Effects of brief mindfulness practice on perceptions of therapist empathy, self-awareness, and the real relationship

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EFFECTS OF BRIEF MINDFULNESS PRACTICE ON PERCEPTIONS OF THERAPIST EMPATHY, SELF-AWARENESS, AND THE REAL RELATIONSHIP

by

Marianne L. Stone

A Dissertation
Submitted to the University at Albany, State University of New York in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy

School of Education
Department of Educational and Counseling Psychology
Division of Counseling Psychology
2017
Acknowledgements

I would like to thank Dr. Micki Friedlander for her remarkable responsiveness, insight and kindness throughout the dissertation process. Micki’s mentorship has shaped me both personally and professionally over the past few years, and I feel profoundly grateful for her support. My special thanks go to Dr. Alex Pieterse, who has been an important influence in my graduate school journey. I am so appreciative also for the helpful contributions of Dr. Mariola Moeyaert and Dr. Mike Ellis.

Many thanks to my mentors and colleagues at the University of Utah Counseling Center for their incredible support – in particular, Lauren, Frances, Glade, and Jake. Their enthusiasm and encouragement gave me the stamina to get through this final hurdle. Cindy and Mark have also been a source of positive energy through the final stages of the dissertation process. Their great company on outdoor adventures was a much-needed counterbalance to the rigor of dissertation work.

Words cannot express my gratitude to my parents, Jarrod and Nancy, for setting me on this course and supporting me every step of the way. My father’s wise and balanced counsel, and my mother’s nurturing, have buoyed me up on countless occasions. My siblings, Kate and Jarvis, with their humor and insight, have also been amazing. I am deeply grateful to my parents-in-law, Doug and Judi, who have been there for me as my loving, stand-in family for the years that I’ve been an ocean away from my own.

And finally, the most heartfelt thank you of all goes to my wonderful husband, Gregory, who has stood by me from start to finish, through all the trials and tribulations associated with this journey. Without him, none of this would have been possible.
# Table of Contents

Acknowledgements........................................................................................................... ii

Table of contents................................................................................................................ iii

Abstract............................................................................................................................... x

Introduction.......................................................................................................................... 1

Method................................................................................................................................. 13

Power Analysis.................................................................................................................... 13

Setting and Participants...................................................................................................... 14

Design................................................................................................................................. 14

Mindfulness Practice.......................................................................................................... 16

Mindfulness Workshop....................................................................................................... 17

Manipulation Checks.......................................................................................................... 18

Measures............................................................................................................................. 18

Barrett-Lennard Relationship Inventory............................................................................ 18

Hindering Self-Awareness Scale......................................................................................... 20

Real Relationship Inventory............................................................................................... 21

State Mindfulness Scale..................................................................................................... 22

Demographic questionnaires.............................................................................................. 23

Procedure............................................................................................................................. 23

Results................................................................................................................................. 26

Preliminary Analyses.......................................................................................................... 26

Missing Data.......................................................................................................................... 26

Manipulation Checks.......................................................................................................... 26
Descriptive Statistics ................................................................. 27
Analysis plan and test of assumptions ........................................ 27
Results for Dyad 1 ...................................................................... 34
Test of Hypothesis 1 ................................................................. 34
  Visual analysis ...................................................................... 34
  Randomization tests and nonoverlap analyses ......................... 36
Test of Hypothesis 2 ................................................................. 37
  Visual analysis ...................................................................... 37
  Randomization tests and nonoverlap analyses ......................... 39
Test of Hypothesis 3a ............................................................... 40
  Visual analysis ...................................................................... 40
  Randomization tests and nonoverlap analyses ......................... 40
Test of Hypothesis 3b ............................................................... 41
  Visual analysis ...................................................................... 41
Results for Dyad 2 ................................................................... 42
Test of Hypothesis 1 ............................................................... 42
  Visual analysis ...................................................................... 42
Test of Hypothesis 2 ............................................................... 43
  Visual analysis ...................................................................... 43
Test of Hypothesis 3a ............................................................... 44
  Visual analysis ...................................................................... 44
  Randomization tests and nonoverlap analyses ......................... 46
Test of Hypothesis 3b ............................................................... 46
Visual analysis.................................................................46

Randomization tests and nonoverlap analyses.......................47

Sensitivity Analysis..........................................................47

Dyad 1.............................................................................48

Dyad 2.............................................................................48

Summary of Results............................................................49

Discussion..........................................................................50

Implications for theory and practice....................................53

Strengths and Limitations..................................................55

Recommendations for future research...............................59

Conclusion........................................................................60

References.........................................................................61

List of Tables

Table 1. Scores on the Study Variables by Dyad....................74

Table 2. Comparison of Mean Scores on Study Variables for Experimental vs.
    Control Conditions by Dyad...........................................75

Table 3. Results for Dyad 1...............................................76

Table 4. Results for Dyad 2...............................................77

List of Figures

Figure 1. Dyad 1: Client ratings on Empathy for experimental and control sessions
    ..................................................................................78

Figure 2. Dyad 1: Variability for client ratings on Empathy for experimental and
    control sessions.............................................................78
Figure 3. Dyad 1: Level for client ratings on Empathy for experimental and control sessions……………………………………………………………………79

Figure 4. Dyad 1: Trend for client ratings on Empathy for experimental and control sessions……………………………………………………………………79

Figure 5. Dyad 1: Overlap for client ratings on Empathy for experimental and control sessions……………………………………………………………………80

Figure 6. Dyad 1: Therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………………………80

Figure 7. Dyad 1: Variability for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………………………81

Figure 8. Dyad 1: Level for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………………………81

Figure 9. Dyad 1: Trend for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………………………82

Figure 10. Dyad 1: Overlap for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………………………82

Figure 11. Dyad 1: Therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………83

Figure 12. Dyad 1: Variability for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………83

Figure 13. Dyad 1: Level for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………84

Figure 14. Dyad 1: Trend for therapist ratings on RRI-T (Real Relationship) for
experimental and control sessions……………………………………………84

Figure 15. Dyad 1: Overlap for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions……………………………………………85

Figure 16. Dyad 1: Client ratings on RRI-C (Real Relationship) for experimental and control session……………………………………………………………85

Figure 17. Dyad 1: Variability for client ratings on RRI-C (Real Relationship) for experimental and control sessions……………………………………………86

Figure 18. Dyad 1: Level for client ratings on RRI-C (Real Relationship) for experimental and control sessions……………………………………………86

Figure 19. Dyad 1: Trend for client ratings on RRI-C (Real Relationship) for experimental and control sessions……………………………………………87

Figure 20. Dyad 1: Overlap for client ratings on RRI-C (Real Relationship) for experimental and control session……………………………………………87

Figure 21. Dyad 2: Client ratings on Empathy for experimental and control sessions…………………………………………………………………………88

Figure 22. Dyad 2: Variability for client ratings on Empathy for experimental and control sessions………………………………………………………………88

Figure 23. Dyad 2: Level for client ratings on Empathy for experimental and control sessions…………………………………………………………………89

Figure 24. Dyad 2: Trend for client ratings on Empathy for experimental and control sessions…………………………………………………………………89

Figure 25. Dyad 2: Overlap for client ratings on Empathy for experimental and control sessions……………………………………………………………90
Figure 26. Dyad 2: Therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions……………………………………………...90

Figure 27. Dyad 2: Variability for therapist ratings on HSA (Hindering Self-Awareness)……………………………………………………………………..91

Figure 28. Dyad 2: Level for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………91

Figure 29. Dyad 2: Trend for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions…………………………………………92

Figure 30. Dyad 2: Overlap for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions……………………………………92

Figure 31. Dyad 2: Therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………………………………93

Figure 32. Dyad 2: Variability for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions………………………………………………93

Figure 33. Dyad 2: Level for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………94

Figure 34. Dyad 2: Trend for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………94

Figure 35. Dyad 2: Overlap for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions…………………………………………………………95

Figure 36. Dyad 2: Client ratings on RRI-C (Real Relationship) for experimental and control sessions…………………………………………………………………………………95

Figure 37. Dyad 2: Variability for client ratings on RRI-C (Real Relationship) for
Appendices

Appendix A. Demographic Questionnaires........................................ 98
Appendix B. Therapist Recruitment Script........................................ 100
Appendix C. Client Recruitment Script........................................... 101
Appendix D. Therapist Informed Consent........................................ 102
Appendix E. Client Informed Consent............................................. 105
Appendix F. Sample Schedule....................................................... 108
Abstract

The present study was designed to extend the psychotherapy literature on the benefits of therapist mindfulness practice. Three factors related to the therapist’s way of being (Fife, Whiting, Bradford, & Davis, 2014) were reasoned to be influenced by a mindfulness exercise practiced by the therapist immediately before beginning a therapy session. It was hypothesized that mindfulness practice, compared to no mindfulness, would result in (a) the client rating the therapist as more empathic on the Barrett-Lennard Relationship Inventory Form OS-Emp+ (Barrett-Lennard, 2015); (b) the therapist experiencing less hindering self-awareness during the session, as indicated by scores on the Hindering Self-Awareness subscale within the Self Awareness and Management Strategies Scale (Williams, Hurley, O'Brien, & DeGregorio, 2003); and (c) the therapeutic relationship being perceived as more genuine and realistic based on clients’ and therapists’ scores on the Real Relationship Inventory (Gelso et al., 2005; Kelley, Gelso, Fuertes, Marmarosh, & Lanier, 2010).

Participants were doctoral trainees in counseling psychology and their clients at a university-based training clinic. In a replicated single-case design with two dyads, mindfulness practice was randomized at the session level. That is, five randomly-determined sessions were preceded by the therapist’s practice of a three-minute mindfulness exercise, whereas preceding the five control sessions, the therapist behaved as usual, i.e., without practicing mindfulness.

Visual analysis, randomization tests and nonoverlap indices showed some different experimental effects across the two dyads. Specifically, the client in Dyad 1 perceived the therapist as more empathic following mindfulness practice, and the therapist reported less
hindering self-awareness and rated the real relationship more favorably. In Dyad 2, both the therapist’s and client’s ratings of the real relationship were influenced by mindfulness practice. In both dyads, a carry-over effect for client-rated empathy seemed likely.

Overall, results suggested that practicing mindfulness briefly before a session may enhance a therapist’s way of being, which is said to be integral to successful psychotherapy. Limitations of the study are discussed along with recommendations for future research on this topic.
INTRODUCTION

Cumulative research suggests that psychotherapy is generally highly effective in bringing about meaningful change in clients’ wellbeing (Lambert, 2013; Wampold, 2001) across different treatment approaches (Cooper, 2008). The question remains, however, as to how psychotherapy brings about change (Castonguay & Beutler, 2006). Therapist factors undoubtedly contribute to some of the variability in treatment success (Cooper, 2008). Indeed, recent research suggests that individual therapist differences are responsible for 12.6% of the variance in client dropout, and 10.1% of the variance in client deterioration (Saxon, Barkham, Foster & Parry, 2016). For this reason, it is important to study therapist factors that may improve the rate of treatment success.

A common factor said to be essential for effective therapy is the therapist’s way of being (Fife, Whiting, Bradford, & Davis, 2014). This construct refers to the therapist’s “in-the-moment stance or attitude towards clients” (Fife et al., 2014, p. 21). Proponents of humanistic psychotherapy in particular have emphasized that therapist attributes, such as authenticity (Bugental, 1965), presence (Bugental, 1987), empathy, warmth, and congruence (Rogers, 1957), are critical to the success of treatment. Fife et al. (2014) theorized that these qualities, reflected in a therapist’s way of being, provide the foundation for a strong therapeutic relationship, which in turn facilitates the effective use of specific therapeutic techniques.

Most graduate programs in psychology focus on knowledge acquisition, rather than a therapist’s way of being (Bibeau, Dionne, & Leblanc, 2016), and there is little consensus about how to train psychotherapists in the cultivation of this or other common factors (Baker, 2016; Fife et al, 2014; Greason & Welfare, 2013). Mindfulness practice,
however, may present an avenue for enhancing a therapist’s way of being (Dunn, Callahan, Swift, & Ivanovic, 2013; Greason & Welfare, 2013; Jooste, Kruger, Steyn & Edwards, 2015).

Mindfulness is defined as an ongoing awareness of one’s immediate experiences, in which each thought, feeling or sensation is acknowledged and accepted as it is, without judgment (Kabat-Zinn, 1990; Segal, Williams, & Teasdale, 2002; Shapiro, Schwartz, & Bonner, 1998). Although the quality of mindfulness is, to some extent, dispositional (called trait mindfulness), it can also be cultivated through practice (often referred to as mindfulness meditation). Mindfulness practice involves deliberately sustaining attention on an object (such as the breath) or on whatever emerges in each moment, which is called choiceness awareness (Hick, 2008). Moreover, mindfulness practice may enhance the therapist’s way of being, insofar as it generates in the practitioner states of loving kindness, compassion, sympathetic joy, and equanimity, according to Buddhist teachings (Aiken, 2006), as well as qualities of acceptance, lack of judgment, and patience (Jooste et al., 2015). Contemporary scholars suggest that the cultivation of these qualities allows a therapist to be highly responsive to clients (Aiken, 2006), relating to them with a high degree of acceptance, kindness, and compassion (Fulton, 2005).

Way of being is a relational construct that refers to the nuanced way in which the therapist relates to the client, as communicated through “attitude, tone, body language, word choice, and timing” (Fife et al., 2014, p. 6). However, way of being with the client may be contingent on the therapist’s way of being with the self. In other words, the extent to which the therapist develops self-acceptance, self-awareness, and self-compassion determines his/her/their ability to extend these qualities to others (Fulton, 2016). Way of
being, first with oneself and then with others, is the mechanism by which contemporary scholars have suggested that mindfulness might enhance the therapeutic process (Baker, 2016; Bruce, Manber, Shapiro, & Constantino, 2010; Schomaker & Ricard, 2015).

Although mindfulness has its roots in eastern philosophy, in recent decades it has increasingly been integrated into western psychology (Martin, 1997). The Intention, Attention, Attitude model is a modern theory of mindfulness (Shapiro, Carlson, Astin, & Freedman, 2006). According to this model, mindfulness is comprised of an intentional component (which involves paying attention on purpose), an attentional component (which involves paying attention to the present moment), and an attitudinal component (which involves paying attention in a particular way, i.e., without judgment).

Although there are few studies on the benefits of therapist mindfulness (Hick, 2008; Ryan, Safran, Doran, & Muran, 2012), research suggests that mindfulness may promote the therapist’s self-care (Cigolla & Brown, 2011; Hermanth & Fisher, 2015), self-acceptance (Solhaug et al., 2016), stress management (Christopher, Christopher, Durinagan, & Schure, 2006), compassion (Cigolla & Brown, 2011; McCollum & Gehart, 2010), and the ability to tolerate negative emotion (Aiken, 2006; Cigolla & Brown, 2011; McCollum & Gehart, 2010; Schure, Christopher, & Christopher, 2008). The therapist’s development of all of these skills and qualities is likely to be beneficial during a therapy session. In a relational sense, therapist mindfulness has been found to enhance therapist-client attunement (Baker, 2016; Keane, 2013), presence (Aiken, 2006; Cigolla & Brown, 2011; Dunn et al., 2013; Fredenberg, 2002; McCollum & Gehart 2010; Hermanth & Fisher, 2015), management of countertransference (Fatter & Hayes, 2013), empathy (Greason & Cashwell, 2009; Fulton, 2016), and self-efficacy (Greason & Cashwell,
Furthermore, with respect to the therapeutic process, mindfulness has been shown to improve perceived session impact (Dunn et al., 2013; Ivanovic, Swift, Callahan & Dunn, 2015; Schomaker & Ricard, 2015) and depth (Fulton, 2016), as well as to reduce clients’ symptoms (Grepmair et al., 2007; Ryan et al., 2012).

