The Process of Therapeutic Change in The attention Training Technique

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ABSTRACT

THE PROCESS OF THERAPEUTIC CHANGE IN THE ATTENTION TRAINING TECHNIQUE

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Background: An unacceptably large proportion of individuals remain symptomatic after receiving first-line interventions. The attention training technique (ATT) is a potentially effective treatment augmentation and standalone treatment that may help improve the treatment of psychological disorders. The mechanisms of therapeutic change of ATT remain understudied. This study is a randomized controlled trial of ATT compared to progressive muscle relaxation (PMR) that examined mindfulness and attentional control as potential mechanisms of therapeutic change.

Method: A convenience sample of 64 participants ($M_{\text{age}} = 20.13, SD = 3.65$; 42.2% Male; 64.1% non-Hispanic White; 23.4% Black; 9.4% Hispanic/Latino; 3.1% Other) were randomly assigned to receive daily ATT or PMR sessions for one week via a smartphone application. Participants reported their attentional control and mindfulness after each session. Attentional control, metacognitions, and psychological symptoms were assessed via self-report immediately before the intervention and after one week.

Results: Hierarchical linear modeling (HLM) assessed changes over time. The impact of experimental group assignment on the effect of time on attentional control was not significant, $\beta$
= -0.174, \( t(309) = -0.903, p = 0.370 \). Additional analyses related to symptom reduction, attentional control, metacognitions, validity of self-report attentional control, and study design feasibility are reported.

Discussion: The study results indicated that ATT and PMR are similar interventions. The study design was unable to identify a mechanism of therapeutic change that was unique to either intervention. The implications for self-report attentional control and study design feasibility are also discussed along with study limitations and future directions.
THE PROCESS OF THERAPEUTIC CHANGE IN THE
ATTENTION TRAINING TECHNIQUE

BY
BENJAMIN J. LAMAN-MAHARG

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A THESIS SUBMITTED TO THE GRADUATE SCHOOL IN
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Doctoral Director:
Holly K. Orcutt
# TABLE OF CONTENTS

