score low on a specific trait (e.g., Baur et al., 2023; Hudson et al., 2019, 2020; Olaru et al., 2023; Stieger et al., 2021). Importantly, non-clinical populations typically show relatively high psychological functioning, which facilitates motivation, willingness, and ability to change—characteristics that clinical groups may often lack (Eryılmaz & Doenyas, 2025; Olaru et al., 2024; Ryan et al., 2011). Together, these considerations underscore the importance of comprehensive approaches that target both psychological illness and well-being among non-clinical populations to maximize mental health improvements.

1.2 The Surge of Contemplative Practices: Mindfulness and Self-Compassion

While most third wave approaches still primarily focus on clinical populations, researchers have developed interventions based entirely on Buddhist concepts to promote

mental health (Galante et al., 2021, 2023; Hofmann & Asmundson, 2008; Shonin et al., 2014). Secularization has made these concepts accessible across different populations regardless of religious or cultural background, facilitating their integration into contemporary psychology and Western mental healthcare (Crane et al., 2017; Goldberg et al., 2018).

The adaptation of mindfulness and mindfulness meditation has been especially influential during this integration (Goldberg & Davidson, 2024; van Gordon et al., 2022). The term mindfulness was presumably first introduced to psychological science by Rhys Davids in 1881 (Anālayo, 2018; Gethin, 2011). It derives from the Pali word *sati* (or Sanskrit *smrti*), which is typically translated as "to remember" (Gethin, 2011; Shonin et al., 2014). Contemporary scientific definitions emphasize two components: (a) the willingness to purposefully direct one's attention to the present moment and (b) the quality of this attention (Baer, 2019; Grossman & van Dam, 2011). The most prominent definition by Kabat-Zinn (1994) describes mindfulness as the ability to intentionally direct attention to present-moment experiences with a non-judgmental and curious attitude.

Compassion, another Buddhist concept receiving immense Western academic interest, originates from the Pali word *mettā* (also commonly translated as loving-kindness; Salzberg, 1997). Compassion involves the ability to recognize and understand perceived suffering and to feel a strong desire to alleviate it (Barnard & Curry, 2011; P. Gilbert et al., 2017; Salzberg, 1997; Strauss et al., 2016). Although Buddhism does not distinguish between different types of compassion (Barnard & Curry, 2011; Salzberg, 1997), researchers typically investigate three directional flows: compassion for others, compassion from others, and self-compassion (P. Gilbert et al., 2017; Zhuniq et al., 2025). Self-compassion is often studied separately from other forms of compassion (Neff, 2023). It describes the willingness to mindfully recognize one's suffering and to respond with kindness, understanding, and a sense of interconnectedness with others who may be experiencing similar suffering (Neff, 2003a, 2003b, 2023).

Because mindfulness constitutes a definitional component of self-compassion, these constructs inherently overlap moderately to strongly (r = .28 - .69; Hölzel et al., 2011; Neff & Dahm, 2015; Shapiro & Fitch, 2023). However, some important differences are worth noting. Mindfulness entails a purposeful yet open attention to *whatever* is present at a given moment (i.e., thoughts, feelings, sounds, sensations, objects, etc.; K. Brown & Ryan, 2003). Self-compassion specifically focuses on one's *negative* experiences, with the explicit intention to cultivate kindness and alleviate distress (Neff, 2003a, 2023). This difference shows that mindfulness can arise independently—while eating, walking, or breathing. Yet, mindfulness of *suffering* is regarded as a prerequisite for self-compassion: Individuals need to first recognize

their suffering before they can extend kindness toward these experiences (Neff & Dahm, 2015). Self-compassion also involves intentionally directing kindness and feeling connected to others through shared humanity. Essentially, mindfulness is directed towards the experience itself, whereas self-compassion is directed towards the person experiencing the negative event (Germer & Barnhofer, 2017; Neff & Dahm, 2015).

Both mindfulness and self-compassion demonstrate high transdiagnostic value: They reduce various psychiatric symptoms and improve multiple psychological well-being indicators across populations (Germer, 2023; Goldberg et al., 2022). Both concepts show overlapping beneficial effects, but self-compassion appears to have stronger predictive power for socio-affective outcomes such as depression, anxiety, worry, happiness, quality of life, and psychological well-being (Baer, Lykins, & Peters, 2012; Hildebrandt et al., 2017; Neff & Dahm, 2015; van Dam et al., 2011). Meanwhile, mindfulness is supposedly the stronger predictor of attentional and cognitive outcomes (Germer & Barnhofer, 2017; Hildebrandt et al., 2017). Given their close relationship, studying both together and separately helps identifying common and unique effects, which is especially meaningful for reaching a more comprehensive understanding of contemplative psychology. As mentioned earlier, their secular adaptations make them accessible to a wide audience, which aligns well with the paradigm shift toward prevention and well-being promotion. Moreover, mindfulness and self-compassion practices are easily scalable across different delivery formats, target groups, and intervention settings (Ferrari et al., 2019; Goyal et al., 2014; Linardon, 2020; Sommers-Spijkerman et al., 2021).

1.3 Conceptualizations of Mindfulness and Self-Compassion

Mindfulness and self-compassion are examined as predictors, mechanisms, and outcomes. For this purpose, they have been conceptualized and measured as traits, states, and learnable skills (Baer, 2019; Neff et al., 2021). *Trait conceptualizations* mainly define mindfulness and self-compassion as relatively stable individual differences, that is, as general dispositions to be mindful or self-compassionate across different situations and over time (Baer et al., 2006; Neff, 2003b). *State conceptualizations* refer to momentary levels of mindfulness and self-compassion in specific situations or time points (Neff et al., 2021; Warren et al., 2023). The repeated experience of these momentary states is thought to represent a density distribution that constitutes trait levels (see Whole-Trait-Theory; Warren et al., 2023). *Practice conceptualizations* investigate mindfulness and self-compassion as activities people engage in, such as formal meditation or informal exercises that are integrated into daily life (Baer, 2019; Neff, 2023; Parsons et al., 2017). These trait, state, and practice conceptualizations are typically

assessed through self-report questionnaires that measure both momentary and characteristic ways of relating to experiences or the self during difficult times (Baer et al., 2019; Neff, 2023). Additionally, practice effects are examined through behavioral tasks (e.g., counting the breath paradigms; Frewen et al., 2016) or neuroimaging (Fox et al., 2016).

1.4 Mindfulness- and Self-Compassion-Based Interventions

The growing scientific interest in contemplative practices has sparked considerable effort to develop mindfulness-based (MBIs) and self-compassion-based interventions (SCBIs). The pioneering Mindfulness-Based Stress-Reduction (MBSR) program showed initial efficacy for chronic pain and stress-related conditions (Jon Kabat-Zinn, 1982, 2013). This laid the foundation for many contemplative-based interventions, which have been rigorously evaluated in both clinical and non-clinical populations. Notable examples include Mindfulness-Based Cognitive Therapy (Segal et al., 2013), Mindful Self-Compassion (MSC; Neff & Germer, 2013), and Cognitively-Based Compassion Training (Ash et al., 2021; Negi, 2014; Pace et al., 2009).

Most MBIs and SCBIs aim to both reduce psychological distress and enhance psychological well-being (Ferrari et al., 2019; Goldberg et al., 2022). These interventions have demonstrated successful transdiagnostic application across a wide range of psychiatric and physical conditions in clinical populations, including affective, eating, substance abuse, and personality disorders, ADHD, psychosis, and cancer recovery (Austin et al., 2020; Cairncross & Miller, 2020; L. Carlson et al., 2013, 2016; Goldberg et al., 2018; Khoury, Lecomte, Gaudiano, & Paquin, 2013; Kirby et al., 2017). In non-clinical populations, MBIs and SCBIs show similar beneficial outcomes, such as reduced psychological distress, anxiety, depressive symptoms, and self-criticism, along with improved psychological well-being, coping skills, prosocial behavior, and emotion regulation (Cha et al., 2022; Donald et al., 2019; Ferrari et al., 2019; Finlay-Jones, 2023; Galante et al., 2021, 2023; Goldberg et al., 2022; Wakelin et al., 2021).

Delivery formats range from short exercises to multi-week courses or intensive retreats, usually delivered in group settings by trained instructors. In-person, online, and smartphone formats demonstrate comparable effectiveness on outcome measures (see e.g., Han & Kim, 2023; Jayawardene et al., 2017; Linardon, 2020; Sommers-Spijkerman et al., 2021; Wasil et al., 2022). Digital formats are especially promising for mental health promotion because of their accessibility, cost-effectiveness, and easy lifestyle integration (Andersson & Titov, 2014; Beecham et al., 2019; Carlbring et al., 2018). These features may especially appeal to individuals with subthreshold symptoms, fear of stigmatization, and healthy individuals

seeking targeted support (e.g., better stress-regulation). Conversely, in-person formats may be preferable for those looking for social connection or interpersonal skill development through real-time practice with nonverbal communication and social feedback that may be difficult to facilitate online (Andersson & Titov, 2014; Norwood et al., 2018). Therefore, considering the target group is essential for designing effective mental health interventions. Previous research indicates that MBIs and SCBIs serve diverse populations, including community members, adolescents, students, parents, athletes, psychiatric and healthcare patients, and employees in healthcare, organizational, and military contexts (Crane et al., 2017; Neff, 2023).

Evidently, the multifaceted conceptualizations of mindfulness and self-compassion are reflected in the heterogeneity of interventions, target populations, and design elements (e.g., delivery format, content, frequency, duration, etc.; Ferrari et al., 2019; Crane et al., 2017; van Dam et al., 2018). This diversity reflects the different ways individuals engage with mindfulness and self-compassion and how these qualities evolve and change over time. However, it also confounds comparison of intervention outcomes within MBI and SCBI research, and more broadly, limits understanding of intervention specificity versus common factors compared to other mental health promotion interventions (Cha et al., 2023; Ferrari et al., 2019; Goldberg, 2022; van Dam et al., 2018). Therefore, a systematic examination of the specificity (or breadth) of MBIs and SCBIs remains to be fully determined.

Comparative studies and meta-analytical results indicate that the effects of MBIs and SCBIs on mental health outcomes are small to moderate relative to waitlist controls and generally small or nonsignificant when compared to active comparison groups and established treatments like CBT (Ferrari et al., 2019; Galante et al., 2021, 2023; Goldberg et al., 2017, 2018, 2022; Goyal et al., 2014; Han & Kim, 2023; Khoury, Lecomte, Fortin, et al., 2013; Kirby, 2017; Kirby et al., 2017; van Agteren et al., 2021; Vrabel et al., 2024; Wakelin et al., 2021; A. C. Wilson et al., 2019). Apparently, these effects vary depending on the previously described characteristics: For instance, MBIs and SCBIs appear to be slightly more effective than other psychological interventions for depression but not for anxiety (outcome), and more effective for adults than for students (target group; Ferrari et al., 2019; Goldberg et al., 2022). Additionally, preliminary evidence suggests that online adaptations do not outperform other active online psychological interventions (delivery format; Han & Kim, 2023; Sommers-Spijkerman et al., 2021).

Although these nuanced findings support the overall effectiveness of MBIs and SCBIs, the evidence for their effect specificity remains vague. The authors of these meta-analyses also highlight several methodological limitations and call for more rigorously designed, high-

quality randomized controlled trials (RCTs). Specifically, comparisons with closely matched, evidence-based active control treatments, longer follow-up periods, adequately powered samples, and more thorough examination of online delivery formats are needed to better understand intervention outcome specificity (Ferrari et al., 2019; Goldberg et al., 2022; A. C. Wilson et al., 2019). Moreover, previous research has largely relied on young and female populations. This is problematic because by systematically overlooking healthy older adults, research fails to account for increasing longevity and demographic changes in Western countries. Although preliminary evidence shows modest benefits of MBIs and SCBIs for improving psychological well-being and reducing depression, anxiety, and perceived stress among older adults, intervention effectiveness in this growing population remains largely unevaluated (L. Brown et al., 2019; Li & Bressington, 2019; Othman et al., 2025; Reangsing et al., 2021; Verhaeghen et al., 2025).

These meta-analyses also reveal that specific MBIs and SCBIs demonstrate broad effects that extend beyond mindfulness, self-compassion, or closely related variables to variables such as prosocial behavior and coping (e.g., Berry et al., 2020; Ewert et al., 2021; Liu et al., 2025; Zheng et al., 2023). Conversely, other psychological interventions increase mindfulness and self-compassion without specifically targeting them (e.g., CBT; Goldberg et al., 2019, 2022; Quaglia et al., 2016; A. C. Wilson et al., 2019). Similar findings exist in psychotherapy research, where personality traits change without explicit targeting (Roberts et al., 2017), and personality intervention effects "spill over" to several domains of life satisfaction and self-esteem (Allemand et al., 2024a; Hähner et al., 2024; Olaru et al., 2023). These broad, and often unintended, effects have often been dismissed, and recently termed "by-product" (Krämer et al., 2025), or "spillover effects" (Hähner et al., 2024).

Although ubiquitous, so far, studies have not yet systematically tried to identify the broad effects of MBIs, SCBIs, or any psychological intervention more generally, nor examined their implications for intervention design. However, this has relevance beyond simply proving the existence of nontargeted effects. By narrowing the focus to specific outcomes, target groups, and frameworks, research has overlooked that healthy individuals may often pursue multiple change goals with high transdiagnostic value simultaneously (e.g., stress regulation or social interaction skills; R. Smith et al., 2024; van Agteren et al., 2021). Therefore, given their usually high functional level, healthy individuals may benefit from interventions that target multiple characteristics at once.

The randomized controlled trials in Chapters 2 and 3 address the identified research gaps through two divergent approaches by examining specific and broad effects of contemplative

interventions across different delivery formats (RQ 1). More specifically, Chapter 2 evaluates the specificity of a brief online, self-guided self-compassion training compared to a closely matched active control stress-reduction training in a convenience sample with a 4-week follow-up. Chapter 3 examines the specific and broad effects of an 8-week in-person, trainer-led socioemotional competence training aimed at stress management and social competences in younger and older adults, including a 3- and 12-month follow-up.

1.5 Mechanisms of Mindfulness and Self-Compassion

The discussion of specificity in psychological interventions extends beyond the outcomes of MBIs and SCBIs to include the change mechanisms that bring about those outcomes. This reflects an ongoing debate in psychotherapy research about whether psychological interventions mainly operate through shared common factors or intervention-specific mechanisms (Cuijpers et al., 2019; Henriques & Gralha, 2024; Wampold & Imel, 2015; Zilcha-Mano, 2021, 2025a; cf. Allemand & Flückiger, 2017 for personality interventions). Understanding how and why psychological interventions work is crucial for optimizing intervention design, tailoring treatments to individual needs, and ultimately developing more effective treatments (Zilcha-Mano, 2025b).

Common change factors are defined as elements that operate across most psychological interventions and typically encompass four aspects: a supportive relationship between facilitator and participant, a treatment rationale that creates expectations for improvement, specific actions that support desired change, and a setting that contributes to the improvement process (Cuijpers et al., 2019; Goldberg, 2022; Wampold & Imel, 2015). These factors can also be conceptualized as providing support (e.g., positive relationship, trust), facilitating learning (e.g., promoting insight), and encouraging action (e.g., trying out new behaviors; Allemand & Flückiger, 2017; Cuijpers et al., 2019; Goldberg, 2022).

Goldberg (2022) presented compelling arguments for the common factors perspective in MBIs, which are likely to apply similarly to SCBIs. These interventions demonstrate comparable effectiveness to other psychological treatments, reveal no differential effects between ingredients in dismantling or component studies, exhibit researcher allegiance effects that positively correlate with RCT outcomes, and display group effects that often exceed therapist-specific effects. The author thus recommends systematically exploring the interaction between common factors (such as group cohesion, therapeutic alliance, and expectancy effects) and specific mechanisms to identify their boundary conditions and relative importance across different outcomes, populations, and contexts.

Numerous theoretical models of specific mindfulness and self-compassion mechanisms exist, mirroring the multifaceted nature of these constructs. Despite their diversity—covering foundational (e.g., Bishop et al., 2004; P. Gilbert, 2010a, 2010b; Neff, 2003a; Shapiro et al., 2006), neurobiological (Hölzel et al., 2011; Vago & Silbersweig, 2012), integrative (e.g., Cha et al., 2023; Garland et al., 2015; Lindsay & Creswell, 2017), and applied models of mindful self-regulation (Ludwig et al., 2020; Schuman-Olivier et al., 2020)—these theories converge on key ideas about how mindfulness and self-compassion work. Notably, one of these key ideas shared by most frameworks suggests that mindfulness and self-compassion improve self-regulation—the ability to regulate attention, emotion, cognition, and behavior—by inducing a fundamental shift in self-referential processing (e.g., Bernstein et al., 2015; Bishop et al., 2004; K. Brown et al., 2007; Cha et al., 2023; Finlay-Jones, 2017, 2023; Garland et al., 2015; Germer, 2023; J. Gu et al., 2015; Hölzel et al., 2011; Lin et al., 2018; Lindsay & Creswell, 2017; Ludwig et al., 2020; Neff, 2003a, 2023; Schuman-Olivier et al., 2020; Shapiro et al., 2006; Vago & Silbersweig, 2012). Self-referential processing refers to how individuals perceive and relate to themselves and self-relevant information (Lin et al., 2018; Northoff et al., 2006)

Specifically, researchers suggest that the non-evaluative, present-moment focus of mindfulness enables conscious awareness of automatic reactive tendencies (e.g., thoughts, feelings, and sensations). Non-evaluative observation of these tendencies provides individuals with more mental space to deidentify from their reactive responses and choose their next actions more consciously (e.g., Bishop et al., 2004; Hölzel et al., 2011; Lin et al., 2018; Ludwig et al., 2020). This process can facilitate self-regulation and behavioral alignment with personal values (cf. Hayes et al., 1999; Shapiro et al., 2006). An attitude of acceptance toward oneself is another key aspect through which mindfulness and self-compassion may exert their effects: Accepting “what is” without adding an evaluative narrative can similarly free capacity to regulate attention, emotion, cognition, and behavior (Cha et al., 2023; Germer, 2023; Lindsay & Creswell, 2017; Shaprio et al., 2006).

Among the many specific mechanisms proposed in MBIs and SCBIs (e.g., Cha et al., 2023; J. Gu et al., 2015; Wang et al., 2025), state-trait relationships probably represent the most proximal mechanism through which interventions create lasting change (cf. Küchler et al., 2025a; Olaru et al., 2024 for similar ideas in personality intervention research). According to Whole-Trait Theory, repeated state experiences accumulate into stable trait changes (Warren et al., 2023). However, research examining this mechanism in MBIs and SCBIs remains surprisingly scarce (but see Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015). Although some studies provide preliminary support for this theoretical idea, they focus exclusively on

explicitly labeled MBIs where expectancy effects may confound interpretation of specific mechanisms.

This limitation is particularly noteworthy given that mindfulness and self-compassion increase even in interventions that do not explicitly target them (Goldberg et al., 2019; A. C. Wilson et al., 2019). One possible explanation for these findings could be that many mental health interventions share a common element: focusing on one‘s cognitive, emotional, and behavioral patterns (Goldberg, 2022). This self-focus, rather than the mindful or self-compassionate stance toward the self per se, could be driving the observed changes. Examining state-trait relationships in contexts without explicit mindfulness labeling could help to determine whether mindfulness and self-compassion operate through specific mechanisms or whether trait changes reflect broader factors, such as attention to the self, which are common across interventions (cf. Krämer et al., 2025). Such evidence would help to clarify the boundary conditions under which mindfulness and self-compassion act as specific change mechanisms and under which common factors may explain their improvement after interventions.

A second avenue for investigation involves examining whether mindfulness and self-compassion predict differences in their proposed core mechanisms. Theoretical accounts suggest that the altered self-referential processing facilitated by mindfulness and self-compassion encompasses a more balanced, less defensive self-evaluation, which ultimately improves self-knowledge (E. Carlson, 2013; Djikic & Langer, 2007; Leary et al., 2007). One way to examine self-referential processes is through self-evaluation in the form of temporal and social comparisons. Comparisons are also considered central to self-knowledge (Festinger, 1954). They are ubiquitous, inherently evaluative, and often happen automatically (Buunk & Gibbons, 2007; Gerber et al., 2018). If the non-judgmental and accepting stance of mindfulness and self-compassion shifts self-relation by promoting recognition of automatic tendencies, this should manifest in their association with both the frequency and evaluation of comparison information (Djikic & Langer, 2007). However, empirical evidence for the role of mindfulness and self-compassion in these naturalistic self-evaluation processes remains largely unexplored (but see Langer et al., 2010).

The studies presented in Chapters 4 and 5 contribute to the second research question: understanding the specificity of mindfulness and self-compassion as change mechanisms and examining whether associations with theoretical mechanisms differ as predicted. More specifically, Chapter 4 examines whether state mindfulness predicts trait changes in mindfulness, self-compassion, and perceived stress within a socioemotional competence training (cf. Chapter 3) using unlabeled mindfulness exercises. This design tests state

mindfulness as a specific change mechanism while minimizing expectancy confounds. Chapter 5 investigates how trait mindfulness and trait self-compassion predict social and temporal comparisons in a longitudinal study, providing insight into their role in shaping self-referential processes. Together, these studies contribute to understanding whether and how mindfulness and self-compassion operate as and predict specific mechanisms beyond common therapeutic factors.

1.6 The Present Research

The current dissertation aims to refine the understanding of the specificity of mindfulness and self-compassion intervention effects as well as their change mechanisms. It therefore focuses on two central research questions: (RQ 1) What are the specific and broad effects of mindfulness exercises and self-compassion-based interventions with different delivery formats in non-clinical samples? And (RQ 2): How do mindfulness and self-compassion predict trait change and self-referential processes? These questions will be addressed in the following chapters, which investigate mindfulness and self-compassion as exercises, interventions, predictors, and mechanisms across varying contexts (i.e., everyday life, in individual and group training), populations (i.e., age diverse, healthy, German and U.S.-based), and target variables (e.g., indicators of psychological ill-, and well-being, and socioemotional competences), within different study designs (i.e., longitudinal and RCTs, online and in-person).

Chapters 2 and 3 of this dissertation present findings from two RCTs examining specific and broad effects of two mental health promotion interventions designed for healthy adults with different durations and delivery modes. More specifically, Chapter 2 examines the specific effects of a 3-week, self-guided online self-compassion training on self-compassion as well as on the arguably opposite concept of self-criticism and closely related downstream outcomes of perfectionism and social anxiety. The study also investigates broader effects on psychological well-being. It uses a strong randomized controlled trial design, comparing the overall efficacy to a generic stress-reduction training and including a 4-week follow-up. Chapter 3 evaluates the specific and broad pre-post training effects of a Socioemotional Competence Training (SECT), focusing on mindfulness, perceived stress, self-compassion, empathy, and perspective taking, in comparison to a waitlist control group. The training for healthy younger and older adults consisted of eight weekly 2-hr in-person, trainer-led group sessions, with follow-ups at 3 and 12 months. Moreover, the SECT included mindfulness exercises without explicitly labeling them as such, providing an excellent framework to distinguish between specific and broad effects and offering some control over expectancy effects.

In a similar attempt, Chapter 4 investigates whether changes in state mindfulness predict trait mindfulness improvements and further downstream outcomes, that is, trait self-compassion and trait perceived stress in the SECT. It thereby contributes to the understanding of mindfulness as a specific change mechanism and extends previous research by examining the previously identified state-trait mindfulness pathways in a training without explicit mindfulness framing. The study outlined in Chapter 5 examines whether mindfulness and self-compassion predict how individuals relate to themselves. More specifically, it investigates the association of mindfulness and self-compassion with the frequency, perceived utility, and perceived outcome of social and past-temporal comparisons across five common comparison dimensions (i.e., extraversion, emotional stability, professional success, physical appearance, and private life). Chapter 5 hereby contributes to further illuminating the potential self-regulatory role of mindfulness and self-compassion through differences in self-referential processes.

Chapter 2: Effects of a Brief Online Self-Compassion Training on Self-Criticism, Perfectionism, and Social Anxiety: A Randomized Controlled Trial

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Abstract

This randomized controlled trial evaluates the effects of a brief online self-compassion training (SCT) on self-compassion, self-criticism, perfectionism, social anxiety, and psychological health in comparison to a generic stress-reduction training (SRT). Both training courses consisted of six brief, format-matched, unsupervised, online sessions with various exercises, and took place in a self-paced manner over 2 to 4 weeks. We collected self-report data on self-compassion, self-criticism, perfectionism, social anxiety, and psychological health. Participants were 200 healthy adults (85.5% female, M_{age} = 30 years; range 18-69), randomly allocated to the SCT or the SRT. In pre-post comparison, effect sizes for the SCT were moderate for self-compassion (*dz* = 0.49, 95% CI [0.26, 0.72]), self-criticism (*dz* = –0.50, 95% CI [–0.72, –0.28]), and perfectionism (*dz* = –0.52, 95% CI [–0.74, –0.30]), but close to zero for social anxiety (*dz* = –0.01, 95% CI [–0.21, 0.18]). Only small differences emerged between the conditions immediately after the training, except for self-compassion (*d* = 0.49, 95% CI [0.02, 0.58]). At 4 weeks follow-up the effects of both trainings on the target variables, including self-compassion, were very similar. However, intervention-specific effects were pronounced and enduring for participants with high initial levels of self-criticism. The results indicate that both training courses yielded similar psychological effect patterns. Effects of the SCT were not specific to self-compassion and conceptually opposite variables like perfectionism or self-criticism. These findings highlight the importance of understanding core mechanisms of self-compassion interventions and identifying appropriate target groups in future research.

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2.1 Introduction

Health is understood not only the absence of disease, but the presence of complete physical, mental, and social well-being (World Health Organization [WHO], 1946). Yet, mental health research has shifted only recently from problem-oriented, curative approaches to mental health preservation and promotion (van Agteren et al., 2021). This randomized control trial examines to what extent a 4-week online Self-Compassion Training (SCT) fosters selective improvements in self-compassion, self-criticism, perfectionism, and social anxiety compared to a generic Stress-Reduction Training (SRT). The study thereby provides an investigation of transdiagnostic indicators of mental health and offers insights for the design of online mental health promotion trainings.

Self-compassion is defined as mindfully directing kindness and understanding toward oneself in difficult times, and the ability to put perceived shortcomings into greater perspective (Neff, 2003a, 2023). Contrarily, self-criticism entails a judgmental and critical attitude towards oneself with an emphasis on disapproving of perceived shortcomings and failings (e.g., Löw et al., 2020). Self-criticism is especially prevalent in perfectionism and social anxiety which are characterized by a pursuit of very high performance standards, an overly critical and harsh evaluation of own behaviour and performance, as well as a strong fear of failing (Schlenker & Leary, 1982; Stoeber, 2018). Both self-compassion and self-criticism have high transdiagnostic value (Finlay-Jones, 2023; Löw et al., 2020), whereby self-compassion is considered a preventive factor (Han & Kim, 2023), and self-criticism a risk factor for several psychological problems and disorders, such as social anxiety, depression, and eating disorders (Löw et al., 2020).

People who report high self-compassion generally score highly on a range of desirable psychological variables such as happiness, optimism, well-being, life satisfaction, adaptive coping strategies, psychological health, and social connectedness (e.g., Asselmann et al., 2024; Ewert et al., 2021; Neff, 2023; Neff et al., 2018; Zessin et al., 2015). Difficulties in emotion regulation and adverse emotional states such as distress, fear of failure, rumination, avoidance, depression and (social) anxiety typically go along with low levels of self-compassion (e.g., Adams et al., 2022; Ferrari et al., 2019; Tobin & Dunkley, 2021; Werner et al., 2012). In contrast, these and similar adverse emotional states often come along with high levels of self-criticism (Dunkley et al., 2020; Löw et al., 2020; With et al., 2024).

Psychological problems associated with self-criticism, such as perfectionism and social anxiety, come at a substantial social and psychological cost. For example, perfectionism is associated with higher mortality and suicidal ideation (Fry & Debats, 2009; M. Smith et al.,

2017). Moreover, it has been repeatedly linked to various psychological (e.g., depression, anxiety, and eating disorder; Bardone-Cone et al., 2007; Hewitt & Flett, 1991; Newby et al., 2017) and personality disorders (e.g., narcissism, borderline personality disorder; Ayearst et al., 2012; Hewitt & Flett, 1991; K. Wu & Cortesi, 2009). Similarly, social anxiety is highly comorbid with psychological disorders such as other anxiety disorders, depression, and substance abuse (Fehm et al., 2008).

People suffering from excessive self-criticism, perfectionism, and social anxiety often refrain from seeking adequate treatment (Shannon et al., 2018; Wittchen et al., 1999). Even if treatment is sought, disorders associated with self-criticism are particularly difficult to treat because self-criticism negatively affects the therapeutic alliance (Löw et al., 2020). However, findings from psychotherapy research indicate that treatment of perfectionism and social anxiety is possible and yields moderate to large effect sizes in clinical and community populations (e.g., Acarturk et al., 2008; Lloyd et al., 2015; Norton et al., 2015). It is therefore essential to further improve the availability and acceptance of (low-threshold) preservation or even preventive offers—that is, before the self-criticism becomes pathologic—for people suffering from harsh self-criticism (Dams et al., 2017).

Self-compassion training (SCT) has been proposed as a transdiagnostic and preventive treatment for a range of psychological problems and disorders, such as maladaptive coping, impaired emotion regulation capacities, or affective disorders (Ewert et al., 2021; Finlay-Jones, 2023). An increasing number of findings suggests that self-compassion interventions improve overall psychological health and well-being and reduce psychological symptoms with overall moderate to large effect sizes (Ferrari et al., 2019; Han & Kim, 2023; Kirby et al., 2017). For example, scripted cognitive routines for enhancing self-compassion are effective for regulating mood (Diedrich et al., 2016; Ehret et al., 2018), feelings of guilt and shame (Leary et al., 2007), or fear of evaluation (Arch et al., 2014).

Although SCT have been shown to considerably reduce self-criticism (for a review see Wakelin et al., 2021), research on specific effects on self-criticism, perfectionism, and social anxiety is limited. Only preliminary research investigated SCT for reducing perfectionism (e.g., Finlay-Jones et al., 2018; James & Rimes, 2018; Woodfin et al., 2021). Yet, several studies found a reduction in social anxiety and related behavior, such as post-event processing or avoidance, after a self-compassion intervention (Arch et al., 2018; Blackie & Kocovski, 2018; Siegel & Kocovski, 2020; Slivjak et al., 2024). Most of these studies delivered training in face-to-face sessions. Yet, people with a sub-clinical symptom load often fear stigmatization and do

not seek sufficient treatment (Schnyder et al., 2017), which should result in a reduced probability of these individuals participating in a face-to-face training.

A promising way to provide broad and unobtrusive access to training in health promotion and prevention settings are internet-based psychological interventions. Such interventions encompass many advantages in terms of accessibility and efficiency, time-, and location-independency (i.e., easier integration into participants' daily life), drop-out rate, treatment of subthreshold levels of psychological symptoms, and participants' fear of stigmatization (e.g., Andersson & Titov, 2014; Beecham et al., 2019; Carlbring et al., 2018).