Contrary to the bulk of research in this area, however, one study found that therapist mindfulness was negatively associated with client outcome, specifically symptom severity, improvement, and overall functioning (Stanley et al., 2006). To explain these findings, the authors suggested that mindfulness may undermine adherence to manualized treatments by focusing the therapist’s attention on the step-by-step details of the treatment, thereby disrupting the procedural memory that guides its successful execution.

In sum, a handful of studies on therapist mindfulness practice suggest its capacity to promote qualities in the therapist that may enhance his/her/their way of being, and could potentially improve the psychotherapy process and client outcomes. Nonetheless, the mixed findings within the relatively small body of literature on therapist mindfulness suggest a need for further research.

The purpose of the present study, a single-case experiment, replicated across two dyads, was to test the effect of the therapists’ brief mindfulness practice (carried out immediately before a therapy session) on three factors related to the therapists’ way of being: empathy, hindering self-awareness, and the quality of the real relationship with clients. Specifically, mindfulness practice was hypothesized to facilitate levels of empathy (as perceived by the client) and reduce therapists’ self-reported levels of hindering self-awareness during the session. Furthermore, mindfulness practice was
predicted to facilitate the therapists’ and clients’ perceptions of the real relationship.

From a theoretical standpoint, mindfulness practice may enhance empathy, which is considered integral to a positive way of being with clients (Fife et al., 2014), in several ways. First, mindfulness practice emphasizes moment-to-moment awareness and openness to all experience (Shapiro et al., 2006), which likely facilitates therapeutic presence (Dunn et al., 2013, McCollum & Gehart 2010), a “necessary precondition” for empathy (Gellar & Greenberg, 2002, p. 85). Second, the quality of “non-attachment to any particular view” inherent in mindfulness practice (Martin, 1997, p. 293) could theoretically promote perspective taking (i.e., the capacity to take the psychological viewpoint of another), a key aspect of empathy (Davis, 1983). Third, mindfulness practice is said to promote self-acceptance and self-compassion (Ryan et al., 2012), which may predispose a person to be more accepting of, sympathetic towards, and concerned about, the suffering of others – another important facet of empathy (Davis, 1983). Finally, mindfulness is said to promote empathy by enhancing self-care (Cigolla & Brown, 2011; Hermanth & Fisher, 2015) and reducing stress (Christopher, et al., 2006; Shapiro & Izett, 2008).

In the earliest study on the relation of therapist mindfulness to empathy, Lesh (1970) demonstrated that participation in a four-week Zen meditation training program enhanced counselors’ empathic abilities. Greason and Cashwell (2009) subsequently found that trait mindfulness among graduate counseling students significantly predicted their levels of self-reported empathy. Additionally, Keane (2013) examined the effects of personal mindfulness practice among psychotherapists. Positive associations emerged between self-reported empathy and self-reported mindfulness. Subsequently, Greason and
Welfare (2013) investigated the effect of therapists’ personal mindfulness practice on clients’ perceptions of empathy, congruence, and positive regard. No significant associations emerged between therapists’ self-reported levels of trait mindfulness and empathy. When examined in relation to therapists’ meditation practice, however, clients’ perceptions of empathy were higher for meditating therapists than for non-meditating therapists, suggesting that mindfulness practice, rather than trait mindfulness, may be related to client-rated empathy.

Most recently, Fulton (2016) investigated the relation between trait mindfulness and client-perceived empathy in 55 dyads. Results indicated a significant relation between these two variables, with non-judgment predicting empathy scores. To explain this finding, Fulton suggested that the ability to suspend judgment of one’s own experience may translate into an ability to be nonjudgmental about others’ experience, which could, in turn, facilitate empathy.

Most of the studies reported here were limited by the use of ex post facto designs (e.g., Fulton, 2016; Greason & Cashwell, 2009; Greason & Welfare, 2013), precluding inferences about causality. Moreover, in two of the previous studies (Lesh, 1970; Greason & Cashwell, 2009) only the therapist’s perspective was examined, despite Rogers’s (1957) specification that empathy must be perceived by the client in order to be effective.

The second therapist quality investigated in the present study was hindering self-awareness. In this context, self-awareness refers to the “therapist’s momentary recognition of, and attention to, their immediate thoughts, emotions, physiological responses and behaviors during a therapy session” (Williams & Fauth, 2005, p. 374).
While one study found no negative effects for heightened self-awareness on counselor trainees’ performance (Ellis, Krengel, & Beck, 2002), other research suggests that heightened self-awareness may adversely affect the qualities and processes associated with the therapist’s way of being (e.g., Nutt-Williams & Hill, 1996; Wei, Tsai, Lannin, Du, & Tucker, 2015). In other words, while generally beneficial in a therapeutic context, self-awareness is considered to be *hindering* when it interferes with the therapist’s ability to be present with a client (Wei, Yi, Carrera, Botello, & Sung, 2017) such as when the therapist has “a distracting thought (‘I left the coffee pot on!’), a strong personal emotion (‘I feel so angry/bored/joyful’), a physiological reaction (a fluttering of the heart), or a personal behavior (how my legs were crossed)” that diverts attention away from the client (Nutt-Williams, 2008, p. 140).

It was reasoned that mindfulness practice may help therapists mitigate the potential hindering effect of self-awareness by helping them to become “centered” before a session (Wei et al., 2017). Additionally, since mindfulness practice involves noticing and accepting inner experiences without judgment, it cultivates an attitude of non-reactivity (Fatter & Hayes, 2013), which may, in theory, offset the potentially hindering effect of heightened self-awareness.

In support of this reasoning, Fatter and Hayes (2013) found that therapists who scored highly on the non-reactivity facet of a mindfulness measure tended to be acutely aware of their thoughts, feelings, sensations and behaviors, and were also able to respond skillfully to these inner experiences. Greason and Cashwell (2009) reported that counselor trainees who scored higher on trait mindfulness were better able to strategically control their attention in therapy and not be distracted by inner dialogue. Together, these
findings suggest that the non-reactivity fostered by mindfulness practice may help therapists become more adept at recognizing and managing internal experiences that interfere with being maximally attuned to their clients. Furthermore, one study found that counselor trainees who reported relatively more trait mindfulness experienced fewer moments of hindering self-awareness than did their less mindful counterparts (Wei et al., 2015). The authors concluded that more mindful trainees are less frequently troubled by internal distractions and are therefore able to direct their mental energy toward making appropriate therapeutic interventions.

The third factor investigated in this study was the real relationship, defined as the “personal relationship existing between two or more persons as reflected in the degree to which each is genuine with the other and perceives the other in ways that befit the other” (Gelso, 2011, p. 13). It was reasoned that this particular aspect of the relationship, which has not previously been investigated in relation to mindfulness practice, is likely to be affected by the therapist’s way of being (Fife et al., 2014). Indeed, research indicates that qualities associated with the therapist’s way of being, such as respect, flexibility, trustworthiness, and openness, contribute significantly to the quality of therapeutic relationships (Ackerman & Hilsenroth, 2003).

Theoretically, mindfulness may enhance the real relationship by facilitating interpersonal attunement, or a “relationship in which one person focuses on the internal world of the other, and the recipient of the attention feels felt, understood, and connected” (Bruce et al., 2010, p. 85). Mindfulness practice is said to promote interpersonal attunement by facilitating the therapist’s attunement to self (Baker, 2016; Bruce et al., 2010; Schomaker & Ricard, 2015), resulting in a deeper connection with the
client, i.e., a “being with” rather than a “doing to” (Baker, 2016).

Finally, from a theoretical standpoint, mindfulness may improve the real relationship by promoting congruence, defined as “the therapist’s awareness of the self which allows a genuine, wholehearted, and honest expression of the actual experiences held at that moment with the client (Rogers, 1957)” (Jooste et al., 2016, p. 555). Since mindfulness practice can increase self-acceptance (Solhaug et al., 2016), it seems likely that by practicing mindfulness, therapists can become increasingly congruent in their thoughts, emotions, and behaviors, allowing them to be more genuine and “real” with the client (Gelso, 2011).

In several previous studies, therapist mindfulness has been investigated in relation to the working alliance (Greason & Welfare, 2013; Ryan et al., 2012; Wexler, 2006). In a replicated single-case study, Schomaker and Ricard (2015) evaluated the difference between client and counselor ratings of the alliance within 5 dyads across 10 sessions, 4 of which occurred prior to a 6-week mindfulness training. The within-dyad comparisons showed some support for the effect of the mindfulness intervention on ratings of the alliance. However, this result may not have solely been due to the effect of mindfulness, since the intervention included a “relational skills building” component (Schomaker & Ricard, 2015, p. 492).

It was reasoned that in addition to the alliance, mindfulness practice may affect the quality of the real relationship, defined as therapist genuineness and realism (Gelso, 2011), by enhancing the therapist’s way of being in the moment (Fife et al., 2014; Jooste et al., 2015). Indeed, some research indicates that perceptions of the real relationship predict client progress and outcome over and above the contribution of the working alliance.
alliance (Fuertes et al., 2007; Lo Coco et al., 2011; Marmarosh et al., 2009).

Each of the three process variables investigated in the present study is theoretically related to mindfulness practice, with varying degrees of empirical support. Moreover, the three constructs are also related to one another. First, empathy requires a “dual level of awareness of being in contact with the client’s experience and with one’s own” (Gellar & Greenberg, 2002, p. 85). Thus, hindering self-awareness, which diverts the therapist’s attention away from the client (Wei et al., 2017), is likely to negatively influence a therapist’s ability to remain empathic throughout the session.

Second, hindering self-awareness is likely to undermine the real relationship if the therapist’s attention to the client and the ongoing therapeutic process are influenced by distracting inner experiences (Nutt-Williams & Hill, 1996). Finally, empathy is said to strengthen the real relationship by offering the client positive regard and understanding (Gelso & Hayes, 1998). Empirical support for this proposition was demonstrated by Fuertes et al. (2007), who found that clients’ ratings of their therapists’ empathy were significantly and positively related to their perceptions of the real relationship.

In the present replicated single-case experiment, which is similar to the design used by Schomaker and Ricard (2015), the therapists were two doctoral students who were seeing clients under faculty supervision at a community-based university training clinic. Prior to beginning the study, the therapists were trained in the practice of mindfulness, which they were asked to practice briefly (in the form of a three-minute exercise) immediately before beginning 5 of their 10 therapy sessions with an individual client. The sessions preceded by this mindfulness exercise were randomly alternated with sessions that were not preceded by mindfulness practice (i.e., control sessions). The
effects of therapist mindfulness practice on perceptions of empathy, hindering self-awareness, and the real relationship were evaluated after each session and contrasted across experimental and control sessions within each of the two dyads.

The study was similar to some aspects of Dunn et al. (2013), who investigated the effects of graduate student therapists’ brief, pre-session mindfulness practice. Like Dunn et al., after receiving specific training in mindfulness, the present therapists were instructed either to engage in guided mindfulness practice or not, immediately before randomly determined sessions; in this way, the experimental manipulation, was randomized at the session level.

In contrast to Dunn et al.’s (2013) group design, however, the present replicated single-subject alternating treatments design permitted an intensive case-by-case examination of the effectiveness of therapist mindfulness practice. Additionally, whereas Dunn et al. used perceived session impact and therapist presence as dependent variables, the current study extended the literature by using three different therapist variables, all of which are reflective of the therapist’s “way of being” with the client (Fife et al., 2014).

The following hypotheses were tested: (1) Clients will report significantly more favorable ratings of the therapists’ empathy during the sessions preceded by mindfulness practice (experimental sessions), as compared to the sessions not preceded by mindfulness practice (control sessions). (2) Therapists will report significantly less hindering self-awareness during the experimental sessions as compared to the control sessions. (3) Both therapists (3a) and clients (3b) will report significantly more favorable ratings of the real relationship following the experimental sessions as compared to the control sessions. Within each dyad, changes in both the therapist’s and client’s perception
of the real relationship were considered necessary in order to support this hypothesis.

Results of the present experiment were expected to clarify and extend the accumulating literature on the benefits of therapist mindfulness practice (e.g., Dunn et al., 2013; Schomaker & Ricard, 2015). Moreover, the clinical usefulness of this study lay in its potential to indicate whether the practice of mindfulness can be an effective training tool for novice therapists.
METHOD

Power Analysis

In a single-case, randomized alternating treatment design, the power estimate pertains to the number of observations within a single case, rather than the number of participants. Specifically, the number of ways in which the observations can be randomly assigned between two conditions determines the power in an ATD (i.e., the number of potential randomizations; Dugard, File, & Todman, 2012).

In the present study, the number of potential randomizations was 242 per dyad, reflecting the number of unique ways in which the designated 10 therapy sessions could be ordered, with 5 sessions in the mindfulness (i.e., experimental) condition and 5 sessions in the control (no mindfulness) condition, and with the restriction that no more than 4 administrations of the same condition be applied consecutively. This number of randomizations falls somewhat short of Onghena’s (1994) recommendation of 500 randomizations to detect meaningful effects in behavior research, rendering low power a limitation of the present study.

In previous similar studies, the impact of therapist mindfulness practice on therapy variables has shown small to medium effects. Greason and Welfare (2013) reported $d = .49$ for the relation between mindfulness and clients’ perceptions of facilitative conditions. Fulton (2016) reported $d = .75$ for the relation between trait mindfulness and empathy. Dunn et al. (2013) reported $d = 0.20$ for clients’ ratings of presence. Ivanovic et al. (2015) and Dunn et al. reported $d = 0.30$ and $d = 0.52$, respectively, for clients’ ratings of session effectiveness. Consequently, the current study sought to achieve at least small to medium effects.
Setting and Participants

The setting was a psychotherapy training clinic, the Psychological Services Center, a fee-for-service agency serving the local community in which doctoral students in clinical and counseling psychology programs accredited by the American Psychological Association provide psychotherapy under faculty supervision. Each of the therapists were asked to identify a client for the study after consulting with his or her faculty supervisor about the client’s appropriateness for the research. That is, clients were only recruited if there were no risk management concerns, such as active suicidality or homicidality, psychosis, or severe medical problems. No exclusion criteria were applied based on client gender, race/ethnicity, diagnosis or presenting problem.

Both therapists were practicum students in their second year of doctoral training. One therapist identified as a man, and the other identified as a woman. The mean age was 27 years, and both therapists identified their race as white. One therapist described her orientation as “client-centered/CBT,” and the other therapist described his orientation as “cognitive/integrative.” Neither therapist had had any formal training in mindfulness practice prior to the study.