<table>
<thead>
<tr>
<th>Chapter</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>LIST OF TABLES</td>
<td>v</td>
</tr>
<tr>
<td>Chapter 1: INTRODUCTION AND LITERATURE REVIEW</td>
<td>1</td>
</tr>
<tr>
<td>Effectiveness of First-Line Treatments</td>
<td>3</td>
</tr>
<tr>
<td>Attempts to Improve Treatment Response Rates</td>
<td>5</td>
</tr>
<tr>
<td>Identifying Why Dropout Occurs</td>
<td>9</td>
</tr>
<tr>
<td>Comorbidities Attenuate Treatment Response</td>
<td>11</td>
</tr>
<tr>
<td>Improving Treatment Response Through Augmentation</td>
<td>13</td>
</tr>
<tr>
<td>The General Applicability of First-Line Treatments</td>
<td>13</td>
</tr>
<tr>
<td>Comorbid Cognitive Problems</td>
<td>14</td>
</tr>
<tr>
<td>Cognitive Problems and Treatment Response</td>
<td>17</td>
</tr>
<tr>
<td>Alternatives to First-Line Treatments</td>
<td>18</td>
</tr>
<tr>
<td>The Attention Training Technique</td>
<td>20</td>
</tr>
<tr>
<td>Theoretical Basis of ATT</td>
<td>21</td>
</tr>
<tr>
<td>The Therapeutic Process of Change in ATT</td>
<td>26</td>
</tr>
<tr>
<td>Ecological Momentary Assessment of ATT</td>
<td>29</td>
</tr>
<tr>
<td>This Study</td>
<td>31</td>
</tr>
<tr>
<td>Hypotheses</td>
<td>32</td>
</tr>
<tr>
<td>Measures Check Hypothesis</td>
<td>32</td>
</tr>
<tr>
<td>Chapter</td>
<td>Page</td>
</tr>
<tr>
<td>---------</td>
<td>------</td>
</tr>
<tr>
<td>A Priori Hypotheses</td>
<td>33</td>
</tr>
<tr>
<td>Exploratory Hypotheses</td>
<td>33</td>
</tr>
<tr>
<td>2. METHODS</td>
<td>34</td>
</tr>
<tr>
<td>Participants</td>
<td>34</td>
</tr>
<tr>
<td>Sample Size</td>
<td>35</td>
</tr>
<tr>
<td>Measures</td>
<td>37</td>
</tr>
<tr>
<td>Attention Network Test (ANT)</td>
<td>37</td>
</tr>
<tr>
<td>Five-Facet Mindfulness Questionnaire – Short Form (FFMQ-SF)</td>
<td>40</td>
</tr>
<tr>
<td>Attentional Control Scale (ACS)</td>
<td>42</td>
</tr>
<tr>
<td>Metacognitions Questionnaires (MCQ-30)</td>
<td>44</td>
</tr>
<tr>
<td>Thought Control Questionnaire (TCQ)</td>
<td>45</td>
</tr>
<tr>
<td>Depression Anxiety Stress Scale-21 (DASS-21)</td>
<td>46</td>
</tr>
<tr>
<td>Procedure</td>
<td>47</td>
</tr>
<tr>
<td>Time 1</td>
<td>47</td>
</tr>
<tr>
<td>Time 2 Through Time 6</td>
<td>49</td>
</tr>
<tr>
<td>Time 7</td>
<td>50</td>
</tr>
<tr>
<td>3. DATA ANALYSIS</td>
<td>51</td>
</tr>
<tr>
<td>Tests of HLM Assumptions, Normality, and Missing Data</td>
<td>51</td>
</tr>
<tr>
<td>Testing the Hypotheses</td>
<td>52</td>
</tr>
<tr>
<td>Hypothesis 0</td>
<td>52</td>
</tr>
<tr>
<td>Hypothesis 1</td>
<td>52</td>
</tr>
<tr>
<td>Hypothesis 2</td>
<td>53</td>
</tr>
<tr>
<td>Chapter</td>
<td>Hypothesis</td>
</tr>
<tr>
<td>---------</td>
<td>------------</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 3</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 4</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 5</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 6</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 7</td>
</tr>
<tr>
<td>4.</td>
<td>RESULTS</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 0</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 1</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 2</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 3</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 4</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 5</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 6</td>
</tr>
<tr>
<td></td>
<td>Hypothesis 7</td>
</tr>
<tr>
<td></td>
<td>Additional Post-Hoc Exploratory Analyses</td>
</tr>
<tr>
<td>5.</td>
<td>DISCUSSION</td>
</tr>
<tr>
<td></td>
<td>Limitations</td>
</tr>
<tr>
<td></td>
<td>Future Directions</td>
</tr>
<tr>
<td></td>
<td>REFERENCES</td>
</tr>
</tbody>
</table>
# LIST OF TABLES

<table>
<thead>
<tr>
<th>Table</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Categorical Variable Differences Between ATT and PMR Groups (χ²) – Time 1</td>
<td>35</td>
</tr>
<tr>
<td>2. Continuous Variable Differences Between ATT and PMR Groups (ANOVA) – Time 1</td>
<td>36</td>
</tr>
<tr>
<td>3. Correlations Between Attention, Mindfulness, and Psychological Symptoms Constructs, Time 1 and Times 7/8 – Combined Groups</td>
<td>60</td>
</tr>
<tr>
<td>4. Correlations Between Attention, Mindfulness, and Psychological Symptoms Constructs, Time 1 and Times 7/8 – ATT Group</td>
<td>62</td>
</tr>
<tr>
<td>5. Correlations Between Attention, Mindfulness, and Psychological Symptoms Constructs, Time 1 and Times 7/8 – PMR Group</td>
<td>64</td>
</tr>
<tr>
<td>6. Correlations Between Self-Report Attention and Cognitive Constructs, Time 1</td>
<td>66</td>
</tr>
<tr>
<td>7. Correlations Between Self-Report Attention and Cognitive Constructs, Time 7</td>
<td>67</td>
</tr>
<tr>
<td>8. Mean Scores Over Time Between Groups and Combined</td>
<td>72</td>
</tr>
</tbody>
</table>
CHAPTER 1