To date, research on internet-based SCT is still in its infancy (Han & Kim, 2023). So far, results suggest positive effects with moderate to large effects on multiple outcomes such as self-compassion, mindfulness, self-esteem, depression, anxiety, stress, self-criticism, and perfectionism (Eriksson et al., 2018; Krieger et al. 2016, 2019; Nadeau et al., 2021; Stevenson et al., 2019). For example, regarding self-criticism, an 8-week online SCT led to lower levels of self-criticism directly after the training and at a 6-months follow-up (Krieger et al., 2019). Results for a 10-week online SCT targeting perfectionism in a small sample of women suggest that participation in the SCT compared to a waitlist control group resulted in reduced self-judgment, shame, and perfectionism (Nadeau et al., 2021). Similarly, undergraduates, who participated in a 2-week online SCT reported reduced shame-proneness, irrational beliefs, and symptoms of social anxiety (Cândea & Szentágotai-Tătar, 2018).

As the research presented above indicates, (online) self-compassion training courses improve a range of psychological health outcomes. However, there is only limited evidence whether SCT selectively counteracts maladaptive perfectionism and self-criticism outside of clinical or pre-screened contexts and compared to active control treatments (Han & Kim, 2023; Kirby et al., 2017; Slivjak et al., 2024). This is somewhat surprising, because perfectionism and self-criticism are arguably conceptual opposites of self-compassion and effects should thus be comparatively strong and specific. Given that emotionally straining levels of perfectionism and self-criticism are widespread in the general population and often precede or accompany more severe psychological disorders (Curran & Hill, 2017; Löw et al., 2020), SCT may provide a simple yet effective preventive measure.

The objective of the current study was to examine the efficacy of an online SCT for improving self-compassion, self-criticism, perfectionism, and social anxiety as primary outcomes. Heeding the call for more rigorously designed randomized controlled trials (RCTs) in compassion research (Han & Kim, 2023; Kirby, 2017), we compared the effects of a purpose-designed self-compassion training (SCT) to a generic stress-reduction training (SRT) without

explicit self-compassion elements and included a 4-week follow-up. To provide easy access, we implemented the training courses as brief, low-threshold, unsupervised, online interventions consisting of six 15-minutes sessions spaced over 2 to 4 weeks.

As primary outcomes, this study focused on self-compassion and the three related constructs self-criticism, perfectionism, and social anxiety. Regarding these primary outcomes, we expected a clear pre–post increase for the SCT and superiority over the SRT. The effect should be strongest for self-compassion and present (if weaker) for the three conceptually related constructs of self-criticism, perfectionism, and social anxiety. As most psychological interventions have broad positive effects (e.g., Borgdorf et al., 2025a; Hildebrandt et al., 2017), we also evaluated the effect of the SCT on general psychological health variables including perceived stress, psychological symptoms, and subjective well-being as secondary outcomes. We expected moderate pre–post improvements on these general variables in both training conditions but no superiority of the SCT over the SRT (cf. van Agteren et al., 2021).

2.2 Method

The anonymized data and analysis code are available on the Open Science Framework. Informed consent was obtained from all participants before participation. This study was performed in line with the principles of the Declaration of Helsinki and was approved by the Ethics Committee of the Faculty of Heidelberg University.

2.2.1 Power Analysis and Participants

Depending on details of the specific analysis, precision-based sample size calculations suggested between 149 and 194 participants to achieve a target CI half-width of .20 or .25 for Cohen's *d* with 99% assurance (see Cumming, 2013). For compatibility with null-hypothesis significance testing conventions and to compensate for expected drop-out, we set the sample size a priori to $N = 200$, which corresponds to 80% power for detecting between-group differences of Cohen's $d = 0.4$ at $\alpha = .05$. Most participants were recruited online, for example using mailing lists, social media, or bulletin boards. The study language was German, and participant recruitment was focused on Germany, Austria, and Switzerland.

2.2.2 Procedure and Participant Flow

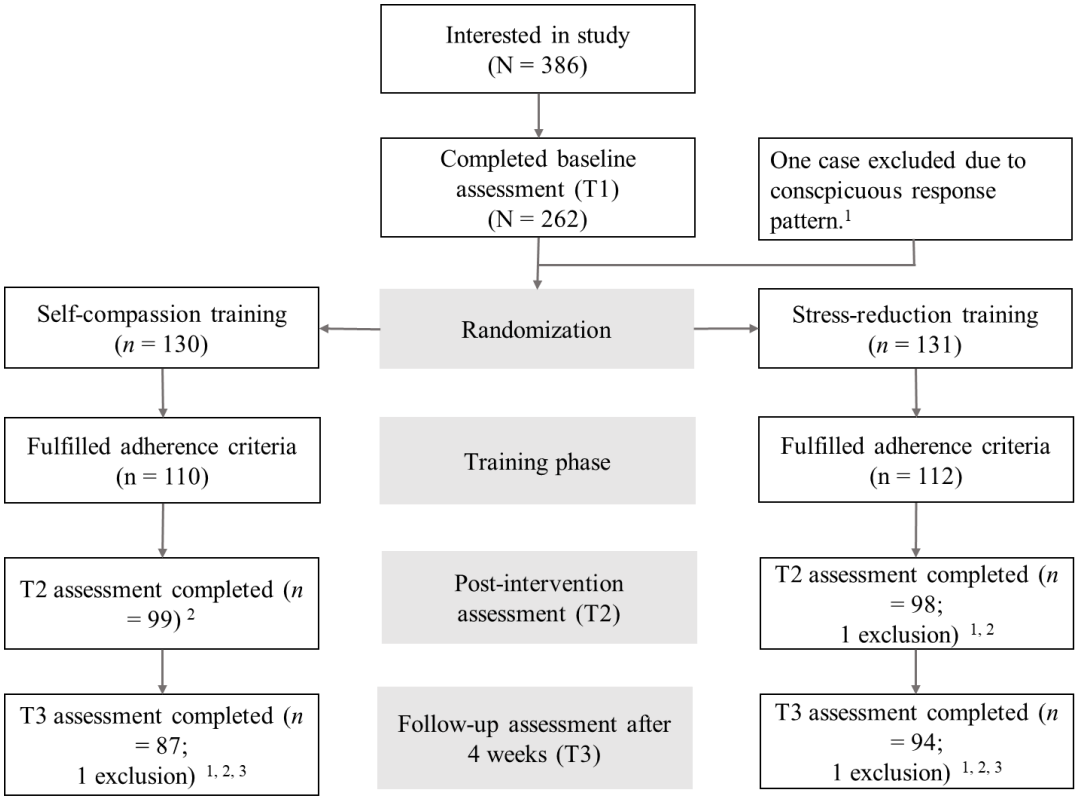
After reading the study information, confirming their age, and giving informed consent, participants received an individualized link to access the study and training materials on the platform SoSci Survey (Leiner, 2018). The study started with the baseline assessment (T1) and included demographic and background information, as well as the outcome measures. Participants were randomly assigned to either the SCT or the SRT by the survey platform. Both

training courses consisted of six online training sessions spread out over 2 to 4 weeks. Every 2 or 3 days, participants received a link to the next session by e-mail, with reminders after 1, 2, and 5 days afterwards. Immediately after the final training session, participants received an invitation to the post assessment (T2) and 28 days later to the follow-up assessment (T3). At the end of the study, participants either received course credit or could register for a lottery to win one of five €50 shopping vouchers.

Figure 2.1 details the structure of the study and the flow of participants according to the CONSORT guidelines (Moher et al., 2010). The drop-out rates between baseline and post-test (SCT: 23.8%; SRT: 26.0%) and between baseline and follow-up measurement (SCT: 33.8%; SRT: 29.0%) were similar between conditions and comparable to previous online self-compassion intervention studies (e.g., Eriksson et al., 2018; Krieger et al., 2016, 2019). We included all participants in the analyses who completed at least three out of six training sessions and provided data at baseline (T1) and one or both of the subsequent assessments (T2 or T3), resulting in a final sample size of 200 participants.

Figure 2.1

Participant Flow



Note. A total of $N = 199$ participants were included in the main analyses. [1]Excluded for exclusive use of a single response category on the Self-Compassion Scale. [2]Differences

in *n* to previous stage: Lost for unknown reasons. [3]Four participants completed T3 but not T2 and were retained in analyses not requiring data for T2.

2.2.3 Training Interventions

Participants were randomly assigned either to a purpose-designed self-compassion training (SCT) or a generic stress-reduction training (SRT) without explicit self-compassion elements. In the first training session (S1), participants watched a short educational video covering key concepts (e.g., "*What is self-compassion?*", "*How does stress affect the body?*"), and received information on the potential benefits of the upcoming training. The following five training sessions (S2 to S6) provided selected reflections, meditations, guided imagery, or physical exercise tasks in audio, video, or text format. Table 2.1 depicts an overview and Supplementary Table S1 contains further details of the session content. The purpose of the training sessions was to foster participants' understanding of self-compassion (SRT: mechanisms of stress generation) and provide practical exercises and techniques to enhance self-compassion (SRT: stress management). After each training session, short versions of the exercises were sent to participants by e-mail to encourage self-practice and integration into everyday life.

Table 2.1

Overview of the Content of the Six Training Sessions

	Self-Compassion Training (SCT)	Stress-Reduction Training (SRT)
S1	Introduction video (*What is self-compassion?*) and short reflection task	Introduction video (*How does stress affect the body?*)
S2	Guided meditation (*Self-Compassion Break,* adapted from Neff, 2017)	Written reflection task (stressors and potential ways to reduce them)
S3	Guided meditation (*Affectionate Breathing,* adapted from C. Braehler, 2017a)	Guided imagery (*Boat at the Lake,* adapted from Schellenberg, n.d. (a))
S4	Written reflection task (change of perspective on a negative event, adapted from Leary et al., 2007)	Guided exercise video (yoga-based relaxation exercises by Morrison, 2015)
S5	Guided meditation (*Soften, Soothe, Allow*; adapted from C. Braehler, 2017b)	Written reflection task (personal values and priorities, adapted from Covey, 2017)
S6	Written reflection task (letter from the compassionate self, adapted from Neff, 2018)	Guided imagery (*Walk at the Beach,* adapted from Schellenberg, n.d. (b))

Note. Please refer to Supplementary Table S1 for further details on the training sessions.

Most exercises of the SCT were adapted from audio instructions (e.g., C. Braehler, 2017a, 2017b; Neff, 2017, 2018) based on the Mindful Self-Compassion program by Neff and Germer (2013). The exercises were revised and adapted for the study's purpose and if necessary, translated to German with kind permission of the Center for Mindful Self-Compassion (https://centerformsc.org). Exercises for the SRT were based on common interventions targeted at reducing stress. These focus on how individuals deal with and perceive stress, using different techniques like modifying behaviours of dealing with stressors (e.g., time management or goal-setting), modifying believes about stress (e.g., cognitive reappraisal; Riepenhausen et al., 2022), modifying the response to stress (e.g., awareness and acceptance of stress; Grossman et al., 2004), or modifying the body's response to stress (e.g., body relaxation techniques; Varvogli & Darviri, 2011). When designing the SRT, we additionally focused on matching the format of the exercises as closely as possible to the SCT.

2.2.4 Measures

All questionnaires were administered in German, required responding on a 5-point Likert scale, and referred to a period of 2 weeks before the assessment, unless stated otherwise. All scales' internal consistencies were good to excellent (Cronbach's $\alpha = .84$ to .95; see Supplementary Table S2 for details). Participants filled out all questionnaires before they started the first training session (T1), 2 to 3 days after the final training session (T2), and at a 4-week follow-up (T3).

Self-Compassion. Self-compassion was measured with the 26-item Self-Compassion Scale (SCS; Hupfeld & Ruffieux, 2011). The SCS assesses three aspects of self-compassion with two scales each: self-kindness versus self-judgment, common humanity versus isolation, and mindfulness versus over-identification. To simplify analyses, we combined the positive and negative scales for each aspect into three scores.

Self-Criticism. The Forms of Self-Criticizing/Attacking and Self-Reassuring Scale (FSCRS; P. Gilbert et al., 2004) examines how people react when things go wrong for them. The FSCRS is composed of 22 items organized in three scales: Inadequate Self (e.g., "I am easily disappointed with myself."), Hated Self (e.g.; "I have a sense of disgust with myself."), and Reassured Self (e.g., "I find it easy to like myself"). We combined these scales into a self-criticism composite with reverse coded Reassured Self items. Carmen Wiencke (Leuphana University Lueneburg, Germany) kindly provided a translated German version because the validated German scale by Biermann et al. (2021) had not been published at the time of data collection.

Perfectionism. Perfectionism was measured with the Self-Oriented Perfectionism (SOPE) subscale of the Hewitt and Flett Multidimensional Perfectionism Scale (HFMPS; Hewitt & Flett, 1991), and the Personal Standards (PS) and Concern over Mistakes (COM) subscales of the Frost Multidimensional Perfectionism Scale (FMPS; Frost et al., 1990; Altstoetter-Gleich & Bergemann, 2006).The SOPE consists of 15 items answered on a 6-point Likert scale (e.g., "One of my goals is to be perfect in everything I do."). The PS scale consists of seven items referring to challenging individual goals (e.g., "I set higher goals than most people"). COM is reflected by the fear of and sensitivity to making mistakes and failing at one's own standards and consists of eight items (e.g., "If I fail partly, it's as bad as being a complete failure."). To simplify analyses, we formed a perfectionism composite of these closely related scales by rescaling the SOPE to a maximum of 5 and averaging the scores.

Social Anxiety. The Social Interaction Anxiety Scale (SIAS; Stangier et al., 1999) measures anxiety in situations of social interaction and is composed of 20 items (e.g., "When mixing socially I am uncomfortable.").

Psychological Health. The Perceived Stress Questionnaire (PSQ; Fliege et al., 2001) measures stress as a subjective feeling without reference to specific events using 20 items with a 4-point Likert scale. For homogeneity of the item format, we reworded items by using the first person ("I" instead of "you") and changed the present to the past tense (e.g., "I had too many things to do"). Psychological symptom load in the past 7 days was assessed with the Brief Symptom Inventory 18 (BSI-18; Franke et al., 2011), which consists of 18 items covering three symptom clusters (somatization, depression, and anxiety). Finally, the WHO-5 Well-Being Index assesses the presence of positive well-being with five items (WHO, 1998; E. Braehler et al., 2007). In the analysis, we used overall scores for each of the psychological health measures.

2.2.5 Primary and Secondary Outcome Measures

The measures selected for evaluating the primary outcomes were: Self-Compassion Scale total score, Forms of Self-Criticizing/Attacking & Reassuring Scale total score, Frost / Hewitt and Flett Multidimensional Perfectionism Scale composite score, and Social Interaction Anxiety Scale total score. Measures for assessing the secondary outcomes: Perceived Stress Scale, Brief Symptom Inventory total score, and WHO-5 Well-Being Index.

2.2.6 Additional Measures

Immediate training effects. To examine the short-term effects of the training, we assessed momentary experiences directly before and after each session. Participants were asked to indicate on a bipolar scale from -5 to +5 how they felt at that moment in terms of mood (bad – good), energy (tired, weak – awake, full of energy), stress (stressed, tense – relaxed,

composed), focus (inattentive, distracted – focused, in the "here and now"), and attitude towards themselves (critical, negative – friendly, positive). The first three items were adapted from the Multi-Dimensional Mood Questionnaire (MDMQ; Wilhelm & Schoebi, 2007).

Satisfaction with the interventions and adverse effects. We examined participants' satisfaction with the training courses using an adapted version of an 8-item scale (ZUF-8) developed by Schmidt et al. (1989; adapted from Krieger et al., 2016). The questionnaire uses 4-point rating scales with anchors adapted to the content of the question (e.g., "How would you generally rate the quality of this training?"). We also asked participants if they experienced any new or aggravated negative effects that they would ascribe to their participation in the study.

2.2.7 Data Analyses

Data analyses were performed with the software R version 4.2.2 (R Core Team, 2021; see Supplementary Information for used R packages). The main analysis is based on standardized change scores, representing an unbiased estimate of true change in RCTs (see Jennings & Cribbie, 2021 for a discussion). Following recent recommendations to move beyond null hypothesis significance testing (NHST) for inferential analyses (Wasserstein et al., 2019), we adopted an estimation statistics approach (Cumming, 2013). We suggest interpreting the confidence intervals as uncertainty ranges around effect sizes (see Cumming, 2013), however, they can alternatively be used as tests of significance: A 95% CI excluding zero corresponds to $p < 0.05$ for a test of difference, a 90% CI excluding both upper and lower equivalence bounds corresponds to $p < 0.05$ for a test of equivalence (Lakens et al., 2018). Equivalence bounds were set to $d = [–0.4; 0.4]$, that is, symmetric to the effect size used for the NHST power calculation mentioned above. Statistical equivalence therefore indicates that the groups differ by less than the typical average effect size of $d \approx 0.4$ found in most psychological studies (Richard et al., 2003). For compatibility with other statistical approaches, *p*-values and corresponding statistics are reported for the main findings. Regarding false positives due to multiple comparisons, we follow the recommendations of Schulz and Grimes (2005) for handling multiplicity in RCTs through careful interpretation rather than applying potentially inconsistent "correction" procedures.

2.3 Results

The current sample was predominantly female (85.4%) with a mean age of 30.0 years. All participants indicated that they were proficient German speakers at an advanced level. Table 2.2 depicts further sociodemographic information. The two training groups did not differ notably on sociodemographic variables or any of the control variables, such as experience with Yoga, Mindfulness-Based Stress Reduction, psychotherapy, counselling, or meditation. The

variable with the statistically highest degree of imbalance between conditions was gender, χ^2 (1, N = 200) = 2.58, p = 0.11.

Table 2.2

Sample Characteristics

	Total (N = 200)	SCT (n = 100)	SRT (n = 100)
Gender (female)	85.5%	90.0%	81.0%
Age (years) [a]			
Mean	30.0	29.2	30.7
Range	18 – 69	18 – 64	18 – 69
Language proficiency (German)			
Native speaker (incl. bilingual)	94.5%	94.0%	95.0%
Secondary language (C1- or C2-level)	5.5%	6.0%	5.0%
Highest education degree			
University	34.0%	36.0%	32.0%
College	56.7%	57.0%	56.0%
Other	9.5%	7.0%	12.0%
Current occupation			
Student	62.0%	62.0%	62.0%
(Self-)Employed	27.0%	28.0%	26.0%
Other	11.0%	10.0%	12.0%
Prior experience			
Psychological therapy or counseling	27.5%	24.0%	31.0%
Mindfulness or relaxation programs[b]	10.5%	10.0%	11.0%
Mindful movement[c]	28.5%	31.0%	26.0%
Meditation experience (10+ hours)	24.0%	24.0%	24.0%
Mindfulness practice (at least monthly)	21.5%	20.0%	23.0%

Note. SCT = Self-Compassion Training. SRT = Stress-Reduction Training. [a]One implausible age value in the SRT was removed. [b]e.g., Mindfulness-Based Stress Reduction or Progressive Muscle Relaxation. [c]e.g., Yoga or Tai Chi.

2.3.1 Main Analyses

Data points exceeding a z-score of |3.29| were identified as univariate outliers and winsorized to the nearest non-extreme value (34 out of 12,735 data points). Distribution statistics and inspection of histograms supported the assumption of an approximately normal distribution for all main outcome variables (skewness between -0.70 and 1.10). The online survey platform ensured that there were no missing values in completed questionnaires. Descriptive statistics of all outcomes are depicted in Supplementary Table S3.

The main results are summarized in Figure 2.2, which depicts standardized change scores (Cohen's *dz*) with confidence intervals. Precise numbers for statistical test results are reported in Table 2.3 and Table 2.4. The general pattern of pre-post comparisons (blue and green bars in Figure 2.2) shows statistically relevant favourable changes in both training conditions on nearly

all outcome variables. The median absolute effect size across all outcomes and measurement occasions was $|dz| = 0.39$ with an interquartile range (IQR) from 0.27 to 0.49. Notable exceptions were social anxiety and perceived stress at follow-up (T3), showing no discernible effect for the SCT.

Primary Outcomes. The analysis shows a robust pre-post effect of the SCT with respect to self-compassion and self-criticism (Figure 2.2, Panel A, blue bars). However, compared to the SRT (Figure 2.2, Panel A, yellow bars) the effect was substantially different only directly after the training (T2) but within equivalence bounds at follow-up (T3). Superiority of the SCT regarding self-compassion and self-criticism was therefore at best partially confirmed. The pattern was similar for the related constructs of perfectionism and social anxiety (Figure 2.2, Panel A), but there were no statistically notable differences between groups at any time. For social anxiety results were slightly surprising, as pre-post effects of the SCT were nearly zero and the SRT was approaching superiority at follow-up (T3).

Secondary Outcomes. Findings for psychological symptoms and subjective well-being (Figure 2.2, Panel B) mirror the general pattern evident in the primary outcomes. Both training conditions show clear pre-post effects but are statistically equivalent to each other, which met our expectations regarding these broader outcomes. One exception to this was perceived stress (Figure 2.2, Panel B), where the SCT showed no lasting effects at follow-up (T3).

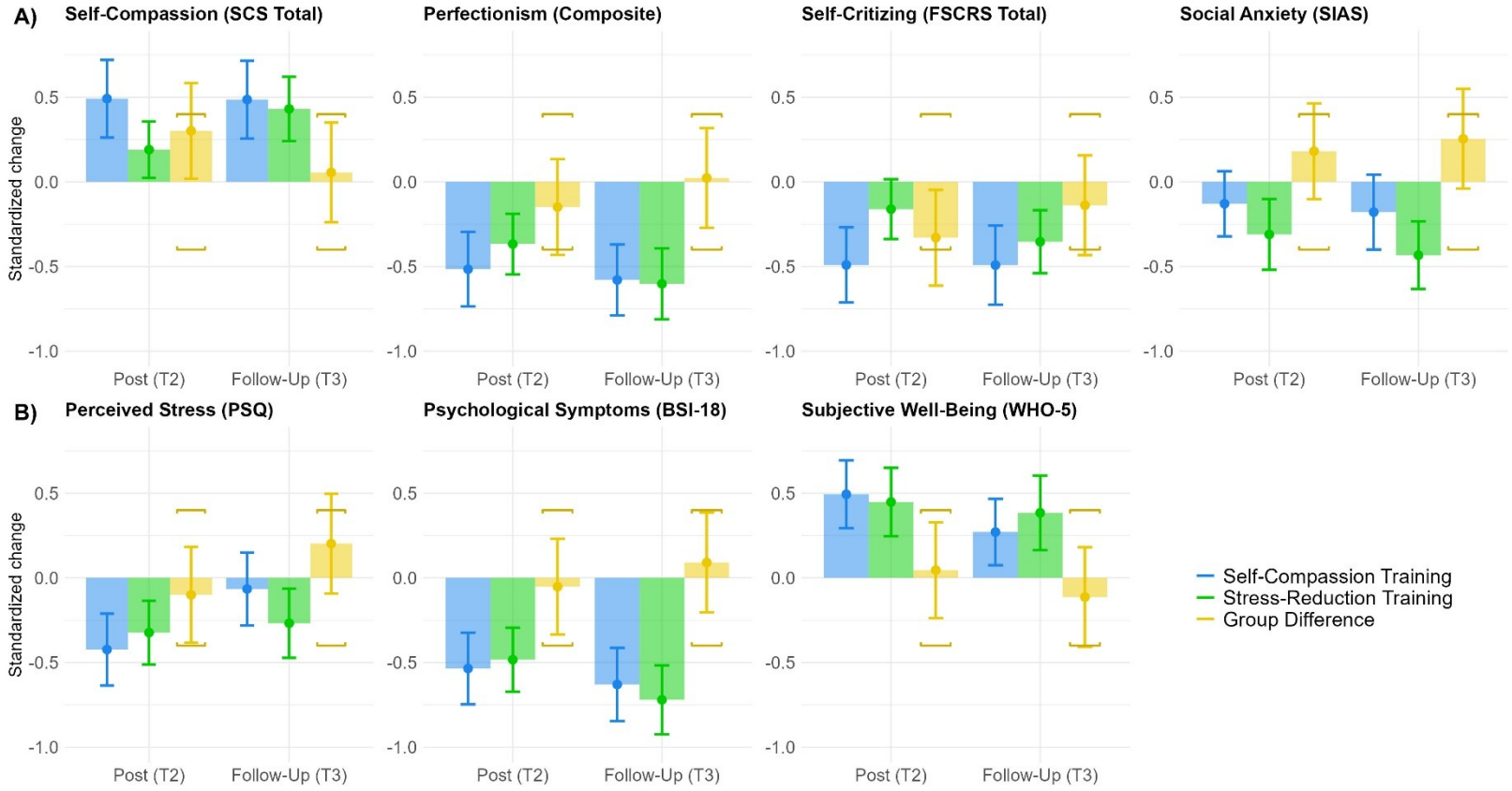
2.3.2 Additional Analyses

Immediate training effects. Both interventions produced considerable within-session effects on all state variables, with the difference of pre- and post-session ratings ranging between Cohen's *dz* 0.51 and 1.04 (see Supplementary Table S4 for details). Surprisingly, no meaningful difference between training conditions emerged regarding kind vs. critical attitude towards the self ($diff_{dz} = 0.18$, 95% CI [–0.11, 0.45]).

Participant satisfaction and adverse effects. Overall, participants were similarly satisfied with the SCT ($M = 3.03$, $SD = 0.54$) and the SRT ($M = 2.93$, $SD = 0.59$) at follow-up (T3) ($t(194) = 1.26$, $p = 0.21$). Both satisfaction scores were close to the anchor *mostly satisfied* (3 out of 4) of the scale.

Figure 2.2

Standardized Change Scores at Post and Follow-Up Measurement by Training Condition



Note. Primary outcomes shown in Panel A), secondary outcomes in Panel B). Solid bars depict standardized change (Cohen's *dz*) relative to T1 for the two conditions (SCT: blue, STR: green) and the difference in standardized change between the two conditions (orange). Error bars indicate 95% CIs, the wide segment of error bars additionally show 90% CIs for group differences. Orange brackets mark group difference equivalence bounds at $\Delta dz = 0.4$.

Table 2.3

Change Scores and Statistical Test Results for Baseline (T1) vs. Post (T2) Comparisons

Variables	Self-Compassion Training				Stress-Reduction Training				Group Difference			Equivalence[c]	
	M (*SD*)	*dz*[a]	*t*[b]	*p*	*M* (*SD*)	*dz*[a]	*t*[b]	*p*	Δ*dz*	*t*[b]	*p*	*t*[b]	*p*
SCS Total	0.23 (0.55)	0.49	4.25	<.001	0.09 (0.39)	0.19	2.27	.03	0.30	2.10	.04	–0.69	.25
SCS Self-Kindness	0.28 (0.62)	0.51	4.53	<.001	0.09 (0.46)	0.17	2.01	.05	0.34	2.38	.02	–0.41	.34
SCS Common Humanity	0.24 (0.66)	0.40	3.63	<.001	0.07 (0.55)	0.12	1.30	.20	0.28	1.94	.05	–0.86	.20
SCS Mindfulness	0.17 (0.58)	0.33	2.85	< .01	0.10 (0.41)	0.20	2.38	.02	0.13	0.93	.35	–1.86	.03
Perfectionism Composite	–0.21 (0.45)	–0.52	–4.65	<.001	–0.15 (0.36)	–0.37	–4.07	<.001	–0.15	–1.04	.30	1.76	.04
SOPE	–0.11 (0.52)	–0.25	–2.16	.03	–0.05 (0.39)	–0.10	–1.20	.23	–0.14	–0.99	.32	1.80	.04
PS	–0.22 (0.50)	–0.47	–4.47	<.001	–0.17 (0.46)	–0.35	–3.61	<.001	–0.11	–0.80	.42	1.99	.02
COM	–0.28 (0.70)	–0.41	–3.92	<.001	–0.24 (0.63)	–0.35	–3.65	<.001	–0.06	–0.44	.66	2.35	< .01
FSCRS Total	–0.21 (0.48)	–0.49	–4.38	<.001	–0.07 (0.38)	–0.16	–1.81	.07	–0.33	–2.30	.02	0.49	.31
SIAS Total	–0.05 (0.36)	–0.13	–1.33	.19	–0.11 (0.38)	–0.31	–2.95	< .01	0.18	1.26	.21	–1.53	.06
PSQ Total	–0.17 (0.43)	–0.42	–3.96	<.001	–0.13 (0.38)	–0.32	–3.41	<.001	–0.10	–0.70	.48	2.09	.02
BSI Total	–0.19 (0.37)	–0.54	–5.02	<.001	–0.17 (0.33)	–0.48	–5.07	<.001	–0.05	–0.36	.72	2.43	< .01
WHO-5 Total	0.37 (0.75)	0.49	4.88	<.001	0.33 (0.74)	0.45	4.41	<.001	0.05	0.32	.75	–2.48	< .01

Note. SCS = Self-Compassion Scale. SOPE = Self-Oriented Perfectionism Subscale of the Hewitt and Flett Multidimensional Perfectionism Scale. PS = Personal Standards Subscale of the Frost Multidimensional Perfectionism Scale (FMPS). COM = Concern over Mistakes Subscale of the FMPS. FSCRS = Forms of Self-Criticizing/Attacking and Self-Reassuring Scale. SIAS = Social Interaction Anxiety Scale. PSQ = Perceived Stress Questionnaire. BSI = Brief Symptom Inventory 18.WHO-5 = WHO-5 Well-Being Index. Means of raw change scores with standard deviations in parentheses. Positive change scores indicate increases from T1 to T2. [a] For comparability across groups, the pooled standard deviation of change scores was used for calculating *dz*.
[b] $df_{SCT} = 98$, $df_{STR} = 96$, $df_{Diff/Equ} = 194$. [c] Equivalence bounds at $\Delta_{dz} = 0.4$.

Table 2.4

Change Scores and Statistical Test Results for Baseline (T1) vs. Follow-Up (T3) Comparisons

	Self-Compassion Training				Stress-Reduction Training				Group Difference			Equivalence[c]	
Variables	*M* (*SD*)	*dz*[a]	*t*[b]	*p*	*M* (*SD*)	*dz*[a]	*t*[b]	*p*	Δ*dz*	*t*[b]	*p*	*t*[b]	*p*
SCS Total	0.23 (0.52)	0.49	4.21	<.001	0.21 (0.45)	0.43	4.50	<.001	0.06	0.37	.71	–2.31	.01
SCS Self-Kindness	0.30 (0.60)	0.53	4.61	<.001	0.21 (0.52)	0.38	3.95	<.001	0.15	1.00	.32	–1.68	.05
SCS Common Humanity	0.20 (0.68)	0.33	2.81	< .01	0.21 (0.58)	0.33	3.49	<.001	–0.01	–0.04	.97	2.64	< .01
SCS Mindfulness	0.20 (0.58)	0.38	3.26	< .01	0.20 (0.48)	0.38	4.03	<.001	0.01	0.06	.95	–2.62	< .01
Perfectionism Composite	–0.28 (0.48)	–0.58	–5.49	<.001	–0.29 (0.50)	–0.60	–5.72	<.001	0.02	0.15	.88	–2.53	< .01
SOPE	–0.21 (0.52)	–0.41	–3.73	<.001	–0.12 (0.49)	–0.24	–2.42	.02	–0.17	–1.14	.26	1.54	.06
PS	–0.34 (0.57)	–0.59	–5.57	<.001	–0.36 (0.59)	–0.62	–5.92	<.001	0.03	0.23	.82	–2.45	< .01
COM	–0.31 (0.70)	–0.43	–4.11	<.001	–0.41 (0.73)	–0.58	–5.45	<.001	0.14	0.96	.34	–1.72	.04
FSCRS Total	–0.24 (0.53)	–0.49	–4.18	<.001	–0.17 (0.44)	–0.35	–3.77	<.001	–0.14	–0.93	.36	1.75	.04
SIAS Total	–0.08 (0.44)	–0.18	–1.61	.11	–0.18 (0.41)	–0.43	–4.32	<.001	0.25	1.70	.09	–0.98	.16
PSQ Total	–0.03 (0.52)	–0.07	–0.61	.55	–0.14 (0.51)	–0.27	–2.61	.01	0.20	1.35	.18	–1.33	.09
BSI Total	–0.27 (0.44)	–0.63	–5.80	<.001	–0.31 (0.43)	–0.72	–7.03	<.001	0.09	0.61	.54	–2.07	.02
WHO-5 Total	0.24 (0.80)	0.27	2.74	< .01	0.34 (0.94)	0.38	3.46	<.001	–0.11	–0.76	.45	1.92	.03

Note. SCS = Self-Compassion Scale. SOPE = Self-Oriented Perfectionism Subscale of the Hewitt and Flett Multidimensional Perfectionism Scale. PS = Personal Standards Subscale of the Frost Multidimensional Perfectionism Scale (FMPS). COM = Concern over Mistakes Subscale of the FMPS. FSCRS = Forms of Self-Criticizing/Attacking and Self-Reassuring Scale. SIAS = Social Interaction Anxiety Scale. PSQ = Perceived Stress Questionnaire. BSI = Brief Symptom Inventory 18.WHO-5 = WHO-5 Well-Being Index. Means of raw change scores with standard deviations in parentheses. Positive change scores indicate increases from T1 to T2. [a] For comparability across groups, the pooled standard deviation of change scores was used for calculating *dz*. [b] $df_{SCT} = 86$, $df_{STR} = 93$, $df_{Diff/Equ} = 179$. [c] Equivalence bounds at $\Delta_{dz} = 0.4$.