Both dyads were mixed gender: The male therapist worked with a female client, and the female therapist worked with a male client. Both clients, who identified their race as white, had a mean age of 34.5 years and reported having had some higher education and previous therapy experience. Their presenting concerns were personal and social in nature, and were comparable to those typically seen at the training clinic.

Design

Single-case designs (SCDs), an alternative to group comparison designs, are
characterized by the use of “an individual ‘case’ [as] the unit of intervention and the unit of analysis” (Kratochwill et al., 2010, p. 2). SCDs are experimental and have as their aim to determine whether there is a causal relationship between an independent variable and a dependent variable within the individual (Kratochwill et al., 2010). The independent variable is manipulated to create different experimental conditions, and since the dependent variable is measured repeatedly across each condition, the individual case under investigation serves as its own control (Kratochwill et al., 2010).

One type of SCD is the alternating treatment design (ATD), also referred to as a randomization design (Barlow, Nock, & Hersen, 2009) and a randomized AB design (Heppner, Wampold, Owen, Thompson, & Wang, 2016). This design involves alternating two or more experimental conditions within a single participant in a randomized fashion, which controls for sequential confounding and order effects (Barlow et al., 2009).

In the current study, mindfulness practice (A) and no-mindfulness (B) were the respective experimental and control conditions that were randomly alternated within dyads at the session level. In each of five experimental sessions, the therapists completed a three-minute mindfulness exercise immediately prior to beginning the session. By contrast, the five control sessions were ones in which therapists behaved as they customarily would prior to a therapy session.

The experimental condition (A or B) was implemented according to a randomized schedule for 10 sessions generated in advance by the researcher for each therapist using the randomize function in Excel (Version 14.4.1, Microsoft Corporation, 2010), with the restriction that no more than four administrations of the same condition could be applied consecutively. Thus, each therapist’s intervention schedule was, in fact, semi-
randomized, to ensure that (a) the ATD would not inadvertently be simplified to an AB design, which would occur if all the control sessions were randomly chosen first, and (b) so that each therapist would have an equal number of $A$ and $B$ sessions (Barlow et al., 2009).

**Mindfulness Practice**

The experimental manipulation was the practice of a brief pre-session mindfulness exercise, in which therapists engaged prior to each of the randomly determined experimental sessions. This pre-session intervention was chosen because engaging in mindfulness practice immediately prior to beginning a therapy session was reasoned to augment its effect on the therapeutic process variables of interest. Moreover, pre-session mindfulness practice was used by Dunn et al. (2013) in their experimental group study of the effects of therapist mindfulness practice on session impact and presence.

The mindfulness exercise was the Three-Minute Breathing Space (Williams & Penman, 2011), a brief audio-recording, which was designed to cultivate mindfulness. A brief intervention was chosen as the focus of the study because, if effective, it may provide therapists with an accessible tool to use on a regular basis, as part of their preparation routine before seeing clients (Dunn et al., 2013). Furthermore, due to the demands of a training clinic, it seemed unrealistic to use a long-term mindfulness intervention, such as the four-week course used by Lesh (1970) or the nine-week course used by Grepmair et al. (2007).

The Three-Minute Breathing Space, an audio-recorded guided meditation is a core tool in mindfulness-based cognitive therapy (Williams, Teasdale, Segal, & Kabat-Zinn, 2007) and was previously used in research to develop mindfulness in clients (e.g.,
The exercise adheres closely to Shapiro et al.’s (2006) Intention, Attention, Attitude model of mindfulness by directing the listener to pay attention deliberately to the self, in the present moment, without judgment. The listener is instructed first to bring awareness to his or her inner experiences, noticing (but not judging) thoughts, feelings, and physical sensations. Next, the listener is directed to narrow the focus of attention to the sensations of the breath. Finally, the listener is instructed to expand the field of awareness to include the entire body.

Mindfulness Workshop

Before the study began, the investigator conducted a manualized mindfulness workshop with the therapists. The workshop was essentially a primer to familiarize the therapists with the principles of mindfulness practice before they were expected to use the Three-Minute Breathing Space exercise during the study.

All sessions of the workshop were conducted by the investigator, who has maintained a personal mindfulness practice for several years. A manualized workshop protocol was used, which was comprised of five, 20-minute sessions, in which the therapists were introduced to the components of mindfulness and various kinds of mindfulness practice.

The content of the workshop was modified from the mindfulness training protocol used by Dunn et al. (2013). Slight adaptations were made to the meditation scripts in the manual to be more accessible to participants and to be more consistent with the mindfulness model on which the current study was based. Additionally, whereas Dunn et al. administered their workshop sessions over five weeks before the study began, the
present workshop took place over two to three weeks, due to time constraints.

**Manipulation Checks**

As a manipulation check for the pre-experiment mindfulness training, the therapists completed the State Mindfulness Scale (SMS; Tanay & Bernstein, 2013) twice: at the beginning of the workshop to provide a baseline of state mindfulness, and again at the end of the workshop. The post-test took place immediately after the therapists listened to the Three-Minute Breathing Space, the mindfulness exercise used in the experimental sessions.

As an additional manipulation check, each therapist was asked to complete a one-item, Likert-type measure of adherence before each session began by providing a rating between 1 (*not at all*) and 7 (*completely*) to the question “How mindful do you feel right now?”. This adherence check was used to determine whether each session was, in fact, conducted according to the randomized schedule.

**Measures**

**Barrett-Lennard Relationship Inventory.** The Empathy Scale within the Barrett-Lennard Relationship Inventory—Form OS-Emp+ was chosen to assess therapist empathy. The Empathy scale in the original Barrett-Lennard Relationship Inventory—Other-to-self (BLRI-OS; Barrett-Lennard, 1964, 1978, 2015) is a widely used instrument that was used in three previous studies of therapist mindfulness (Fulton, 2016; Greason & Welfare, 2013; Vinca, 2009). Frequently used as a stand-alone measure (e.g., Fulton, 2016; Hayes & Gelso, 1991; Hayes, Yeh, & Eisenberg, 2007; Sripada et al., 2011; Vinca, 2009), the original Empathy scale has demonstrated adequate reliability estimates ranging from .71 (Sripada et al., 2011) to .82 (Hayes et al., 2007).
Recently, Barrett-Lennard (2015) published the Form OS-Emp+, in which 12 of the original Empathy items are interspersed with a mix of 12 non-empathy items from the three other BLRI scales (Level of Regard, Unconditionality, and Congruence). This modification was created to reduce the potential for consistency response bias when the original Empathy scale is used as a stand-alone measure. The non-empathy items, which are worded both positively and negatively, can be used to provide an approximation of the other three scales or as filler statements (Barrett-Lennard, 2015). In the current study, the non-empathy items were used as filler statements and not included in the Empathy score analysis.

Participants indicate agreement or disagreement with each statement on the BLRI-OS-Emp+ on a 6-point Likert scale, ranging from -3 (strongly disagree) to +3 (strongly agree). Sample items are ‘X nearly always sees exactly what I mean’ and ‘X doesn’t listen and pick up on what I think and feel.’ For half of the items, the signs are reverse scored (e.g., -3 becomes +3). All of the items are then summed, with the items preceded by a minus sign detracting from the total score. The Empathy score ranges from -36 to +36, with higher scores reflecting greater perceived client-rated empathy (Barrett-Lennard, 2015).

Since the Form OS-Emp+ scale was recently published, reliability and validity data are not yet available (Barrett-Lennard, 2015). However, it is reasonable to assume that the psychometric properties of the OS-Emp+ are equivalent to those for the original BLRI-OS (Barrett-Lennard, 2015). Test-retest reliability for the Empathy scale in the original measure was .89 (Gurman, 1977), and its split-half reliabilities ranged between .82 and .93 (Gurman, 1977). Content validity of the BLRI was enhanced by the
involvement of experienced client-centered judges in the construction of the original measure (Gurman, 1977), and the predictive validity of the Empathy scale was supported by its correlation with various psychotherapy outcome measures (Kurtz & Grummon, 1972), including change scores on the MMPI and the Tennessee Self-Concept Scale (Fitts, 1965).

**Hindering Self-Awareness Scale.** The Hindering Self-Awareness scale (HSA) within the Self Awareness and Management Strategies scale (SAMS; Williams, Hurley, O'Brien, & DeGregorio, 2003) was used because it is the only existing measure of “temporary, in-session experiences of therapist self-awareness” (Williams et al., 2003, p. 278), rather than global self-awareness. The SAMS scale includes a 15-item measure of Management Strategies, which was not included in the current study. Using the Hindering Self-Awareness scale as a stand-alone measure in the current study was reasoned to be acceptable, since the items on the two scales are not interspersed and separate psychometric data are available for each scale (Williams et al., 2003).

On the 10-item HSA, participants rate the frequency of their experience with distracting self-awareness on a 5-point scale (1 = *never*; 5 = *always*) (Williams et al., 2003). An example is, “How often do you become aware of negative self-talk (e.g., self-critical thoughts, distracting thoughts) during a session?” To obtain the total score, the ratings are summed and then divided by the total number of items (i.e., 10), resulting in a total score range of one to five. Although the scale was developed to assess therapists’ general experiences of distracting self-awareness while conducting therapy (Williams et al., 2003), the directions were modified to instruct the participants in the present study to fill out the measure with reference to the session just completed.
In a recent study investigating the relation between mindfulness and hindering self-awareness, the internal consistency coefficient was $\alpha = .83$ (Wei et al., 2015). Convergent validity was supported by a significant relation ($p < .001$, $\eta^2 = .09$) between scores on the HSA, and private-self-consciousness as measured by the Self-Consciousness Scale (SCS, Fenigstein, Scheier, & Buss, 1975) (Williams et al., 2003). Williams et al. (2003) concluded that the scale’s discriminant validity was supported by the lack of a significant relation with Snyder’s (1974) Self-Monitoring Scale; these authors reported the internal consistency of the HSA to be $\alpha = .76$.

**Real Relationship Inventory.** The Real Relationship Inventory (RRI; Gelso et al., 2005; Kelley, Gelso, Fuertes, Marmarosh, & Lanier, 2010) was selected since it is the only published self-report measure of the real relationship. Theoretically, the real relationship is characterized by genuineness and realism, constructs that constitute the two subscales of the RRI (Gelso, 2011).

In the current study, the total RRI score was used in the analysis, since there was no basis for predicting differential results for Genuineness and Realism. The RRI has a therapist version (RRI-T; Gelso et al., 2005) and a client version (RRI-C; Kelley et al., 2010). Both versions include 24 items on a 5-point Likert scale, ranging from 1 (*strongly disagree*) to 5 (*strongly agree*). Ten of the RRI-T items are reverse-scored, as are eight items on the RRI-C (Gelso et al., 2005; Kelley et al., 2010). The range for the total scores on both measures is 24 to 120, with higher scores indicating more favorable perceptions of the real relationship.

Total scale scores on the RRI-T have demonstrated strong internal consistency reliabilities (Cronbach’s $\alpha = .93$; coefficient of stability = .87), as well as convergent and
discriminant validity (Gelso et al., 2005). Specifically, convergence was reported with (a) positive associations between the total RRI and the WAI (Horvath & Greenberg, 1989; \( r = .47; r^2 = .22 \)) and the Session Evaluation Questionnaire (Stiles & Snow, 1984; Depth: \( r = .36, r^2 = .13 \); Smoothness: \( r = .43, r^2 = .18 \)) (Gelso, Kivlighan, Wine, Jones, & Friedman, 1997); and (b) inverse associations with a measure of negative transference (\( r = -.29, r^2 = .08 \); Gelso et al., 1997). Discriminant validity was supported in that the RRI-T was not significantly associated with the Marlowe-Crowne Social Desirability Scale (SDS; Crowne & Marlowe, 1960; \( r = .08, r^2 = .01 \)).

Similar to the RRI-T, the RRI-C has demonstrated strong internal consistency reliability (\( \alpha = .95 \)), as well as convergent and discriminant validity (Kelley et al., 2010). Convergence was demonstrated between scores on the RRI-C and several theoretically related measures, including the WAI (Horvath & Greenberg, 1989; \( r = .79, r^2 = .62 \)), Eugster and Wampold’s (1996) measures of the real relationship (Patient real relationship: \( r = .67, r^2 = .44 \); Therapist real relationship: \( r = .60, r^2 = .36 \)), as well as the Congruence scale of the BLRI (\( r = .71, r^2 = .50 \)). Discriminant validity was demonstrated by a near-zero correlation between scores on the RRI-C and scores on the SDS (\( r = .05, r^2 = .00 \)).

State Mindfulness Scale. The 21-item State Mindfulness Scale (SMS; Tanay & Bernstein, 2013) was used as a manipulation check after the manualized training workshop. The SMS uses a 5-point Likert-type response format, ranging from 1 (not at all) to 5 (very well); participants are asked to rate their degree of attention and awareness to their present-moment experiences during a specific time frame (e.g., past 15 minutes) and context (Tanay & Bernstein, 2013). An example item is, “I noticed thoughts come
and go” (Tanay & Bernstein, 2013, p. 4). Scores are summed to provide an index of state mindfulness, ranging from 21 to 105, with higher scores reflecting greater levels of mindfulness during the specified time frame/context (Tanay & Bernstein, 2013).

The SMS was considered the most appropriate measure of mindfulness for the purposes of a manipulation check, since it has demonstrated greater construct and face validity than other similar measures (Tanay & Bernstein, 2013). In research with non-clinical adult populations, the SMS demonstrated convergent and discriminant validity with other measures of mindfulness, as well as strong test-retest reliability ($r = .72$) when administered immediately following weekly mindfulness meditation sessions (Tanay, Lotan, & Bernstein, 2012). Cronbach’s alphas have ranged from .92 to .97 across samples (Tanay & Bernstein, 2013).

**Demographic questionnaires.** The therapist questionnaire (see Appendix A) inquired about gender, age, education, race/ethnicity, theoretical orientation, clinical experience (in years and months), and previous experience with mindfulness practice (yes/no). Clients were asked to provide gender, age, race/ethnicity, education level, presenting problem, and previous experience in therapy (yes/no) (also in Appendix A).

**Procedure**

Therapists were recruited in person at the training clinic (see script in Appendix B). Therapists who agreed to participate in the study required their supervisor’s written consent prior to the start of the mindfulness training workshop (five sessions over two weeks).

Client recruitment took place during the period that therapists were participating in the mindfulness training workshop. Based on the therapist’s recommendation, the
researcher approached eligible clients and after describing the study, requested their participation (see the client recruitment script in Appendix C).

Informed consent was obtained in writing from both therapists/faculty supervisors and clients (see Appendices D and E, respectively). Specifically, participants (and supervisors) were informed of the study’s purpose, procedures, duration, risks and benefits, as well as confidentiality and participant rights (e.g., to withdraw at any point). As explained below, the monetary incentives provided to both clients and therapists were outlined in the consent form.