INTRODUCTION

The fields of psychology and psychiatry have worked in tandem to provide a broad portfolio of high-quality evidence-based treatments for most common psychological disorders. However, a significant portion of individuals who complete one of these standalone first-line treatments does not experience adequate symptom reduction (e.g., Cuijpers, Berking, et al., 2013; Foa et al., 2005; Monson et al., 2006; Powers, Halpern, Ferenschak, Gillihan, & Foa, 2010; Resick, Nishith, Weaver, Astin, & Feuer, 2002; Rush et al., 2006; Schnurr et al., 2007). In fact, a high proportion of individuals seeking treatment are likely to undergo several different trials of different treatments (Rush et al., 2006). Several efforts to address treatment nonresponding are currently underway. A popular strategy for addressing treatment nonresponse is attempting to develop entirely new treatments (e.g., Twohig & Levin, 2017). However, effect sizes for these new treatments tend to be similar to those they are seeking to supplant and may even be found to be inferior after undergoing rigorous scrutiny (e.g., Hacker, Stone, & MacBeth, 2016). Furthermore, diagnostic heterogeneity makes it difficult for mental health practitioners to select an overly specific treatment for a given symptom presentation (Cuijpers, Berking, et al., 2013).

Another strategy to address treatment nonresponse comes in the form of treatment augmentation. Treatment augmentation attaches an additional therapeutic element to a pre-existing treatment that has already been shown to be effective in an effort to improve potential
weaknesses of the treatment such as poor patient retention or less-than-ideal rates of treatment response. For example, treatment augmentation has been shown to reduce dropout rates (e.g., Biesheuvel-Leliefeld et al., 2015; Thase et al., 2007; Vogel, Stiles, & Götestam, 2004) and increase treatment adherence (e.g., Balán, Moyers, & Lewis-Fernández, 2013).

One promising but understudied treatment augmentation is the attention training technique (ATT; Wells, 2009). ATT is a brief neurobehavioral intervention that trains an individual to increase their attentional control, an executive function that has been found to be a protective factor against several anxiety disorders (Susa, Pitica, Benga, & Miclea, 2012). While some preliminary evidence has provided support for the effectiveness of ATT (for a review, see Knowles, Foden, El-Deredy, & Wells, 2016), the mechanisms of therapeutic change in ATT treatment are not well-understood. Furthermore, mindfulness-based interventions (MBIs) appear to offer similar benefits to ATT, but the theoretical basis for each intervention is quite different. The mechanism of therapeutic change of ATT is not well-understood, but may function via a neurobehavioral pathway. The present study seeks to investigate whether ATT significantly influences anxiety, depression, and stress symptoms over time, and whether these improvements are likely due to the purported process of therapeutic change of ATT (attentional control) or are likely due to a tendency for the training technique to promote mindfulness, a construct also associated with reduced psychological symptoms and also related to attention.

In this paper, a literature review of clinical trials of several different first-line treatments, recommended treatments for which there is strong evidence of comparative effectiveness, of a variety of psychological disorders is presented. Current evidence suggests that first-line treatments are typically effective for most individuals seeking treatment but fail to significantly improve the symptoms of others. Following the review of the treatment literature, current efforts
to improve already existing first-line treatments will be reviewed and contrasted with treatment augmentation. The general difficulties in clinical research are then briefly covered. Because of the sizeable minority that fail to experience an adequate response to treatment, factors that contribute to first-line treatment failures are explored with a particular emphasis on dropout and comorbidity. Research is then presented on cognitive problems, which are a ubiquitous correlate or symptom of psychological disorders and may also affect treatment response. The attention training technique (ATT) is then offered as a potential augmentation to psychological treatments. The theoretical basis for ATT is then reviewed and extant research on the processes of therapeutic change of ATT are discussed. Based on this review of the literature, a study was designed and executed in order to examine these processes of therapeutic change, an important step in establishing the validity of a potential intervention. The results of this study are then presented, followed by a discussion of their significance to the broader extant literature.