Fourteen participants (seven in each condition) reported that they experienced new symptoms during the training period but only two of the participants ascribed the new symptoms to the training without further specifying their complaints. Likewise, 16 participants experienced an aggravation of their symptoms. Of these, four participants (all in the SRT) indicated that this was due to their participation in the study. Two participants reported lower mood or nervousness, the other two criticized the content of the exercises.

2.3.3 Exploratory Analyses

Effect of initial self-criticism levels. To investigate whether participants with high initial levels of self-criticism responded particularly well to the SCT, we repeated the main analysis for participants with high "Inadequate Self" (IS) scores on the Forms of Self-Criticizing/Attacking and Self-Reassuring Scale (P. Gilbert et al., 2004) before the intervention, using the criterion of Krieger et al. (2019; FSCRS IS score ≥ 20). This subsample of 86 participants showed notably stronger and enduring differences between SCT and SRT in self-compassion (T2: $\Delta_{dz} = 0.60$, 95% CI [0.16, 1.03]; T3: $\Delta_{dz} = 0.46$, 95% CI [0.00, 0.92]) and self-criticism (T2: $\Delta_{dz} = –0.71$, 95% CI [–1 .06, –0.19]; T3: $\Delta_{dz} = –0.72$, 95% CI [–1.20, –0.27]). However, the effect of selection on group differences in perfectionism, social anxiety, and secondary outcome variables was statistically negligible.

2.4 Discussion

The present randomized controlled study examined the specificity of a brief, low-threshold, unsupervised online self-compassion training (SCT) compared to a matched generic stress-reduction training (SRT) on self-compassion, self-criticism, perfectionism, and social anxiety as primary outcomes and several general psychological health indicators as secondary outcomes. As expected, participation in the SCT led to increases in self-reported self-compassion and a reduction in self-criticism at post-training and 4-week follow-up assessments. Contrary to our expectations, the lasting effects of the SCT were not specific, that is, they were not statistically superior to the effects of the SRT on self-compassion at follow-up, with a nearly identical results for self-criticism. To a lesser extent, the other primary outcome variables showed a similar pattern: The SCT led to considerable reductions in perfectionism and social anxiety but there was no statistically meaningful difference on these outcomes compared to the SRT at any time. As expected, the current findings indicate that both online training courses were effective in maintaining and enhancing subjective well-being and mental health—immediately and up to 4 weeks. Furthermore, results indicate that participation in all six training sessions of both, SCT and SRT, led to immediate improvements in mood and a more positive self-perception. Additionally, both SCT and SRT had broad positive effects on psychological

symptom load, subjective well-being, and—except for the SCT at follow-up—perceived stress. Last, participants in both training conditions were equally satisfied with the two training courses and only a few participants reported minor negative side effects.

We assumed that the SCT would have a specific effect not only on self-compassion but on conceptually related variables when compared to a generic stress-reduction training. Yet, although the SRT did not include any exercises targeting self-critical beliefs or perfectionistic standards, it led to similar improvements on these variables. Considering this and previous results from research on broad stress-reduction interventions (e.g., Domes et al., 2019; Richardson & Rothstein, 2008), we argue that simply directing attention towards oneself and taking time to consciously unwind from stressful moments may already support an individual to regulate self-critical thoughts. Spending 20 to 30 minutes on self-reflection tasks, guided imagery and/or meditation per week may be a crucial factor in preserving and promoting mental well-being (cf. van Agteren et al., 2021). Thus essentially, both training courses were different, but similarly effective means to the same end.

Surprisingly, and contrary to previous research (e.g., Krieger et al., 2019), the effects of the SCT on perceived stress were not maintained at the 4-week follow-up whereas the effects after participation in the SRT were maintained. A potential explanation may come from the context of self-compassion as an emotion-regulation strategy (e.g., Finlay-Jones, 2023): Self-compassion may not necessarily lead to a reduction in the *perception* of stress but rather to an enhanced effectiveness in *dealing* with it. This explanation is consistent with a study on Mindfulness-Based Cognitive Therapy (MBCT; Kuyken et al., 2010) which shows that increases in self-compassion neutralized the relationship between cognitive reactivity and depression. The study's authors argued that it was not the dysfunctional thinking style per se that was changed but that the response to those thoughts changed through the intervention. In future studies, researchers may hence benefit by focusing on how individuals *deal* with rather than measuring how they *perceive* stress. Consequently, future research should aim to define and differentiate specific and broad effects more systematically, clearly distinguishing between training-specific outcomes and broader, potentially inconsistent psychological changes (Borgdorf et al., 2025a; Hildebrandt et al., 2017; Krieger et al., 2019).

The question of specificity of measures extends to the other primary outcomes of this study. For example, it could be worthwhile to investigate whether even more specific components, such as cognitive (e.g., perfectionistic cognitions; Flett et al., 1998) or behavioural aspects of perfectionism (e.g., perfectionistic self-presentation tendencies; Hewitt et al., 2003) are better targeted by the SCT than the generic SRT. Also, cognitive components of social

anxiety, such as critical post-event processing, may be especially prone to change through an SCT (Blackie & Kocovski, 2018).

Overall, our finding that neither training was superior to the other is consistent with work on mindfulness-based interventions (MBIs; Mander et al., 2018; Shallcross et al., 2015) and recent meta-analytic results of SCT (Han & Kim, 2023). Studies suggest that MBIs and other SCT are as effective as but not more effective than active control conditions (Palmer et al., 2023). At the same time, our results are in line with research showing that online and mobile-based self-compassion interventions may be as effective as other well-established offline practices, such as cognitive restructuring (Stevenson et al., 2019), mindfulness-based training, or cognitive behavioural psychoeducation (Mak et al., 2018).

Does this mean that we should stop investing in the development of new online (contemplative) interventions that help people preserve or improve their mental health and just rely on what is already on the market? In terms of cost-benefit considerations, we could arguably invest more in improving the interventions that already exist (Beecham et al., 2019). However, different people have different preferences and benefit from different approaches. An interesting line of future research would therefore entail a more person-centred approach, extracting the explicit core mechanisms of interventions, the optimal dose, and combination (or repetition) of exercises to define more clearly which individuals prefer and respond to which content (Blanck et al., 2018; Han & Kim, 2023). Recently, research on MBIs has begun to examine these aspects more thoroughly, for example by considering dose-response interactions in comparison to an active control condition, previous trait mindfulness levels, or previous experience with meditation (Palmer et al., 2023), as well as practice-outcome relationships (Goldberg et al., 2020). Accordingly, through further identification of core mechanisms and target groups of SCT combined with careful study designs, we expect better and more targeted mental health promotion as well as preventive care.

2.4.1 Limitations and Future Research

The sample investigated in this study consisted of mostly healthy young adults, who are a relevant population for this type of intervention because perfectionism and strong self-criticism have been repeatedly identified as a key mental health issues in this group (Curran & Hill, 2017). Moreover, internet access and experience with web-based applications is very common in this population, which makes the online format particularly feasible. However, the composition of our sample of mostly young, well-educated women, limits generalizability. The current study is also limited by the rather short the follow-up period of 4 weeks, therefore we cannot make assumptions on the long-term stability of the training effects. A similar problem

of psychological studies is the use of psychological self-report measures. Although the measures used in this study were reliable and empirically validated indicators, future research could compare other- and self-reports or investigate psychophysiological measures as alternative measures of stress (Di Bello et al., 2020) to compensate for social desirability effects. As mentioned above, more specific investigation of other cognitive, emotional, or behavioural indicators of the target outcomes is warranted.

Finally, we cannot conclude for whom and in which dose either the SCT or the SRT is a better fit. Filtering our sample by initial levels of self-criticism supports the findings of previous studies that people with high levels of self-criticism respond particularly well to self-compassion interventions (e.g., Kelly et al., 2010; Krieger et al., 2019; Stevenson et al., 2019). However, this effect seems to be confined to self-compassion and self-criticism and did not generalize to other outcome variables in our study. Future research may take further individual differences and pre-training (baseline) conditions into account.

2.5 Conclusion

In summary, teaching people to compassionately accept themselves and their imperfections led to improvements in self-compassion, and psychological well-being, as well as considerable decreases in psychological symptom load, self-criticism, perfectionism, and social anxiety. Contrary to former opinions that perfectionism and self-criticism are particularly hard to treat (e.g., Ayearst et al., 2012), our results show that even two brief, low-threshold, unsupervised online training courses can lead to meaningful changes on these variables. Nonetheless, the specific effects and mechanisms of SCT are yet to be identified and understood for the purpose of developing more targeted interventions.

The two brief online training courses may be integrated into more intensive face-to-face programs or therapies, to support and maybe even enhance the effectiveness of these interventions. However, ever since the outbreak of the COVID-19 pandemic, online interventions have become even more relevant. The low-threshold format of our two training courses could easily be scaled up at low costs for mental health promotion purposes. For example, the training courses allow for a very feasible and time-efficient integration into various settings, for example in workplace, educational, or health care contexts. Consequently, personal suffering, as well as societal, and economic costs could be reduced.

Chapter 3: Mindful and Well: The Effects of a Socioemotional Competence Training (SECT) in a Randomized Controlled Trial

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Abstract

Many psychological interventions are designed to address specific characteristics of socioemotional functioning. This randomized controlled trial innovatively examined to what extent effects of such an intervention generalize to broader domains. The new socioemotional competence training (SECT), designed to improve social and emotional functioning and well-being in healthy adults, consists of eight weekly 2-hr sessions and was conducted in three cohorts. We collected data on both specific outcomes that were directly targeted during the training (i.e., mindfulness, perceived stress) and broader psychological indicators of emotional and social functioning (e.g., self-compassion, empathy, perspective taking, well-being) before, during, and after the training, with follow-ups after 3 and 12 months. Participants were 166 young (19–39 years, $M = 28.16$, $SD = 4.7$) and older (50–78 years, $M = 63.55$, $SD = 7.2$) healthy German adults (75.2% female). Of these, 73 and 54 participants were randomly allocated to the socioemotional competence training or to a waitlist control group, respectively. Thirty-nine participants were allocated to the last training cohort. Results of the mixed effects models showed significant Group-by-Time effects on mindfulness ($b = 0.28$, 95% CI [0.16, 0.40]), perceived stress ($b = -0.36$, 95% CI [−0.56, −0.16]), and well-being ($b = 0.43$, 95% CI [0.09, 0.78]), but no meaningful effects on the other broader outcomes. Training effects were stable across 3- to 12-months follow-up. The socioemotional competence training yielded specific improvements in characteristics that were explicitly and consistently addressed throughout the training. This underscores the importance of tailored interventions in promoting specific aspects of socioemotional well-being.

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3.1 Introduction

In a world marked by an increasing number of global and social challenges like worldwide pandemics, political instabilities, as well as rising numbers of psychological disorders (Y. Wu et al., 2023), preserving and promoting mental well-being is more vital than ever. Many psychological interventions are designed to target specific characteristics of socioemotional functioning. This study examines whether the 8-week socioemotional competence training (SECT) can foster long-term improvements on specific outcomes and, innovatively, to what extent effects generalize to broader indicators of emotional and social functioning and well-being in healthy adults. More specifically, mindfulness and perceived stress were explicitly targeted during the SECT, therefore named specific effects. Self-compassion, empathy, and perspective taking are considered broader indicators, that is, they were not explicitly trained. This study thereby provides new insights for the design of mental health promotion trainings, as well as preventive, counseling, and clinical interventions.

Individuals naturally strive to change aspects of their affective, cognitive, and behavioral experience (Hudson et al., 2020), that is, many people want to change one or more of their socioemotional characteristics. The umbrella term *socioemotional competences or skills* is often used to describe a diverse range of characteristics that are commonly summarized as "people's capacities to maintain social relationships, regulate emotions, and manage goal- and learning-directed behaviors" (Soto et al., 2022, p. 193). In line with this, empirical research in clinical and personality psychology has consistently shown that individuals especially seek to improve their social interaction skills, their ability to manage stress, and regulate emotions: This is evident in psychotherapy patients' goals for better regulation of depressive or anxious thoughts (Baur et al., 2023; Grosse & Grawe, 2002), in meditation beginners who hope to improve their mental health and stress management (Jiwani et al., 2022), and in personality change interventions where participants seek to become more relaxed and less anxious (Hudson et al., 2019; Stieger et al., 2021). Interpersonal goals for assertiveness and connectedness often rank second in importance among psychotherapy goals (Baur et al., 2023; Grosse & Grawe, 2002). Likewise, increased extraversion is a frequently named change goal in personality intervention research (Hudson et al., 2019; Lücke et al., 2021; Stieger et al., 2021).

Individuals express a higher desire to change, particularly on characteristics whose levels are initially low (Hudson et al., 2020), yet they change to a greater extent if they engage in behaviors, thoughts, and emotions that align with the desired changes (e.g., Hudson et al., 2019; Singer & Engert, 2019). In addition to the curative approach for psychiatric patients, researchers recognized the value of psychological interventions targeted at preserving and promoting well-

being in individuals without a clinical diagnosis. Research shows that such interventions effectively increase psychological and subjective well-being in both nonclinical and clinical populations (van Agteren et al., 2021). These interventions differ in origin, duration, and delivery mode (e.g., Allemand & Flückiger, 2022; Goldberg et al., 2022; Hähner et al., 2024; Roberts et al., 2017; van Agteren et al., 2021) and span very diverse designs, including third-wave approaches (e.g., acceptance and commitment therapy; Hayes et al., 1999), systemic counseling (von Schlippe & Schweitzer, 2016), contemplative practices (e.g., mindfulness-based stress reduction, cognitively based compassion training; Ash et al., 2021; Kabat-Zinn, 2013), personality change interventions (e.g., Hudson et al., 2019; Stieger et al., 2021), and educational programs like the Social, Emotional, Ethical Learning program (Center for Contemplative Science and Compassion-Based Ethics, 2019).

Originally, most of these diverse interventions were developed for specific aims and target populations, but previous research indicates that their effects extend beyond these primary targets. For example, in the ReSource project, the Affect Module led to the anticipated specific increases in compassion, self-compassion, and acceptance (Hildebrandt et al., 2017). Yet, the authors also report broad—and nontargeted—effects on mindful attentional subfacets such as presence, observing, describing, and acting with awareness. Mindfulness-based stress reduction was originally developed to support chronic pain patients to relate to their pain and stress differently (Kabat-Zinn, 1982, 2013). Today, the program itself and adaptations of it are recognized as having broad effects, including reduced stress, depression, or smoking, as well as increased well-being and prosocial behavior (Berry et al., 2020; Goldberg et al., 2022; Zhang et al., 2021). Mindfulness-based stress reduction and mindfulness-based interventions more generally have been adapted to a broad variety of contexts, including somatic, psychiatric, workplace, school, or military populations (Crane et al., 2017; Zhang et al., 2021). Research on personality change interventions also provides evidence for broad effects in two ways: First, personality traits changed after clinical interventions that were not targeted at changing personality traits, such as cognitive behavioral therapy, psychodynamic therapy, or even pharmacological treatment (Roberts et al., 2017). Second, specific personality change interventions showed broad effects, for example, on domain specific and life satisfaction, as well as self-esteem (termed “spillover effects” in Hähner et al., 2024; cf. Allemand et al., 2024a; Olaru et al., 2023).

Together, these findings across a wide array of psychological interventions demonstrate the potential of interventions to operate on both, specific and broad levels, extending effects beyond their initially targeted behaviors and experiences. However, current research is limited

in two ways: First, a notable research gap exists regarding psychological interventions that simultaneously assess multiple characteristics in more general populations in order to intentionally and systematically examine potential broader training effects. Previous studies have often focused on circumscribed frameworks (e.g., Big Five) or addressed specific populations (e.g., psychotherapy patients, target groups in education, or those drawn to contemplative practices). Second, research rarely examines the stability of these effects over longer time frames in healthy adults. Healthy individuals, too, actively wish for an improved ability to deal with (stressful) challenges throughout life (R. Smith et al., 2024; van Agteren et al., 2021), and research needs to determine whether and how long the positive effects of psychological interventions last. Extending and developing trainings that address these limitations potentially add to the understanding of broader, nontargeted effects of psychosocial interventions which significantly contribute to protect from or at least act as a buffer against adverse effects of daily hassles and enhance overall resilience.

3.1.1 Present Research

The present research addresses these limitations and advances the field in several ways: We designed an 8-week group SECT for a broad public of healthy younger and older adults, wishing to improve their stress management and social relationships. Previous interventions often focused on either stress management or social skills. The SECT is distinct from these approaches by uniquely combining both aspects within a systemic and values-based framework. Moreover, our integrated approach innovatively combines and adapts well-founded techniques and exercises from diverse systemic, clinical, contemplative, and personality intervention approaches (e.g., Aguilar-Raab et al., 2023; Allemand & Flückiger, 2017; Ash et al., 2021; Center for Contemplative Science and Compassion-Based Ethics, 2019; Hayes et al., 1999; Hinsch & Pfingsten, 2015; Kabat-Zinn, 2013; von Schlippe & Schweitzer, 2016). It thereby allows participants to benefit from several evidence-based common change mechanisms, such as awareness, understanding (e.g., through reflection and insight), regulation of thought, emotion, and behavioral patterns, as well as motivational clarification and resource activation (e.g., Allemand & Flückiger, 2017; Gibbons et al., 2009; Goldberg, 2022; Sønderland et al., 2024; Wampold & Imel, 2015).

At the core, the SECT was designed with the goal of understanding the *processes* of personality development in healthy individuals, that is, *how* emotional stability and extraversion, two important socioemotional characteristics (Abrahams et al., 2019), change over the course of the training (i.e., through self-reflection and comparisons; see Küchler et al., 2025a). However, the present investigation had the goal of understanding potential spillover

effects, as found in previous research, by systematically examining specific and broader effects of the training. We therefore included a broad range of outcome variables—because we specifically trained them and/or expected indirect effects given the foundation, the content, and the exercises of the training. Particularly, we were interested in the specific effects of the SECT on mindfulness and perceived stress, which were core components of the SECT. More broadly, we expected effects on self-compassion, empathy, and perspective taking, given that these topics were explicitly or implicitly addressed in psychoeducation and exercises during the training.

Mindfulness entails a nonjudgmental awareness of the present moment (Kabat-Zinn, 2013), whereas self-compassion comprises a friendly and balanced perspective on the self. Both mindfulness and self-compassion promote emotional regulation and well-being (Goldberg et al., 2022; Leyland et al., 2019; Neff, 2023) and supposedly increase alongside each other (Golden et al., 2021). However, in line with recent discussions, we argue that mindfulness is a necessary, but not a sufficient condition for self-compassion to arise (Neff, 2023). This is underscored by results suggesting that mindfulness increases after compassion and gratitude practices, but not vice versa (Hildebrandt et al., 2017). As mentioned above, specific effects refer to outcomes that are the explicit target of a training, while broad effects refer to effects that were indirectly addressed. Mindfulness is a core component of the SECT and repeatedly practiced through different meditation exercises (e.g., body scan, open monitoring). We therefore suggest that the effect of the SECT on mindfulness is specific. In contrast, we consider self-compassion a broad effect because it was expected to develop through the integration of multiple training components (e.g., psychoeducation on negative self-talk), but it was not directly practiced, nor does it necessarily arise just because mindfulness is practiced (Hildebrandt et al., 2017). Feeling in control of perceived stress is crucial for management of daily stressors (Schönfeld et al., 2016). We argue that mindfulness, self-compassion, and perceived stress fall under the umbrella term of socioemotional characteristics that indicate and facilitate awareness, understanding, and regulation of own and others' thoughts, emotions, and behavior. Consequently, well-being should be preserved and enhanced the more people understand and regulate their emotions. Empathy—put simply, the ability to understand and share the feelings of others—and perspective taking, that is, adopting and understanding others' viewpoints (Davis, 1983), are other key socioemotional characteristics that indicate social awareness and relationship skills. These skills should maintain and improve the perceived quality of personal relationships further downstream.

The characteristics examined in the present study are related to the Big Five traits (Bainbridge et al., 2022) and map onto or are an integral part of various socioemotional frameworks (e.g., Center for Contemplative Science and Compassion-Based Ethics, 2019; Schoon, 2021). More importantly, we argue that these specific and broad indicators support individuals to reach their two common change goals outlined above: improving stress management and social interaction skills. Specifically, our hypotheses were:

Hypothesis 1: Participation in the socio-emotional competence training facilitates changes in socioemotional competences, i.e., it leads to increases in a) mindfulness, b) self-compassion, c) empathy, d) perspective taking, and e) decreased perceived stress.

Hypothesis 2: Participation in a socio-emotional competence training leads to increases in a) well-being, and b) perceived relationship quality.

We expected the training to lead to stable effects on the target variables across both a 3- and a 12-month follow-up period but did not preregister this prediction. Several theoretical assumptions of training effects could apply, that is, a continuous increase (or decrease) after training participation (linear), a trend back to baseline (quadratic), or an increase (or decrease) during the training followed by a leveling off after training participation (logarithmic). These models will be exploratorily examined and compared across the 3- and 12-months follow-up period, respectively. Showing that participating in the SECT improves a wide range of socioemotional characteristics in a general sample of healthy younger and older adults in the long-term will benefit the progression of intervention research at the intersection of counseling, personality, and contemplative psychology.

3.2 Method

3.2.1 Transparency and Openness

The hypotheses were preregistered on the Open Science Framework (https://osf.io/3abyn; Borgdorf et al., 2023; see deviations from the preregistration in Supplemental Table S1). Anonymized data, analysis code, complete code book, and other materials of this study are also available on the Open Science Framework (https://osf.io/ftas6/; Borgdorf et al., 2025b). Informed consent was obtained at all stages from all participants. This study was performed in line with the principles of the Declaration of Helsinki and was approved by the Ethics Committee of the Faculty of Behavioural and Cultural Studies at Heidelberg University, Germany (Agu 2022 4/1). We report how we determined our sample size, all manipulations, and all measures in the study, and we follow the Journal Article Reporting Standards for Quantitative Research (Appelbaum et al., 2018).

3.2.2 Procedure

Recruitment and Screening. Through diverse online and offline channels (e.g., lectures, flyers, social media, print, and cinema advertising), we specifically addressed individuals who wanted to become better at dealing with everyday stressors and challenging social situations. All advertisements contained information on the training and study content, procedure, timeline, cost, and compensation, as well as a link to the screening.

Given that the SECT was designed as a group training aimed at preserving and promoting mental health, our target population consisted of healthy adults. After giving informed consent, all potential participants were therefore screened for eligibility. Participants were excluded if they met any of the following criteria: (a) lack of internet access or appropriate hardware for online questionnaires, (b) current psychotherapy or participation in other socioemotional or compassion/ mindfulness-based trainings, (c) age between 40 and 50 years (other research questions studied age differences and required a sample of younger and older adults), or (d) clinically relevant levels of depression and generalized anxiety indicated by scores above cutoff values for major depression on the Patient Health Questionnaire-9 (as recommended by Löwe et al., 2002) or scores on the Generalized Anxiety Disorder Screener-7 ≥10 (Löwe et al., 2008). Participants meeting clinical criteria were referred to websites for counseling and therapeutic help.

Study Requirements, Compensation, and Enrollment. Participants who were eligible for the training received an individual study ID and a detailed information package on data privacy, collection, and processing, as well as the training and study content, requirements, procedure (e.g., random group assignment), timeline, cost, and compensation. For adherence and commitment purposes, participants had to pay 80 EUR (or 50 EUR at a reduced rate) during enrollment. Participants were reimbursed with up to 110 EUR, depending on their commitment to the study and training (e.g., filling out all questionnaires, regular participation in the training, etc.). If participants completed at least five out of eight training sessions, they additionally received a refund of half the training fee. If the participants agreed to the specified requirements, they were asked to enroll in the study online and provide informed consent again.

Participants enrolled in the study in three cohorts. The first cohort of trainings started in January 2023 (training group) and April 2023 (training start for first waitlist control group). Recruitment continued, resulting in a second cohort, starting in April 2023 (together with the first cohort waitlist control group) and in June 2023 (training start for second waitlist control group). In the third cohort, we recruited additional participants who started their training with the second cohort waitlist control group in June 2023, to achieve more power for analyzing time

effects. The questionnaire software SoSci Survey (Leiner, 2022) randomly assigned participants to either the training group or the waitlist control group. Participants were informed about this procedure prior to agreeing to participate.

3.2.3 Sample Size and Eligibility

We aimed to recruit 220 participants in two age groups ($N = 110$ young adults, 18–33 years, $N = 110$ older adults, 55+ years). This number resulted from sample size estimations for different research questions within the project which also required the two age groups (power estimation settings: $1 - \beta = .80$, $\alpha = .05$, medium and large effect sizes) and feasibility issues within this randmoized controlled trial (e.g., organizing parallel in-person training groups).

In total, we collected 1,150 data points at screening. Of the respondents who consented to the screening requirements and provided preliminary information on the second screening page ($n = 1{,}019$), 75.3% were female and 58.5% belonged to the younger age group. Figure 3.1 depicts the study and attrition flow as well as reasons for exclusion.

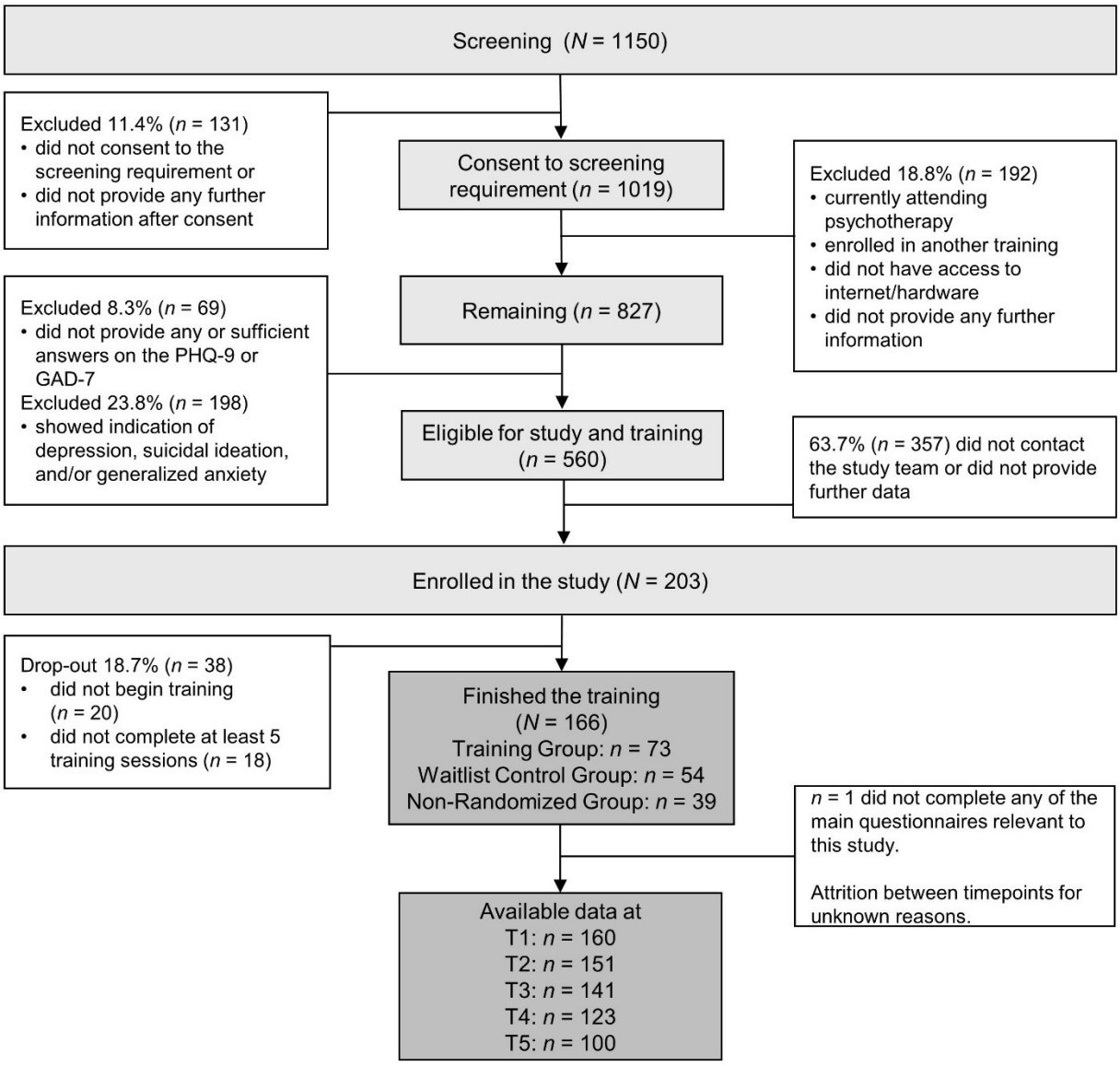
Of 203 individuals who enrolled in the training, 166 participants finished the training ($M_{Age} = 46.3$, $SD_{Age} = 18.7$, 75.2% female; see Figure 3.1 for attrition). Of these, 73 participants were randomly allocated to the training group, and 54 were assigned to the waitlist control group. Ten participants were not randomly allocated but were allowed to change from the training to the waitlist condition, or vice versa (e.g., because of time constraints). Another 39 participants enrolled in the third cohort without randomization for purposes of higher power.