After the training workshop had concluded, the therapists were provided with randomly generated schedules that specified which sessions should be preceded by the brief mindfulness exercise or a control activity of their choosing (e.g., checking email) other than mindfulness practice (see sample schedule in Appendix F). Before the experimental sessions, the therapist listened to the audiorecorded Three-Minute Breathing Space on a personal electronic device. When the schedule specified that the therapist should not practice mindfulness, the therapist was asked to carry out any activities that he or she might normally do before a session, such as using the restroom, checking email, or chatting with other therapists. Each therapist received a separate schedule in hard copy form.

For the manipulation check, therapists were asked to rate on the schedule (Appendix F) how mindful they felt after finishing either the mindfulness or control activity, immediately before going into session with their client. The therapist then conducted therapy as usual for approximately 50 minutes, as is typical at the training clinic.
The investigator placed the paper-and-pencil measures for both client and therapist in the therapist's mailbox at the start of each week for the 10 weeks of the study period. Immediately following each session, the therapist gave the client the two measures to complete in the waiting room; therapists completed their two measures in the staff office.

Completion of the measures took fewer than 10 minutes. Upon completion, the participants placed the measures in secure boxes in the office area (therapists) and waiting room (clients).

As an incentive, therapists were eligible to receive up to three gift cards, each worth $25. One card was awarded upon completion of the training workshop, another card after six sessions of participation in the study, and a third card upon completion of the study. Clients were eligible to receive up to two gift cards, worth $25 and $50 respectively. They received the first gift card after six sessions of participation, and the second gift card after the 10th session.
RESULTS

Preliminary Analyses

Missing data. Missing values were present only in Dyad 2. Specifically, the client did not respond to three items (25%) after experimental Sessions 1 (two items) and 8 (one item). None of the same items was omitted in the two sessions, suggesting that the data were missing completely at random (Rubin, 1976). Nevertheless, the Session 1 Empathy responses were not included in the analysis, since these missing values exceeded 20% (Peng, Harwell, Liou, & Ehman, 2006). Mean substitution was used to impute the single missing value from Session 8 on the basis of the other Empathy responses for that session (Schlomer, Bauman, & Card, 2010).

Additionally, the therapist in Dyad 2 neglected to complete the RRI-T for Session 3, which was a control session. Thus, this dyad’s RRI-T data for Session 3 were excluded from the analysis.

Manipulation checks. The State Mindfulness Scale (SMS), administered before and after the mindfulness training workshop, was used to determine whether the brief exercise achieved the intended effect. Results indicated that the Dyad 1 therapist scored 68 on the SMS at the start of the workshop and 86 after the workshop (scores can range from 21-105). The Dyad 2 therapist scored 32 before the workshop and 70 after the workshop. Thus, both therapists reported a notable increase in state mindfulness after practicing the mindfulness exercise during training.

As a second manipulation check, the therapists rated their state mindfulness before each therapy session on a 1 (not at all) to 7 (completely) scale. The therapist in Dyad 1 provided $M_s = 5.2$ for the experimental sessions and 2.6 for the control sessions.
Similarly, the therapist in Dyad 2 provided $M_s = 4.8$ (experimental) and 2.8 (control). Based on these self-reported ratings, it was concluded that the experimental manipulation was effective for both therapists. It is worth noting, however, that self-report measures of mindfulness may pose validity challenges, such as social desirability bias (Bergomi et al., 2013).

**Descriptive statistics.** Table 1 summarizes the scores for each study variable by dyad and by session, and Table 2 presents the mean scores by experimental condition for each of the two dyads. The score range for Empathy is to -36 to +36 (Barrett-Lennard, 2015), although no published means and standard deviations are available for this measure. The score range for Hindering Self-Awareness is 1 to 5 (Williams et al., 2003), and the mean and standard deviation in Williams et al.’s (2003) validation sample were 2.37 and 0.33, respectively. The score range for the Real Relationship Inventory is 24 to 120 (Gelso et al., 2005). For the RRI-T, the average score in Gelso et al.’s (2005) validation sample was 90 (no standard deviation was provided). For the RRI-C, the mean and standard deviation in Kelley et al.’s (2010) validation sample were 98.74 and 12.74, respectively.

As shown in Table 2, the mean Empathy and RRI-T and RRI-C scores were higher in Dyad 2 than Dyad 1, and the mean HSA scores were lower in Dyad 2. Whereas Dyad 1’s mean scores were typically in the moderate range, Dyad 2 demonstrated low scores on the HSA scale and very high scores on Empathy, suggestive of a possible ceiling effect.

**Analysis plan and test of assumptions.** The data from each dyad were analyzed separately. As the first step, scores on each dependent measure were plotted graphically
and evaluated within and between the two conditions (Kratochwill, Levin, Horner, & Swoboda, 2014). Visual analysis in single case studies, used to assess numerous features of the data (Manolov & Moeyaert, 2017), is recommended prior to the calculation of effect size via quantitative analysis (Kratochwill et al., 2010). Indeed, the decision to proceed with quantitative testing rests on the visual analysis, because the visual analysis can illuminate whether a functional relation exists between the independent and dependent variables, provided that certain conditions are met (i.e., systematic manipulation of the independent variable, systematic measurement of the dependent variable, three attempts to demonstrate an effect at three different time points, and at least three data points per condition; Kratochwill et al., 2010).

Nonparametric randomization tests and nonoverlap analyses were conducted only when the visual analysis suggested the possible presence of an experimental effect, as described below. In other words, when an experimental effect was not observed visually, no further tests were conducted, since effect-size estimation should only be calculated when the graphs provide strong or moderate evidence of an experimental effect (Kratochwill et al., 2010).

Whereas parametric tests require that the data conform to a theoretical distribution derived under certain assumptions, such as normality, independence, and equal variance (Ferron & Levin, 2014), nonparametric tests do not require distributional assumptions (Barlow et al., 2009). Indeed, nonparametric hypothesis tests are considered most appropriate for single-case designs for several reasons. First, these designs typically produce short data series (Parker, 2006), which preclude a fair assessment of whether parametric assumptions have been met (Parker, Vannest, & Davis, 2014). Second, single-
case data rarely demonstrate constant variance (Parker, 2006) and are frequently skewed and non-normal (Parker, Vannest & Davis, 2011), violating parametric assumptions. Third, the data are gathered from a single case, rather than selected at random from a population (Parker, 2006). Thus, unlike parametric tests, the test statistic cannot be compared to a theoretical distribution. Non-parametric measures also help to address the problem of autocorrelation in time series [repeated measures] designs/data, which means that every observation in a repeated series is “likely related to, or is a function of (in varying degrees), the preceding observation(s)” (Kratochwill et al., 2014, p. 100).

In a nonparametric randomization test, the obtained test statistic is compared to the sampling distribution generated through permutation of the data set (Ferron & Levin, 2014). The $p$ value, which is based on the data permutation (Edgington & Onghena, 2007), is used to test the null hypothesis that the intervention had no impact on the dependent variable by “rearranging the observed scores to all permutations of the possible randomization orders and examining different outcomes” (Barlow et al., 2009, p. 284). In the present study, the test statistic for each hypothesis was the difference between the mean scores for the mindfulness (experimental) sessions versus those for the control sessions, i.e., $\bar{A} - \bar{B}$ (Edgington & Onghena, 2007). This test statistic was compared to the set of 242 permutations (i.e., all possible assignments, with restrictions) in each of the two therapy dyads observed over 10 sessions (Ferron & Levin, 2014). Randomization tests were conducted using $R$ (Version 3.2.3 “Wooden Christmas Tree;” The R Foundation for Statistical Computing, 2015).

A one-tailed $p$ value was determined by calculating the proportion of all possible permutations with a value equal to, or larger than, the observed test statistic. A one-tailed
A value (i.e., $\bar{A} - \bar{B}$) was deemed more appropriate that a two-tailed $p$ value (i.e., $|\bar{A} - \bar{B}|$) due to the three directional hypotheses. All tests were conducted with $\alpha = .05$. While this approach increased the likelihood of Type I error, it was considered acceptable in the present study because probability values reflecting the practical significance of the results are less useful in single-case research than are effect sizes (Kratochwill et al., 2010).

Next, various nonoverlap indices were computed to gauge the evidence for an experimental effect (Lenz, 2013) by providing a directly interpretable index of the vertical “separation of two ‘data clouds’” (Parker, Vannest, Davis, & Sauber, 2011b, p. 285) that corresponds strongly with visual data representation (in ideal cases) (Parker, Vannest, & Davis, 2011a). Nonoverlap analyses are a particularly useful supplement to visual analysis when the “results are not large and obvious” and can provide clarity when features of the visual analysis are not consistent (Parker & Hagan-Burke, 2007, p. 95). However, there are also certain drawbacks associated with the use of nonoverlap statistics, such as their difficulty distinguishing between medium and large effects, and in some cases, their reliance on a single data point and their susceptibility to being influenced by outliers (Manolov & Moeyaert, 2017; Parker et al., 2011a).

There are at least nine nonoverlap indices available for assessment of single-case data (Parker et al., 2011a), and by using a variety of these indices, a researcher can have more confidence in the results when they demonstrate consistency (Kratochwill et al., 2010; Manolov & Moeyaert, 2017). Four nonoverlap indices were computed in the present analysis: (1) percentage of data exceeding the median (Ma, 2006), (2) TauU (Parker et al., 2011b), (3) percentage of all nonoverlapping data (Parker, Hagan-Burke, & Vannest, 2007), and (4) nonoverlap of all pairs (Parker & Vannest, 2009). It should be
noted that the interpretation of the effect sizes differs across the various nonoverlap indices, as explained below.

First, the percentage of data exceeding the median (PEM) provides an estimate of effect size by indicating the proportion of data from the experimental condition that falls above or below the median of the control data (Lenz, 2013). PEM was chosen for its ease of calculation and because it does not rely on the most extreme score in the baseline/control condition (Parker et al., 2014). PEM is expressed as a value between 0 and 1, with higher values representing relatively stronger effects. Interpretation of effect sizes relied on the recommendations of Scruggs and Mastropieri (1998), who indicated that effect sizes below .50 should be considered ineffective, whereas those between .50 and .69 are questionably effective, those between .70 and .89 are moderately effective, and those ≥ .90 are highly effective.

Second, the TauU (Parker et al., 2011b) is a nonoverlap index that places equal emphasis on all data points. Thus, TauU is a “complete” nonoverlap index (Parker et al., 2014), which was chosen for inclusion in the present analysis because it is a useful supplement to PEM, an “incomplete” index that emphasizes a single score (i.e., the median) (Parker et al., 2014). Furthermore, the TauU was selected due to its appropriateness for “non-conforming, atypical, and very short data series” (Parker et al., 2014, p. 129). An additional asset of TauU is its suitability for meta-analysis (Parker et al., 2014), due to its underlying sampling distribution and standard error (Manolov & Moeyaert, 2017). It is worth bearing in mind, however, that TauU has several drawbacks, including inflated values that do not range between -1 and 1, and an inability to be graphed visually (Tarlow, 2017). TauU is calculated from all pairwise comparisons
between the A and B conditions (Parker et al., 2014). Specifically, for each pairwise comparison, a positive, negative, or neutral value is assigned depending on whether a Phase B data point is higher than, lower than, or tied with the preceding Phase A data point, respectively. A computerized calculation of TauU was used in the present analysis (http://www.singlecaseresearch.org/calculators/tau-u). Effect sizes for TauU are interpreted similarly to those for the nonoverlap of all pairs index, described below.

Third, the percentage of all nonoverlapping data (PAND) (Parker et al., 2007) was chosen because, like the original and most popular nonoverlap index, PND (Scruggs et al., 1987), it lends itself to hand calculation and typically demonstrates strong correspondence with visual analysis (Parker & Vannest, 2009). PAND is an improvement on PND, however, in that it does not rely on a single data point and possesses a sampling distribution (Parker et al., 2014). Representing the degree of nonoverlap between the data from two different conditions (Parker et al., 2007), PAND is calculated by determining the fewest number of data points that must be removed to completely separate the experimental and control data clouds (Parker et al., 2014). In this way, PAND represents the “percentage of data remaining after removing the fewest data points that would eliminate all overlap” (Parker et al., 2011, p. 310). The PAND value is scaled from 50 – 100, with 50 representing the level of chance (Parker et al., 2014). Effect sizes for PAND between .50 and .69 are considered questionably effective, .70 to .89 moderately effective, and ≥ .90 highly effective (Scruggs & Mastropieri, 1998).

Fourth, the nonoverlap of all pairs (NAP) index was chosen because, like TauU, it is a complete nonoverlap index, that is a “robust option for nonconforming, atypical, and very short data series” (Parker et al., 2014, p. 129). Furthermore, NAP has strong
precision power and good correspondence with visual judgments (Parker & Vannest, 2009). NAP uses all pairwise comparisons to generate an index representing the percentage of data that showed improvement from one condition to the other (Parker et al., 2014). Unlike TauU, which reflects the percentage of nonoverlapping data minus the percentage of overlapping data, NAP refers only to the percent of nonoverlap (Parker et al., 2014).

NAP values can range from 0.5 – 1.0 (Parker & Vannest, 2009), but can readily be rescaled to 0 – 1.0 using the calculation \( \frac{\text{NAP}}{0.5} - 1 \). Rescaling the NAP value serves to equate zero with chance level, such that negative scores represent a deteriorating performance from one condition to another (Parker et al., 2011a). The following guidelines are used for interpreting NAP (Parker & Vannest, 2009): 0 - .31 = weak effects; .32 - .84 = moderate effects, and .85 – 1.0 = strong effects. The guidelines for interpreting NAP may be used for TauU (without trend control or other variation) due to the similarity between these two indices (Parker et al., 2011b). A computerized calculation of NAP was used (http://www.singlecaseresearch.org/calculators/NAP).

The final step was a sensitivity analysis for each dyad. This analysis involves comparing results across the various visual and nonparametric analyses in order to determine the degree of consistency in the documented effects (Kratochwill et al., 2010). Consistency across the six analyses (visual analysis, randomization test, PEM, TauU, PAND and NAP) in each dyad was used to determine whether each of the three hypotheses was supported.
Results for Dyad 1

Test of Hypothesis 1: Effect of mindfulness practice on the client’s perceptions of therapist empathy (BLRI).

Visual analysis. The Empathy scores for the experimental and control sessions are presented in Figure 1. The level, trend and variability of the data were initially examined within each condition to ascertain whether they demonstrated consistency (Kratochwill et al., 2010), i.e., the extent to which the next three data points in the series could be predicted with confidence (Horner, 2013). Next, the variability, level, trend, and overlap of the data were compared between conditions, to evaluate whether the manipulation of the independent variable was associated with a change in the dependent variable (Horner, 2013; Kratochwill et al., 2010). The data were subsequently examined for evidence of a functional relation between the independent and dependent variables, as indicated by demonstrations of an experimental effect at three different points in time, and by the absence of non-effects, in other words, intervention failures (Kratochwill et al., 2010).