Effectiveness of First-Line Treatments

The fields of psychiatry and clinical psychology offer high quality treatments for most psychological disorders. First-line treatments will typically improve symptoms for a majority of patients who participate in them. However, these first-line treatments do not always work for everyone. For example, significant proportions of individuals who undergo first line PTSD treatments remain significantly symptomatic (Foa et al., 2005; Monson et al., 2006; Powers et al., 2010; Resick et al., 2002; Schnurr et al., 2007). This has also been found in other common psychological disorders. At least 30% of patients suffering from major depressive disorder (MDD) demonstrate treatment resistance to antidepressants, with some estimates reaching as high as half of the patients (Rush et al., 2006). Furthermore, it is estimated that approximately
40% of individuals undergoing psychotherapy for MDD still meet DSM-5 MDD criteria after completing a course of treatment (Cuijpers et al., 2014). This pattern of a sizeable minority of nonresponding patients can be found in a variety of other literatures across multiple psychological disorders. Furthermore, symptom reduction rates are not useful in evaluating treatment effectiveness without comparison to control groups. While a majority of patients undergoing psychotherapy no longer meet DSM-5 criteria for MDD, as many as 40% of individuals on wait-list controls in cognitive behavioral therapy (CBT) randomized controlled trials (RCTs) spontaneously improve (Cuijpers, Berking, et al., 2013) reducing the level of effectiveness that can be ascribed to the actual treatment. These findings taken together highlight a lack of knowledge regarding underlying therapeutic mechanisms that lead to symptom reduction.

A meta-analysis of psychotherapy and psychopharmacotherapy for generalized anxiety disorder (GAD) showed similar results to reviews of depression treatments. Among all studies included in this review, as few as 20% and as many as 70% of patients responded to the active treatment (Goncalves & Byrne, 2012). However, similar to findings in the Cuijpers, Berking, et al. (2013) review for depression, Goncalves and Byrne (2012) found that as many as 45% of individuals in wait-list control groups spontaneously remitted, which again reduces the level of effectiveness that can be ascribed to the treatment itself. Further examples of mediocre responses to treatment can be found in the OCD literature, where in one review, approximately half of all patients with OCD significantly improve after receiving CBT and SSRI treatment, and about 25% fully remit (Eddy, Dutra, Bradley, & Westen, 2004). Notably, there are more effective treatments for OCD, namely exposure with response prevention (ERP), in which approximately half of patients fully remit (Middleton, Wheaton, Kayser, & Simpson, 2019). But, due to an
undeserved reputation for high dropout rates (Ong, Clyde, Bluett, Levin, & Twohig, 2016; ERP for OCD has dropout rates that are no worse than, for instance, CBT for Depression), not everyone seeking treatment for OCD receives ERP (Hezel & Simpson, 2019).

Attempts to Improve Treatment Response Rates

No treatment for any psychological disorder is perfect, and several sequential treatment steps are often needed to obtain remission with a tolerated treatment (Adli, Bauer, & Rush, 2006; Crismon et al., 1999). As with any mental disorder being treated with selective serotonin reuptake inhibitors (SSRI), treatment response may only be seen after several medications have been trialed. Researchers in the field of psychiatry have noted that a possible pragmatic solution to this problem would be to determine an evidence-based standard for medication switching and titration. However, controlled data on switching medications are lacking for the anxiety disorders (Bandelow et al., 2008; Samuel, Zimovetz, Gabriel, & Beard, 2011), and some studies support augmentation rather than switching (Samuel et al., 2011) which further complicates a psychiatrist’s decision-making process when attempting to find a tolerable and effective medication for any given patient.

Psychological researchers have focused on selecting a handful of treatments that are effective for each individual psychological disorder as a potentially pragmatic and cost-effective way to improve treatment response. However, it is often difficult to determine the true effectiveness of a psychotherapy for a specific psychological disorder. For example, effect sizes for the treatment of various anxiety disorders with CBT are thought to vary depending on the type of anxiety disorder (Hofmann & Smits, 2008; Norton & Price, 2007). However, one meta-analysis of cognitive and exposure therapies for a variety of anxiety disorders found higher effect
sizes for the treatment of acute stress disorder, OCD, PTSD, and social anxiety disorder, and lower effect sizes for the treatment of GAD and panic disorder (Hofmann & Smits, 2008). A separate meta-analysis differed dramatically from Hoffmann and Smits (2008), finding that CBT interventions for anxiety disorders had higher effect sizes for the treatment of GAD and PTSD, and a generally low effect size for the treatment of SAD (Norton & Price, 2007). Diagnostic heterogeneity may be a likely explanation for such divergent findings. Given the well-documented controversy over diagnostic specificity in the DSM, it may be more fruitful to acknowledge this limitation in the search for pragmatic solutions to treatment nonresponse.