3.2.4 Socioemotional Training and Trainers

Overview. The SECT was designed as an 8-week, 2-hr group format for groups of five to 12 participants, each led by two trainers. The training had a parallel format (i.e., groups ran concurrently on different weekdays) and took place in rooms of the University and University Hospital of Heidelberg. The first four sessions of the training covered topics on stress, resilience, attention, and emotion regulation. The second half of the training focused on interpersonal competences and included an introduction to social dynamics, systemic perspectives on social interactions, practicing social interactions with video feedback, and a summary of the training contents (see Supplemental Table S2 for a detailed overview on the training modules). Between the sessions, participants were asked to complete everyday exercises (e.g., meditations, self-reflection, and behavioral tasks). The study protocol and training material have not yet been published. Training material will be provided upon request.

Figure 3.1

Study Flow and Attrition



Note. The figure depicts the study flow and attrition. Some individuals may have repeatedly participated in the screening. More than one exclusion criterion may apply to one individual. Not all participants who completed the training filled out all study questionnaires at all timepoints; therefore, the *n* varies across timepoints. PHQ = Patient Health Questionnaire; GAD = Generalized Anxiety Disorder.

Trainers and Treatment Fidelity. Twenty-seven graduate students of psychology and educational sciences facilitated the trainings and received course credit for their role as trainers. As part of their preparation, the students had to participate in a 12-week train-the-trainer seminar during which they completed the training as participants themselves. During this period, they were also trained in didactical principles (i.e., attitude as a trainer, dealing with difficult situations/participants, etc.). While facilitating the training, they had to participate in

at least two group supervision sessions per training cohort. The supervision and the train-the-trainer seminar were facilitated by the two project principals, CAR and CW, with several years of experience in teaching and training facilitation. Additionally, CAR works as a licensed psychotherapist and mindfulness and compassion trainer. Furthermore, trainers were encouraged to discuss any questions, concerns, difficulties, or deviances during the training phase with KB and GK, before and after each training session.

To ensure close adherence to the training manual, trainers received a detailed time schedule they had to adhere to, as well as a fixed set of slides, worksheets, and prerecorded audio files. They also had access to video recordings of their own training facilitated by the project principals. After each training session, trainers documented adherence to the manual, reasons for deviation, and atmosphere during the session. Ultimately, the trainers declared that they adhered to the training manual closely (M = 5.80, SD = 1.44 on a 7-point scale ranging from very strong deviation to exactly as intended). The most frequently documented reason for deviance from the training manual was time management (e.g., time shortage, longer group discussions). Trainers therefore cut discussions short or asked participants to fill out worksheets at home. Details on adherence to the training manual and frequent reasons for deviance on the module level can be found in Supplemental Table S3.

3.2.5 Measures

Validated German versions of all questionnaires were administered before the training started (T1), 4 weeks into the training (T2), and in the week after the last training session (T3). Moreover, data were collected 3 months (T4) and 12 months (T5) after T3[1]. The waitlist control group answered questionnaires at T1 and T3, together with the training group. To reduce participant burden, data collected from the waitlist control group at T3 (post–control) was used as T1 (baseline/pretraining) data, allowing for within-training comparisons over time without additional data collection. If not indicated otherwise, items were rated on a 5-point Likert scale. Reliability scores for all measures at all time points ranged between ω = .72 and ω = .94 (see Supplemental Tables S4–S6). The codebook, detailing all collected measures, including those not reported in this article, can be found on the Open Science Framework (https://osf.io/ftas6/; Borgdorf et al., 2025b).

Mindfulness was measured with the 39-item Five Facet Mindfulness Questionnaire (Michalak et al., 2016), assessing the five facets Describe, Observe, Act with Awareness, Non-

[1] To reduce participant burden, we collected data on the FFMQ and self-compassion at T4, and on the CHIME subscales, empathy, perspective taking, and perceived relationship quality at T5. Data on well-being and perceived stress were collected at both follow-ups.

Judge, and Non-React. We included two subscales of the Comprehensive Inventory of Mindfulness Experiences (Bergomi et al., 2014), namely, Awareness of Thoughts' Relativity and Openness to Experience, which were answered on a 6-point Likert scale. *Self-compassion* was measured with the 12-itemshort version of the Self-Compassion Scale (Hupfeld & Ruffieux, 2011), assessing the six dimensions of self-compassion (positive: Mindfulness, Self-Kindness, Common Humanity; negative: Overidentification, Self-Criticism, Isolation). The 10-item *Perceived Stress* Scale (Schneider et al., 2020) measured the perceived ability to appropriately deal with and control stressful events.

Empathy and *perspective taking* were measured with the 18-item questionnaire for Empathy and Perspective Taking (Maes et al., 1995). Nine items captured the individual's capacity to consider and understand other people's feelings and problems (empathy), and nine items measured the willingness to consider other people's point of view (perspective taking). *Perceived relationship quality* was measured with the 10-item Evaluation of Social Systems Scale (Aguilar-Raab et al., 2015). Participants evaluated the social interactions in a relationship of their choice (e.g., partnership, friendship) regarding aspects, such as communication quality, decision-making quality, and atmosphere. Items were rated on a 4-point Likert scale. At T1, most participants chose a family relationship (31.3%, T2: 25.3%, T3: 24.1%), followed by romantic relationships (30.1%; T2: 33.7%, T3: 33.1%), friendships (26.5%; T2: 25.9%, T3: 22.9%), professional relationships (5.4%; T2: 3.0%, T3: 3.0%), and other relationships (3.0%; T2: 1.8%, T3: 1.2%). At T1, T2, and T3, 52.4% thought of the same category, 32.5% indicated the same categories at least at two time points, and 15.1% chose three different categories at the three time points. *Well-being* was measured with the 5-item WHO-5 Well-Being Index (E. Braehler et al., 2007). The scale assesses the presence of positive well-being on a 6-point Likert scale.

Meditation experience was operationalized by asking for how long (*several weeks* to *more than 5 years*) and how often (*not at all* to *daily*) participants had meditated. However, meditation experience did not significantly predict any of the outcome variables at T1; hence, we did not include these control variables in the analyses.

3.2.6 Analyses

Data were analyzed using R, Version 4.3.1 (R Core Team, 2023; see Supplemental Material for used R packages), and MPlus, Version 8.6 (L. K. Muthén & Muthén, 1998–2017). Data were collected before, during, and after the training (i.e., T1, T2, T3), with each assessment 4 weeks apart, as well as 3 months (T4) and 12 months (T5) after T3. To accommodate the nested structure of these data, we ran mixed effects models. Time was coded analogous to the

months that passed, that is, as 0 at T1, 1 at T2, 2 at T3, 5 at T4 (3 months after T3), and 14 at T5 (12 months after T3). The linear, quadratic, and logarithmic models for the exploratory analysis of time effects were not nested in each other. We therefore used the Deviance Information Criterion to compare model fit (ΔDIC > 7; Cain & Zhang, 2019).

As mentioned above, different research questions required the recruitment of two different age groups. Therefore, in the present study, we controlled for potential age effects in all analyses. We included time as a within-person (Level 1) random effect in all analyses and age group or training/control group as fixed between-person (Level 2) predictors. Age group was grand-mean centered. Analyses were conducted with and without outliers ($M \pm 3$ *SD*; outliers were winsorized). We identified eight outliers (two on empathy, and perspective taking, three on perceived relationship quality, and one on perceived stress, respectively). Because results did not differ, we only report results with winsorized outliers.

To examine whether changes over time in the outcome variables were different between the training and the waitlist control group, we included two-way cross-level interactions between time and group for the group comparison. These analyses only included T1 and T3 measurements as the waitlist control group did not provide T2, T4, or T5 measures in this function to keep participant burden to a minimum and because of the long follow-up time.

Separate mixed effects models for each outcome variable were estimated in MPlus using the Bayes estimator (Markov Chain Monte Carlo with Gibbs sampler) with default, noninformative priors (B. Muthén & Asparouhov, 2012). The estimations used two chains with 30,000 iterations per analysis, in which the first half is considered burn-in. Convergence was evaluated with the Gelman–Rubin diagnostic with values >1.1 taken as a sign of convergence (Potential Scale Reduction Factor; Gelman & Rubin, 1992; B. Muthén & Asparouhov, 2012). Convergence was double-checked with a higher number of iterations following recommendations by B. Muthén and Asparouhov (2012). Estimates and Potential Scale Reduction Factor with higher iterations did not essentially differ. The analyses provide point estimates and 95% credibility intervals (CI) of the posterior distribution. Effects are considered significant if the CI does not include zero.

3.3 Results

3.3.1 Sociodemographic Information and Baseline Characteristics

Study participants were mainly female (75.2%) and highly educated (63.0%with a university degree). We achieved a similar number of participants in the younger ($n = 80$; $M = 28.16$, $SD = 4.7$) and older ($n = 84$; $M = 63.55$, $SD = 7.2$) age group. Most of the younger participants were students or employees (92.6%), most of the older participants were employed

or retired (84.5%), and no participant was unemployed. Most students indicated a net household income below EUR 1,000. However, most retired and working participants indicated a higher net household income (*Md* = EUR 2,000 to 3,000), suggesting a high socioeconomic status of our sample. Almost half of the participants had prior meditation experience, but a majority of them did not meditate frequently (i.e., less than twice a month). Supplemental Table S7 contains detailed sociodemographic information on the overall sample and separately for training, control, and age groups. Table 3.1 shows descriptive statistics at T1 and T3 separately for the training and control group, and Table 3.2 depicts descriptive statistics over time for the complete sample. The training and waitlist control group did not meaningfully differ on demographics and baseline characteristics of the study's main variables (all *p*s > .05). Reliability scores, bivariate correlations of all outcome variables at T1, and retest reliabilities between T1 and T3 are depicted in Supplemental Table S4.

3.3.2 Dropout, Cohort, and Adherence Analyses

Approximately 36.3% (*n* = 203) of the eligible participants (*N* = 560; see Figure 3.1) eventually enrolled in the study. Of these, 18.7% (*n* = 38) decided not to start (*n* = 20) or to end participation prematurely (i.e., completed ≤ 4 training sessions; *n* = 18) and are considered a dropout. Dropouts did not differ meaningfully from participants who finished the training, except in levels of empathy: The dropout group was significantly more empathetic (*d* = 0.49, *p* = .01; for details, see Supplemental Table S8). Likewise, different training cohorts did not differ meaningfully on sociodemographic and baseline characteristics; therefore, we collapsed the data for our analyses (see Supplemental Table S9). At T3, participants who discontinued participation after T3 did not differ from participants who provided data at T4 and/or T5 (see Supplemental Table S8). The final sample consisted of 166 participants who finished the training. On average, participants completed 6.88 training sessions (*SD* = 0.89). One participant in the nonrandomized group did not complete any of the main questionnaires relevant to this study and was excluded from further analyses. Not all participants who completed the training filled out all study questionnaires. However, in sum, at least 160 participants provided data at T1, 151 at T2, 141 at T3, 123 at T4 (attrition rate of 25.9% compared to T3), and 100 at T5 (attrition rate of 39.8% compared to T3).

3.3.3 Training Efficacy

As evident from the intraclass correlation coefficients, which ranged between 0.40 and 0.83, the effect of time varied substantially both within and between persons (see Supplemental Table S10). Therefore, we assumed that application of mixed effects models was warranted

(Raykov, 2011). Table 3.1 illustrates the results of the linear mixed effects models on each outcome variable for the differential effects on the training and the waitlist control group at T1 and T3 (for full model results, see Supplemental Table S10). Partly in line with Hypothesis 1a, a significant Group × Time effect (b = 0.28, 95% CI [0.16; 0.40]) indicates greater pre–post increases in mindfulness in the training group compared to the waitlist control group. We did not find a significant Group × Time effect for the subscales of the Comprehensive Inventory of Mindfulness Experiences, that is, relativity of thoughts (b = 0.17, 95% CI [−0.14, 0.48]) and openness to new experiences (b = 0.02, 95% CI [−0.23, 0.27]). Furthermore, and contrary to Hypothesis 1b, we did not observe a significant Group × Time effect indicating greater pre–post changes in the training group compared to the waitlist control group on self-compassion (b = 0.13, 95% CI [−0.06, 0.31]).

We did not find support for Hypotheses 1c or 1d either, that is, the results indicate no differential pre–post changes in the training compared to the waitlist control group in empathy (b = −0.04, 95% CI [−0.15, 0.08]) or perspective taking (b = 0.06, 95% CI [−0.07, 0.19]). In line with Hypothesis 1e, however, participation in the training resulted in greater pre–post reductions compared to the waitlist control group on perceived stress (b = −0.36, 95% CI [−0.56, −0.16]). In line with Hypothesis 2a, the significant Group × Time effect (b = 0.43, 95% CI [0.09, 0.78]) indicates that participation in the training led to greater improvements in well-being compared to the waitlist control group. The Group × Time effect for perceived relationship quality was not significant (b = 0.11, 95% CI [−0.13, 0.34]), indicating that pre–post changes in perceived relationship quality in the training group did not differ from pre–post changes in the waitlist control group as suggested in Hypothesis 2b. The results did not differ when we only included those participants who indicated the same relationship category at all three time points.

3.3.4 Training Effects Over Time

Table 3.2 shows the results of the exploratory follow-up analyses over time (T1–T4/T5, respectively) for all participants independent of group status. Indicated by lower DIC of at least ΔDIC > 7, the results suggest that the logarithmic models fit our data best (see Supplemental Table S10 for an overview of all models). That is, results suggest a strong initial increase (or decrease, respectively) followed by a subsiding increase (or decrease) or leveling off after the training ended in mindfulness (Five Facet Mindfulness Questionnaire: b = 0.14, 95% CI [0.11, 0.17]; Comprehensive Inventory of Mindfulness Experiences subscale openness to experience: b = 0.07, 95% CI [0.01, 0.13]) and self-compassion (b = 0.21, 95% CI [0.16, 0.26]). The training

also led to significant logarithmic increases in perspective taking (b = 0.04, 95% CI [0.01, 0.06]) and in perceived relationship quality (b = 0.14, 95% CI [0.09, 0.19]), across the 12-months follow-up, but had no effect over time on empathy (b = −0.01, 95% CI [−0.04, 0.02]). The effects on perceived stress (b = −0.09, 95% CI [−0.14, −0.05]) and well-being (b = 0.14, 95% CI [0.07; 0.20]) were also significant across the 3- and 12-months follow-up. Nonsignificant Age × Time effects indicate that the training effects over time did not differ between younger or older participants in the current sample (see Table 3.2). Figure 3.2 displays the change in standard deviations from T1 at each time point for all participants.

3.3.5 Training Evaluation

The training aimed at preserving and promoting stress management, social interaction skills, and well-being in a prescreened, nonclinical sample. Yet, participants self-selected into the training, indicating that they perceived the need to train their socioemotional competences. To evaluate whether participants experienced the benefit they wished for, we included measures of perceived benefit, quality, satisfaction, adverse effects, and support from the training (see Supplemental Material for further details). In summary, at T3, on a scale from 1 to 4, 139 participants rated the quality of the training as good to excellent (M = 3.21, SD = 0.50) and indicated that they were generally satisfied with the training (M= 3.37, SD = 0.73). Moreover, participants reported that they were better able to deal with stress (M = 3.09, SD = 0.64) and to stay more relaxed in challenging social situations (M = 3.06, SD = 0.59) due to training participation.

Table 3.1

Differential Change Between Groups: Descriptive Statistics, Within-Group Effect Sizes, and Group x Time Interaction Effects

Variable	T1[a] *M* (*SD*)			T3[b] *M* (*SD*)		$d_{T1\text{-}T3}$[c]		Group x Time
	TG	WG	*p*[d]	TG	WG	TG	WG	Estimate (SE) [CI]
Mindfulness (FFMQ)	3.26 (0.46)	3.29 (0.43)	.725	3.50 (0.47)	3.24 (0.48)	0.70	−0.20	**0.28** (0.06) [0.16; 0.40]
Relativity of Thoughts (CHIME)	3.82 (0.88)	3.85 (0.84)	.875	4.16 (0.96)	4.00 (0.84)	0.43	0.18	0.17 (0.16) [−0.14; 0.48]
Openness to New Exp. (CHIME)	3.52 (0.91)	3.61 (0.83)	.599	3.74 (0.99)	3.68 (0.86)	0.30	0.21	0.02 (0.13) [−0.23; 0.27]
Self-Compassion	2.97 (0.66)	2.92 (0.63)	.674	3.27 (0.70)	3.08 (0.66)	0.57	0.33	0.13 (0.09) [−0.06; 0.31]
Perceived Stress	2.91 (0.58)	2.95 (0.55)	.638	2.47 (0.61)	2.88 (0.62)	−0.80	−0.10	**−0.36** (0.10) [−0.56; −0.16]
Empathy	3.78 (0.50)	3.90 (0.51)	.173	3.80 (0.46)	3.92 (0.52)	−0.10	0.06	−0.04 (0.06) [−0.15; 0.08]
Perspective Taking	3.58 (0.52)	3.64 (0.53)	.563	3.68 (0.54)	3.68 (0.54)	0.20	0.05	0.06 (0.07) [−0.07; 0.19]
Relationship Quality	2.71 (0.56)	2.70 (0.63)	.970	3.09 (0.65)	2.99 (0.62)	0.58	0.43	0.11 (0.12) [−0.13; 0.34]
Well-Being	3.57 (0.97)	3.60 (0.93)	.828	4.07 (0.94)	3.68 (1.06)	0.56	0.08	**0.43** (0.18) [0.09; 0.78]

Note. TG = Training Group; WG = Waitlist Control Group. [a]n_{TG} = 72-73; n_{WG} = 54. [b]n_{TG} = 66-68; n_{WG} = 50. [c]Cohen's *d* represents within-group changes from T1 to T3. [d]*p*-values from Welch's independent *t* tests comparing groups at T1. Group × Time effects are estimates from linear mixed effects models testing differential changes between TG and WG from T1 to T3. Bold estimates indicate significant effects. Complete model results are available in Supplemental Table S10.

Table 3.2

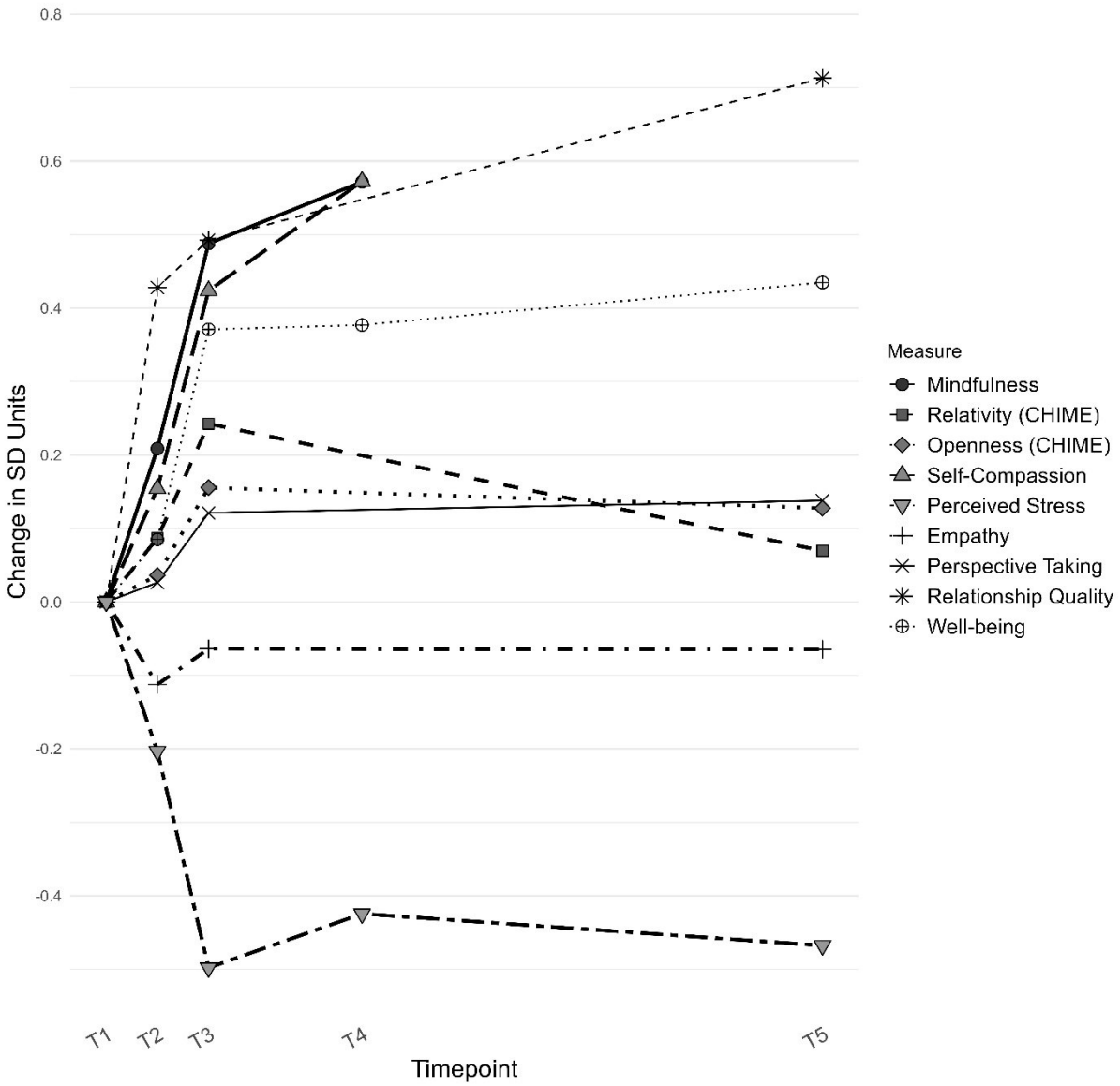
Changes over Time: Descriptive Statistics, Effect Sizes, Time and Age x Time Effects.

	***M* (*SD*)**					**Cohen's *d*** [b]			**Log Time**	**Age x Time**
Variables	**T1**[a]	**T2**[a]	**T3**[a]	**T4**[a]	**T5**[a]	$d_{T1\text{-}T3}$	$d_{T1\text{-}T4/T5}$	$d_{T3\text{-}T4/T5}$	**Estimate (SE) [CI]**	**Estimate (SE) [CI]**
Mindfulness (FFMQ)[c]	3.26 (0.44)	3.34 (0.48)	3.46 (0.45)	3.50 (0.47)		0.47	0.55	0.09	**0.14** (0.02) [0.11; 0.17]	−0.01 (0.03) [−0.07; 0.05]
Relativity of Thoughts (CHIME)[d]	3.86 (0.86)	3.99 (0.87)	4.12 (0.90)		3.98 (0.86)	0.24	0.08	−0.12	0.05 (0.03) [−0.01; 0.11]	−0.05 (0.06) [−0.17; 0.07]
Openness to New Exp. (CHIME)[d]	3.58 (0.90)	3.66 (0.96)	3.78 (0.90)		3.75 (0.96)	0.17	0.29	0.02	**0.07** (0.03) [0.01; 0.13]	−0.03 (0.06) [−0.15; 0.10]
Self-Compassion[c]	2.93 (0.64)	3.08 (0.66)	3.26 (0.69)	3.36 (0.69)		0.38	0.53	0.15	**0.21** (0.02) [0.16; 0.26]	0.06 (0.05) [−0.03; 0.16]
Perceived Stress	2.94 (0.58)	2.79 (0.65)	2.62 (0.65)	2.66 (0.66)	2.64 (0.67)	−0.46	−0.36/ −0.42	0.08/ 0.06	**−0.09** (0.02) [−0.14; −0.05]	−0.01 (0.04) [−0.09; 0.07]
Empathy[d]	3.84 (0.50)	3.79 (0.46)	3.81 (0.48)		3.81 (0.44)	−0.12	−0.17	0.04	−0.01 (0.01) [−0.04; 0.02]	0.04 (0.03) [−0.01; 0.09]
Perspective Taking[d]	3.62 (0.53)	3.65 (0.51)	3.70 (0.53)		3.71 (0.48)	0.11	0.12	0.09	**0.04** (0.01) [0.01; 0.06]	0.01 (0.03) [−0.04; 0.05]
Relationship Quality[d]	2.68 (0.59)	3.04 (0.66)	3.08 (0.67)		3.21 (0.63)	0.47	0.67	0.09	**0.14** (0.03) [0.09; 0.19]	0.04 (0.05) [−0.06; 0.14]
Well-Being	3.54 (0.96)	3.65 (1.03)	3.93 (1.03)	3.94 (1.07)	4.00 (1.06)	0.38	0.32/0. 30	−0.04/ −0.07	**0.14** (0.03) [0.07; 0.20]	0.02 (0.07) [−0.10; 0.16]

Note. [a]n_{T1} = 158-160; n_{T2} = 148-151; n_{T3} = 139-141; n_{T4} = 123; n_{T5} = 99-100. [b]Cohen's *d* for mean differences between indicated timepoints. Follow-up assessments varied by measure: [c]Follow-up only at T4. [d]Follow-up only at T5. Time and Age x Time effects are estimates from mixed effects models with log-transformed time. Bold estimates indicate significant effects. Complete model results are available in Supplemental Table S10.

Figure 3.2

Change in Standard Deviation Over Time (Relative to T1)



Note. The figure displays the changes in standard deviation for the outcome variables between baseline (T1) and each timepoint (T2, T3, T4, T5), respectively. Values were calculated by $(M_{TX} - M_{T1}) / SD_{T1}$.

3.4 Discussion

This randomized controlled trial examined the specific and broad effects of an 8-week SECT on mindfulness, self-compassion, perceived stress, empathy, perspective taking, well-being, and perceived relationship quality and the stability of the effects across a 3- and 12-months follow-up. Results suggest that participation in the SECT led to specific effects, indicated by increases in mindfulness and decreases in perceived stress compared to a waitlist control group. Contrary to expectations, no broader effects of the SECT on self-compassion,

empathy, or perspective-taking compared to the waitlist control group emerged. In line with our hypothesis, the training also led to higher well-being in the training group. Participation in the training did not, however, lead to higher perceived relationship quality with an indicated target person. Nonetheless, time effects on most variables—that is, mindfulness, self-compassion, perceived stress, perspective taking, well-being, and perceived relationship quality—were significant and could be maintained over a 3- and/or 12-months follow-up period. Additionally, we did not find different effects for older and younger adults, indicating that the SECT is applicable across the adult lifespan, which is consistent with previous results (Roberts et al., 2017).

The SECT is embedded in a systemic and value-based framework, relies on common change mechanisms, and offers a unique combination of established exercises in order to simultaneously address two highly prevalent change goals: improving stress management and social interaction skills. The systemic foundation and contemplative focus of the SECT may be particularly effective in supporting participants in understanding and modifying their patterns of thinking, feeling, and behaving in response—rather than reaction—to internal and external circumstances (e.g., Bernstein et al., 2015). Furthermore, connecting the desire for change with personally meaningful values enhances motivation and sustains goal pursuit. Taken together, the current findings suggest that the SECT is a feasible and well-accepted training option for healthy younger and older adults that may enable participants to preserve their mental health and become better at dealing with emotional and social challenges throughout life.

Contrary to our expectations, we did not find broad effects of the SECT, despite previous evidence for such effects in similar interventions (Hähner et al., 2024; Hildebrandt et al., 2017). Still, this result pattern resembles previous research on near- and far-transfer effects of cognitive trainings. Two meta-analyses did not find convincing evidence for far-transfer effects of working memory training compared to control groups (Melby-Lervåg et al., 2016; Sala et al., 2019). The critique offered in this domain that cognitive tasks trained in a lab are too abstract to apply to real-world problems (Niebaum & Munakata, 2022), however, does not apply to our context. The SECT is highly adapted to the real world, and integration into daily life was suggested at various points.

Two explanations for the current findings appear more appropriate. First, effects of psychological interventions in healthy samples are potentially rather specific. This is in line with previous findings on trainings with a mental health promotion focus (e.g., Singer & Engert, 2019; R. Smith et al., 2024; Stieger et al., 2021). By analogy with physical exercise, any activity benefits individuals with a sedentary occupation or restricted mobility. However, healthy adults

aiming for specific goals, like doing a pull-up, require targeted training of specific muscle groups. Similarly, healthy adults typically already exhibit high levels of emotional and social functioning and, therefore, may benefit less from common change factors that simultaneously address a broad range of psychological issues—factors that are highly beneficial for psychotherapy patients (e.g., Gibbons et al., 2009). In contrast, healthy individuals may need more targeted or even person centered training approaches in order to flourish and go beyond preserving mental health—just like when training the latissimus muscle for a pull-up.

Preassessing people's levels of socioemotional functioning would allow to tailor the training to their needs (as is routinely done in psychotherapy or in the gym). As of now, this approach is limited by financial and time constraints. However, with artificial intelligence and machine learning rapidly evolving, the idea of being able to analyze individuals' initial levels of certain characteristics and then choosing the appropriate training from a vast pool of validated exercises, content, and mechanisms seems well within reach.

A second alternative explanation for the current result pattern might be that broad effects may be smaller and need more time to unfold—like muscle growth in surrounding, but not directly targeted muscles. This assumption is strengthened by the significant time effects across the follow-up period in our study and results from trainings with more intensive designs (e.g., Hinsch & Pfingsten, 2015; Singer & Engert, 2019; R. Smith et al., 2024; Stieger et al., 2021). Hence, broad effects may only show after longer time periods or more intensive training. This may be particularly relevant for changes on interpersonal characteristics which might be harder to implement for participants and take longer to unfold due to the complexity of social dynamics (Sembill et al., 2017). While perspective taking shows a significant increase over time (not controlling for group status), we even found a slight, although nonsignificant, decrease in empathy. Next to methodological reasons (i.e., ceiling effects, regression to the mean), a potential reason could be that participants might have aimed to become less empathetic. Results on personality change goals assessed before the training for a different research question align with previous research and revealed that 11.4%of participants wanted to be less agreeable (similar to 6.4% in Stieger et al., 2021) and 86.1% wanted to be more extraverted as indicated by means above or below the scale midpoint ("I don't want to change this aspect"), respectively.

We achieved the primary objective of this study, that is, preserving *and* promoting mental health in healthy adults. Still, the interpretation of accumulated result pattern in combination with previous research findings points to several future research endeavors: First, we suggest that future intervention research should aim to sharpen the definition of specific and broad effects, bearing in mind the specificity or breadth of the indicators themselves, as well as the

content and exercises of the intervention. Potentially future research can lean on the distinction between near- (explicitly and repeatedly trained) and far- ("spillover") transfer effects from cognitive psychology. Moreover, research should specify mechanisms and temporal dynamics of these effects in controlled designs. This would allow for even more effective design of specific (and broad) psychological interventions considering the needs of healthy adults.

This need for individualization extends to cultural considerations as well. Although the SECT was developed and evaluated in Germany, the effects will likely generalize to other individualistic cultures given that the theoretical (i.e., common change mechanisms) and empirical (i.e., exercises fromwell-validated trainings) foundation is based upon research conducted in these cultures. We also argue that especially the second part of the training focusing on social interaction skills (e.g., the videotaped fictive role plays of Module 6 and 7) could be easily adapted to culturally specific aspects of social interactions. Future cultural adaptations of the SECT may consider different social norms, communication styles, and value orientations at various points of the training. Understanding how change goals and individual preferences vary across cultures could inform both the theoretical framework of specific versus broad effects and the practical implementation of individualized interventions.