For Dyad 1, the within-condition comparisons revealed that the control condition demonstrated an inconsistent pattern of Empathy scores, due to high variability in the data (Fig. 2). The amount of variability may be ascertained by determining the percentage of data points that fall within a 15% range of the mean of all data points in the condition, with a percent of less than 80 reflecting high variability (Tawney & Gast, 1984, as cited in McCormick, 1995). With regard to the experimental data, the within-condition comparisons indicated adequate predictability due to the lack of trend, little variability (i.e., 100% of data points fell within 15% of the mean), and the observation that the level
well represented the data points (Fig. 3). In the control condition, however, high variability was demonstrated by only 40% of the data points falling within 15% of the mean. Thus, due to the inconsistent pattern of the control data, it would be difficult to predict the next three data points in the series with confidence (Horner, 2013). While visual judgment suggested the presence of a trend in the control condition (Fig. 4), the data did not demonstrate “three consecutive data points in the same direction,” which is the conventional method for detecting a trend (Barlow & Hersen, 1984, as cited in McCormick, 1995, p. 10).

The between-condition comparisons revealed a small difference in level, with higher Empathy scores in the sessions preceded by mindfulness practice compared with those preceded by the control activity. A carry-over effect was suggested by the observation that the Empathy scores in the control sessions were initially lower than those in the experimental sessions and increased with repeated implementation of the mindfulness intervention. In other words, the empathy experienced by the client in the randomly determined experimental sessions seems to have carried over to the client’s experience of the therapist’s empathy in the subsequent control sessions.

An experimental effect can also be determined by the proportion of overlap between the data in different conditions, where fewer overlapping data points indicate a relatively stronger effect (Kratochwill et al., 2010). To conduct this analysis, a horizontal line is inserted to mark the highest data point in the control condition. Then, the number of data points from the experimental condition that fall above the horizontal line reveals the extent of overlap. Figure 5 indicates a high degree of overlap (i.e., a low Percentage of Nonoverlapping Data; Scruggs, et al. 1987), in that only one Empathy score in the
experimental condition exceeded the single highest Empathy score in the control condition.

When the evidence for a functional relation between the independent and dependent variables was examined, the data revealed four demonstrations of an effect at different points in time. An effect may be understood as a deviation in the data pattern in one condition (e.g., the experimental condition) from what would be expected based on the data pattern in the previous condition (e.g., the control condition) (Horner et al., 2005, as cited in Kratochwill et al., 2010). Specifically, Empathy scores decreased in control Session 2, increased in experimental Session 4, decreased in control Session 5, and increased again in experimental Session 6. However, no effect was observed in experimental Sessions 8 and 10, when the Empathy scores were identical or decreased, respectively.

In summary, the visual analysis showed four demonstrations of an experimental effect at four different points in time, although non-effects were also observed. These findings, considered together with the observation that the control data did not demonstrate within-phase predictability, suggested weak to moderate evidence of a functional relation between mindfulness practice and client empathy ratings in this dyad. Of note, the data pattern in the control condition indicated the possible presence of a carry-over effect.

**Randomization test and nonoverlap analyses.** The randomization test result, $\Delta = 3.2, p = .03$, was statistically significant, indicating that eight of the values in the randomization distribution equaled or exceeded the test statistic of 3.2. That is, 242 data randomizations were generated (with the restriction that a maximum of 4 administrations
of the same condition could be applied consecutively), and 8 of these randomizations (i.e., 3%) provided a mean difference between the mindfulness and control condition that was equal to, or greater than, the value of 3.2 obtained from the data (i.e., $\bar{A} - \bar{B}$). This result signifies that the mean of the experimental condition was significantly higher than the mean of the control condition.

The four nonoverlap indices showed, first, that all five of the experimental data points exceeded the median of the five control data points (PEM). Thus, the effect size was 1.0, indicating a strong experimental effect of mindfulness practice on the client’s perception of the therapist’s level of empathy (Scruggs & Mastropieri, 1998). Second, the obtained TauU for the Empathy data in Dyad 1 was 0.72, indicating a moderate effect (Parker & Vannest, 2009). Third, based on a hand calculation of PAND (Parker et al., 2014), one of the 10 data points needed to be removed in order to separate the data clouds, resulting in a value of .90 (1 – removed/total data points, or $1 - (1/10) = .90$). The PAND value of 90% suggests a strong effect (Scruggs & Mastropieri, 1998). Finally, NAP = .86, representing a strong nonoverlapping effect (Parker & Vannest, 2009).

**Test of Hypothesis 2: Effect of mindfulness practice on the therapist’s report of hindering self-awareness (HSA).**

*Visual analysis.* As shown in Figure 6, the initial within-condition analysis of the HSA data revealed a predictable pattern of responding for the control sessions, for which there was little variability (i.e., 100% of data points fell within 15% of the mean) (Fig. 7) and the level was a good representation of the data (Fig. 8). Poor consistency was demonstrated in the experimental sessions, however, in that high variability was observed (i.e., 60% of data points falling within 15% of the mean) (Fig. 7), rendering the level
(Fig. 8) a poor summary of the data. While visual judgment suggests the presence of a downward trend in the experimental condition (Fig. 9), the data did not demonstrate three consecutive data points in the same direction.

The between-condition analysis showed a discrepancy between the experimental and control levels of the HSA data (Fig. 8), indicating that the therapist reported a higher mean rating of hindering self-awareness in the control sessions. The impact of this difference was undermined somewhat by the variability in the HSA data from the experimental sessions.

The apparent difference in trend between the two conditions (Fig. 9) provided strong support for the experimental effect. That is, while the HSA scores remained relatively constant in the control sessions, they decreased over time in the experimental sessions. Although a trend was not specifically hypothesized, these results supported the predicted effect of mindfulness practice in decreasing HSA.

The proportion of overlap is shown in Figure 10. Because it was hypothesized that HSA scores would be lower in the mindfulness condition, a horizontal line was inserted in the figure to mark the lowest data point in the control condition. In contrast to the analyses of the other measures, the number of experimental data points that fell below the horizontal line was used to assess the amount of overlap. Results indicated that 90% of the data were nonoverlapping, which is considered a significant effect (Parker et al., 2014).

Moreover, six demonstrations of an effect were observed at different points in time, providing strong evidence for a functional relation between the independent and dependent variable. Specifically, HSA scores decreased in experimental Sessions 4, 6,
and 9, and increased in control Sessions 5, 8, and 10. One non-effect was observed when HSA decreased from experimental session 1 to control session 2.

In summary, the visual analysis showed a large percentage of nonoverlap and six demonstrations of an effect in this dyad. Taken together, the presence of one non-effect and some inconsistency in the experimental data indicated a moderate rather than a strong effect.

**Randomization test and nonoverlap analyses.** Since decreased HSA was hypothesized for the experimental sessions, the test statistic was derived by subtracting the mean of the experimental scores from the mean of the control scores. The resulting value for the difference between the means of the experimental and control conditions was $\Delta = 0.42$, $p = .05$. That is, 13 of the 242 data randomizations (i.e., 5%) provided a mean difference between the mindfulness and control condition that was equal to, or greater than, the test statistic of 0.42 obtained from the data (i.e., $B - A$). This result signified that the mean of the experimental condition was significantly lower than the mean of the control condition.

In terms of the nonoverlap analyses, PEM results indicated that 4 of the 5 experimental data points fell below the median of the control data points, yielding a moderate effect size of .80. TauU was -0.60, also a moderate effect, with the negative value indicating that HSA scores generally decreased as predicted. The PAND value was .90, indicating that 90% of the data was nonoverlapping and representing a strong effect. Finally, the NAP value was .80 ($p = .14$), suggesting a moderate effect.
Test of Hypothesis 3a: Effect of mindfulness practice on the therapist’s perceptions of the real relationship (RRI-T).

Visual analysis. Figure 11 showed predictable patterns of RRI-T scores for both conditions. In each condition there was little variability (i.e., 100% of data points fell within 15% of the mean) (Fig. 12), rendering the level an adequate representation of the data (Fig. 13). Evidence of an upward trend was present in the experimental data (i.e., three consecutive data points in the same direction) (Fig. 14).

Between-condition comparisons indicated a gap between the levels in each dyad (Fig. 13), demonstrating that the therapist provided a higher mean rating of the real relationship in the experimental sessions as compared with the control sessions. As shown in Figure 15, minimal overlap was observed between the two conditions, with 4 of the 5 experimental data points exceeding the highest data point in the control condition, indicating a strong effect of .80.

Seven demonstrations of an experimental effect were observed. Specifically, RRI-T scores decreased in control Sessions 2, 5, 8, and 10, whereas the scores increased in experimental Sessions 4, 6, and 9. There was no demonstration of non-effects.

In sum, the visual data from the therapist’s rating of the real relationship was consistent with predictions. Both conditions showed predictable data patterns, the therapist provided a higher mean rating of the real relationship when mindfulness was practiced, there was little data overlap between conditions, and there were seven demonstrations of an effect with no intervention failures.

Randomization test and nonoverlap analyses. For the randomization test, the mean difference between the experimental and control conditions was $\Delta = 9.2, p = .01$. That is,
3 of the possible 242 randomizations (i.e., 1%) provided a mean difference between the mindfulness and control condition that was equal to, or greater than, the value of 9.2 obtained from the data. This result signifies that the mean for the experimental condition was significantly higher than the mean for the control condition.

In terms of PEM, all 5 of the RRI-T data points in the experimental condition exceeded the median of the scores in the control condition, indicating a strong effect of 1.00. TauU = 0.88, representing a strong effect. One data point needed to be removed to eliminate all overlap between the two conditions resulting in PAND = .80 (i.e., 80% nonoverlapping data), which is a moderate effect. Finally, NAP = .94 (p = .02), which represented a strong effect.

**Test of Hypothesis 3b: Effect of mindfulness practice on the client’s perceptions of the real relationship (RRI-C).**

*Visual analysis.* Figure 16 shows the client’s ratings on the Real Relationship Inventory (RRI-C) across the two conditions. Within-condition comparisons revealed adequate consistency, in that there was little variability in both conditions (i.e., 100% of data points fell within 15% of the mean) (Fig. 17), and the levels were good representations of the data (Fig. 18). The data in the experimental condition demonstrated a downward trend (i.e., three consecutive data points in the same direction) (Fig. 19).

Between-condition comparisons did not provide support for an experimental effect. Contrary to prediction, the client provided a higher mean rating of the real relationship in the control condition, although the between-level discrepancy was small. The data trend in the experimental condition was also inconsistent with the prediction, in that the experimental sessions demonstrated a decline in RRI-C scores. Furthermore,
Figure 20 revealed a 100% overlap between the conditions, indicating that none of the experimental data points exceeded the highest control data point.

Whereas there were three demonstrations of an experimental effect (i.e., RRI-C scores increased when mindfulness practice was implemented at Sessions 4 and 6, and decreased when the control activity was implemented at Session 5), there were also four intervention failures. The level differences and trend were not in the hypothesized direction, there was a large amount of overlap, scores in the experimental condition decreased over time, and there were multiple demonstrations of non-effects. Based on these results, no further analyses were conducted.

Results for Dyad 2

Test of Hypothesis 1: Effect of mindfulness practice on the client’s perceptions of empathy (BLRI).

Visual analysis. As shown in Figure 21, there were only four Empathy data points in the experimental condition. Within-condition comparisons indicated that the control data did not follow a predictable pattern. Specifically, the data demonstrated high variability (i.e., 40% of the data points fell within 15% of the mean) (Fig. 22), and the level was not representative of the data (Fig. 23). As shown in the respective figures, the experimental data demonstrated greater consistency, in that the level was a good reflection of the data points and the variability was low (i.e., 100% of the data points fell within 15% of the mean). It was not possible to predict the next three data points with confidence, however, since there were only four data points in the series. Neither the control nor the experimental data demonstrated a notable trend (i.e., there was an absence of three consecutive data points in the same direction) (Fig. 24).
Between-condition comparisons revealed a small discrepancy between the levels in the two conditions, with the client providing somewhat higher Empathy scores after the experimental sessions. The meaningfulness of this finding was called into question, however, by the inconsistency in the control data, along with the limited number of experimental data points.

Figure 25 shows complete overlap, as none of the four experimental data points exceeded the highest control data point. An examination of consistency across the conditions suggested that the two data patterns were indicative of a carry-over effect in empathy scores (similar to the pattern observed in Dyad 1). That is, the last two control scores, which were obtained after two experimental sessions, were substantially higher than the other control scores, which were obtained prior to the manipulation.

Finally, only one experimental effect was observed: the Empathy score increased in experimental Session 5. On the other hand, several intervention failures were observed. That is, Empathy scores remained constant irrespective of whether the session was preceded by the mindfulness practice or a control activity.

In sum, there was no support for the presence of an experimental effect based on the visual analysis. For this reason, no further analyses were conducted.

**Test of Hypothesis 2: Effect of mindfulness practice on the therapist’s report of hindering self-awareness (HSA).**

*Visual analysis.* The therapist’s HSA scores in the two conditions are shown in Figure 26. The experimental condition demonstrated good consistency, in that there was little variability (i.e., 100% of data points fell within 15% of the mean) (Fig. 27), rendering the level a representative summary of the data (Fig. 28). By contrast, in the
control condition the level was a poor summary of the data (Fig. 28) due to the substantial variability within this condition (i.e., 60% of data points fell within 15% of the mean) (Fig. 27). Neither the control nor the experimental data demonstrated a notable trend (i.e., there was an absence of three consecutive data points in the same direction) (Fig. 29).

The between-condition comparison showed a difference in levels for the two conditions, with the therapist’s HSA score higher in the control condition. On the other hand, the inconsistency in the control data limited the meaningfulness of this finding.

Figure 30 indicates complete overlap between the two conditions, as none of the experimental data points fell below the horizontal line. Four demonstrations of an experimental effect were observed at different points in time. That is, HSA scores decreased when mindfulness practice was implemented before Sessions 5, 8 and 10, and increased when the control activity was implemented before Session 9. There were two intervention failures.

In summary, the findings for HSA in Experimental Dyad 2 did not provide sufficient evidence of an effect to proceed with effect size estimation, due to the inconsistency of the control data, the high degree of overlap between conditions, and the intervention failures.

**Test of Hypothesis 3a: Effect of mindfulness practice on the therapist’s perceptions of the real relationship (RRI-T).**

*Visual analysis.* Figure 31 shows the therapist’s ratings on the Real Relationship Inventory for the two conditions. As noted earlier, there were only four control data points because the therapist neglected to complete the RRI-T after Session 3.
As evident from within-condition comparisons, both the experimental and control conditions demonstrated little variability (i.e., 100% of data points fell within 15% of the mean) (Fig. 32), rendering the levels representative of the data (Fig. 33). Thus, both conditions demonstrated adequate stability, but the control condition did not contain sufficient data to document a predictable pattern of responding. That is, the limited number of data points made it difficult to predict the subsequent data points with confidence. Neither the control nor the experimental data demonstrated a notable trend (i.e., there was an absence of three consecutive data points in the same direction) (Fig. 34).

The between-condition comparison revealed a substantial gap between the levels (Fig. 33), indicating that the therapist provided relatively higher mean RRI-T ratings after the experimental sessions. Three of the five experimental data points (60%) exceeded the highest data point in the control condition (Fig. 35), also indicating the presence of an effect.