Rather than seeking out and developing specialized treatment manuals for amorphous and potentially arbitrary diagnostic criteria, it may be more beneficial to identify general treatments that are flexible enough to allow augmentation and adaptation based on individual client needs. While this method of improving treatment response is conceptually sound there is also evidence to support it. With evidence suggesting that individual treatment modalities are less effective at treating MDD than combined or sequential modalities (Rush et al., 2006), it is not surprising that effect size estimates for the effectiveness of standalone CBT for MDD may have been overestimated previously due to publication bias and poor-quality methodology (Cuijpers, Berking et al., 2013). As new CBT treatments are developed, initial studies are lower-quality and may have systematic flaws that lead to error inflation. A recent meta-analysis found that high quality studies (those with larger sample sizes, intention-to-treat analysis, blinding of assessors, allocation concealment, etc.) showed a moderate effect size for CBT for MDD, while lower quality studies, typically those that were published initially upon the development of the treatment, found large effect sizes (Cuijpers, Berking, et al., 2013). This puts CBT for MDD more in line with other treatments, even though it was previously thought to be an outstanding
psychological treatment. A similar trend of smaller effect sizes over time appears to be
developing in the acceptance and commitment therapy (ACT) for depression literature (Hacker et al., 2016; Twohig & Levin, 2017). While developing entirely new treatments for narrow, well-defined psychological disorders initially appears to provide significant improvements compared to already well-established treatments, meta-analytic evidence suggests that initial studies of new treatments tend to exaggerate effect sizes. It may be more beneficial to augment or alter existing, proven treatments.

Why Treatments Fail

In addition to diagnostic heterogeneity, treatment failures that occur for a significant minority of the population seeking treatment for common psychological disorders may be explained by a variety of other factors including noncompliance, dropout, confounding comorbidities, and unknown individual differences affecting treatment response. It is important to identify factors that lead to treatment failures in order to adapt interventions in ways that reduce the likelihood that these factors may occur. Treatment failures happen at about the same rates in psychotherapy and psychopharmacotherapy interventions (Adli et al., 2006; Cuijpers, Berking, et al., 2013), but it is not clear what specific factors lead to treatment resistance for any particular treatment modality, with the exception of medication adherence that is unique to psychopharmacotherapy due to the unequivocal cause-and-effect relationship between medication nonadherence and treatment response attenuation. Two common reasons for treatment resistance in pharmacotherapy are difficulties in titrating the ideal dose for an individual patient and treatment adherence (Fava & Davidson, 1996).
Noncompliance Relates to Dropout

Noncompliance is a frequently cited reason for treatment failure across treatment modalities (Burns & Spangler, 2000; Fava & Davidson, 1996; Ward et al., 2000), and depression has been found to be a risk factor for noncompliance with medical interventions (DiMatteo, Lepper, & Croghan, 2000). In psychopharmacotherapy, noncompliance typically takes the form of medication nonadherence. Although medication adherence is not directly applicable to psychotherapy, treatment nonadherence is unsurprisingly associated with poorer outcomes in psychotherapy (Simpson, Marcus, Zuckoff, Franklin, & Foa, 2012). Additionally, treatment dropout may be a commonly studied close analogue to medication nonadherence. While psychotherapies do not have side effects in the traditional sense, some forms of psychotherapy, particularly exposure therapies and those meant to directly challenge long-held beliefs, can be aversive to some patients (Hoge et al., 2004). Psychotherapy also takes a considerable amount of time which has been hypothesized to be a significant barrier to treatment and also a possible reason for high dropout rates. However, efforts to reduce overall time commitment via telehealth strategies tend to suffer from higher dropout rates than in person therapies (Cuijpers, Berking, et al., 2013). This suggests that motivation from direct interpersonal communication outweighs potential time and cost savings of a telehealth solution, although at this time it is not known whether individuals in telehealth studies tend to have higher levels of barriers to treatment than individuals in in-person studies.