3.4.1 Limitations and Directions for Future Research

Despite the notable strengths and promising findings of this randomized controlled trial, some limitations of the present study should be noted. First, our study is limited to a numerous, yet still selected set of indicators of better stress management and social interaction skills. Although these variables are theoretically sound, empirically validated, and trainable competences, future research may consider including extant comprehensive socioemotional competence measures (e.g., Soto et al., 2022). Incorporating these alongside specific outcome measures would support a better understanding of the relationship between targeted training effects and other indicators of socioemotional functioning. Moreover, personalizing the training to individual needs would make the training simpler, more accessible, and potentially more effective for healthy individuals. Examining potential mechanisms and preconditions for effectiveness, that is, identifying mediators and moderators, should also be considered. Second, compared to other universal trainings (e.g., Singer & Engert, 2019; R. Smith et al., 2024; Stieger et al., 2021), the current sample was relatively small. Broader effects, if present at all, are probably of a smaller size. Significant training effects over time may hint at (small) broad effects. Moreover, we assessed a relatively large number of variables. Future research needs to replicate the study for more confident results. Including other measures beyond self-reports, like biological markers, could also strengthen findings. On a related note, participants often did

not remember which relationship they had evaluated for perceived relationship quality which casts doubt on how to interpret these findings. Future studies should take the operationalization of measuring social dynamics and contexts into account more carefully (e.g., Roos et al., 2023). Although the study consisted of an age heterogeneous sample including older adults—an aspect often neglected in other intervention research—the current sample was mostly female, highly educated, and with a rather high socioeconomic background (i.e., Western, educated, industrialized, rich, and democratic; Henrich et al., 2010). Future studies should aim to include participants from more diverse backgrounds and cultures and evaluate the effectiveness of the SECT in specific (cultural) groups and minorities (e.g., regarding sexual or religious orientation, disability status, race, socioeconomic status, etc.). This would increase confidence in statements on the reliability and generalizability of the training effects. Even stronger evidence could be derived by including an *active* control group, assessed across the same measurement occasions as the training group. This would allow for a more nuanced evaluation of the long-term trajectories of the training than the current pre–post comparison. Due to limited resources, we could not realize such a design in this study. Limited resources also restrained use of more elaborate measures to evaluate compatibility and treatment fidelity (i.e., using trainers' self-report instead of observational data from videotapes), which should be addressed in future research.

Despite these limitations, the current results demonstrate that the SECT adds significant and long-term value to healthy individuals in mindfulness, perceived stress, and well-being. The effect sizes are comparable to other mental health trainings (e.g., Hildebrandt et al., 2017; R. Smith et al., 2024; Stieger et al., 2021), potentially offering a more comprehensive approach by addressing both stress management and social interaction skills at the same time. The SECT is a high-quality training with a comparatively low attrition rate of only 18.7% (T1–T3). We also controlled for potential adverse effects, with only 4.2% of participants reporting minor negative effects—an essential aspect often neglected by intervention research in healthy adults (Halfond et al., 2021). The evaluation of whether the SECT (universally) prevents psychological problems, however, remains to be determined in future work.

3.5 Conclusion

The findings of this randomized controlled trial suggest that the 8-week SECT benefits healthy adults of different age groups who seek to improve their ability to manage stress and navigate challenging social interactions. The observed improvements in mindfulness, perceived stress, and well-being, which were maintained between 3 and 12 months, indicate that the SECT, similar to other mental health trainings, has specific beneficial effects. This highlights

the importance of designing tailored interventions that focus on specific competencies because individual needs and desires for change may differ. Yet, broader effects may be smaller and may need longer to unfold which is to be determined by future research. This study emphasizes the potential for psychological interventions to prevent adverse effects from life's challenges and to long-lastingly promote the preservation and cultivation of psychological health and well-being.

Chapter 4: When State-Trait Trajectories do not Align: State Mindfulness does not Predict Trait Changes of Mindfulness, Self-Compassion, or Perceived Stress.

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Abstract

Previous research has identified state mindfulness as a key change mechanism in explicitly mindfulness-based interventions to increase trait mindfulness and reduce stress. This study extends previous work by examining trajectories of state mindfulness and their associations with trait indicators of mindfulness, self-compassion, and perceived stress when mindfulness practices are delivered within a Socioemotional Competence Training (SECT) without explicit mindfulness labeling. A sample of 166 healthy younger and older German adults (75.2% female; M_{age} = 46.26; SD_{age} = 18.70) participated in the 8-week training. State mindfulness was assessed weekly, whereas trait mindfulness, self-compassion, and perceived stress were assessed before, during, and after the training. Second-order latent growth models suggest significant pre-post improvements in all state and trait variables (d = |0.38| to |0.62|), but trait changes were not predicted by changes in state mindfulness. These findings indicate that while state mindfulness increases during the Socioemotional Competence Training, these increases alone appear insufficient to drive changes in trait mindfulness and related outcomes. Results suggest that intervention framing could play a crucial role in facilitating explicit connections between state experiences and trait development, contributing to our understanding of mindfulness as a change mechanism in psychological interventions.

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4.1 Introduction

The concept of mindfulness and mindfulness practices has seen an impressive surge in scientific interest over the last three decades (van Dam et al., 2018). Mindfulness is generally defined as a purposeful present-moment awareness with a non-judgmental and accepting attitude (Bishop et al., 2004; K. Brown & Ryan, 2003; Creswell, 2017; Kabat-Zinn, 2013; van Dam et al., 2018). Research has approached mindfulness as both a relatively stable *trait*-like characteristic and a fluctuating *state* experience. Mindfulness construed as a trait-like characteristic has been defined, and mostly measured with self-report questionnaires, as a general tendency or predisposition to be attentive and non-judgmental towards present-moment experiences (e.g., Baer et al., 2006; Bergomi et al., 2014; K. Brown & Ryan, 2003; K. Brown et al., 2007; Chems-Maarif et al., 2025; Kabat-Zinn, 2013; Karl et al., 2021; Lindsay & Creswell, 2017). The *momentary* non-judgmental orientation to present-moment experiences is referred to as state mindfulness (Blanke & Brose, 2017; Bravo et al., 2018; Tanay & Bernstein, 2013; Warren et al., 2023). Ecological momentary assessment studies suggest that state mindfulness naturally varies in everyday life and between individuals (Warren et al., 2020, 2023).

Extensive research demonstrates that mindfulness-based interventions (MBIs) yield numerous positive outcomes, including improved emotion and behavioral regulation, cognitive functioning, increased well-being, better physical health indicators, and more prosocial attitudes (Creswell, 2017; Donald et al., 2020; Giluk, 2009; Goldberg et al., 2023; Keng et al., 2011; Leyland et al., 2019; Singer & Engert, 2019; Tomlinson et al., 2018; Trautwein et al., 2020; for a discussion of boundary conditions, see Britton, 2019). While these benefits are well-documented, researchers continue investigating the underlying change mechanisms. Similar to psychotherapy (Wampold & Imel, 2015) and personality interventions (Allemand & Flückiger, 2017), MBIs are hypothesized to operate through both common and specific change mechanisms (e.g., Fresco & Mennin, 2019; Goldberg, 2022; J. Gu et al., 2015; Hölzel et al., 2011). Common change factors include therapeutic alliance, group cohesion, and expectation of beneficial outcomes (e.g., Goldberg, 2022). Specific mechanisms encompass state mindfulness, emotion regulation, interoceptive awareness, cognitive processing changes, and shifts in self-perspective (e.g., J. Gu et al., 2015; Hölzel et al., 2011; Lindsay & Creswell, 2017; Tang et al., 2015; Vago & Silbersweig, 2012). Most relevant to the current investigation, repeated experiences of state mindfulness are thought to facilitate cognitive and emotional meta-awareness, allowing for more conscious selection of evaluative information and less automatic, reactive responses to internal and external

experiences (Bernstein et al., 2015; Borgdorf et al., 2024; Fresco & Mennin, 2019; Ludwig et al., 2020; Schuman-Olivier et al., 2020).

To understand how these mechanisms work, or more specifically, how state experiences relate to lasting trait change during interventions, researchers have suggested several theoretical frameworks. By applying ideas from Whole Trait Theory (WTT; Fleeson & Jayawickreme, 2015), scholars have aimed to reconcile the state and trait approach to mindfulness by conceptualizing trait mindfulness as a stable density distribution of fluctuating, within-person mindfulness state experiences (Warren et al., 2023). WTT conceptualizes traits as consisting of two interactively emerging parts: (a) as descriptions of how individuals typically function (descriptive part; "density distributions of states") as well as (b) outcomes of socio-cognitive processes that explain how and why trait expression differs across context and time (explanatory part). This perspective aligns with broader theories of personality development and psychotherapy efficacy, which emphasize that repeated state experiences can accumulate into trait-level changes over time (e.g., Wrzus & Roberts, 2017; Zilcha-Mano, 2021). Empirical findings corroborate these theoretical ideas showing, for example, that weekly changes in personality characteristics correlate with personality trait change in app-based and in-person interventions (Küchler et al., 2025a; Olaru et al., 2024). Applying the tenets of WTT to mindfulness, Warren and colleagues (2023) suggest that the frequency and duration of mindful state experiences constitute trait mindfulness. Consequently, if individuals experience mindful states more frequently—for example, during MBIs—indicators of their trait mindfulness should change accordingly (K. Brown & Ryan, 2003; Warren et al., 2023).

Consistent with these theoretical assumptions, recent studies incorporating measures of state mindfulness have provided first evidence for the outlined state-trait associations (e.g., Aguilar-Raab et al., 2021; Baer, Carmody, & Hunsinger, 2012; Garland et al., 2017; Kiken et al., 2015). Two studies examining state trajectories across 8-week Mindfulness-Based Stress Reduction (MBSR) courses found that weekly increases in state mindfulness predicted pre-post changes in trait mindfulness and psychological stress indicators (Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015). These findings underscore that state-trait mechanisms may be crucial for how MBIs create lasting trait change (Schuman-Olivier et al., 2020; Warren et al., 2023) and aligns with the understanding that mindfulness is trainable through repeated practice (Creswell, 2017).

Interestingly, meta-analytic evidence suggests that interventions not explicitly focused on mindfulness also yield increases in mindfulness, although to a smaller extent than MBIs

(Goldberg et al., 2019; cf. Golberg et al., 2016; Quaglia et al., 2016). Notably, the meta-analysis compared traditional MBIs (e.g., MBSR and Mindfulness-Based Cognitive Therapy) to non-mindfulness-based active bona fide conditions intended to be therapeutic (e.g., Cognitive Behavioral Therapy [CBT]) on trait mindfulness and clinical outcomes, such as depression, anxiety, and psychological distress (Goldberg et al., 2019). These comparisons are especially informative because they control for non-specific and specific treatment factors (e.g., cognitive restructuring as in CBT) simultaneously. The results of the meta-analysis imply that, to some extent, other psychological interventions without explicit mindfulness content implicitly or explicitly convey mindfulness skills or at least aspects thereof (e.g., through observation of thoughts and emotions; Goldberg, 2022; Goldberg et al., 2019; J. Gu et al., 2015; Quaglia et al., 2016). The open question remains whether the state-trait pathway found previously (Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015) generalizes beyond explicit MBIs to contexts where mindfulness components are embedded more implicitly within broader intervention frameworks (cf. Fresco & Mennin, 2019; Mennin et al., 2013).

Self-compassion—treating oneself with kindness when facing difficulty—shares conceptual overlap with mindfulness, as mindful awareness of difficult experience is an integral part to self-compassion's definition (Neff, 2003a, 2023). Substantial empirical evidence demonstrates bidirectional relationships between these constructs: Some researchers identified self-compassion as a change mechanism in MBIs (J. Gu et al., 2015; Kuyken et al., 2010; López et al., 2016), while others consider mindfulness as a prerequisite for self-compassion to arise (Beshai et al., 2018; Hildebrandt et al., 2017; Neff, 2003a, 2023; van Dam et al., 2011). Still others have found a combination of both to mediate effects on well-being (Baer, Lykins, & Peters, 2012). Hence, the relationship between mindfulness and self-compassion provides a particularly compelling context for examining state mindfulness mechanisms. In the current study, we included self-compassion as an additional outcome to further elucidate on the assumed direction that mindful awareness is a precondition for self-compassion to arise (cf. Hildebrandt et al., 2017). Thus, if state mindfulness drives trait changes as suggested by WTT and previous research as reviewed above, increases in state mindfulness might also predict improvements in downstream outcomes, such as self-compassion and perceived stress. alongside traditional mindfulness outcomes.

4.1.1 Present Research

The goal of the current research was twofold: First, we wanted to investigate whether the effects of state mindfulness extend beyond traditional MBIs to a Socioemotional

Competence Training (SECT) that incorporated mindfulness practices without explicit mindfulness framing. This design allows examination of whether state mindfulness operates as a specific change mechanism while reducing expectancy effects associated with mindfulness labeling. And second, beyond replicating previous findings on trait mindfulness and perceived stress (Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015), we included self-compassion as potential "downstream" consequences of state mindfulness changes.

More specifically, we examined whether state mindfulness increases throughout the 8-week Socioemotional Competence Training (SECT; Borgdorf et al., 2025a) and examined the relationships between weekly changes in state mindfulness and changes in trait mindfulness, perceived stress, and self-compassion. The SECT included some mindfulness practices (e.g., body scan, awareness of the breath, open monitoring) but was not designed, structured, or advertised as an MBIs. Specifically, our preregistered hypotheses were:

Hypothesis 1 (H1): Over the course of the training, state mindfulness improves.

Hypothesis 2a (H2a): More pronounced changes in state mindfulness are associated with stronger changes in trait mindfulness.

Hypothesis 2b (H2b): More pronounced changes in state mindfulness are associated with stronger changes in trait self-compassion.

Hypothesis 2c (H2c): More pronounced changes in state mindfulness are associated with stronger changes in trait perceived stress.

4.2 Method

4.2.1 Transparency and Openness

Hypotheses were preregistered on the Open Science Framework (OSF; see minor deviations from the preregistration in Supplementary Table S6). The anonymized dataset, analysis code, and code book with an overview of all variables of the project are also stored on the OSF. Data from the same dataset but with distinct research questions have been previously used in Borgdorf et al. (2025a) and Küchler et al. (2025a). The study was approved by the Ethics Committee of the Faculty of Heidelberg University and aligns with the principles of the Declaration of Helsinki. We obtained informed consent from all study participants prior to participation.

4.2.2 Procedure

Participants were recruited on- and offline (e.g., through social media advertising, flyers, or talks at various institutions). We provided a link through which interested people reached the online survey platform SosciSurvey (Leiner, 2022). After giving informed consent approximately 1,150 people were screened for study eligibility criteria, that included

(1) belonging to one of the two age groups of younger (18-40 years) or older (50-80 years) adults for other research questions, (2) appropriate access to study material and questionnaires, and (3) no participation in another training, or psychotherapy/psychological counseling. Moreover, respondents who (4) exceeded clinical cut-off values for depression and/or generalized anxiety were excluded (Löwe et al., 2002; Löwe et al., 2008). In total, of the 560 participants who were eligible for the training, 203 enrolled in it, and 166 ultimately finished the training.

Following successful screening, participants were informed on the study purpose and requirements, data collection and privacy, as well as training cost and reimbursement. Informed consent was collected a second time at this point. Participants paid EUR 80 (or EUR 50 at a reduced rate) for participation but were reimbursed with up EUR 110 plus half of the training fee depending on their adherence to the training sessions and study requirements. While filling out the baseline questionnaires, participants were randomly allocated to either a training group or a waitlist control group. Because this study only assessed state mindfulness during active training participation, we did not include analyses on group comparisons. Additional information on recruitment, enrollment, screening, random group allocation, and drop-out of participants is detailed in Borgdorf et al. (2025a).

4.2.3 Socio-Emotional Competence Training

Participants took part in an 8-week Socio-Emotional Competence Training (SECT) with weekly 2-hours sessions. Group size ranged from five to twelve participants. Each group was led by two graduate students who were trained by the principal investigators, CAR and CW. In total, 27 trainers conducted one or more of the 22 training groups that took place between January 2023 and June 2023.

The training consisted of two parts. The first 4 training weeks concentrated on the development of emotional competences. Participants learned about stress synthesis, resilience, as well as attention and emotion regulation through psychoeducation, self-reflection, and various practical exercises (e.g., body awareness, meditation, noticing of stressful emotions). Based on this knowledge and while continuing the practical exercises, the next 4 weeks concentrated on strengthening social competences. Participants were educated on systemic perspectives on social dynamics, and practiced new behavior in fictious videotaped role plays with the trainers. In-between training sessions, participants were asked to continue learning and practicing in their daily lives by themselves and with a training buddy, via self-reflection, guided audio exercises, and behavioral tasks (e.g.,

practicing new and/or challenging social behavior). For more detailed information on the training structure and content see Borgdorf et al. (2025a).

4.2.4 Sample Size

In consideration of feasibility issues with conducting an RCT with a training and a waitlist control group, as well as different research questions demanding two age groups (N = 110 young adults, 18–33 years, N = 110 older adults, 55+ years), power analyses ($1-\beta = .80$, $\alpha = .05$) suggested that 220 participants would be needed to find medium and large effect sizes. As mentioned, 203 participants enrolled in the training, and 166 participants finished at least four out of eight training sessions.

4.2.5 Measures

All trait questionnaires were administered before (T1), during (T2), and after the training (T3). Data from the follow-up timepoints, 3 and 12 months (T4 and T5, respectively) after the end of the training, were not included in the current analyses. Data on weekly mindfulness were collected 4 to 6 days after each of the eight training modules.

State Mindfulness was measured with the Multidimensional State Mindfulness Questionnaire (MSMQ; Blanke & Brose, 2017). The questionnaire measures three facets of state mindfulness: Acting with Awareness, Nonjudgmental Acceptance, and Present-Moment Attention. The 9 items were answered on a 6-point Likert scale ranging from *does not apply at all* to *applies strongly* and showed good reliability in this sample ($\omega = .74 - .82$).

Trait Mindfulness was assessed with the Five Facet Mindfulness Questionnaire (FFMQ; Michalak et al., 2016), measuring the five facets Describe, Observe, Act with Awareness, Nonjudge (8 items each), and Nonreact (7 items) with a total of 39 items on a 5-point Likert scale ranging from *never or very rarely true* to *very often or always true*. Reliability in the current sample was good ($\omega = .90 - .92$).

Self-Compassion was assessed with the short version of the Self-Compassion Scale (SCS; Hupfeld & Ruffieux, 2011). The scale consists of 12 items that measure the six dimensions of self-compassion (two items per dimension; positive: Mindfulness, Self-Kindness, Common Humanity; negative: Overidentification, Self-Criticism, Isolation) on a 5-point Likert scale ranging from *almost never* to *almost always*. McDonald's ω indicated good reliability of the scale in the current sample ($\omega = .87 - .91$).

Perceived Stress was assessed with the 10-item Perceived Stress Scale (PSS; Schneider et al., 2020) on a 5-point Likert scale ranging from *never* to *very often*. The scale measures the perceived capability to deal with stressors of the past two weeks and to control stressful events and showed good reliability in this sample ($\omega = .84 - .91$).

4.2.6 Data Analyses

Data were analyzed with 4.3.1 (R Core Team, 2023; see Supplemental Material for used R packages) and MPlus, version 8.6 (Muthén & Muthén, 1998-2017). In sum, 10.8% of data were missing (1,379 out of 11,403 data points). Moreover, we detected 16 outliers (out of 237,319 data points) and winsorized them to $M \pm 3$ *SD* for further analyses.

To test for the increases in state mindfulness, we calculated multilevel analyses with time as a Level 1 random within-person predictor (coded from 0 to 7). We included age group (0 = younger, 1 = older adults) as a grand-mean centered Level 2 predictor, given that other research questions required these two age groups (see Küchler et al., 2025a). By including a cross-level interaction between Time x Age Group we examined whether state mindfulness developed differently between older (= 0) and younger adults (= 1).

To test Hypotheses 2a, 2b, and 2c, whether state mindfulness predicted trait change, we first fitted second-order latent growth models to state mindfulness and each trait variable separately (see minor deviation in analysis strategy from preregistered analyses in Supplementary Table S6). These models should replicate findings from the multilevel analyses conducted to test H1 and for the trait increases as found in Borgdorf et al. (2025a). In a second step, we regressed the latent trait slope on the latent state slope (see Figure 4.2; exemplarily for state and trait mindfulness). For the trait slope, loadings were set to 0, 4, and 8 to reflect the approximate 4-week intervals between the three timepoints. Loadings for the state slope were set to 1 through 8 across the eight weekly assessments. We slightly changed the time coding in order to depict the time delay in trait and state questionnaires. Results with a coding from 0 through 7 did not change results. To examine whether the state-trait relationship differed with age, we extended the latter models with age group as a moderator in the relationship and regressed the respective trait slope on the interaction of state slope with age group.

Latent growth analyses were estimated with Bayes estimator with 20.000 iterations (10.000 iterations in the extended age model), because estimation with MLR for state mindfulness and combined models did not converge, potentially because of high complexity and a comparatively small sample size of these models. Default, non-informative priors were used with two chains (Markov Chain Monte Carlo with Gibbs sampler), in which the first half is considered burn-in (Muthén & Asparouhov, 2012). Convergence was determined by evaluating trace plots and examining the Potential Scale Reduction Factor (PSRF), with values > 1.1 indicating convergence (Gelman & Rubin, 1992; Muthén & Asparouhov, 2012). Convergence was double-checked with a higher number of iterations (Muthén &

Asparouhov, 2012). Estimates and PSRF with higher iterations did not essentially differ. The analysis provides point estimates and 95% credibility intervals (CI) of the posterior distribution. Effects are considered significant if the CI does not include zero.

In all models, mindfulness, self-compassion, and perceived stress were modeled as latent variables. Items were parceled according to the respective subscales of the three trait measures (e.g., Describe, Observe, Nonjudge, Nonreact, and Act with Awareness for the FFMQ), which then loaded on the respective latent trait. All models included indicator-specific method factors, uncorrelated with all other variables, which offer more reliable estimations than correlated residuals (Geiser, 2011). Based on theoretical and data-driven (i.e., sensitivity analyses) considerations, the facets Nonreact (FFMQ), Self-Kindness (SCS), and Self-Efficacy (PSS) were chosen as reference factors, respectively (Geiser, 2011). Strong and partial strong measurement invariance was separately determined before calculating all final models (Chen, 2007; Geiser, 2011; see Supplementary Table S7). Overall, model fit for all models was evaluated using the standard cutoff criteria in psychological research (Hu & Bentler, 1999), where comparative fit index (CFI) values ≥ 0.90 and root-mean-square error of approximation (RMSEA) values ≤ 0.08 indicate an acceptable fit.

4.3 Results

The SECT was conducted in an in-person format over 8 consecutive weeks and was carried out in three training cohorts, starting in January, April, and June 2023, respectively. The training focused on emotional stability (weeks 1-4: stress synthesis, resilience, attention- and emotion-regulation) and social skills (weeks 5-7: social dynamics, behavioral training), concluding with a wrap-up session in week 8 (for an overview of the training content see Borgdorf et al., 2025a). The study was designed as a randomized controlled trial, with two waitlist control groups participating in the training in the second or third cohort, respectively. State mindfulness was, however, only assessed during the active training period. Therefore, analyses focused on longitudinal changes. Participants completed self-report trait measures (mindfulness, self-compassion, and perceived stress) before, 4 weeks into, and after the training. State mindfulness was assessed weekly, approximately 5 days after each training module. Data were analyzed using multilevel models and second-order latent growth models to examine within-person and between-person effects and correlated change over time. Due to other research questions in the same project regarding age differences in socioemotional development, our sample consisted of younger and older adults. Thus, age group was included as a moderating factor (see Method section for more details).

4.3.1 Sociodemographic Information

In total, 166 adults (M_{Age} = 46.3, SD_{Age} = 18.7) participated in the training, of whom 75.2% were female. A majority of the 80 younger (M_{Age} = 28.16, SD_{Age} = 4.70, range = 19-42 years) and 84 older adults (n = 84; M_{Age} = 63.55, SD_{Age} = 7.20, range 50-78 years) indicated holding a university degree as their highest educational achievement (63.0%). Almost all younger participants were either studying (55.6%) or employed (37.0%), whereas most older adults were either employed (46.4%) or retired (42.9%). Approximately half of the sample (50.3%) had prior meditation experience, of which 60.5% did not meditate at all or only infrequently (ranging from *not at all* to *once a month*), and 39.5% meditated at least *several times a month* to *daily*. Further sociodemographic information can be found in Supplementary Table S1. On average, the 166 participants completed 6.88 training sessions (SD = 0.89) and 7.18 weekly questionnaires (SD = 1.40). However, they did not provide information at all timepoints: In total, 160 participants filled out questionnaires at T1, 151 at T2, and 141 at T3. Participants who started in different cohorts did not differ on important baseline characteristics (all ps > .05). Moreover, participants who dropped out between T1 and T3 did not differ from those who completed the training (all ps > .05; see Table S2 and Borgdorf et al., 2025a, for further details).

4.3.2 Increases in State and Trait Variables During the Intervention

Figure 4.1 shows the trajectories of state mindfulness, trait mindfulness, trait self-compassion, and trait perceived stress overall and on facet level. Descriptive statistics, reliabilities, and bivariate correlations are depicted in Supplementary Table S3.

State changes of mindfulness were analyzed with a multilevel model with time as a Level 1 random within-person predictor and age group (0 = younger, 1 = older adults) as a grand-mean centered Level 2 predictor. In line with H1, results indicate that state mindfulness significantly increased over time (b = 0.05, 95% CI [0.04; 0.07]). Age did not moderate this effect (b = −0.01, 95% CI [−0.03; 0.03]).

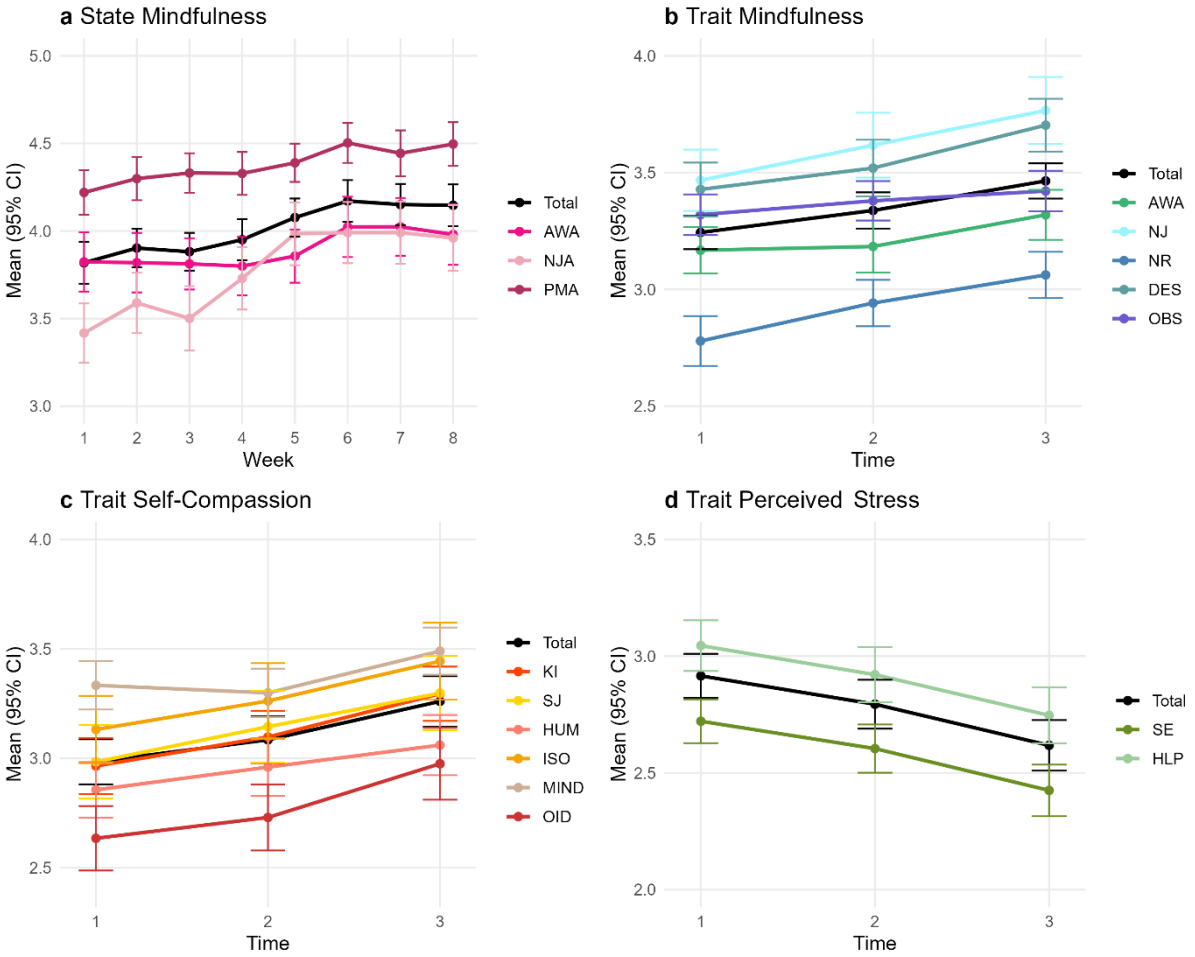
In preparation for testing H2a, H2b, and H2c, we first fit a second-order latent growth model to state mindfulness (see Figure 4.2 lower part). The results of this model with strong measurement invariance (CFI = 1.00; RMSEA = 0.004, 90% CI [0.00; 0.024]) replicated the results of the multilevel model: State mindfulness significantly increased over the training course (M_{slope} = 0.03, 95% CI [0.01; 0.06]), with different change rates between participants (Var_{slope} = 0.01, 95% CI [0.004; 0.01]; see further details in Supplementary Table S4).

In the next step, we calculated separate second-order latent growth model to examine trait change during the training (see Figure 4.2 upper part, exemplarily for trait mindfulness;

and Supplementary Table S4). Model fit with strong measurement invariance for trait mindfulness was good (CFI = 0.99; RMSEA = 0.04, 90% CI [0.02; 0.05]). Results indicate a significant increase in trait mindfulness over the timepoints (M_{slope}= 0.14, 95% CI [0.10; 0.18]) at different change rates (Var_{slope} = 0.02, 95% CI [0.003; 0.04]). Similarly, trait self-compassion significantly increased throughout training participation (M_{slope} = 0.15, 95% CI [0.09; 0.20]; Var_{slope} = 0.03, 95% CI [0.002; 0.06]). However, we could only establish partial strong measurement invariance (Chen, 2007) for self-compassion with the subfacet Isolation being allowed to vary over time. The subfacets suggest considerable heterogeneity and hence we caution with regards to model interpretation. Last, results of the single latent growth model for perceived stress (strong measurement invariance; CFI = 1.00; RMSEA = 0.00, 90% CI [0.00; 0.04]) suggest that overall means significantly decreased over the timepoints at differing rates (M_{slope} = −0.14, 95% CI [−0.19; −0.09]; Var_{slope} = 0.03, 95% CI [0.05; 0.07]). Detailed results on trait change during the training can be found in Supplementary Table S4 and in Borgdorf et al. (2025a).