Moreover, the data revealed six demonstrations of an effect at different points in time. Specifically, whereas the RRI-T scores decreased at control Sessions 2, 7 and 9, the scores increased at experimental Sessions 5, 8, and 10. There were no demonstrations of non-effects.

Taken together, the visual analysis provided moderate evidence for an effect of the mindfulness intervention on the therapist’s perceptions of the real relationship. This conclusion was supported by the difference in levels between the conditions, the amount of nonoverlap, and the presence of six basic effects with no intervention failures. The presence of only four control data points indicated that the data did not meet criteria for
strong evidence. It was concluded, however, that there was sufficient evidence to conduct the subsequent nonparametric tests.

**Randomization test and nonoverlap analyses.** As there were 9 observations rather than 10, it was not possible to calculate a randomization test since the R software requires a balanced design. In the PEM analysis, all 5 data points in the experimental condition exceeded the median of the RRI-T scores in the control condition, yielding a strong effect of 1.0. The obtained TauU = 0.80 indicated a moderate effect. The obtained PAND value was .78 (i.e., 78% nonoverlapping data), which was a moderate effect. Finally, the obtained NAP = .90, which represented a strong effect.

**Test of Hypothesis 3b: Effect of mindfulness practice on the client’s perceptions of the real relationship (RRI-C).**

**Visual analysis.** The client’s scores on the Real Relationship Inventory for the two conditions are shown in Figure 36. The pattern of responding was fairly consistent in both conditions, in that there was little variability (i.e., 100% of data points fell within 15% of the mean) (Fig. 37), rendering the level an adequate summary of the data (Fig. 38). There was an upward trend for the experimental condition (i.e., three consecutive data points in the same direction) (Fig. 39).

The between-condition comparison revealed a slightly higher mean RRI-C score in the experimental condition as compared with the control condition (Fig. 38), as predicted. Regarding the amount of overlap, only one experimental data point exceeded the highest control data point (Fig. 40), indicating no observed effect.

On the other hand, four demonstrations of an experimental effect were found. Specifically, RRI-C scores increased when mindfulness practice was implemented at
Sessions 5, 8 and 10, and decreased when the control activity was implemented at Session 9. There were two experimental failures, in which RRI-C scores increased or remained the same (in control Sessions 2 and 7, respectively).

In summary, some evidence of an effect emerged in that RRI-C scores were, on average, higher in the experimental sessions. Moreover, four demonstrations of an effect were observed. However, the degree of overlap was high, and there were two demonstrations of non-effects. Thus, the visual analysis showed a moderate experimental effect.

**Randomization test and nonoverlap indices.** The result of the randomization test was \( \Delta = 6.6, \ p = .11 \). That is, 26 of the possible 242 randomizations (i.e., 11\%) provided a mean difference between the mindfulness and control condition that was equal to, or greater than, the value of 6.6 obtained from the data. This result signifies that the mean for the experimental condition was not significantly higher than the mean for the control condition.

In terms of the nonoverlap indices, 4 of the 5 data points (80\%) exceeded the median RRI-C score in the control condition. This PEM value suggested a moderate experimental effect. Similarly, \( \text{TauU} = 0.48 \), indicating a moderate effect. Three data points needed to be removed to eliminate all overlap, resulting in \( \text{PAND} = .70 \) (i.e., 70\% nonoverlapping data), also indicating a moderate effect. A moderate effect was also observed with \( \text{NAP} = 0.74 \).

**Sensitivity Analysis**

For each dependent variable in each dyad, six estimators (visual analysis, randomization test, PEM, TauU, PAND and NAP) were reviewed to evaluate whether the
respective hypothesis was supported. It should be noted that the rejection of the null hypothesis and the determination of the effect size was not based on any single estimator, but rather on the cumulative evidence provided by all six estimators for each dependent variable. A summary of these determinations is provided in Tables 3 and 4.

**Dyad 1.** In terms of client-reported empathy, all of the results (visual analysis of Empathy data, a significant randomization test, and moderate to strong effects on all four nonoverlap indices) were highly consistent. It was concluded that the mindfulness intervention exerted a moderate effect on the client’s perception of the therapist’s empathy in this dyad, supporting Hypothesis 1. Of note, a carry-over effect was evident in the visual analysis, in that the scores in the control condition increased with repeated implementation of the therapist’s mindfulness practice.

In terms of the therapist’s reported level of hindering self-awareness, Hypothesis 2 was also supported in the visual data, in all four of the nonoverlap analyses (which indicated moderate to strong effects), and in the statistically significant randomization test ($p = .05$). Taken together, these data indicated a moderate effect of the mindfulness intervention on the therapist’s experience of hindering self-awareness, supporting Hypothesis 2.

The two hypotheses concerning the real relationship scores were not consistent. The RRI-T visual data supported Hypothesis 3a, as did the significant randomization test and the four nonoverlap indices. By contrast, the visual analysis of the RRI-C data failed to support Hypothesis 3b, indicating that the experimental intervention exerted no effect on the client’s perception of the real relationship in this dyad.

**Dyad 2.** In this dyad, the Empathy data failed to support Hypothesis 1. However,
similar to Dyad 1, the data pattern was indicative of a carry-over effect in Empathy scores. The HSA data also provided no support for Hypothesis 2.

With respect to the therapist-rated real relationship, the visual analysis of the RRI-T data provided support for Hypothesis 3a, as did the moderate to strong effects for all four nonoverlap indices. Based on the general consistency of these results, it was concluded that the mindfulness intervention exerted a moderate to strong effect on the therapist’s perception of the real relationship in this dyad.

On the other hand, the client-rated real relationship provided only partial support for Hypothesis 3b. Specifically, the visual analysis of the RRI-C data and all four nonoverlap indices provided moderate support for the hypothesis, but the randomization test was not significant ($p = .11$). Overall, it was concluded that these results documented a moderate effect.

**Summary of Results**

As shown in Table 3, Hypotheses 1, 2 and 3a were supported in Dyad 1. In Dyad 2, on the other hand, only Hypothesis 3a received clear support, and Hypothesis 3b received partial support, as shown in Table 4.
An emerging body of research suggests that mindfulness practice may help therapists enhance their “in-the-moment stance or attitude towards clients” (Fife et al., 2014, p. 21) and thereby improve their in-session effectiveness (e.g., Dunn et al., 2013; Fulton, 2016; Greason & Welfare, 2013; Ivanovic et al., 2015). In line with this thinking, the present study was designed to extend our understanding of the immediate effects of brief mindfulness practice on a therapist’s way of being with clients. Specifically, it was hypothesized that by practicing mindfulness immediately before a session begins, a therapist would experience less hindering self-awareness during the session, the client would perceive the therapist as more empathic, and the therapeutic relationship would be seen as more “real” (i.e., genuine and realistic) by both parties. All of these in-session phenomena have been shown to be associated with therapeutic effectiveness (e.g., Fauth & Williams, 2005; Norcross & Wampold, 2011; Lo Coco et al, 2011).

In a replicated single-case design with two therapy dyads, the therapists practiced a brief, pre-session mindfulness exercise in 5 of 10 randomly determined sessions after having received specific training in mindfulness practice. Scores on the three outcome measures, administered immediately following each therapy session, were compared across the experimental and control (no mindfulness) sessions.

Similar to a previous group investigation on novice therapists’ mindfulness practice (Dunn et al., 2013), post-session data were collected from both therapists and clients, and the clients had no knowledge of the experimental manipulation. In contrast to Dunn et al., however, the single-case experimental design allowed for a replicated test of causality, in that each therapist served as his or her own control. Moreover, the present study extended
Dunn et al.’s investigation of the impact of mindfulness on therapist presence and session quality by assessing therapists’ levels of hindering self-awareness and two relational phenomena that have repeatedly been associated with therapeutic success, client-perceived empathy and the quality of the real relationship.

The graphed data were initially examined visually. If an experimental effect seemed evident, the next steps were a randomization test to compare the mean scores for the experimental (mindfulness practice) versus control (no mindfulness) sessions, and the computation of four nonoverlap indices. Results indicated that the two therapy dyads demonstrated differing levels of support for the three hypotheses. Specifically, comparisons of the experimental to control sessions in Dyad 1 showed that when mindfulness was practiced prior to the session, (a) the client perceived the therapist to be more empathic, and (b) the therapist reported less hindering self-awareness during the session and (c) rated the quality of the real relationship more favorably. These experimental effects were moderate to strong. By contrast, in Dyad 2, weak to moderate experimental effects were found for both the client’s and the therapist’s perceptions of the real relationship. The hypotheses regarding empathy and hindering self-awareness were not supported.

It is worth noting a general discrepancy between the two dyads. Dyad 2 provided more extreme scores than Dyad 1 across all three dependent measures. That is, regardless of the experimental manipulation, the Dyad 2 therapist reported notably less hindering self-awareness, was seen as more empathic by the client, and the relationship was perceived as more genuine and “real” by both parties. The somewhat differing results for the two dyads might have been due to a ceiling effect on the dependent measures in Dyad
2. That is, even in the absence of mindfulness practice, the Dyad 2 therapist experienced relatively little hindering self-awareness during therapy, he was rated as highly empathic by the client, and the real relationship was seen as solid by both parties.

The notable discrepancy in scores across the two cases suggests another explanation for the results. It seems that mindfulness practice can enhance the therapeutic process more so for some therapists than others, due to differences in dyad composition (based on gender or other demographic factors), in the clients (e.g., presenting concern, readiness for change), or in the therapists themselves. The generally lower scores in Dyad 1 suggest that only in this dyad was there room for positive change as a result of practicing mindfulness, possibly because the therapist was particularly challenged in working with this client. Alternately, the Dyad 2 therapist identified his approach as cognitive/integrative, whereas the Dyad 1 therapist identified her orientation as partly client-centered. It is possible that the “being with,” non-judgmental aspect of mindfulness practice may resonate more strongly for therapists who approach therapeutic work from a humanistic perspective.

Regardless of how the two cases differed, the present results underscore Heppner et al.’s (2016) observation that clients and therapists “process information about themselves, their experiential worlds, and counseling in idiographic ways, and even differently from one time to the next” (p. 349). In other words, the different findings in the two dyads highlight how the complex, idiographic nature of psychotherapy can be investigated in a replicated single-case design, in which observations are contrasted over time and across dyads.
Implications for Theory and Practice

Based on the theoretical interrelations among the dependent variables, one would expect empathy and the real relationship to increase in concert, and hindering self-awareness to decrease. In Dyad 1, results were fairly consistent with this expectation. In Dyad 2, where an experimental effect was found only for the real relationship but not for empathy or hindering self-awareness, the results did not support the theoretical interrelations among the dependent variables.

Consistent with the literature (e.g., Lesh, 1970; Keane, 2013; Greason & Cashwell, 2009; Greason & Welfare, 2013; Fulton, 2016), the results from Dyad 1 suggested that a novice therapist’s level of empathy, as perceived by the client, can be positively affected by mindfulness practice. This finding has potential implications for clinical training. That is, empathy is generally valued as a “vital and universal therapeutic skill” (Shapiro & Izett, 2008, p. 162). The experimental effect on perceived empathy in one of the two dyads lends support to the assertion that mindfulness practice can be an effective means for teaching this important skill to therapy trainees (Bibeau et al., 2016; Fulton, 2005; Shapiro & Izett, 2008).

Notably, this was the first study to investigate the effect of mindfulness practice on hindering self-awareness. The effect of reduced HSA in the experimental sessions in one of the dyads extends the literature by demonstrating a functional relation between the two phenomena, at least in one therapy case. That is, mindfulness practice, which helps a person develop non-reactivity, may help some therapists become more effective at recognizing and managing their internal distractions, allowing them to be more fully present with their clients in the moment (Fatter & Hayes, 2013; Greason & Cashwell,
It is possible that one therapist in this study had a greater ability than the other to be nonreactive to begin with. Notably, Dyad 2’s therapist reported less difficulty with hindering self-awareness than did the average participant in Williams et al.’s (2003) HSA validation sample. For this reason, his relatively low HSA scores in the control sessions may account for the contrasting experimental effects for HSA in the two cases.

Moreover, the present adaptation of the Hindering Self-Awareness Scale (Williams et al., 2003), in which therapists were directed to report their experience of hindering self-awareness at the session level (rather than in general), represents a specific contribution of this study. That is, the results from both dyads indicate that HSA levels fluctuate across sessions, a finding that has implications for future research on this important therapist variable, such as its relation to session effectiveness. The absence of an experimental effect for this variable in Dyad 2, however, highlights the importance of replication with both single-case and group designs.

The present study was the first to investigate the immediate effects of brief mindfulness practice on perceptions of the therapeutic relationship. In particular, the construct of interest was the real relationship (Gelso, 2011), which is conceptually linked to mindfulness practice in that the two aspects composing the construct are genuineness and realism. Although previous research showed positive associations between therapist mindfulness and the working alliance (Greason & Welfare, 2013; Ryan et al., 2012; Wexler, 2006), a conceptually different aspect of the therapeutic relationship, no other studies have investigated the real relationship as a consequence of therapist mindfulness.

A significant effect was found in both dyads for the real relationship from the
therapist’s perspective, extending the literature on this important construct. An effect was found for the client’s perception of the real relationship in Dyad 2, but not in Dyad 1. In the latter dyad, positive changes in the therapist’s perception of the real relationship as a result of mindfulness practice are rendered less meaningful by the absence of the client’s matching perception. Like Rogers’s (1957) specification that empathy must be experienced by the client in order to be effective, we reasoned that the client’s perception of improvement in the real relationship would be necessary to support the hypothesis regarding the effect of therapist mindfulness practice on this relational variable. On the other hand, the experimental effects observed in Dyad 2 for both client- and therapist-rated real relationship quality suggests that brief mindfulness practice before a session begins may be able to effect changes in this aspect of the relationship in some dyads.

In sum, the results from this study have the potential to extend the literature on the benefits of therapist mindfulness practice, although undoubtedly, further study is needed to better understand its effect on empathy, hindering self-awareness, and perceptions of the real relationship. Although differing aspects of the therapeutic process were affected in each dyad, the findings across dyads suggest that therapist mindfulness practice may be able to enhance way of being in some therapy trainees. These results add to the incipient body of literature (e.g., Dunn et al., 2013) supporting brief mindfulness practice as an accessible tool that therapists can use as part of their pre-session preparation routine, if they have had some prior training in mindfulness practices.

**Strengths and Limitations**

A strength of this study was the use of a replicated single-case design, which permitted an experimental investigation of the effects of therapist mindfulness practice in
different dyads. Unlike large-N designs, which can obscure individual differences through the reliance on group means, single-case designs are ideally suited to exploring the “complex and highly variable” nature of the therapy process (Heppner et al., 2016, p. 349). Since each dyad serves as its own control, various threats to internal validity are mitigated, permitting inferences about a functional relationship. Of course, since it is not possible to eliminate all potential confounds (e.g., maturation, history, convenience assignment of clients to therapists), the present study may be more accurately described as quasi-experimental.