All of these factors, as well as unknown individual differences, can lead to treatment dropout, which is prevalent to some degree in every human-subject field of research. However, the problem of dropout may be more severe in psychological intervention studies and practice.
Treatment dropout in these contexts may result in an inadequate dose of therapy, thus leading to the continuation of possibly severe symptoms. There are myriad examples of problematic dropout rates across studies involving many psychological disorders and psychotherapies. For example, one study found that almost half of participants seeking treatment for bulimia nervosa (BN) dropped out before treatment completion. Individuals who dropped out tended to have higher levels of comorbid depression, specifically related to hopelessness and an external locus of control (Steel et al., 2000). Other research in this area has not been able to identify any significant differences between individuals who drop out of CBT for BN and those who complete treatment (Waller et al., 2014). The overall trend of being unable to identify significant differences between individuals who complete treatment and drop out of treatment is common across many literatures (Cooper & Conklin, 2015; de Haan, Boon, de Jong, & Vermeiren, 2018; Karyotaki et al., 2015).

Identifying Why Dropout Occurs

Dropout rates between different treatments for the same disorder also appear to be very similar, despite different protocols and theoretical underpinnings of differing forms of treatments used in comparison studies. For example, in some comparative effectiveness trials for PTSD treatments, dropout rates were identical across all treatments included in the comparison, suggesting that there may be unknown individual differences between people who dropout of treatment and people who stay in treatment that are unrelated to treatment modality (Stapleton, Taylor, & Asmundson, 2006; Taylor et al., 2003). An analysis of 25 different PTSD studies found no difference in dropout rates between exposure therapy, cognitive therapy, stress inoculation training, and EMDR (Hembree et al., 2003). This effect has also been found in other
areas of psychotherapy research. A review of common evidence-based treatments for anxiety disorders found that a novel virtual reality exposure therapy had a nearly identical dropout rate compared to traditional exposure therapies. (Opris et al., 2012).

Identifying reasons why patients drop out of treatment remains difficult. An analysis of two different community samples of mixed-trauma PTSD patients undergoing exposure therapy found that dropout was only related to substance use (van Minnen, Arntz, & Keijsers, 2002). Specifically, alcohol use and benzodiazepine use predicted dropout among both samples, a finding replicated by other studies (Foa et al., 2013; Mills et al., 2012). However, substance use alone did not account for the vast majority of dropout. While many gold-standard RCTs report acceptable dropout rates from trials and use this as a proxy measure for treatment tolerability, recent real-world studies within community clinics paint a different picture (Najavits, 2015). In a very large \( n = 1924 \) study of VA patients seeking treatment for PTSD, only 2% of individuals remained in PE or CPT for all 12 sessions, and the median number of sessions attended was five (Watts et al., 2014). Further complicating matters are findings that dropout rates between studies of the same treatment type and population vary greatly (Imel, Laska, Jakupcak, & Simpson, 2013).

Other findings related to dropout are somewhat nonintuitive. Some researchers have suggested that behavioral interventions fail in part due to behavioral noncompliance and an inability to understand and adopt the treatment rationale (Hopko, Magidson, & Lejuez, 2011). Additional evidence points to patients dropping out due to a misinterpretation of early improvements in well-being at the outset of therapy initiation (Fernandez, Salem, Swift, & Ramtahal, 2015). In other words, patients who felt some improvement at the beginning of treatment no longer felt the need to return to subsequent therapy sessions. Again, this only
explains why a small portion of individuals drop out of treatment and may just be a manifestation of previously discussed phenomena of spontaneous recovery unrelated to treatment. Regardless, most of the variance associated with dropout remains unexplained.