Figure 4.1

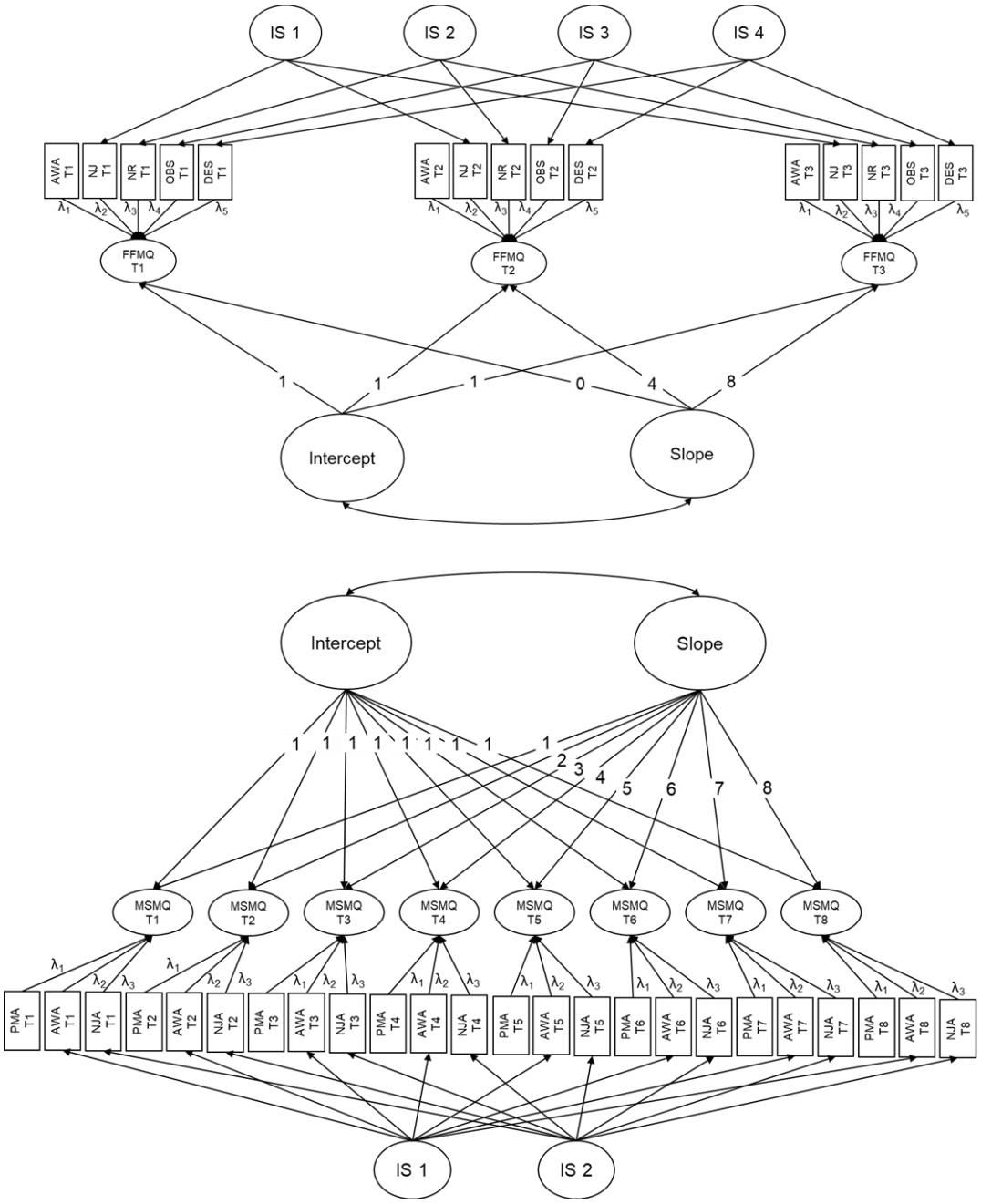
Mean Trajectories of State Mindfulness, Trait Mindfulness, Trait Self-Compassion, and Trait Perceived Stress



Note. Trajectories of state mindfulness (a), trait mindfulness (b), trait self-compassion (c), and trait perceived stress (d), total mean scores and subscales with 95% Confidence Interval (CI). AWA = acting with awareness, NJA = nonjudgmental acceptance, PMA = present-moment attention; NJ = nonjudge; NR = nonreact; DES = describe; OBS = observe; KI = self-kindness; SJ = self-judgment; HUM = common humanity; ISO = isolation; MIND = mindfulness, OID = overidentification; SE = self-efficacy (reverse coded); HLP = helplessness.

Figure 4.2

Second-Order Latent Growth Models: State Mindfulness Trajectories Predict Trait Mindfulness Change



Note. Second-order latent growth model for state mindfulness (lower half; MSMQ) and trait mindfulness (upper half; FFMQ). Residuals terms are not depicted. Loadings on indicator-specific method factor are not depicted and restrained to 1. λ = time invariant state factor loading. MSMQ = Multidimensional State Mindfulness Questionnaire; FFMQ = Five-Facet Mindfulness Questionnaire; AWA = acting with awareness, NJA = nonjudgmental acceptance, PMA = present-moment attention; NJ = nonjudge; NR = nonreact; DES = describe; OBS = observe; IS = indicator specific method factor.

4.3.3 Correlated Changes of State Mindfulness and Trait Mindfulness, Trait Self-Compassion, and Perceived Stress

With regards to the combined second-order latent growth models for state and trait mindfulness, model fit estimates suggested acceptable fit (CFI = 0.94; RMSEA = 0.04, 90% CI [0.04; 0.05]). Contrary to H2a, results indicate that increases in state mindfulness did not predict the increases in trait mindfulness (b = 0.04, 95% CI [−0.12; 0.22]). Age did not moderate the state-trait relationship (b = −0.03, 95% CI [−0.52; 0.41]; see Supplementary Table S5). Similarly, and contrary to H2b, changes in state mindfulness did not predict the increases in trait self-compassion (b = 0.13, 95% CI [−0.10; 0.38]). The model fit for the combined second-order latent growth model was excellent (CFI = 0.99; RMSEA = 0.02, 90% CI [0.00; 0.024]). The correlated change between state mindfulness and trait self-compassion did not differ between younger or older adults (b = 0.17, 95% CI [−0.30; 0.68]; see Supplementary Table S5). And last, contrary to H2c, the results of the combined second-order latent growth model with excellent fit (CFI = 0.98; RMSEA = 0.03, 90% CI [0.02; 0.03]) suggest that changes in state mindfulness did not predict decreases in perceived stress either (b = −0.11, 95% CI [−0.34; 0.12]). Again, the association in changes of state and trait was not moderated by age (b = −0.07, 95% CI [−0.55; 0.49]; see Supplementary Table S5).

4.4 Discussion

This longitudinal study provides novel insights into the complex relationship between state trajectories of mindfulness with trait indicators of mindfulness, self-compassion, and perceived stress. The current findings extend previous research and theory by innovatively examining these state-trait associations in an 8-week Socioemotional Competence Training (SECT). More specifically, we replicated the previous finding that a psychological intervention without explicit mindfulness label led to increases in trait mindfulness (cf. Goldberg et al., 2019). Additionally, the results of the current longitudinal study suggest that participation in the SECT led to significant improvements in state mindfulness, trait self-compassion, and trait perceived stress (cf. Borgdorf et al., 2025a). Surprisingly, and contrary to our expectations, changes in state mindfulness did not predict changes in any of the traits. At a first glance, these findings contradict previous theoretical assumptions and former empirical findings from explicit MBIs (Baer, Carmody, & Hunsinger, 2012; Goldberg, 2022; J. Gu et al., 2015; Kiken et al., 2015; Warren et al., 2023). However, our results may simply suggest a more nuanced state-trait relationship bearing important implications for both MBIs and other psychological interventions.

This study conveys a critical insight: Increases in state mindfulness alone appear insufficient to drive changes in trait mindfulness and other trait outcomes when the intervention is not explicitly mindfulness-based. Although trait mindfulness increased in the current study, this occurred without the significant state-trait association previously observed in traditional MBIs (Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015; Warren et al., 2023). Yet, this pattern aligns with previous findings from personality intervention and psychotherapy research, where sometimes significant state-trait associations occurred (Küchler et al., 2025a, 2025b; Olaru et al., 2024), while at other times these could not be observed (Küchler et al., 2025a, 2025b; Quintus et al., 2021; Zilcha-Mano, 2021). Taken together, the inconsistent result patterns suggest that the state-trait pathway depends on mediating conditions, such as common and specific change mechanisms.

The current findings contribute to the ongoing debate about common and specific change mechanisms across different intervention types (Fresco & Mennin, 2019; Goldberg et al., 2022; Zilcha-Mano, 2021). Traditional MBIs seem to contain specific elements beyond mindfulness exercises that strengthen state-trait associations found previously. Although the SECT included mindfulness practices (e.g., attention to the body or breath), these were not embedded in a mindfulness psychoeducational context and required substantially less daily practice time than traditional MBIs (10-20min vs. 45min daily in traditional MBIs; Goldberg et al., 2019). According to theoretical assumptions of the WTT, the repeated experience of more mindfulness in daily life should result in trait change over time (Warren et al., 2023). However, the reduced dose in the SECT may not have generated sufficient state mindfulness experiences to reach a threshold for meaningful state-trait association. Previous research shows that the outcome is reciprocally linked with practice time (Palmer et al., 2023), and that a perceived positive outcome may be a better predictor of longer practice times than vice versa (Goldberg et al., 2020). Hence, especially without specific mindfulness framing of the exercises, participants may have prioritized other SECT exercises—potentially because they were more obviously aligned with the SECT goals or because they were perceived as more beneficial. This could also explain the trait improvements in downstream outcomes, that is self-compassion and perceived stress. In addition to practice time, trainer characteristics likely influence state-trait associations. SECT teachers did not have to follow a mindfulness practice of their own, as often required from MBSR teachers (Kabat-Zinn, 2013). Yet, trainers' embodiment of mindfulness principles may facilitate participants' recognition of state-trait connections through explicit instruction and implicit modeling.

Trait improvements in the SECT may have occurred through alternative pathways, such as increased emotional stability, or engagement in broader reflective or other self-regulatory processes (J. Gu et al., 2015; Küchler et al., 2025a), This aligns with perspectives on general, pan-theoretical change mechanism in psychotherapy, which suggest that increased differentiation between external triggers and affective reactions represents a common therapeutic mechanism (Sønderland et al., 2024). Mindfulness can be one means to create this emotional space between a trigger and the response to it (Bernstein et al., 2015; Ludwig et al., 2020) but may exert these effects only through explicit instruction in MBI contexts. Hence, we argue that state mindfulness changes may be necessary but not sufficient for trait mindfulness development and related variables such as trait self-compassion and perceived stress, particularly in the absence of mindfulness-specific elements that characterize traditional MBIs.

Another intriguing thought comes from personality change research, which suggests that explicit awareness and reflection of behavioral (state) changes and their attribution to aggregated self-perceptions may be central to translating momentary experiences into lasting trait-level change (Küchler et al., 2025a, 2025b; Wrzus & Roberts, 2017). Mindfulness may facilitate the awareness part, but conscious reflection may need more instruction or explicit framing. Given that the SECT included mindfulness exercises but did not provide a comprehensive contemplative framework, participants may have attributed state mindfulness fluctuations to momentary and contextual influences rather than enduring trait change. Additionally, state and trait changes may not occur for all individuals equally: Some participants may show behavioral (state) changes without self-concept integration while others may exhibit self-concept changes without obvious behavioral adaptation. How to measure and overcome the informational loss in state-trait translations during an intervention—and whether it affects the size, stability, or the duration of intervention effects—remains an intriguing question for future research.

Our results may also point toward expectancy effects. Participant expectations and intentions shape both subjective experiences and objective outcomes (Krämer et al., 2025; Suelmann et al., 2018; Zilcha-Mano et al., 2019; Zilcha-Mano, 2021), potentially explaining why state-trait relationships differ between intervention types despite similar directional improvements. These expectations are, among others, created by contextual framing of exercises and program structure (see above; Krämer et al., 2025; Warren et al., 2023). While participants in MBIs likely anticipate improvements in mindfulness and reductions in stress—for example when participating in MBSR—the SECT addressed individuals seeking to become more relaxed in face of daily stress and to handle challenging social situations more calmly.

Participants in MBIs may experience greater trait changes specifically because they anticipate mindfulness benefits, receive explicit framing about mindfulness mechanisms, and are instructed to attribute their state experiences to broader trait changes (cf. Wenzel et al., 2024). In contrast, the mindfulness exercises incorporated in the SECT were presented as (attention and emotion) regulation techniques rather than mindfulness training. This expectancy divergence is reflected in the effect sizes, which are medium in size but still considerably smaller (d = 0.62 for mindfulness, d = −0.51 for perceived stress) than those reported in traditional MBIs (up to d = 1.14 for trait mindfulness and d = −0.64 to −1.04 for perceived stress; Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015).

4.4.1 Limitation and Future Research

Our findings should be considered in light of important limitations. First, measuring state mindfulness retrospectively over one week has been criticized as lacking the fine-grained resolution necessary for studying momentary fluctuations (Blanke & Brose, 2017), potentially representing trait-like rather than state characteristics (cf. Warren et al., 2023). Second, reliance on self-report measures with their typical short-comings as well as using different instruments for state (Multidimensional State Mindfulness Questionnaire; Blanke & Brose, 2017) versus trait mindfulness (Five-Facet Mindfulness Questionnaire; Baer et al., 2006), potentially measuring distinct constructs, may limit our ability to capture true state-trait associations. Yet, our study was the first to use two validated measures in their intended purpose, whereas previous research relied on adapted trait measures or ad-hoc designed questions (e.g., Baer, Carmody, & Hunsinger, 2012; Carmody et al., 2008; Kiken et al., 2015; Warren et al., 2023). Future research could employ more fine-grained measures to better understand state-trait relationships in mindfulness interventions, for example, by using validated momentary assessments in ecological momentary assessment periods during the intervention or neurobiological indicators of mindfulness. However, repeatedly measuring momentary state mindfulness may be an intervention in itself, reminding participants of their intention to be non-judgmentally aware of the present-moment (Suelmann et al., 2018).

Third, we did not include measures of weekly changes in self-compassion or perceived stress. It would be interesting to examine reciprocal associations of these trajectories in future research, in both explicit (compassion or stress-reduction) interventions and broad psychological interventions such as the SECT, in order to test whether the current result patterns generally replicate.

Last, due to the design of an in-person psychological intervention, although spanning a large age range, our sample was mostly female, highly educated, and with a comparatively high

socio-economic status. Future research is needed to determine whether the current results extend to more heterogenous, minority, or clinical populations, ideally with additional measures, such as observer reports or (neuro-)biological markers of state and trait variables.

The current findings suggest that mindfulness-based practices can be integrated into a Socioemotional Competence Training, but they may not exert the same effects as in explicit MBIs due to person, context, and process specific and common change mechanisms. Trait changes occurred even without a strong state-trait association, which suggests a complex relationship between state and trait variables in this study, and potentially in psychological interventions using mindfulness techniques without explicitly labeling them more generally. Replication of these findings with a larger and more heterogeneous sample, in other psychological interventions without explicit mindfulness labeling, and potentially different variables may make the assumptions drawn here more reliable.

Chapter 5: Less Frequent but Equally Useful: Social and Temporal Comparisons in Light of Mindfulness and Self-Compassion

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Abstract

Objectives Social and temporal comparisons are ubiquitous and considered important sources of an individual's self-knowledge. Yet, comparisons are inherently evaluative and may result in negative affective consequences. In contrast, mindfulness and self-compassion are characterized by a present-moment, non-evaluative, and accepting stance toward the self and one's experiences and are associated with numerous positive psychological health indicators. This study innovatively examined the associations of mindfulness and self-compassion with frequency and perceived utility of social (i.e., with others) and past-temporal (i.e., with the past self) comparisons across different life domains.

Method In a binational online study, we examined the associations between mindfulness, self-compassion, and comparisons across five different domains (i.e., extraversion, emotional stability, appearance, professional success, and private life) in a gender- and age-diverse sample at two measurement points 6 months apart (NT1 = 615, NT2 = 310, 18–84 years, 51.5% female).

Results Results of multiple regression analyses indicated that the more mindful or self-compassionate individuals were, the less they compared themselves with others or their past self (across domains $\beta = -0.25$ to -0.51; all p-values < 0.001). Contrary to our expectations, the overall pattern of results suggests that mindfulness and self-compassion were generally not significantly associated with perceiving comparisons as less useful (after family-wise error correction; $\beta = -0.04$ to -0.14; all p-values ≥ 0.008).

Conclusions The findings suggest that comparisons, when done mindfully and self-compassionately, can be a valuable source of information for self-knowledge. At the same time, mindfulness and self-compassion may buffer against negative affective outcomes of comparison processes.

Preregistration This study is preregistered on the Open Science Framework: https://osf.io/6hfb2.

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5.1 Introduction

As humans, we frequently compare ourselves with others or with past versions of ourselves across a range of dimensions, for example, how successful we are compared to our colleagues or how athletic we were (or were not) in younger years. While there are many standards individuals can compare to, social comparisons (i.e., with others) and past-temporal comparisons (i.e., with the past self) are among the most prevalent standards people use for comparisons (Morina, 2021). Moreover, comparisons can occur with regard to nearly every human characteristic. Common dimensions along which people tend to compare themselves include professional performance, physical appearance, personality traits, and their personal life (Wheeler & Miyake, 1992; A. E. Wilson & Ross, 2000).

Comparisons are ubiquitous and considered to fulfill the fundamental need for self-knowledge (Buunk & Gibbons, 2007; Festinger, 1954), serving as "reference frames in evaluating attributes that constitute the self" (Morina, 2021, p. 1281). Moreover, research suggests that self-assessment, self-improvement, or self-enhancement motives may be other important drivers for comparisons (Morina, 2021; Sedikides & Strube, 1997; Wayment & Taylor, 1995).

Accordingly, comparisons are evaluative in nature (Gerber et al., 2018): Individuals use comparisons to determine whether someone else or a past version of oneself is relatively "better," "equal," or "worse" (Morina, 2021; A. E. Wilson & Ross, 2000). Some comparisons result in beneficial outcomes (Gerber et al., 2018). For example, individuals may use downward comparisons (i.e., to worse-off comparison standards) to feel better about themselves (e.g. "I am more athletic than I used to be", i.e. self-enhancement; Wayment & Taylor, 1995). Or they may compare to better-off standards (e.g., comparing to a more successful colleague) in order to initiate self-improvement motivation (Morina, 2021; Wayment & Taylor, 1995). Yet, results from a recent ecological momentary intervention study suggest that the positive effects of upward social comparison on motivation are of rather short duration and negative consequences prevail, such as negative affect and lower goal approach (Diel et al., 2024).

Comparisons often happen automatically or involuntarily (Buunk & Gibbons, 2007; D. Gilbert et al., 1995), and the adequacy of comparison outcomes is rarely questioned or contextualized (Djikic & Langer, 2007). This may substantially impact an individual's self-concept (Morina, 2021): Two recent meta-analyses indicate that most social comparisons are upward, result in less favorable self-evaluations, and are therefore accompanied by negative consequences such as negative emotions or a deflated sense of self-worth (Gerber et al., 2018;

McComb et al., 2023). Similarly, upward temporal comparisons led to less positive self-concepts in students (Vogel et al., 2020; Wolff et al., 2018), and adolescents (Gürel et al., 2022).

Given these findings, two critical questions arise: First, can individuals consciously control their use of comparative information, especially when comparisons often occur automatically and involuntarily outside experimental contexts? And second, can individuals deliberately deploy comparisons to achieve neutral or even beneficial outcomes, or at least gain accurate self-knowledge as Festinger (1954) suggested 70 years ago?

One promising approach to addressing these questions may lie in the research on mindfulness and self-compassion. Potentially, mindfulness and self-compassion allow individuals to consciously choose accurate information for self-evaluation, acquire self-knowledge, and inspire self-improvement, and autonomous motivation more sustainably (Breines & Chen, 2012; E. Carlson, 2013; Djikic & Langer, 2007; Donald et al., 2020; Leary et al., 2007). Moreover, mindfulness and self-compassion may enable individuals to more consciously decide when to consider or disregard comparison information, to discern which comparison information is useful, and how to deal with, and integrate comparison information into their self-evaluation.

Contrary to the evaluative nature of comparisons, mindfulness is inherently non-evaluative. While there is an ongoing debate on one single definition, most researchers agree that mindfulness contains two main aspects: (i) attention and awareness of the present moment and (ii) a non-judgmental, accepting quality about the nature of this attention (van Dam et al., 2018). Mindfulness is closely linked with self-compassion, but while mindfulness entails an impartial focus on the present moment, self-compassion explicitly promotes a benevolent focus on the self, especially regarding perceived flaws (Neff, 2003a). Both, mindfulness and self-compassion, and practices aimed at enhancing them, have been associated with numerous positive psychological, cognitive, emotional, and behavioral outcomes (e.g., Farb et al., 2014; Ferrari et al., 2019; Keng et al., 2011; Neff, 2023; Singer & Engert, 2019; Tomlinson et al., 2018; van Vugt, 2015; Zessin et al., 2015).

Research on the association between mindfulness, self-compassion, and comparisons is relatively scarce, with most studies focusing on *social* comparisons. These studies suggest that mindfulness and self-compassion are negatively associated with social comparison orientation (Egan et al., 2021; C. Gu et al., 2022; Neff & Vonk, 2009). No research has examined the association with *temporal* comparisons, but it seems reasonable to assume that the findings for social comparisons extend to temporal comparisons as well, given that all comparisons are

evaluative and may consequently lead to negative affective reactions (Buunk & Gibbons, 2007; Wolff et al., 2018).

Several mechanisms of mindfulness and self-compassion may facilitate different processing of self-evaluative information and therefore explain the negative association. First, a mindful and self-compassionate awareness enables individuals to quickly and consciously notice arising internal thoughts and emotions, allowing for quicker and more comprehensive information processing (E. Carlson, 2013). Second, this awareness creates a space between a stimulus (e.g., a sensation, thought, or action of another person) and an individual's response to it (Bernstein et al., 2015; Ludwig et al., 2020; Vago & Silbersweig, 2012). This allows individuals to consciously decide *how* to evaluate and respond to their experience rather than reacting automatically (cf. Teper et al., 2013). Third, mindfulness and self-compassion may generally alter how evaluative information is perceived, selected, and processed. According to the mindful social comparison theory (Djikic & Langer, 2007), mindful comparisons, as opposed to automatic, mindless comparisons, encourage an understanding of one's own and others' thoughts and behaviors as complex, context dependent, and evolving. Moreover, mindful and self-compassionate individuals potentially process information less defensively (Djikic & Langer, 2007; Terry & Leary, 2011), promoting a healthy self-focus, which is central to acquiring self-knowledge (E. Carlson, 2013; Morin, 2017).

Numerous empirical studies support these mechanisms, suggesting that mindfulness and self-compassion are associated with increased awareness and cognitive-regulation capacities, such as executive cognitive control (Cásedas et al., 2020; Ferrari et al., 2019; Finlay-Jones, 2017; Im et al., 2021; Tang et al., 2015; van Vugt, 2015). They are also linked to improved emotion- and self-regulation (Ferrari et al., 2019; Finlay-Jones, 2017; Tang et al., 2015), including less emotional lability, higher emotional differentiation (Hill & Updegraff, 2012), improved negative affect regulation (Leyland et al., 2019; Yip & Tong, 2021), and reduced repetitive negative thinking after negative performance feedback (Blackie & Kocovski, 2018, 2019). And last, individuals with higher levels of mindfulness and self-compassion evaluated themselves more positively and accurately during self- and performance evaluations (Langer et al., 2010; Leary et al., 2007), and could better regulate feelings of embarrassment, humiliation, guilt, and failure (Leary et al., 2007).

In the context of self-knowledge and self-evaluation, the above mentioned mechanisms and related empirical findings bear important implications for comparison processes. First, the higher availability of processed information in individuals with higher levels of mindfulness and self-compassion should reduce the number of comparisons, simply because more and

different information is available in the self-evaluation process (E. Carlson, 2013; Djikic & Langer, 2007; Morin, 2017). Second, the created space facilitates a more conscious evaluation of the comparison information. If individuals with higher levels of mindfulness and self-compassion deem a comparison information relevant or useful, they may continue the comparison process. If a comparison information is categorized as irrelevant, unhelpful, or even detrimental, the comparison process may be terminated. This is in line with research showing that higher mindfulness was associated with less frequent use of upward social comparisons in the context of social media usage (C. Gu et al., 2022), and with results showing that self-compassion seems to buffer the negative effects of social comparisons on the self-image (e.g., Egan et al., 2021; Seekis et al., 2020). Potentially, more mindful and self-compassionate individuals are more likely to identify and stop unhelpful comparisons. This should, again, result in fewer comparisons overall, as well as less perceived utility of comparisons. Last, a mindful, self-compassionate approach to self-evaluation is potentially associated with less negative affective consequences relative to typical comparison outcomes in self-evaluation (Donald et al., 2020; Gerber et al., 2018; Leary et al., 2007; Ludwig et al., 2020). Consequently, mindfulness and self-compassion may be associated with more neutral or better comparison outcomes. Additionally, mindful and self-compassionate individuals may come to better comparison outcomes, because they can better deal with negative comparison outcomes (i.e., less defensive processing and better emotion regulation). That means that these individuals interpret and integrate threatening comparison information differently, resulting in neutral or positive self-evaluations.

To our knowledge, no studies have investigated yet the associations of mindfulness or self-compassion with comparison characteristics such as comparison frequency, perceived comparison utility, and perceived comparison outcome. In the present research, we focused on social and temporal comparisons across five dimensions (i.e., professional performance, physical appearance, two personality traits, and personal life), because these standards and dimensions are among the most common forms (Morina, 2021; Wheeler & Miyake, 1992; A. E. Wilson & Ross, 2000). Based on the reviewed literature, we derived the following three assumptions: First, we assumed that individuals higher in mindfulness and self-compassion may show an overall decreased tendency to engage in comparisons. They have access to more and different information for self-evaluation. Tentatively, we supposed that this relationship is stronger for social comparisons. This is, because temporal comparisons have an inherent self-focus, similar to mindfulness and self-compassion (E. Carlson, 2013; Neff, 2003a), and inform individuals on their developmental progress (A. E. Wilson & Ross, 2001), making them more

readily accessible to the individual. Therefore, we proposed the following two hypotheses: In Hypothesis 1 (H1), we assumed that higher mindfulness is associated with less frequent use of comparisons overall and less frequent use of social comparisons compared with temporal comparisons, both cross-sectionally and longitudinally. In Hypothesis 2 (H2), we suggested that higher self-compassion is associated with less frequent use of comparisons overall and less frequent use of social comparisons compared with temporal comparisons, both cross-sectionally and longitudinally.

The second assumption was that individuals higher in mindfulness and self-compassion may perceive comparisons as less useful for acquiring self-knowledge. They may be more aware of the pitfalls of comparisons, and disregard the comparison information or seek alternative sources to attain an accurate self-understanding. We had no assumptions on differences between the perceived utility of social relative to temporal comparisons. Although individuals with higher levels of mindfulness and self-compassion may compare themselves to their past self more frequently, their orientation toward the present moment and acceptance of past experiences as immutable may reduce the perceived utility of these comparisons for their current self-evaluation. Therefore, we suggested the following hypotheses: In Hypothesis 3 (H3), we assumed that higher mindfulness is associated with less perceived utility of comparisons, both cross-sectionally and longitudinally. In Hypothesis 4 (H4), we suggested that higher self-compassion is associated with less perceived utility of comparisons, both cross-sectionally and longitudinally.

Lastly, we assumed that individuals higher in mindfulness and self-compassion may contextualize comparison information more flexibly and therefore come to less detrimental outcomes. Alternatively, these individuals may more easily tolerate and regulate negative affect following comparisons, leading to more neutral or positive self-evaluations. In addition to the preregistered primary hypotheses (H1-H4; see https://osf.io/6hfb2; see deviations from the preregistration in Table S1 in the Supplementary Information), we therefore analyzed, in an exploratory manner, how mindfulness and self-compassion relate to the perceived comparison outcome.

5.2 Method

5.2.1 Participants and Procedure

Power analyses ($1-\beta = 0.90$, $\alpha = 0.05$) indicated that we needed 108 participants to detect an effect size of at least $f^2 = 0.1$. However, data were collected within a larger research project, and power analyses were based on research questions that required structural equation modeling for interaction effects. We therefore aimed for a sample of 330 participants, which would allow

us to detect a small effect size (f^2 = 0.03) for our purposes. For purposes of generalizability beyond single country studies, we collected data from 330 German participants and 330 U.S. participants, respectively. Participants' data from both countries were combined for the present study's purpose. The data set, analysis code, and study materials are available at https://osf.io/f6qc8/. Along with the hypotheses, exclusion criteria, measures, analyses, and inclusion of control variables were preregistered on the OSF: https://osf.io/6hfb2. The study was approved by the Ethics Committee of the Faculty of Behavioural and Cultural Studies at Heidelberg University, Germany (Wrzus 2019 1/1).

Between August 2021 and May 2022, we collected self-report data at two time points 6 months apart from a German and a U.S. sample on the online platform Clickworker. Participants received monetary compensation for their participation (up to €13.00, approximately 15.05 USD). We aimed for an age- and gender-diverse sample: We created five age groups (18‑30 years, 31‑44 years, 45‑58 years, 59‑72 years, and 73‑86 years) in the German sample, and four age groups in the U.S. sample (the last group included participants between 59‑75 years). We did this separately for people who identified as either female or male which resulted in a total of 10 and 8 strata, respectively, and a total of 648 participants. These participants gave informed consent, provided sociodemographic information, and filled out the study questionnaires. After data collection, we thoroughly checked data quality. We excluded participants with suspicious response patterns as indicated by (unrealistic) speediness (< 15 min), longstrings (> 10), failed attention checks, and obvious misunderstanding or neglect of instructions. This resulted in an exclusion of 14 participants in the German sample and 19 participants in the U.S. sample.

The final sample at Time 1 (T1) consisted of 615 participants ($n_{Germany}$ = 313 and n_{USA} = 302; M_{Age} = 42.3, SD_{Age} = 14.5, 52% female). Six months after T1, we invited the participants to fill out the same questionnaires again. At Time 2 (T2), after applying the same exclusion criteria, 310 participants provided self-report data ($n_{Germany}$ = 229, 26.84% dropout; n_{USA} = 81, 73.18% dropout; M_{Age} = 45.1, SD_{Age} = 13.9; 53% female). Sample descriptives can be found in Table 5.1.

5.2.2 Measures

Mindfulness was measured with the 39-item Five Facet Mindfulness Questionnaire (FFMQ; Baer et al., 2006; Michalak et al., 2016). The FFMQ measures five facets of mindfulness, namely Observe, Describe, Act with Awareness, Non-Judge (8 items each), and Non-React (7 items). Items were answered on a 5-point Likert scale from 1 = *never or very*

rarely true to 5 = *very often or always true*. Indicators for internal consistency (McDonald's ω) of all measures at T1 are depicted in Table 5.2 (values for T2 can be found in Table S2 in the Supplementary Information).

We preregistered the inclusion of two subscales of the Comprehensive Inventory of Mindfulness Experiences (CHIME; Bergomi et al., 2014) as an additional measure of mindfulness. Description of these subscales and results for the CHIME can be found in the Supplementary Information.

Self-compassion was measured with the 12-item short version of the Self-Compassion Scale (Hupfeld & Ruffieux, 2011; Raes et al., 2011). The scale captures the positive dimensions of self-compassion, Mindfulness, Self-Kindness, and Common Humanity, and their negative counterparts, Overidentification, Self-Criticism, and Isolation, with two items, each rated on a 5-point scale from 1 = *almost never* to 5 = *almost always*.