In addition to replication across cases, the experimental design allowed mindfulness practice to be manipulated systematically by randomly assigning the five experimental and five control sessions differently in the two dyads. Each dyad had at least four data points in each condition, and the randomization schedules permitted five repetitions of the alternating sequence of experimental and control sessions. Additionally, three psychometrically sound measures were administered under the same conditions after each session, and manipulation checks (after the mindfulness training workshop and before each session) contributed to the study’s validity. Yet another strength of the study was that the clients, who were unaware of the experimental manipulation, reported their perceptions of the therapist’s empathy and the quality of realism and genuineness in the therapeutic relationship after each session. Taken together, these features satisfied the design requirements necessary to meet evidence standards for single-case research (Kratochwill et al., 2010).

Two aspects of the study that were informed by Dunn et al. (2013) also added to its strength. First, the temporal proximity of mindfulness practice (i.e., immediately before
sessions) allowed for confident inferences about its effects. Second, data were collected from both the clients’ and therapists’ perspectives, in contrast to previous studies of mindfulness practice that relied exclusively on therapist self-report (e.g., Greason & Cashwell; 2009; Keane, 2013). Particularly with regard to therapist empathy, which must be perceived by the client in order to be effective (Rogers, 1957), obtaining the client’s perspective was particularly important.

On the other hand, one limitation of this study was low statistical power, which may have contributed to the nonsignificant randomization test of the client-perceived real relationship in Dyad 2, despite the observed moderate effect sizes in the other indices for this variable. The use of only two dyads, rather than multiple replications across a greater number of therapists and clients, diminishes confidence in the conclusions. Moreover, a lack of generalizability is a notable limitation of single-case designs. The purpose of single-case research in psychotherapy, however, is to shed light on therapeutic processes as they unfold over time and to advance theory by illuminating mechanisms of change within a specific dyad, rather than to generalize results across settings and participants.

Further limitations pertained to the likelihood of ceiling and carry-over effects. Dyad 2’s Empathy scores were high in both the experimental and control sessions, making it difficult to detect differences on this measure. Moreover, both dyads demonstrated a carry-over effect for empathy. That is, Empathy scores on the BLRI-OS-Emp+ in the control condition increased after mindfulness was practiced in the experimental sessions. It makes sense that once empathy is improved due to mindfulness practice, it is unlikely that the client would perceive a difference in the therapist’s attunement during subsequent control sessions.
Indeed, the strong likelihood of carry-over effects makes it difficult to conduct single-case experiments on relational variables like empathy, which is one reason why so few alternating treatments designs are conducted on psychotherapeutic processes (Heppner et al., 2016). On the other hand, the absence of carry-over effects for real relationship quality and hindering self-awareness, which were also assessed as session variables, demonstrates the potential value of using an alternating treatments design to study therapeutic change.

Although a strength of the study was the administration of measures to both clients and therapists, the reliance on therapist self-report data was an additional limitation. Since the therapists were well aware that the experiment involved testing the effect of mindfulness practice, subject expectancy needs to be acknowledged. Similarly, social desirability constituted another validity threat, since the therapists were undoubtedly aware of the importance of the therapeutic process variables under investigation.

The element of subjectivity inherent in the visual analysis of single case data may constitute a further limitation in the present study. Despite the introduction of formal design and evidence standards for single-case research (Kratochwill et al., 2010), the determination of effect size based on visual analysis still relies on a researcher’s subjective judgment (Horner, 2013). On the other hand, the decision to make inferences from the findings did not rely solely on visual analyses in the present study. That is, the inclusion of randomization tests and nonoverlap indices strengthened conclusions about which of the hypotheses were or were not supported in each dyad.

Finally, this study presents mindfulness as a brief, stand-alone intervention that is divorced from the “contextualized set of attitudes, exercises and practices” in which it
ideally should be embedded, according to the traditional Buddhist perspective (Brito, 2014, p. 353). While appropriate for a psychotherapy training context in which time is limited, the use of a three-minute mindfulness practice may be considered a “reductionist” approach that does not facilitate a “culturally informed understanding of mindfulness – and the spiritual nature of this practice” (Brito, 2014, p. 352). In other words, the lack of support for some of the hypotheses may be because the experimental condition was not powerful enough to result in all of the predicted effects across dyads.

**Recommendations for Future Research**

Consideration of the strengths and limitations of the present study suggests several clear directions for future research in this area. First, it would be advisable to replicate the experiment in additional dyads, not only among therapy trainees but also among experienced practitioners. Second, using a different single-case design might prevent carry-over effects, such as a multiple baseline design in which mindfulness practice is introduced at randomly-determined start points. Third, to offset the subject expectancy effect, behavioral data could be obtained, such as the observed number of empathic statements made by the therapist in each session. Interview data may also be useful to shed light on why mindfulness practice might have a greater influence on some therapists than others. Finally, the inclusion of outcome data in future studies could determine whether the therapeutic processes affected by the therapist’s mindfulness practice ultimately affect the success of the treatment.

Finally, future research might be planned to better honor the cultural and contextual roots of mindfulness practice (cf. Brito, 2014). For example, therapist participants could be supported in sustaining and extending their mindfulness practice after the study period.
through ongoing mindfulness groups. Such an approach may promote the “personal transformation and the cultivation of wisdom and compassion” that Brito (2014, p. 357) contended are necessary for mindfulness to have a strong impact on the therapeutic process.

**Conclusion**

On the whole, the present findings suggest that brief, pre-session mindfulness practice (following an introductory training in mindfulness) may be a useful tool for cultivating empathy, reducing hindering self-awareness, and improving perceptions of the real relationship. Collectively, these effects may enhance a therapist’s way of being, which is integral to successful psychotherapy. Replication with additional dyads is needed to advance this novel area of research.
References


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Integration, 7, 291-312.


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Psychology and Psychotherapy, 24, 575-588.


Table 1.

*Scores on the Study Variables by Dyad*

<table>
<thead>
<tr>
<th>Session</th>
<th>Empathy</th>
<th>HSA</th>
<th>RRI-T</th>
<th>RRI-C</th>
<th>Session</th>
<th>Empathy</th>
<th>HSA</th>
<th>RRI-T</th>
<th>RRI-C</th>
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<td>1*</td>
<td>17</td>
<td>2.7</td>
<td>91</td>
<td>90</td>
<td>1*</td>
<td>--</td>
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<td>91</td>
</tr>
<tr>
<td>2</td>
<td>12</td>
<td>2.4</td>
<td>85</td>
<td>94</td>
<td>2</td>
<td>29</td>
<td>1.6</td>
<td>90</td>
<td>94</td>
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<td>88</td>
<td>84.5</td>
<td>3</td>
<td>32</td>
<td>2.3</td>
<td>--</td>
<td>90</td>
</tr>
<tr>
<td>4*</td>
<td>18</td>
<td>1.9</td>
<td>96</td>
<td>93</td>
<td>4</td>
<td>23</td>
<td>2.1</td>
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<td>5*</td>
<td>34</td>
<td>1.7</td>
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<td>100</td>
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<td>20</td>
<td>2.2</td>
<td>99</td>
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<td>6*</td>
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</tbody>
</table>

*Note.* Mindfulness sessions. Empathy = Barrett-Lennard Relationship Inventory – Form OS Emp+ (Barrett-Lennard, 2015); HSA = Hindering Self-Awareness Scale (Williams, Hurley, O'Brien, & DeGregorio, 2003); RRI-T = Real Relationship Inventory – Therapist (Gelso et al., 2005); RRI-C = Real Relationship Inventory – Client (Kelley, Gelso, Fuertes, Marmarosh, & Lanier, 2010)
Table 2.

*Comparison of Mean Scores on Study Variables for Experimental vs. Control Conditions by Dyad*

<table>
<thead>
<tr>
<th></th>
<th>Dyad 1</th>
<th></th>
<th>Dyad 2</th>
<th></th>
</tr>
</thead>
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<tr>
<td></td>
<td>Empathy</td>
<td>HSA</td>
<td>RRI-T</td>
<td>RRI-C</td>
</tr>
<tr>
<td><strong>Experimental</strong></td>
<td>18.2</td>
<td>2.0</td>
<td>97.6</td>
<td>86.8</td>
</tr>
<tr>
<td><strong>Control</strong></td>
<td>15.0</td>
<td>2.4</td>
<td>88.4</td>
<td>88.5</td>
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</table>
Table 3.

*Results for Dyad 1*

<table>
<thead>
<tr>
<th>Visual analysis effect</th>
<th>Rand. test</th>
<th>PEM</th>
<th>TauU</th>
<th>PAND</th>
<th>NAP</th>
<th>H_A support</th>
<th>Overall effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>Empathy Weak/moderate†</td>
<td>0.03*</td>
<td>1.00</td>
<td>0.72</td>
<td>0.90</td>
<td>0.86</td>
<td>Yes</td>
<td>Moderate</td>
</tr>
<tr>
<td>HSA Moderate</td>
<td>0.05*</td>
<td>0.80</td>
<td>-0.60</td>
<td>0.90</td>
<td>0.80</td>
<td>Yes</td>
<td>Moderate</td>
</tr>
<tr>
<td>RRI-T Strong</td>
<td>0.01*</td>
<td>1.00</td>
<td>0.88</td>
<td>0.80</td>
<td>0.94</td>
<td>Yes</td>
<td>Strong</td>
</tr>
<tr>
<td>RRI-C None</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>No</td>
<td>None</td>
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</table>

*Notes.* Rand. = significance of randomization test. PEM = percentage of data exceeding the median; PAND = percentage of all nonoverlapping data; NAP = nonoverlap of all pairs. For PEM and PAND, effect sizes .50–.69 are considered weak, .70–.89 are considered moderate, and ≥ .90 are considered strong (Scruggs & Mastropieri, 1998). For TauU and NAP, effect sizes 0–.31 are considered weak, .32–.84 are considered moderate, and 85–1.0 are considered strong (Parker & Vannest, 2009).

* p ≤ .05. † Suggestive of a carry-over effect.
Table 4

*Results for Dyad 2*

<table>
<thead>
<tr>
<th>Visual analysis effect</th>
<th>Rand. test</th>
<th>PEM</th>
<th>TauU</th>
<th>PAND</th>
<th>NAP</th>
<th>H_A support</th>
<th>Overall effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>Empathy</td>
<td>None†</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>No</td>
<td>None</td>
</tr>
<tr>
<td>HSA</td>
<td>None</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>No</td>
<td>None</td>
</tr>
<tr>
<td>RRI-T</td>
<td>Moderate</td>
<td>--</td>
<td>1.00</td>
<td>0.80</td>
<td>0.78</td>
<td>0.90</td>
<td>Yes</td>
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<tr>
<td>RRI-C</td>
<td>Moderate</td>
<td>0.11</td>
<td>0.80</td>
<td>0.48</td>
<td>0.70</td>
<td>0.74</td>
<td>Partial</td>
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</table>
Figure 1. Dyad 1: Client ratings on Empathy for experimental and control session

Figure 2. Dyad 1: Variability for client ratings on Empathy for experimental and control sessions
Figure 3. Dyad 1: Level for client ratings on Empathy for experimental and control sessions

Figure 4. Dyad 1: Trend for client ratings on Empathy for experimental and control sessions
Figure 5. Dyad 1: Overlap for client ratings on Empathy for experimental and control sessions

Figure 6. Dyad 1: Therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions
Figure 7. Dyad 1: Variability for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions.

Figure 8. Dyad 1: Level for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions.
Figure 9. Dyad 1: Trend for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions

Figure 10. Dyad 1: Overlap for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions
Figure 11. Dyad 1: Therapist ratings on RRI-T (Real Relationship) for experimental and control sessions

Figure 12. Dyad 1: Variability for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions
**Figure 13.** Dyad 1: Level for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions

**Figure 14.** Dyad 1: Trend for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions
Figure 15. Dyad 1: Overlap for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions

Figure 16. Dyad 1: Client ratings on RRI-C (Real Relationship) for experimental and control session
Figure 17. Dyad 1: Variability for client ratings on RRI-C (Real Relationship) for experimental and control sessions

Figure 18. Dyad 1: Level for client ratings on RRI-C (Real Relationship) for experimental and control sessions
Figure 19. Dyad 1: Trend for client ratings on RRI-C (Real Relationship) for experimental and control sessions.

Figure 20. Dyad 1: Overlap for client ratings on RRI-C (Real Relationship) for experimental and control session.
Figure 21. Dyad 2: Client ratings on Empathy for experimental and control sessions

Figure 22. Dyad 2: Variability for client ratings on Empathy for experimental and control sessions
Figure 23. Dyad 2: Level for client ratings on Empathy for experimental and control sessions

Figure 24. Dyad 2: Trend for client ratings on Empathy for experimental and control sessions
Figure 25. Dyad 2: Overlap for client ratings on Empathy for experimental and control sessions

Figure 26. Dyad 2: Therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions
Figure 27. Dyad 2: Variability for therapist ratings on HSA (Hindering Self-Awareness)

Figure 28. Dyad 2: Level for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions
Figure 29. Dyad 2: Trend for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions

Figure 30. Dyad 2: Overlap for therapist ratings on HSA (Hindering Self-Awareness) for experimental and control sessions
Figure 31. Dyad 2: Therapist ratings on RRI-T (Real Relationship) for experimental and control sessions

Figure 32. Dyad 2: Variability for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions
Figure 3. Dyad 2: Level for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions

Figure 4. Dyad 2: Trend for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions
Figure 35. Dyad 2: Overlap for therapist ratings on RRI-T (Real Relationship) for experimental and control sessions

Figure 36. Dyad 2: Client ratings on RRI-C (Real Relationship) for experimental and control sessions
Figure 37. Dyad 2: Variability for client ratings on RRI-C (Real Relationship) for experimental and control sessions

Figure 38. Dyad 2: Level for client ratings on RRI-C (Real Relationship) for experimental and control sessions
Figure 39. Dyad 2: Trend for client ratings on RRI-C (Real Relationship) for experimental and control sessions

Figure 40. Dyad 2: Overlap for client ratings on RRI-C (Real Relationship) for experimental and control sessions
Appendix A. Demographic Questionnaires

Demographic Questionnaire for Therapists

1. What is your gender? __________

2. What is your age? __________

3. What is your highest level of education completed?
   a. Bachelor’s degree
   b. Master’s degree (Please specify _______________________________)
   c. Doctoral degree (Please specify _______________________________)
   d. Professional degree (Please specify _______________________________)

4. If you are currently enrolled in a graduate program, please specify the type of program and your year in the program: ________________________________

5. What is your racial/ethnic background?
   a. Native American/Alaskan Native/First Nation
   b. Black/African American
   c. East Asian/Pacific Islander
   d. Hispanic/Latino/a
   e. Middle Eastern/West Asian
   f. South Asian
   g. White/Caucasian
   h. Multiracial
   i. Other ______________

6. What is your primary theoretical orientation? ________________________________

7. How much clinical experience have you had? _______ months ___ years

8. Are you currently being supervised? ___ yes ___ no
   If so, how long have you been working with this supervisor? _____ months

9. Have you had previous formal training in mindfulness practice?
   a. Yes
   b. No
Demographic Questionnaire for Clients

1. What is your gender? _________

2. What is your age? ___________

3. What is your racial/ethnic background?
   a. Native American/Alaskan Native/First Nation
   b. Black/African American
   c. East Asian/Pacific Islander
   d. Hispanic or Latino/a
   e. Middle Eastern/West Asian
   f. Multiracial
   g. South Asian
   h. White/Caucasian
   i. Other ________________

4. What is your highest level of education?
   a. Some high school, no diploma
   b. High school graduate/GED
   c. Some college, no degree
   d. Trade/technical/vocational training
   e. Associate degree
   f. Bachelor's degree
   g. Master's degree
   h. Professional degree
   i. Doctorate degree

5. Please identify your primary reason for coming to counseling:
   a. Relationship
   b. Stress/anxiety
   c. Family issues
   d. Educational/vocational issues
   e. Depression
   f. Academic concerns
   g. Physical problem(s)
   h. Substance/alcohol use
   i. Eating concerns
   j. Grief
   k. Sexual assault
   l. Other ________________________

6. Have you ever been in counseling before this time?
   a. Yes
   b. No
   If so, for how long, approximately? _____weeks
Appendix B. Therapist Recruitment Script

Hi _______, I’d like to tell you about the study that I’m doing for my dissertation here at PSC, and invite you to participate. Would this a convenient time to talk to you about it for a couple of minutes?