Comorbidities Attenuate Treatment Response

Comorbidities may be a large contributing factor to treatment dropout and more broadly to treatment failure. Comorbidities contribute to treatment failure by increasing dropout rates and adding complexity to treatment protocols that may not be well-suited to treating multiple diagnoses. As the field of psychopathology has progressed, it has tended to split large areas of psychological dysfunction into smaller, more specific categories of disorders (Roy-Byrne, 2017). Using PTSD as an example, in order to meet DSM-5 criteria, an individual must endorse at least nine of the 20 individual symptoms, a handful from each symptom category. This rigid structure of symptom requirements may exclude many individuals suffering from post-trauma psychological problems. Researchers have conducted network analyses in an effort to determine which symptoms from the specified list (other than Criterion A) are most central to PTSD but have had mixed results. One study of Chinese earthquake survivors found hypervigilance, concentration difficulties, and nightmares to be most central to PTSD (McNally et al., 2015), meaning that these three symptoms capture the majority of the variance and were more predictive of the presence of other symptoms in Criteria B through E than were other symptoms of PTSD. Another study of US military veterans found that trauma-related emotions, flashbacks, detachment, and physiological cue reactivity were most central to PTSD (Armour, Fried, Deserno, Tsai, & Pietrzak, 2017). A third study of adult sexual assault survivors found physiological cue reactivity, nightmares, and loss of interest in previously enjoyed activities were
most central (McNally, Heeren, & Robinaugh, 2017). It is possible that each individuals’ presentation of PTSD is too unique to strictly adhere to one manualized treatment meant to target a handful of symptoms.

Mirroring the progression of psychological disorder classification, psychological treatments have also tended to target individual disorders and are less broadly applicable to multiple disorders or nonstandard symptom presentations. At the same time, the field is struggling to respond to the large-scale presence of comorbid disorders (Kessler, Ruscio, Shear, & Wittchen, 2010). Approximately 85% of patients with depression also experience significant symptoms of anxiety, and approximately 90% of patients with an anxiety disorder also experience symptoms of depression (Gorman, 1996). It is estimated that half of all patients diagnosed with PTSD also carry a diagnosis of depression (Rytwinski, Scur, Feeny, & Youngstrom, 2013), and comorbid substance use disorder is common among individuals with any mood disorder (Merikangas et al., 1998). Furthermore, individuals with comorbid psychological disorders tend to respond poorly to treatment as compared to those who have just one psychological diagnosis (Roy-Byrne, 2017).

As a result, many psychological disorders are associated with “secondary” symptoms that are not typically the direct focus of first-line psychotherapies. For example, people with mood disorders often suffer from sleep disturbances even post-treatment (Bunney et al., 2015; Chan et al., 2014; Lamarche & De Koninck, 2007). Sleep disturbances are frequently part of diagnostic criteria for mood disorders in the DSM-5, but this symptom is not necessarily the target for treatment. This symptom is often found to be a common residual symptom post-treatment (Nierenberg et al., 2010; Zayfert & DeViva, 2004) and may require adjunctive treatment in order to remit (Chung et al., 2015).
Improving Treatment Response Through Augmentation

In some instances, adding additional components of treatment have been shown to reduce dropout and noncompliance. Adding cognitive therapy elements to an exposure therapy intervention for individuals diagnosed with OCD lessened dropout rates (Vogel et al., 2004; effect size was “large” but unreported). This type of CBT add-on may be particularly effective for individuals undergoing somewhat aversive treatment protocols like exposure therapy for PTSD (Foa et al., 1999) and ERP for OCD (Najmi & Amir, 2010). For psychopharmacotherapy, it may be particularly beneficial to add an additional cognitive or behavioral therapy, like motivational interviewing, to increase medication adherence (Balán et al., 2013). It has been well-established that, for many mood disorders, combining psychotherapy and pharmacotherapy improves treatment response (Fava & Rush, 2006), but this also has been found in some studies to reduce dropout (Biesheuvel-Leliefeld et al., 2015; Thase et al., 2007). These additional components have also been found to be beneficial when treating populations with multiple comorbidities (Jones et al., 2011).