Based on previous research (e.g., Wayment & Taylor, 1995; A. E. Wilson & Ross, 2000), we developed items to measure comparison frequency, perceived comparison utility, and perceived comparison outcome in five domains: extraversion, emotional stability, appearance, professional success, and private life. For the U.S. sample, the items were translated and harmonized by the project team and then checked for spelling, grammar, and cultural appropriateness by a professional translation company.

For each domain, the frequency of social and temporal comparisons was assessed with one item each (e.g., "How often do you compare yourself with other people regarding your appearance?) using a 7-point bipolar scale (1 = *never*, 7 = *always*). Perceived comparison utility of social and temporal comparisons was assessed with one item each (e.g., "How useful do you find comparisons with yourself in the past regarding your private life?") using a 7-point bipolar scale with three anchors (1 = *not at all useful*, 4 = *neither/nor*, 7 = *very useful*). Perceived comparison outcome of social and temporal comparisons was measured with one item each (e.g., "How do you evaluate yourself compared with other people regarding how sociable you are?") on a 5-point bipolar scale ranging from 1 = *much less [sociable/calm/…]* to 5 = *much more [sociable/calm/…]*. This resulted in a total of 30 items, 15 items for each comparison standard (social and temporal). The mean and standard deviation for the items on dimension level (i.e., separately for social and temporal comparison frequency, perceived utility, and perceived outcome for extraversion, emotional stability, appearance, professional success, and private life) can be found in Table S3 in the Supplementary Information. Confirmatory factor analyses revealed that items loaded on assumed higher-order factors with loadings of at least 0.32 (see Table S4 in the Supplementary Information). For the current purposes, we therefore

aggregated the dimensions to measure a general comparison frequency, a general perceived comparison utility, and a general perceived comparison outcome, and the same for social and temporal comparisons separately. For a comprehensive analysis of the factor structure, more information on the items on dimension level, and more details on the psychometric properties, please refer to Wrzus et al. (2024).

We included age and meditation experience as covariates because these had a significant effect on the study's main variables. Meditation experience was operationalized as a factorial variable indicating how much experience with meditation, in hours, participants had. For analysis purposes, we dummy coded this variable into two variables (i) little meditation experience (< 50 hr) versus none and (ii) some meditation experience (> 50 hr) versus none, respectively.

5.2.3 Data Analyses

Data were analyzed with multiple regressions using the software R (Version 4.0.5; R Core Team, 2021). We conducted the data analyses with outliers and with winsorized values ($M \pm 3$ *SD*). The analyses did not differ, therefore, we continued with winsorized outliers and report only these analyses. Family-wise error correction was applied within the same family of regressions (Bonferroni correction; new $\alpha = 0.05/6 = 0.008$).

Table 5.1

Sample Descriptives at T1 and T2

Variable	Overall (N_{T1} = 615; N_{T2} = 310)	Germany (N_{T1}= 313; N_{T2}= 229)	USA (N_{T1} = 302; N_{T2} = 81)
Gender T1 (% female)	51.5	49.8	52.0
Gender T2 (% female)	53.4	51.5	56.8
Age T1 (years)			
M (*SD*)	42.3 (14.47)	43.3 (14.91)	41.32 (13.94)
Md	41	42	41
Range	18-84	18-84	18-78
Age T2 (years)			
M (*SD*)	45.1 (13.93)	46.07 (14.61)	42.27 (11.39)
Md	45	47	42
Range	18-84	18-84	21-68
Prior meditation experience[a], *n* (%)	310 (50.4)	147 (47.0)	163 (54.0)
Experience in hours[a], *n* (%)			
Less than 10 hours	100 (32.3)	43 (29.3)	57 (35.0)
10-50 hours	142 (45.8)	75 (51.0)	67 (41.1)
50-100 hours	37 (11.9)	15 (10.2)	22 (13.5)
More than 100 hours	31 (10.0)	14 (9.5)	17 (10.4)
Mindfulness practice[a], *n* (%)			
Daily	19 (6.1)	4 (2.7)	15 (9.2)
A few times a week	45 (14.5)	20 (13.6)	25 (15.3)
A few times a month	73 (23.5)	35 (23.8)	38 (23.3)
Once a month	40 (12.9)	21 (14.3)	19 (12.0)
One or two times in half a year	63 (20.3)	29 (19.7)	34 (20.9)
Less than once a year	26 (8.4)	10 (6.8)	16 (9.8)
Never	44 (14.2)	28 (19.0)	16 (9.8)

Note. [a]Information on meditation experience was only collected at T1.

5.3 Results

Descriptive statistics, McDonald's ω at T1, bivariate correlations at T1, and retest-reliabilities are depicted in Table 5.2 (values for T2 are given in Table S2 in the Supplementary Information). All associations between the study's main variables were in line with the suggested relationships. Unexpectedly, we found mean differences in mindfulness, self-compassion, comparison frequency, and perceived comparison utility between the German and U.S. samples (see Table S5 in the Supplementary Information). However, when we additionally controlled for the participants' nationality, the overall result patterns did not change (see additional analyses on OSF: https://osf.io/f6qc8/).

Dropout analyses indicated significant differences between participants who continued and discontinued participation at T2 on age, mindfulness, comparison frequency, perceived comparison utility, and perceived comparison outcome (but not on self-compassion) at T1. In summary, participants who completed both surveys were older, more mindful, compared less frequently, perceived comparisons as less useful, and perceived their comparison outcomes as worse than did non-completers (see Table S6 in the Supplementary Information).

The results of the regression analyses are depicted in Figure 5.1 and Figure 5.2 (as well as in Tables S7-S10 in the Supplementary Information). In line with H1, mindfulness was associated with less frequent use of comparisons overall, both cross-sectionally and longitudinally (see Figure 5.1, Panel A, and Table S7). Because the 95% confidence intervals (CIs) of mindfulness regression weights for social (T1 [− 0.50, − 0.33], T2 [− 0.52, − 0.27]) compared with temporal (T1 [− 0.37, − 0.19], T2 [− 0.37, − 0.13]) comparison frequency overlapped, we could not infer that mindfulness was associated with less frequent use of social comparisons compared with temporal comparisons.

The same result pattern emerged for self-compassion. Self-compassion was associated with less frequent use of comparisons overall, both cross-sectionally and longitudinally (H2; see Figure 5.1, Panel B, and Table S8). Again, both comparison standards did not seem to differ in their association with self-compassion because 95% Cis overlapped (social comparison: T1 [− 0.50, − 0.33], T2 [− 0.63, − 0.39]; temporal comparison: T1 [− 0.36, − 0.19], T2 [− 0.41, − 0.18]).

Contrary to Hypothesis 3, the overall pattern of results suggested that mindfulness was not associated with less perceived utility of comparisons (see Figure 5.2, Panel A, and Table S9). Similarly, higher self-compassion was not associated with less perceived utility of comparisons, both cross-sectionally and longitudinally (H4; see Figure 5.2, Panel B, and Table S10).

Exploratory analyses indicated that mindfulness was associated with a better perceived comparison outcome overall, both cross-sectionally and longitudinally (see Figure 5.3, Panel A, and Table S11). This was also true for self-compassion, for which higher values were associated with better perceived comparison outcomes overall, both cross-sectionally and longitudinally (see Figure 5.3, Panel B, and Table S12).

Table 5.2

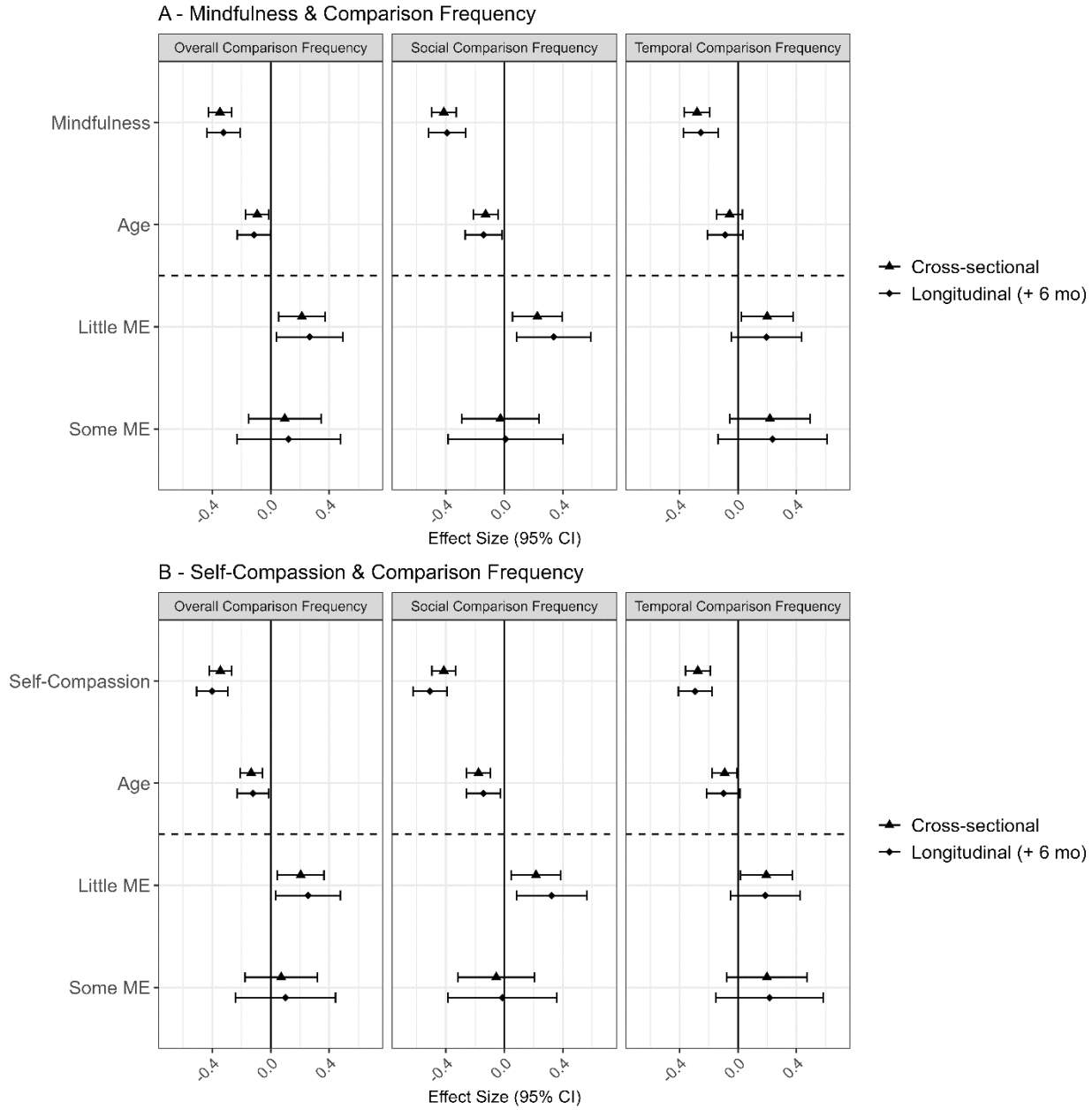
Bivariate Correlations of Study Variables at T1 Below the Diagonal, Retest-Reliabilities Between T1 and T2 Measures on the Diagonal

Variable	*M* (*SD*)	ω	FFMQ	SCS	Frequency			Utility			Outcome		
					Overall	Social	Temporal	Overall	Social	Temporal	Overall	Social	Temporal
FFMQ	3.28 (0.51)	0.92	0.85										
SCS	3.02 (0.69)	0.86	0.68	0.76									
Frequency	3.72 (1.02)	0.86	−0.37	−0.37	0.77								
Social	3.73 (1.12)	0.78	−0.42	−0.41	0.93	0.78							
Temporal	3.70 (1.09)	0.75	−0.27	−0.27	0.92	0.71	0.70						
Utility	3.21 (1.19)	0.87	−0.15	−0.09	0.65	0.57	0.63	0.68					
Social	2.99 (1.23)	0.78	−0.16	−0.09	0.59	0.58	0.51	0.93	0.68				
Temporal	3.43 (1.31)	0.78	−0.13	−0.07	0.61	0.47	0.66	0.94	0.73	0.61			
Outcome	2.92 (0.52)	0.73	0.27	0.42	0.00	−0.04	0.04	0.31	0.26	0.32	0.76		
Social	2.81 (0.57)	0.60	0.35	0.49	−0.06	−0.12	0.01	0.25	0.23	0.23	0.86	0.76	
Temporal	3.03 (0.62)	0.60	0.14	0.26	0.05	0.04	0.06	0.30	0.22	0.33	0.88	0.53	0.68

Note. $N = 615$ below the diagonal, $n = 310$ on the diagonal. FFMQ = Five-Facet Mindfulness Questionnaire; SCS = Short Self-Compassion Scale; Frequency = comparison frequency; Utility = perceived comparison utility; Outcome = perceived comparison outcome. All $r \geq |0.12|$ are significant at $p < 0.05$.

Figure 5.1

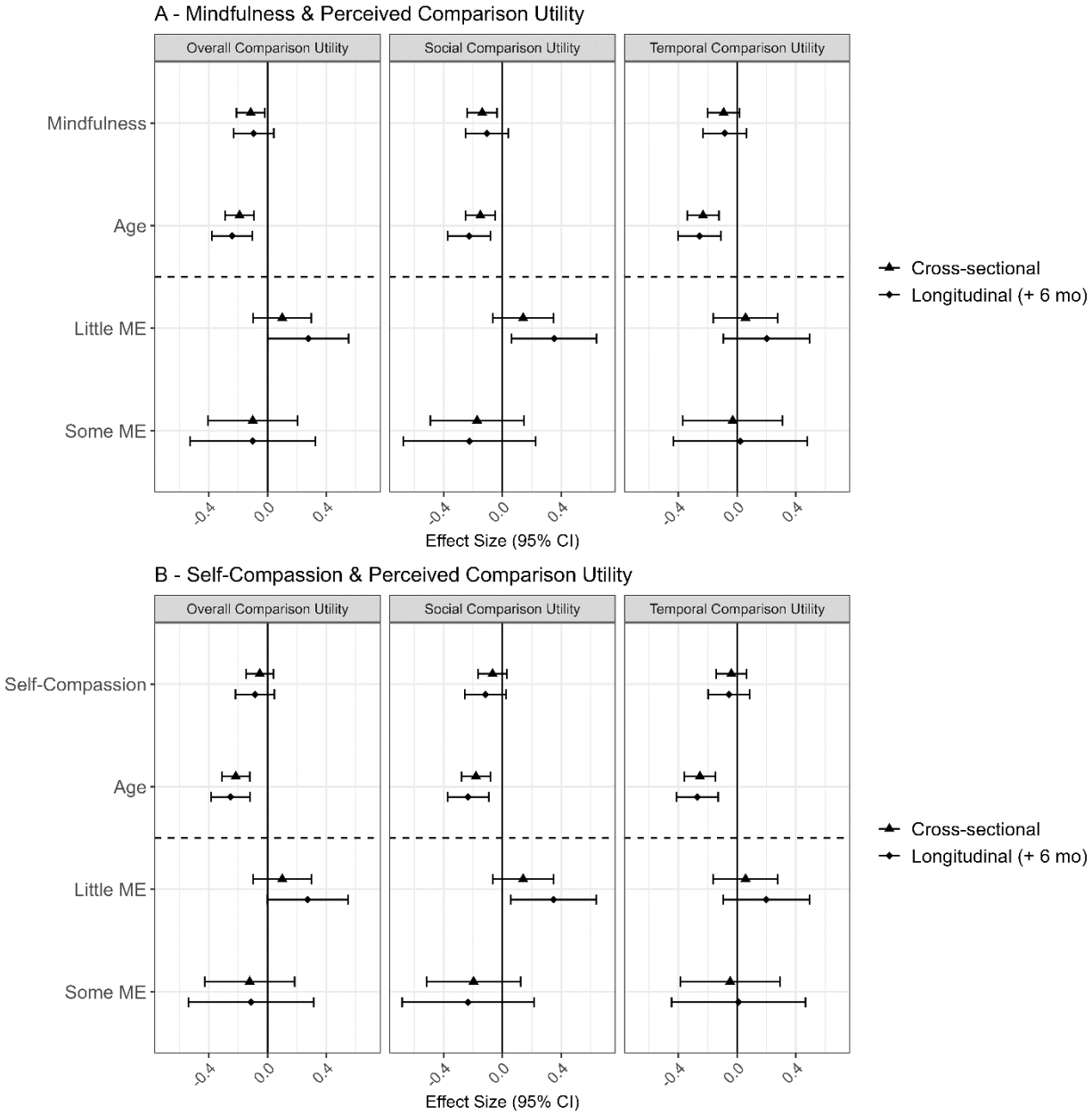
Results of the Multiple Linear Regressions for Prediction of Comparison Frequency by Mindfulness (Panel A) and Self-Compassion (Panel B)



Note. This figure displays the regression weights and the respective 95% CIs of the regression of comparison frequency at T1 and T2 on mindfulness at T1 (with control variables; Panel A) and self-compassion at T1 (with control variables; Panel B), both overall and separately for social and temporal comparisons. Values above the dotted line are standardized; values below the dotted line are unstandardized. Values on the left mean less comparison frequency. Little ME = no versus little meditation experience (<50 hr); Some ME = no versus some meditation experience (>50 hr).

Figure 5.2

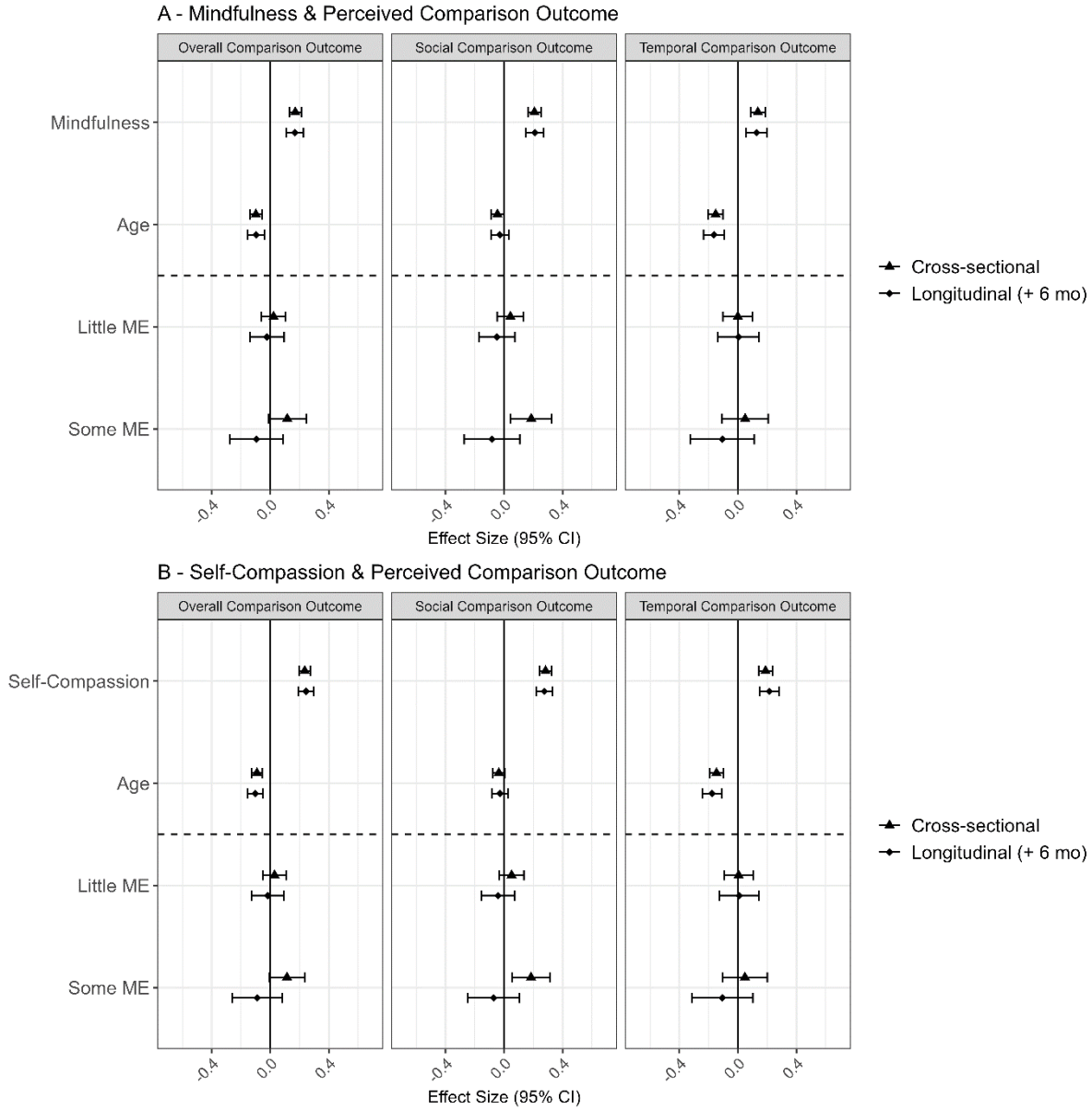
Results of the Multiple Linear Regressions for Prediction of Perceived Comparison Utility by Mindfulness (Panel A) and Self-Compassion (Panel B)



Note. This figure displays the regression weights and the respective 95% CIs of the regression of perceived comparison utility at T1 and T2 on mindfulness at T1 (with control variables; Panel A) and self-compassion at T1 (with control variables; Panel B), both overall and separately for social and temporal comparisons. Values above the dotted line are standardized; values below the dotted line are unstandardized. Values on the left mean less perceived comparison utility. Little ME = no versus little meditation experience (<50 hr); Some ME = no versus some meditation experience (>50 hr).

Figure 5.3

Results of the Multiple Linear Regressions for Prediction of Perceived Comparison Outcome by Mindfulness (Panel A) and Self-Compassion (Panel B)



Note. This figure displays the regression weights and the respective 95% CIs of the regression of perceived comparison outcome at T1 and T2 on mindfulness at T1 (with control variables; Panel A) and self-compassion at T1 (with control variables; Panel B), both overall and separately for social and temporal comparisons. Values above the dotted line are standardized; values below the dotted line are unstandardized. Values on the left indicate a worse comparison outcome. Little ME = no versus little meditation experience (<50 hr); Some ME = no versus some meditation experience (>50 hr).

5.4 Discussion

The objective of this study was to investigate the associations between mindfulness and self-compassion with basic comparison characteristics, that is, frequency and perceived utility of social and temporal comparisons. Results from this large, binational, age- and gender-diverse sample suggest that the more mindful and self-compassionate individuals are, the less they use social and temporal comparisons to evaluate their standing across professional performance, physical appearance, personality characteristics, and their personal life. Contrary to our expectations, more mindful and self-compassionate individuals did not perceive social or temporal comparisons as less useful than individuals with lower scores on mindfulness and self-compassion regarding the same domains. However, individuals with higher mindfulness and self-compassion scores perceived themselves as having a better standing across professional performance, physical appearance, personality characteristics, and their personal life overall compared with others or with their past selves. Because we chose a prospective longitudinal design, this also offers preliminary evidence for the stability and predictive value of mindfulness and self-compassion of basic comparison characteristics.

We suggest that individuals higher in mindfulness and self-compassion use fewer social and temporal comparisons for self-evaluations because of enhanced processing and integration of more information from various sources (e.g., nonverbal cues in social situations, enhanced interoceptive perception; E. Carlson, 2013). These individuals may also process the available information less defensively and consider it as valid feedback rather than ego-threatening comparison information. This is in line with earlier findings suggesting that mindfulness is related to a clearer self-concept and autonomous forms of motivation (Donald et al., 2020; Dummel, 2018), and that self-compassion increases self-improvement motivation (Breines & Chen, 2012; Chwyl et al., 2021). Rather than focusing on aspects these individuals do *not* have (comparatively), they may place more importance on finding out where they are standing now (clarity) and how they can get to another state (self-improvement). The finding could also indicate that individuals with higher mindfulness and self-compassion may generally have a lower self-evaluation motive, which results in fewer comparisons.

Interestingly, we found that the control variables, age and little meditation experience (< 10‑50 hr, compared with no meditation experience), showed some significant associations with the outcome variables as well. We suggest that, as they age, people have less desire to compare themselves because they may already have a clear self-concept (Lodi-Smith et al., 2017). With regard to meditation experience, the results indicate that with little experience compared to no experience, people compare themselves more frequently. This may hint at the

use(fulness) of comparisons to evaluate one's relative standing especially while learning a new skill, such as meditation, and more feedback on one's progress in form of comparisons is sought.

Altogether, the participants in the current study judged social and temporal comparisons as barely useful. This may be because most individuals anticipate the potentially negative outcomes of comparisons, especially social comparisons (Gerber et al., 2018). At the same time, comparisons are an important source for evaluating attributes that constitute the self; they seem to happen automatically and are ubiquitous (D. Gilbert et al., 1995; Morina, 2021). Thus, people can hardly avoid them. And perhaps for good reason: To evaluate one's development and learn from previous experiences, an individual needs to know how they behaved, thought, and felt compared with others and at an earlier timepoint (e.g., for a review, see Wrzus, 2021). Nonetheless, we suggest that individuals can change their perspective on comparison standards and information. As previous research suggests, the motivational focus of comparisons determines the choice of the comparison standard (i.e., lateral, upward, downward comparison), and the goal of the comparison (e.g., self-verification; Wayment & Taylor, 1995). A mindful and self-compassionate focus that aims at a clear and accepting understanding of oneself in reference to others or the past self may lead to a more positive or neutral processing of information. This suggestion is reflected in the results of the exploratory analyses, which indicate that mindfulness, and especially self-compassion, is associated with a better perceived outcome of social and temporal comparisons. These findings challenge the assumption that upward comparisons necessarily lead to negative outcomes (Gerber et al., 2018; McComb et al., 2023). The association between higher mindfulness and self-compassion and more positive perceived comparison outcomes suggests that these qualities may buffer against the negative effects of upward comparisons. Thus, when performed with mindfulness and self-compassion, social and temporal comparisons may help individuals get a clearer, more adequate picture of their standing that is less blurred by a negative processing bias, and facilitate social learning and personal growth (Leary et al., 2007).

5.4.1 Limitations and Future Research

Despite the notable strengths of the current data, which were gathered from a large, binational, age- and gender-diverse sample, this study has some limitations. First, the analyses do not allow for any causal conclusions on the relationship between mindfulness, self-compassion, and basic comparison characteristics despite the fact that we analyzed these associations longitudinally. Future research could address this issue by comparing mindfulness and/or self-compassion interventions with control groups and their effect on comparison processes in a randomized-controlled design. Moreover, it would be interesting to investigate

the specific mechanisms of mindfulness and self-compassion as well as differential effects on the dimension level of comparisons. Second, as dropout analyses suggest, the variance in this longitudinal sample was somewhat limited: Individuals higher in mindfulness, and lower in comparison frequency, perceived comparison utility, and perceived comparison outcomes tended to continue participation. The selective participation at follow-up should hence be considered when interpreting the longitudinal findings. Third, the study was limited by the use of self-report measures, which may have led to a biased or socially desirable response pattern. In future studies, researchers could use different settings (e.g., observational studies, as often done in social comparison research; Gerber et al., 2018), potentially in combination with the aforementioned interventions. Last, when we did not control for age, mindfulness and self-compassion were negatively associated with perceived comparisons utility. This finding hints at a potential third variable, for example, a clearer self-concept and thus a lower perceived comparison utility in general. Future research could aim to identify moderating or mediating variables.

The current findings suggest that social and temporal comparisons can provide useful information for self-evaluation and that a negative outcome of a comparison can be mitigated by adopting a kind and non-evaluative perspective on the given information. Individuals higher in mindfulness and self-compassion engage in fewer comparisons and may be able to more carefully discern which comparison information to choose (and which to disregard) to acquire self-knowledge. This study sets the foundation for further investigations into how mindfulness and self-compassion contribute to a kind and non-judgmental self-evaluation, a more thorough inclusion of information for self-evaluation, and potentially a more realistic self-perception.

Chapter 6: General Discussion

The present dissertation investigated mindfulness and self-compassion across their multiple conceptualizations—as practices, interventions, predictors, and mechanisms of change—to elucidate their contribution to mental health promotion. Through three empirical studies employing diverse methodologies, samples, and assessment approaches across different contexts, this work showed that mindfulness and self-compassion meaningfully contribute to mental health promotion. Specifically, the dissertation addressed two central research questions: (RQ 1) It examined the specific and broad effects of mindfulness-based and self-compassion-based interventions (MBIs and SCBIs) in two different samples from the non-clinical population in different delivery formats (i.e., online and in-person). Moreover (RQ 2), it zoomed in on state mindfulness as a predictor of trait change and investigated how mindfulness and self-compassion are associated with self-evaluation tendencies.

6.1 Universality vs. Specificity of Mindfulness- and Self-Compassion-Based Interventions

The findings from Chapters 2 and 3, investigating the universality versus specificity of MBIs and SCBIs, carry important theoretical and conceptual implications for intervention effectiveness and design. The two randomized controlled trials described in these chapters examined distinct training approaches across different delivery formats and durations while addressing previous research limitations. More specifically, the study presented in Chapter 2 was among the first of its kind in several ways: It investigated a brief, self-guided, online self-compassion training compared to an active stress-reduction control group in a non-clinical community sample. This design addressed multiple gaps in existing research (Ferrari et al., 2019; Han & Kim, 2023; Kirby et al., 2017): it (a) included a format-matched active control group, (b) recruited a healthy community adult sample compared to previous studies often using pre-selected samples (e.g., Krieger et al., 2019), (c) examined a brief, accessible, cost-efficient, and easily scalable online format in contrast to previous research which has relied on in-person formats with either very short inductions or extended training periods (e.g., Arch et al., 2014; Leary et al., 2007; Neff & Germer, 2013), and (d) expanded the range of outcome variables by exploring effects on perfectionism—a transdiagnostic, often subthreshold pathological construct with high comorbidity that significantly impairs mental health (Callaghan et al., 2024). Surprisingly, both the self-compassion training and the control condition demonstrated similar effects on self-compassion, self-criticism, perfectionism, social anxiety, and psychological health, with effects persisting up to at least 4 weeks. Intervention-specific effects on self-compassion and self-criticism emerged only for highly self-critical individuals in the

self-compassion training, indicating that baseline self-criticism may moderate intervention effectiveness (cf. Krieger et al., 2019).

The study presented in Chapter 3 took a different approach, examining the 8-week in-person Socioemotional Competence Training (SECT) that uniquely targets both stress management and social competence development simultaneously. The SECT innovatively combines well-founded techniques from systemic, value-based, personality, and contemplative intervention frameworks. The study systematically examined spillover effects to broader domains, which have often been overlooked in previous research but may be more common than previously recognized (Hähner et al., 2024; Krämer et al., 2025). Moreover, the sample included two age groups, addressing age-related questions on older adults' responsiveness and development through training (L. Brown et al., 2019; Verhaeghen et al., 2025).

Unlike the relatively broad effects observed in Chapter 2, the SECT demonstrated rather specific effects only on directly trained outcomes (i.e., mindfulness and perceived stress) compared to a waitlist-control condition, with no broader spillover effects on self-compassion, empathy, or perspective-taking. However, the overall training effects demonstrated remarkable durability, persisting up to at least 12 months, in mindfulness, perceived stress, self-compassion, perspective taking, relationship quality, and psychological well-being. Additionally, the results indicate that both younger and older adults similarly benefited from the SECT.