(If yes:) The purpose of the study is to see how therapist mindfulness affects the therapy process. I’m specifically looking at what happens when therapists practice a 3-minute mindfulness exercise right before session. I’m interested in how it affects the therapeutic relationship, as well as the therapist’s empathy and self-awareness. In this context, self-awareness refers to the awareness of internal distractions that you experience during session, such as thoughts, feelings or physical sensations.

The first part of the study involves an introductory workshop that I’ll conduct here at PSC, to familiarize participants with mindfulness. It will involve five, 20-minute sessions at a time that’s convenient for the therapists who are participating.

While the workshop is going on, client recruitment will be taking place. I’ll ask you to consult with your supervisor to identify one client from your caseload who you think might be interested in participating. You can tell me the time and day that you meet with the client, and I’ll approach them in the waiting room to describe the study and to find out if they want to take part. If they are not interested or not eligible, you can nominate another client. If none of your clients wants to take part in the study, then your participation would be over at the end of the workshop. But if any of your clients do choose to participate, you would continue with the second part of the study.

What the second part would involve is putting your mindfulness skills into practice for the rest of the year, by doing a short, 3-minute mindfulness exercise before certain sessions with each of your clients who’s participating. So, at most, that would mean practicing the mindfulness exercise before two sessions per week. Afterwards, you would be asked to fill out two paper-and-pencil questionnaires about your perceptions of the session, and your clients would be asked to fill out 2 questionnaires about their perceptions— that’s all that the clients would have to do. I anticipate that the questionnaires won’t take more than 10 minutes to fill out each time, for either you or the client.

Participation in this study is completely voluntary, so please feel free to say ‘no.’ If you’re interested in participating, I’ll ask you to get your supervisor’s approval as well. As a thank-you for participating in the study til the end, you would receive $75 worth of gift cards to your choice of the places on this list *(show list)*.

Do you have any questions?
Do you think you’d like to take part in my study?

*[if no: “Okay, thank you very much for your time.”]*
*[if yes: “Okay, thank you very much! Could you please read and sign this consent form, and demographic questionnaire?”]*
Appendix C. Client Recruitment Script.

Hi, my name is Marianne Ball, and I’m a graduate student at the University at Albany. I’d like to tell you about a research study that I’m doing here at PSC, and invite you to participate. Would this be a convenient time to talk to you for a couple of minutes?

(If yes:) The purpose of the study is to learn more about what makes therapists effective. To shed light on this, I’m interested in hearing how you feel about your experience of counseling. All that you would have to do is fill out a short demographic survey now, and then fill out 2 questionnaires about how you experienced your therapist, and your connection with him/her, after each session for the next few months. It will take less than 10 minutes each time.

Participation in this study is completely voluntary, so please feel free to say ‘no.’ As a thank-you for participating in the study till the end, you would receive $75 worth of gift cards to your choice of the places on this list (show list).

Do you have any questions?

Do you think you’d like to take part in my study?

[if no: “Okay, thank you very much for your time.”]
[if yes: “Okay, thank you very much! Could you please read and sign this consent form, and complete this demographic questionnaire?”]
Appendix D. Therapist Informed Consent

<table>
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<tr>
<th>Protocol (Study) Number</th>
<th>1940</th>
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<tr>
<td>Study Title</td>
<td>Investigating perceptions of the therapy process at the Psychological Services Center</td>
</tr>
<tr>
<td>Study Principal Investigator Name</td>
<td>Marianne Ball, M.A. PhD Candidate Division of Counseling Psychology University at Albany/SUNY</td>
</tr>
<tr>
<td>Study Principal Investigator Phone #</td>
<td>518 618 9390</td>
</tr>
<tr>
<td>Study Principal Investigator Email address</td>
<td><a href="mailto:mball@albany.edu">mball@albany.edu</a></td>
</tr>
</tbody>
</table>

Introduction

You are being invited to participate in a study that I am conducting for my dissertation, here at the Psychological Services Center. Therapists at the Psychological Services Center are being recruited to participate in this research, to help us understand how therapist mindfulness affects the therapeutic process.

Why is this study being done?

The purpose of the study is to examine whether practicing mindfulness meditation briefly before therapy sessions has any effect on the therapeutic relationship and therapist variables, including empathy and self-focused attention.

What are the study procedures? What will I be asked to do?

If you agree to participate, I’ll first ask you to complete a brief demographic questionnaire today. Then, you will be asked to attend an introductory workshop that I will conduct in the coming weeks, to familiarize you with the practice of mindfulness. The workshop will comprise of five, 20-minute sessions. It will be conducted here at the Psychological Services Center, at a time that is convenient for the therapists who agree to participate.

Clients will also be recruited to participate in the study. Up to 2 therapy clients from your caseload can take part. You will be asked to mention the study when you phone your clients to set up appointments over the next couple of weeks, using a script that I’ll provide. If any of your clients are amenable to being recruited, I’ll ask you to let me know when their next appointments are (without providing names), and I’ll approach them in the waiting room beforehand to tell them more about the study and ask if they want to take part. If any of your clients agrees to take part, I’ll ask you about whether they have any severe risk management issues, as this is one of the exclusion criteria for clients.

If any of your clients enrolls in the study (up to a maximum of 2), your involvement in this research will continue after the workshop. Specifically, for the remainder of the year, you’ll be
asked to put your mindfulness skills into practice by doing a 3-minute mindfulness exercise before randomly-selected sessions with each participating client. Alternatively, there is a chance that you will be assigned as a control participant, in which case you may be asked to practice the mindfulness exercise before every session with your participating client(s), or not to practice the mindfulness exercise at all. If none of your clients enrolls in the study, then your involvement will be over at the end of the workshop.

So that we have data from both the client’s and therapist’s perspective, both you and your participating client(s) will be asked to complete two paper-and-pencil measures about your experience of each session for the remainder of the semester. One of the measures is about your perception of the therapeutic relationship during the session, and the other is about your experiences with hindering self-focused attention, which means any experience where you felt troubled by awareness of internal distractions, such as thoughts, feelings or physical sensations.

How long will it take?

The demographic questionnaire is expected to take less than 10 minutes. The workshop will comprise of five, 20-minute sessions. After the workshop, there will be a small time commitment associated with each session for each participating client for the remainder of the semester. Specifically, practicing the mindfulness exercise before the session will take 3 minutes, and completing the questionnaires after each session will take up to 10 minutes each time.

What are the risks or inconveniences of the study?

There is no risk associated with participating in this study. Inconvenience may arise if two participating clients are scheduled back to back, leaving insufficient time to complete the questionnaires and to practice the mindfulness exercise between sessions. Thus, we will consult with you regarding your schedule before enrolling clients, to avoid this predicament.

What are the benefits of the study?

The potential benefit to you for participating in the study is the opportunity to learn mindfulness meditation, a practice that has been shown to have personal benefits as well as benefits to clients. Furthermore, taking part in this study could be a helpful professional development experience.

Will I receive payment for participation? Are there costs to participate?

There are no costs to participants. Depending on the length of your participation, you will receive gift cards for up to $75, as a token of appreciation for your participation in the study. One $25 gift card will be presented after completion of the workshop. A second $25 gift card will be presented after 6 weeks’ participation in the study. A third $25 gift card will be presented at the end of the study period. If your participation in the study ends after the workshop (i.e., if none of your clients enroll), you will only receive the first gift card.
How will my personal information be protected?

The questionnaires you fill out after each session will not contain any identifying information about you. All information obtained in this study is strictly confidential unless disclosure is required by law. In addition, the Institutional Review Board, the sponsor of the study (e.g. NIH, FDA, etc.) and University or government officials responsible for monitoring this study may inspect these records.

Can I stop being in the study and what are my rights?

You should also know that participation in this research is entirely voluntary. If you agree to participate, you may decide to leave the study at any time without penalty or loss of benefits to which you may otherwise have been entitled. You should also be aware that the investigator may withdraw you from participation at his/her professional discretion.

Whom do I contact if I have questions about the study?

Take as long as you like before you make a decision. We will be happy to answer any questions you have about this study. If you have further questions about this project or if you have a research-related problem, you may contact the principal investigator.

Whom do I contact if I have questions about my rights as a study participant?

Research at the University Albany involving human participants is carried out under the oversight of the Institutional Review Board (IRB). If you have any questions concerning your rights as a research subject or if you wish to report any concerns about the study, you may contact University at Albany Office of Regulatory & Research Compliance at 1-866-857-5459 or hscconcerns@albany.edu.

You will be given a copy of this document to keep.

Signing your name below indicates that you agree to participate in this study. Thank you very much for your time.

_________________________  ______________________  ________________
Signature of Participant    Printed Name       Date

_________________________  ______________________  ________________
Signature of Supervisor    Printed Name       Date

_________________________  ______________________  ________________
Signature of Principal Investigator  Printed Name       Date
Introduction

You are being invited to participate in a study that I am conducting for my dissertation on clients’ perceptions of therapy. You are being asked to participate because you are receiving services at the Psychological Services Center.

Why is this study being done?

The purpose of the study is to identify factors that might help therapists to be more effective. One of the ways to do this is to find out about clients’ perceptions of therapy.

What are the study procedures? What will I be asked to do?

If you agree to participate, I will ask you to sign this form and complete a brief demographic questionnaire today. For the remainder of the year, you will be asked to fill out 2 questionnaires immediately after each session, about how you experienced the session. You will not be asked about what you discussed with your therapist, but rather about how you experienced your therapist, and your relationship with him/her. Your therapist will hand you the questionnaires at the end of each session. You can complete them in the waiting room, and put them in a secure box in the reception area when you are done.

If you are interested in participating, I will ask for some information from your therapist to make sure that you are a good fit for this study. For example, I will be checking with your therapist to find out whether you are at risk for hurting yourself or someone else. If any concerns arise at any point, your participation in the study may be terminated.

How long will it take?

The demographic questionnaire is expected to take less than 10 minutes. I anticipate that the questionnaires you complete after each session will take no more than 10 minutes to fill out each time.

What are the risks or inconveniences of the study?

There is no risk associated with participating.
What are the benefits of the study?

The potential benefit to you is the opportunity to express your thoughts and feelings about your therapy experience. By reflecting on each session immediately afterwards, you may even benefit more from your therapy experience.

Will I receive payment for participation? Are there costs to participate?

There are no costs to participants. As a token of appreciation for your participation in the study, you will receive up to 2 gift cards, worth $25 and $50 respectively. You will receive the first gift card after 6 sessions of participating in the study. You will receive the second gift card at the end of the study, provided that you participate until the end.

How will my personal information be protected?

The questionnaires you fill out will not contain any identifying information about you, and your responses will not be shown to your therapist. All information obtained in this study is strictly confidential unless disclosure is required by law. In addition, the Institutional Review Board, the sponsor of the study (e.g., NIH, FDA, etc.) and University or government officials responsible for monitoring this study may inspect these records.

Can I stop being in the study and what are my rights?

You should also know that participation in this research is entirely voluntary. If you don’t want to participate, it won’t affect your therapy here at all. If you do agree to participate, you may decide to leave the study at any time without penalty or loss of benefits to which you may otherwise have been entitled. You should also be aware that the investigator may withdraw you from participation at his/her professional discretion.

Whom do I contact if I have questions about the study?

Take as long as you like before you make a decision. We will be happy to answer any questions you have about this study. If you have further questions about this project or if you have a research-related problem, you may contact the principal investigator.

Whom do I contact if I have questions about my rights as a study participant?

Research at the University Albany involving human participants is carried out under the oversight of the Institutional Review Board (IRB). This research has been reviewed and approved by the IRB. If you have any questions concerning your rights as a research subject or if you wish to report any concerns about the study, you may contact University at Albany Office of Regulatory & Research Compliance at 1-888-857-6459 or h收录 concerns@albany.edu.
You will be given a copy of this document to keep.

Signing your name below indicates that you agree to participate in this study. Thank you very much for your time.

Signature of Participant  Printed Name  Date

Signature of Person Obtaining Consent  Date
Appendix F. Sample Schedule

THERAPIST INITIALS: 

CLIENT INITIALS:

Instructions

1) Before each session please refer to the table below to determine whether you should practice mindfulness (M) by listening to the 3-minute breathing space, or do a control activity (C), such as use the restroom, check email, or chat with other therapists.

2) Please fill in the date.

3) Immediately after finishing the mindfulness activity or the control activity (i.e., right before you meet with the client), please rate your level of mindfulness.

<table>
<thead>
<tr>
<th>Session Number</th>
<th>Activity</th>
<th>Date</th>
<th>Mindfulness rating</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>M</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>M</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>C</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>C</td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>C</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6</td>
<td>M</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

How mindful do you feel right now?

1 | 2 | 3 | 4 | 5 | 6 | 7

Not at all | Completely

How mindful do you feel right now?

1 | 2 | 3 | 4 | 5 | 6 | 7

Not at all | Completely

How mindful do you feel right now?

1 | 2 | 3 | 4 | 5 | 6 | 7

Not at all | Completely

How mindful do you feel right now?

1 | 2 | 3 | 4 | 5 | 6 | 7

Not at all | Completely
<table>
<thead>
<tr>
<th>THERAPIST INITIALS:</th>
<th>CLIENT INITIALS:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
</tr>
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</table>

<table>
<thead>
<tr>
<th>7</th>
<th>C</th>
<th>How mindful do you feel right now?</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>1 2 3 4 5 6 7</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Not at all Completely</td>
</tr>
<tr>
<td>8</td>
<td>C</td>
<td>How mindful do you feel right now?</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1 2 3 4 5 6 7</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Not at all Completely</td>
</tr>
<tr>
<td>9</td>
<td>C</td>
<td>How mindful do you feel right now?</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1 2 3 4 5 6 7</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Not at all Completely</td>
</tr>
<tr>
<td>10</td>
<td>M</td>
<td>How mindful do you feel right now?</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1 2 3 4 5 6 7</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Not at all Completely</td>
</tr>
</tbody>
</table>