Despite substantial variation in intervention content, format, duration, framing, and findings, several results from Chapters 2 and 3 suggest convergent implications that align with previous research. Most notably, both training courses (and the active control condition in Chapter 2) led to positive mental health outcomes and a reduction in negative mental health indicators. Similarly, the contemplative qualities could be cultivated through diverse approaches, whether explicitly labeled (self-compassion training; Chapter 2), trained but not explicitly labeled (SECT, Chapter 3), and neither labeled nor trained (control condition; Chapter 2). This pattern aligns with meta-analytic evidence that diverse psychological interventions, whether contemplative-based or otherwise, promote mental health and contemplative qualities (cf. Chapter 1, Goldberg et al., 2019; Krämer et al., 2025; van Agteren et al., 2021).

Moreover, both brief and extended interventions demonstrated notable durability. Six brief self-guided online sessions produced effects that persisted at 4-week follow-up, while the 8-week SECT maintained effects through 3- and 12-month follow-up. Combined, these results suggest that short online and extended interventions can catalyze relatively lasting change when well-designed (cf. Goldberg et al., 2019; Ferrari et al., 2019; van Agteren et al., 2021). Last, these findings carry important implementation implications. The online, unguided self-

compassion training is readily scalable for stepped-care models. It can serve as a first offer in a stepped system that provides the most effective yet least resource-intensive treatment to individuals first (Bower & Gilbody, 2005; Richards et al., 2012). Likewise, the SECT does not require extensive teacher training compared to traditional MBIs and SCBIs (e.g., MBSR or MSC programs), can be facilitated by graduate students, and can be easily tailored to specific contextual needs (e.g., organizational challenges, cultural characteristics). Therefore, it offers a cost-effective implementation option across diverse populations and settings.

Despite these similar implications, the divergence between relatively broad effects of the stress-reduction training (Chapter 2) and the rather specific effects of the broad SECT (Chapter 3) is surprising yet intriguing. A direct comparison between the interventions is not warranted from the available data given the very different approaches and their different target outcomes. Yet, considering the results alongside each other unveils some interesting hypotheses for future examination. Specifically, the diverging pattern may indicate that variation in intervention content, format, duration, timing, and framing potentially affects specificity in multiple ways.

First, all interventions shared a core element: dedicating time for oneself to reduce stress. This self-focus may naturally impact "inward-oriented" constructs (e.g., mindfulness, self-compassion, self-criticism, perfectionism) more than "outward-oriented" social competencies (e.g., empathy, perspective-taking; Miller & Verhaeghen, 2022), making the former more amenable to change. The self-guided, at-home format may have amplified this inward focus more than the trainer-led group format. In this case, the specificity would refer to the self-focus: Training effects could be more likely to spill over to other "inward-oriented" variables rather than "outward-oriented" ones.

Second, the act of taking time for oneself may already increase self-compassion. Self-compassion conceptually opposes self-criticism (Neff, 2003a, 2023), which underlies both perfectionism and social anxiety (Dunkley et al., 2020; Werner et al., 2012). Thus, if self-compassion increases, the other constructs may decrease naturally (cf. Krieger et al., 2019). In other words, effects might have spilled over due to an inherent interdependence. This could explain why both training courses in Chapter 2 showed similar broad effects, especially given the amplified self-focus of the online format. However, in comparison to the waitlist control group, self-compassion did not increase during SECT participation, although previous research suggests it can be developed in group formats (Neff & Germer, 2013). This warrants further investigation of the proposed effect chain across different intervention formats.

Third, the results may reflect differential motivation toward avoidance versus approach goals (Elliot & Church, 1997): Reducing negative self-directed experiences (e.g., perceived

stress, self-criticism, perfectionism) may be more appealing to healthy individuals than developing complex positive (social) competencies (Baumeister et al., 2001; Galante et al., 2023). Moreover, this preference may reflect traditional deficit-oriented therapeutic and societal approaches to mental health, whereas the focus on building positive competences has only emerged relatively recently (cf. Chapter 1; Goldberg et al., 2023; Seligman & Csikszentmihalyi, 2000). This general deficit focus may shape participants' expectations and engagement, making avoidance goals seem more attainable for healthy adults, whereas learning new or improving adaptive skills may require more time or training (Fredrickson, 2001; Lyubomirsky et al., 2005). Future studies could explore whether specific training is necessary for increases in positive competences and whether broader approaches may be sufficient to address negative thought, emotion, and behavioral patterns in healthy adults.

The concentrated delivery of the online training may have potentiated these effects. The training provided brief sessions and reminders more frequently than the SECT. Preliminary research suggests that perception of beneficial outcomes predicts practice time (Goldberg et al., 2020). The faster pace in the online training may have enhanced perceived success, resulted in more practice time, and thereby created a positive feedback loop. However, this effect may be attributable to training pace rather than online delivery per se. Future research may also want to directly compare the stability of effects between short, intensive courses and longer, distributed training. This would provide valuable insights into dose-response questions, especially regarding optimal volume and density.

Finally, repeated assessment of target variables may have amplified effects through heightened salience of the target constructs. Both studies repeatedly asked participants to report on mood, stress level, and self-view before and after each training session (Chapter 2), or on weekly mindfulness and stressful events (Chapter 3). These frequent measurements may act like micro-interventions or may have created expectancy (or social desirability) effects that enhanced (or diluted) intervention outcomes (cf. Krämer et al., 2025). In summary, further systematic investigation of design elements, such as intervention focus (inward vs. outward, deficit- vs. competence-oriented), session characteristics (e.g., timing, volume, pacing), and delivery format (individual vs. group, online vs. in-person) would help to identify which intervention components are most effective for different populations and target outcomes.

Taken together, two valuable insights arise from this research—one encouraging, one challenging. First, and consistent with previous findings (Goldberg et al., 2019, 2022), individuals apparently benefit from intentionally dedicating time to themselves, which enhances psychological well-being and reduces perceived stress regardless of intervention

duration, delivery format, or specific content. Second, these studies highlight the need to move beyond questions of efficacy toward understanding mechanisms and boundary conditions among the numerous psychological interventions currently available. Zilcha-Mano (2021) critically reflected that, after more than eight decades of psychotherapy research, most treatments appear effective for approximately 50% of patients, with different approaches showing comparable levels of effectiveness, regardless of differences in their theorized mechanisms. The author further argues that without understanding how a treatment works, little can be done to improve it. Future research on psychological interventions in general, and MBIs and SCBIs in particular, should thus invest targeted effort in comparative analyses of which training approaches work best, in which formats and doses, and for whom. Such nuanced investigations will be essential for advancing the field and optimizing intervention effectiveness (cf. Chapter 6.4 for a more detailed discussion).

6.2 Mindfulness and Self-Compassion as Predictors and Change Mechanisms

The studies in Chapters 4 and 5 examined mindfulness and self-compassion as mechanisms of psychological change and predictors of self-evaluations. These chapters aimed to contribute to the second research question of this dissertation: How do mindfulness and self-compassion predict trait change and self-referential processes?

Specifically, Chapter 4 revealed an unexpected disconnect between state and trait trajectories of mindfulness in the SECT described in Chapter 3. Contrary to expectations based on previous empirical evidence from explicit MBIs (Baer, Carmody, & Hunsinger, 2012; Kiken et al., 2015), and despite significant improvements in all state and trait indicators throughout the training, increases in state mindfulness did not predict changes in trait mindfulness, trait self-compassion, or trait perceived stress. This may suggest that experiencing change in state mindfulness may not be sufficient for trait-level change without appropriate training framing.

The disconnect between state-trait associations adds nuance to the diverging intervention specificity results discussed earlier. Perhaps an explicit and repeated reference to mindfulness concepts is necessary for individuals to integrate state experiences into lasting trait shifts. This interpretation is consistent with personality development theories and psychotherapy research, which suggest that trait change requires not only behavioral repetition but also integration of new self-perceptions through conscious reflection and associative learning (Warren et al., 2023; Wrzus & Roberts, 2017; Zilcha-Mano, 2021, 2025a). Instructing individuals to consciously reflect on what they learned and how it applies to daily life may therefore be a critical component of effective intervention design (cf. Allemand et al., 2024b; Küchler et al., 2025a, 2025b for empirical investigation).

The findings also emphasize the role of expectancy and intervention framing in shaping outcomes. In the study presented in Chapter 4, we intentionally avoided labeling exercises as meditation or mindfulness practice, aiming to reduce potential expectancy effects. In traditional MBIs, participants are repeatedly instructed to mindfully direct attention to the present moment over extended periods and thus expect mindfulness improvements. In contrast, the SECT included only short practices (about 15 minutes) and offered various other exercises such as self-reflection and behavioral tasks. Moreover, the SECT participants, who sought to improve stress management and social skills, may have attributed fluctuations in state mindfulness to contextual factors rather than to skill development. These (sub-)conscious assumptions about treatment goals likely affect participants' motivation, as well as their interpretation and integration of training experiences (Olaru et al., 2024; Suelmann et al., 2018). This aligns with previous empirical results showing that eliciting anticipation of treatment success in mental health patients improves treatment outcomes (Rutherford et al., 2017). Therefore, instead of viewing expectancy as a confounding variable, recent studies suggest leveraging it as a potential "active ingredient" (Goldberg, 2022; Krämer et al., 2025). Consequently, future theoretical and empirical work may benefit from including expectancy-enhancing elements while maintaining rigorous designs, such as dismantling or component trials (Goldberg, 2022). Also, choosing an appropriate control condition would allow further analysis of expectancy as well as broad and specific outcomes and mechanisms (see Krämer et al., 2025 for an illustrative example).

The study presented in Chapter 5 provides insight into potential self-referential mechanisms through which mindfulness and self-compassion may exert beneficial effects on mental health. Findings indicated that both concepts predicted reduced comparison frequency and more favorable comparison outcomes. This finding could indicate that mindfulness and self-compassion promote a more balanced integration of comparative information within self-evaluative frameworks. More mindful and self-compassionate individuals may (1) consider more information in their self-evaluation, (2) be kinder during the self-evaluation process, (3) perceive comparisons as more neutral (i.e., objective) or positive evaluation information, and (4) show reduced tendency for self-evaluation overall. The finding that the two constructs did not predict perceived utility may reflect that mindfulness and self-compassion can mitigate the potentially detrimental consequences of comparisons and instead process them as less ego-threatening, useful information. These interpretations align with theoretical accounts of enhanced self-knowledge through contemplative practice (E. Carlson, 2013; Djikic & Langer, 2007), though the correlational nature of Chapter 5's data underscores the need for more empirical evidence to establish causal relationships. That is, alternative explanation like

avoidance of comparisons rather than different processing, shared third variables (e.g., higher psychological well-being), and methodological confounds (i.e., floor effects in perceived utility ratings) may apply.

Together, the findings from Chapters 4 and 5 raise an intriguing question: Do MBIs and SCBIs, and psychological interventions more generally, produce actual changes in a person's characteristics, or do they primarily alter self-referential processes? This distinction is supported by theoretical and empirical work suggesting that psychological interventions may foster greater self-acceptance (E. Carlson, 2013; Goldberg et al., 2023; Krämer et al., 2025; Lindsay & Creswell, 2017). In a series of randomized controlled online studies, researchers found that both a self-improvement and a self-acceptance intervention led to similar pre- to post-intervention improvements in Big Five personality traits and psychological well-being indicators (Krämer et al., 2025). These changes were driven by shifts in current self-perceptions. Participants in both groups showed personality change towards their ideal self rather than lowering the standards for the ideal self. Yet, the open question remains whether the participants in the self-acceptance group genuinely changed or whether they interpreted their standing on their characteristics differently post-intervention. For example, they may have been more benevolent or included additional information in the evaluation of the Big Five post-assessment.

This interpretation aligns with the response shift theory, which suggests that individuals might change their self-evaluations as a result of altered internal measurement standards, values, or redefinition of the construct (Sprangers & Schwartz, 1999). For example, as a consequence of the intervention, a participant may become more aware of situations in which they responded with more awareness and compassion, re-interpret previously overlooked behavior as signs of emotional regulation (e.g., staying composed during a disagreement, managing feelings of frustration instead of acting impulsively), stop evaluating such responses as insufficient, or weigh instances of self-regulation more strongly than moments of irritability.

From this perspective, mindfulness and self-compassion may not just improve psychological functioning directly but may also modulate how individuals evaluate themselves in self-report assessments. The findings from Chapter 5 illustrate that mindfulness and self-compassion are associated with better comparison outcomes, supporting the notion that these qualities potentially help individuals reinterpret evaluative information with greater self-acceptance and objectivity. Alternatively, these effects might reflect a self-positivity bias (i.e., an overly positive or inflated self-view). However, previous research contradicts this hypothesis, suggesting that mindfulness and self-compassion are associated with a clearer self-concept, greater self-improvement and autonomous motivation, and more honest self-feedback

(Breines & Chen, 2012; Chwyl et al., 2021; Donald et al., 2020; Dummel, 2018; Leary et al., 2007). Thus, the combined results of Chapters 4 and 5 suggest that shifts in self-referential processes may play a central role in the effects of contemplative-based interventions. Future models of intervention effects should therefore distinguish between state change, trait change, and changes in self-referential processes, while accounting for the roles of reflection, expectancy, and intervention framing.

6.3 Limitations of the Present Studies, Critical Considerations, and Future Directions in Mindfulness and Self-Compassion Research

Despite the insights presented in this dissertation, several fundamental conceptual, methodological, and contextual limitations apply both to the studies presented here and to the field more broadly (cf. Goldberg & Davidson, 2024; van Dam et al., 2018; van Gordon et al., 2022). The study-specific limitations have been discussed in the respective chapters, but some overarching challenges prevail that have important implications for future research.

6.3.1 Conceptual Challenges

First, a central limitation of mindfulness and self-compassion research is the definitional and conceptual ambiguity, with researchers operationalizing mindfulness and self-compassion differently across studies (Cha et al., 2023; van Dam et al., 2018). This inherent challenge manifests in the diverse operationalizations of the constructs for intervention research (e.g., Chapters 2 and 3), making it difficult to determine whether positive outcomes reflect acquired skills or enduring trait changes. More granular designs (e.g., Chapter 4) significantly contribute to addressing this challenge. Still, future research needs to find additional ways to theoretically and methodologically distinguish between different conceptual levels (e.g., trait vs. state, practice vs. outcome) and identify how these levels interact over time (e.g., Warren et al., 2023).

Second, the secularization of these Buddhist concepts has provoked criticism that was termed "McMindfulness" or “capitalist spirituality” (Purser, 2019). These arguments contend that contemplative practices have been detached from their original ethical foundations and adapted into commercialized versions that emphasize individual self-optimization and ego-centeredness over addressing systemic causes for suffering, thereby inhibiting societal change (Chachignon et al., 2025; Choi et al., 2021; Hyland, 2017; Purser, 2019; Quaglia et al., 2021; Schindler et al., 2019). It remains, however, an open empirical question whether incorporating ethical components into contemplative interventions would additionally enhance psychological well-being outcomes.

Moreover, mindfulness and self-compassion considerably overlap with or map onto Big Five personality traits, specifically neuroticism, conscientiousness, and agreeableness

(Almenräder et al., 2024; Altgassen et al., 2023; Bainbridge et al., 2022; Geiger et al., 2018; Giluk, 2009; Pfattheicher et al., 2017). Future research needs to further disentangle conceptualizations theoretically and clarify empirically how these concepts relate to and differ from each other.

6.3.2 Methodological Challenges

Another notable limitation is the exclusive reliance on self-report measures in the studies presented here, which are susceptible to recall biases, social desirability, and demand effects (e.g., Dunning et al., 2004). Compounding this issue, the present studies relied on two widely adopted measures—the Five Facet Mindfulness Questionnaire (FFMQ) and the Self-Compassion Scale (SCS)—which continue to raise psychometric concerns. Previous research suggests poor discriminant validity and lack of longitudinal measurement invariance (Pfattheicher et al., 2017; van Dam et al., 2018). These issues undermine confidence in the robustness of intervention outcomes over time.

Although these measures are currently the best option to asses mindfulness and self-compassion with self-report and the presented studies thus provide valuable insight into subjective experience, future research should integrate more objective, complementary methodologies such as ecological momentary assessments (EMA), behavioral tasks, observer ratings, psychobiological indicators, and digital tools (e.g., wearables, mobile sensing, virtual/augmented reality; see, e.g., Blanke & Brose, 2017; Blanke et al., 2018, 2020; Halim et al., 2023; Kirk & Axelsen, 2020; J. Ma et al., 2023; Olasz et al., 2024; Seabrook et al., 2020; Zhao et al., 2025). Notably, in one study, EMA data demonstrated intervention effects of an MBSR program on anxiety and depression that traditional self-report measures apparently failed to capture (Moore et al., 2016). This divergence highlights limitations of trait self-report assessments in capturing dynamic psychological processes, potentially depicting biased snapshots of actual functioning. Combining instruments suggests another potential research avenue. Previous research showed that wearable technology can capture effects on physiological correlates during a 10-day online mindfulness course (i.e., heart-rate variability; Kirk & Axelsen, 2020). Moreover, psychological and physiological measures converged, showing pre-post reduction in anxiety after mindfulness meditation in a virtual reality environment (Olasz et al., 2024; see also Halim et al., 2023 for similar results with self-compassion).

6.3.3 Study Design

Although we achieved comparatively large sample sizes in all studies, intervention studies are often criticized for limited statistical power, which might apply to the current

research given the complex models we used (Geiser, 2011). Additionally, only the study presented in Chapter 2 employed an active control group. Therefore, replication and validation of the SECT using an active control condition, such as established contemplative (e.g., MBSR, MSC, etc.) or socioemotional competence interventions (e.g., Hinsch & Pfingsten, 2015), are still pending. Appropriate control group conditions would also allow further analysis of expectancy effects as well as broad and specific outcomes and mechanisms.

Moreover, longer follow-up periods in comparison to active controls would be interesting. The 6-session, self-guided online training courses demonstrated follow-up effects of 4 weeks. Still, future research needs to determine the stability of training effects of brief intervention effects across extended periods of several weeks or months. For the SECT study (Chapter 3), we included follow-up assessments after 3 and 12 months. However, due to limited resources and participant burden, we did not collect follow-up data from the control group (i.e., they participated in the SECT directly after the post-training assessment), limiting conclusions regarding longer-term comparative effects and interpretation of specificity and expectancy effects. Addressing these design limitations would also enable examination of whether interventions designed for mental health promotion, such as the approaches presented here, actually prevent the onset of clinical disorders.

6.3.4 Contextual Challenges

Another limitation is the systematic non- and underreporting of adverse effects of mindfulness and self-compassion interventions, which may lead to the overestimation of the beneficial effects of these programs (Baer et al., 2019; Baer et al., 2021; Britton, 2019; Britton et al., 2021; Goldberg et al., 2017). Research indicates that 37% of participants in MBIs report negative functional impacts such as hyperarousal or insomnia, with 6% reporting negative effects lasting over a month (these numbers are comparable to psychotherapy related adverse effects; Britton et al., 2021). To my knowledge, systematic evaluation on adverse effects of SCBIs is still outstanding (but see Germer, 2023, for a discussion of the “backdraft” phenomenon). Although the present studies assessed adverse effects and found only minor complaints, more systematic documentation is needed to ethically inform teacher training, intervention design, and implementation standards (cf. Britton et al., 2021; Crane et al., 2017).

Finally, another critical limitation pertains to the lack of cultural diversity in mindfulness and self-compassion research, including the current studies. The present studies included an age-heterogenous sample in the SECT study (Chapters 3 and 4), and an age- and gender-stratified, bi-national sample in the study presented in Chapter 5. Yet, these studies, like much mindfulness and self-compassion research, relied on samples from WEIRD populations (i.e.,

Western, educated, industrialized, rich, democratic; Henrich et al., 2010), with predominantly female and highly educated participants. The studies thereby neglected culturally diverse and marginalized groups. Given that the examined concepts predominantly stem from Buddhism, most prevalent in Asian countries, this limitation is particularly concerning. Comparative research found similar effects for psychological well-being and distress across cultures (Chio et al., 2021; Kirby et al., 2017). However, psychometric studies on self-report measures revealed cross-cultural differences in factor structure and mean levels, indicating potential bias toward Western individualistic interpretations, and dismissing potentially more intuitive understandings from religious Asian contexts (Arimitsu, 2023; Christopher et al., 2009; Karl et al., 2020; Montero-Marin et al., 2018; Neff et al., 2019; Tóth-Király & Neff, 2021).

6.4 Implications for Future Psychological Interventions

6.4.1 Personalized Psychological Treatment

Research has gone a long way in identifying predictors and mechanisms of change in psychological interventions. However, recent critiques of psychological interventions show that one recurrent conclusion prevails: Many mental health training approaches of different formats may increase psychological well-being and reduce psychological ill-being, but we do not have a thorough understanding of why and how (e.g., Eronen & Bringmann, 2021; Goldberg, 2022; Zilcha-Mano, 2021). Future research should therefore aim to become more precision-oriented (Cohen et al., 2021; Zilcha-Mano, 2025b).

Methodologically, micro-randomized intervention designs offer promising approaches. These designs allow systematic manipulation of order, duration, intensity, framing, and other factors, which could help clarify which ingredients of a given intervention work best for whom (Goldberg, 2022; Krämer et al., 2025). Correspondingly, fine-grained measurement techniques, such as data from wearables, mobile sensing, or intensive ecological momentary assessments should mirror this more granular level of analysis. For example, future research could test whether a specific order of exercises is comparatively more or less effective than alternative sequences or the repetition of only one exercise. Beyond these component questions, dismantling trials could help identify the specific active training ingredients that produce intervention effects (cf. Goldberg, 2022; Krämer et al., 2025).

The presented studies suggest stronger effects of self-compassion for highly self-critical individuals (Chapter 2, cf. Krieger et al., 2019) and consistent benefits of the SECT across age groups (Chapter 3), hinting at potential leverage points for specific and broad target groups. For example, the efficacy of the self-compassion training could be examined for specific components of perfectionistic self-criticism (i.e., perfectionistic cognitions or behavior; Flett et

al., 1998; Hewitt et al., 2003). This approach aligns with previous research suggesting that specific treatment may be more beneficial for individuals with higher symptom load (e.g., subclinical individuals; Galante et al., 2023; L. Ma et al., 2019). The SECT findings may reflect motivational differences between age groups, suggesting that older adults may compensate reduced cognitive plasticity through increased motivational engagement (cf. Küchler et al., 2025a). Once we understand these intervention nuances, we may be able to develop individualized treatment protocols (Zilcha-Mano, 2025b).

Mental health interventions have yet to adopt what is routinely practiced in medical treatment and physical health interventions (B. Fernandes et al., 2017): pre-assessing individual characteristics and tailoring treatment plans to individual factors such as neurological, genetic, habitual, and other preconditions, as well as lifestyle, needs, goals, abilities, and motivation. For example, when starting a new membership in the gym, the customer typically undergoes a physical pre-assessment and discusses existing preconditions, workout goals, and motivation strategies. Similarly, in cardiovascular medicine, physicians do not prescribe identical treatments for all patients with cardiac issues. Instead, they conduct comprehensive assessments including symptomatic presentation, medical history, comorbid conditions, and lifestyle factors before selecting from various therapeutic options (e.g., medication or lifestyle modification; B. Fernandes et al., 2017). In both cases, the workout or treatment plan is continuously adjusted based on individual development and changing circumstances.

Mental health interventions could benefit from similar personalized approaches. This would involve systematically assessing relevant indicators such as change goals, baseline characteristics, genetic and neurobiological preconditions, learning preferences and motivation, social support systems, technological knowledge, and symptom profiles to optimize intervention selection and delivery format (Delgadillo et al., 2022; Lutz et al., 2022; Pena Loray et al., 2025). Preliminary evidence supports this approach, with results demonstrating the superiority of personalized treatment over standard treatment protocols for depression (Chekroud et al., 2021; Cohen et al., 2021; Nye et al., 2023).

With artificial intelligence (AI) and machine learning (ML) gaining momentum, analysis of large data sets, examination of complex result patterns, and identification of predictive models that can inform individualized intervention selection have become increasingly easy (Delgadillo et al., 2022; Olawade et al., 2024). These technological advances shift the focus from previous one-size-fits-all approaches towards precision mental health care, where intervention decisions are based on scientifically informed algorithms that consider the unique constellation of factors characterizing each individual (Johnson et al., 2020; Lee et al., 2021;

Lutz et al., 2022). Furthermore, adaptive interventions could respond to changes in real-time based on continuous monitoring and engagement with users (e.g., through wearables or mobile sensing). Early detection of symptomatic deviations from individual baseline and more granular indicators of progress, will contribute to substantial optimization of the intervention process. Such advances will likely result in significant reductions in participant burden and dropout rates (Pena Loray et al., 2025; Swift & Greenberg, 2012). Despite these advantages, concerns about data security, transparency, and ethical questions, among others, need to be carefully addressed in AI-assisted psychological intervention research and development (Lee et al., 2021).

6.4.2 Personalized Preventive Mental Healthcare

Building upon these technological possibilities and extending the ideas of individualized treatment in the light of the prevention context discussed in Chapter 1, the integration of mental health prevention strategies into routine healthcare may significantly reduce psychological burden and associated societal costs. Again, medical care is ahead of mental health care in this regard. Many healthcare systems worldwide have implemented and financially cover systematic preventive medical offers, including vaccinations, cancer screenings, and cardiovascular risk assessments (OECD, 2023). These offers are informed by assessing an individual's risk of developing a condition, considering factors such as age, genetic predisposition, lifestyle behaviors, and environmental exposures (e.g., Effertz, 2023; B. Fernandes et al., 2017; Sisti et al., 2018). Based on the presence or combination of risk factors, screening intervals and intervention intensity can be individually tailored. These prevention models have achieved substantial reductions in morbidity and mortality rates for conditions such as cervical cancer, cardiovascular events, and infectious diseases (Carter et al., 2021; OECD, 2023; Peirson et al., 2013; Rahimi et al., 2021; but see Krogsbøll et al., 2019).

In Germany, many health insurances cover one or two mental health promotion interventions per year (such as Yoga or MBSR; e.g., Techniker Krankenkasse, 2025) as well as the first three to four therapy sessions ("Probatorik"; Gemeinsamer Bundesausschuss, 2024). However, limited knowledge and acceptance of these offers in the general population, as well as constraints from long waiting lists and geographical barriers, remain. Translating the prevention paradigm to mental health care could involve regular and systematic screening for (a) psychological risk factors, such as (chronically) elevated stress levels or depressive symptoms, as well as (b) psychological resilience factors, including social support or coping resources (cf. Pena Loray et al., 2025). These preventive mental health assessments could detect subthreshold levels of psychological disorders—particularly if concrete risk profiles have been identified—and offer timely implementation of prevention efforts. For example, research shows

that depression has genetic components, with heritability rates for first-degree relatives ranging between 30% to 50% (e.g., Kendall et al., 2021). Yet, to the best of my knowledge, no preventive mental health measures are taken to inform, let alone offer routine psychological check-ups for these individuals in Germany.

Routine implementation of mental health prevention could also address the current gap between intervention need and availability. These offers might serve healthy individuals struggling with temporary circumstantial or contextual difficulties as well as individuals who might not otherwise seek mental health services. This approach might reduce stigma through normalization of (personalized) psychological interventions as a standard component of healthcare.

6.5 General Conclusion

This dissertation examined mindfulness and self-compassion, two secularized constructs with Buddhist origins, across multiple conceptualizations, namely as practices, interventions, predictors, and mechanisms. Through three empirical studies in different contexts, using diverse methodologies, samples, and assessment approaches, this work showed that mindfulness and self-compassion meaningfully contribute to mental health promotion in non-clinical Western populations. It extended previous research by systematically investigating specificity questions, both at the level of intervention design and potential underlying mechanisms. Two novel contemplative training approaches demonstrated both specific and broad effects, highlighting how intervention framing and design may distinctly affect outcome specificity. Moreover, the research findings on predictive power and mechanistic functions of mindfulness and self-compassion underscore the importance of distinguishing between actual state or trait change and differences in self-referential processes.

This dissertation revealed promising avenues for future analysis, with the aim of inspiring more innovative investigation into the specific effects and mechanisms of mindfulness and self-compassion. By building on the momentum of this work, future research will hopefully move closer to solving the challenges posed by the complex nature of these constructs. Ultimately, I hope to have sparked meaningful questions that contribute to both empirical advancement and theoretical refinement, especially given the immense potential of mindfulness and self-compassion in mental health promotion. By leveraging the power of AI and machine learning combined with closer, unobtrusive monitoring, we will hopefully soon understand when, under which circumstances, and for whom these constructs are most beneficial, paving the way for personalized mental health treatment and prevention.

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List of Submitted Scientific Publications and Personal Contribution

Chapter 2

Borgdorf, K. S. A., Aguilar-Raab, C., & Holt, D. V. (2025). *Effects of a brief online self-compassion-training on perfectionism, self-criticism and social anxiety: A randomized controlled trial.* [Manuscript submitted for publication; currently under review at Internet Interventions]. Department of Clinical Psychology, Interaction- and Psychotherapy Research, Institute for Compassionate Awareness and Interdependence Research and Practice IN-CARE, University of Mannheim, Mannheim, Germany.

All authors contributed to the conceptualization. My contribution included methodology, investigation, software, data curation and preparation, formal analysis, and writing-original draft preparation. CAR contributed to supervision, and writing-reviewing and editing. DH contributed to formal analysis, supervision, and writing-reviewing and editing

Chapter 3

Borgdorf, K. S. A., Küchler, G., Wrzus, C., & Aguilar-Raab, C. (2025). Mindful and well: The effects of a socioemotional competence training (SECT) in a randomized controlled trial. *Journal of Counseling Psychology*, *72*(4), 329–341. https://doi.org/10.1037/cou0000802

I had the lead role in methodology, formal analysis, software, visualization, and writing–original draft and an equal role in conceptualization, data curation, and investigation. GK played a supporting role in methodology and writing–review and editing and an equal role in conceptualization and investigation. CW played a lead role in funding acquisition and project administration and a supporting role in conceptualization, supervision, and writing–review and editing. CAR played a lead role in funding acquisition and project administration and a supporting role in conceptualization, supervision, and writing–review and editing.

Chapter 4

Borgdorf, K. S. A., Küchler, G., Wrzus, C., & Aguilar-Raab, C. (2025). *When state-trait trajectories do not align: State mindfulness does not predict trait changes of mindfulness, self-compassion, or perceived stress.* [Manuscript submitted for publication; currently under review at Scientific Reports]. Department of Clinical Psychology, Interaction- and Psychotherapy Research, Institute for Compassionate Awareness and Interdependence Research and Practice IN-CARE, University of Mannheim, Mannheim, Germany.

All authors contributed to conceptualization and methodology. My contribution included investigation, software, data curation and preparation, formal analysis, writing-original draft preparation, and visualization. GK contributed to investigation, data curation and preparation, and writing-reviewing and editing. CW contributed to project administration, funding acquisition, supervision, and writing-reviewing and editing. CAR contributed to project administration, funding acquisition, supervision, and writing-reviewing and editing.

Chapter 5

Borgdorf, K. S. A., Küchler, G., Wrzus, C., & Aguilar-Raab, C. (2024). Less frequent but equally useful: Social and temporal comparisons in light of mindfulness and self-compassion. *Mindfulness, 15*, 2906–2918. https://doi.org/10.1007/s12671-024-02472-w

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Declaration in accordance to § 8 (1) c) and d) of the doctoral degree regulation of the Faculty

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