The General Applicability of First-Line Treatments

First-line treatments may not target symptoms that one might expect. For example, psychotherapy for treating individuals with MDD may not actually be very effective for hopelessness or suicidal ideation. A meta-analysis of studies reporting the effects of CBT on hopelessness and suicidal ideation found that this particular treatment modality was insufficient at reducing these symptoms in individuals with MDD (Cuijpers, de Beurs, et al., 2013). Most studies define treatment response as either a set level of symptom reduction or a reduction in
symptoms that fall under a clinical cutoff. However, since most symptoms that fall under the umbrella of a diagnosis in the DSM-5 are weighted equally in symptom questionnaires (e.g., the PTSD Checklist for DSM-5 weights all 20 PTSD symptoms equally, and each question is scored as 0-4), there is room for certain symptoms to fall between the cracks and fail to improve in a substantial proportion of patients receiving a particular targeted psychotherapy.

Comorbid Cognitive Problems

In addition to residual sleep problems, another major category of “secondary” symptoms is cognitive problems. Difficulty concentrating is a frequent symptom among people with psychological disorders and is the most common diagnostic criterion found across all emotional disorders in the DSM-5 (Hallion, Steinman, & Kusmierski, 2018). Other cognitive problems are also significant to individuals with psychopathology and these factors are not always present in diagnostic manuals. Cognitive problems, especially problems with attention, memory, and executive functioning, have all been found to be associated with a variety of mental illness. This association has been well studied within several specific disorder literatures.

A meta-analysis found that there is high quality evidence to support the notion that PTSD is associated with poor memory performance beyond the specific dysfunction of processing and recalling trauma-relevant memories (Brewin, Kleiner, Vasterling, & Field, 2007). Memory for emotionally neutral information was found to be significantly worse (average and median effect sizes were moderate) in individuals with PTSD compared with healthy controls. Epidemiologic data in the US military veteran population confirms that prevalence rates of memory impairment are significantly elevated for individuals diagnosed with PTSD (Stricker et al., 2017). Research into other cognitive domains that are associated with PTSD has continued since the Brewin
meta-analysis. A more recently updated and broadly focused meta-analysis found that of those who develop PTSD after experiencing a traumatic event, many also develop cognitive problems associated with the disorder (Zoellner, Pruitt, Farach, & Jun, 2014). These cognitive problems include attention, verbal memory, speed of information processing, and general executive functioning (Scott et al., 2017; Scott et al., 2015; Wrocklage et al., 2016). These findings hold even when controlling for mild traumatic brain injury (mTBI; Scott et al., 2015). Further, sleep quality explains only a small portion of the relationship (Martindale, Morissette, Rowland, & Dolan, 2017).

Meta-analytic studies from other areas of clinical and neurological psychology suggest that cognitive impairment is a common symptom across many psychological disorders. A review of cognitive impairment within the depression literature almost exactly mirrors the previously discussed review on cognitive impairment within the PTSD literature, finding that individuals suffering from a current major depressive episode, as well as individuals diagnosed with MDD currently in remission, had cognitive deficits in executive function, memory and attention relative to controls (Rock, Roiser, Riedel, & Blackwell, 2014). Several other studies not included in this analysis due to strict inclusion criteria also found that cognitive impairments most commonly associated with depression include executive function, processing speed, attention, and verbal memory (Landro, Stiles, & Sletvold, 2001; McDermott & Ebmeier, 2009; Murrough, Iacoviello, Neumeister, Charney, & Iosifescu, 2011; Stordal et al., 2004). Importantly, these cognitive symptoms have been found to remain after treatment (Hasselbalch, Knorr, & Kessing, 2011), a finding that runs contrary to the previously discussed finding that cognitive impairment significantly diminished in individuals successfully treated for PTSD.
Individuals with GAD have been found to exhibit cognitive impairments in selective attention and working memory (Yang et al., 2015) which has been connected with decreased amplitude of N270, an endogenous event-related potential component also associated with cognitive impairment in depression (H. Wang et al., 2002) and Parkinson’s Disease (Mao, Wang, & Wang, 2005). Individuals with GAD have also been found to have poorer concentration abilities compared to healthy controls (Hallion et al., 2018) which is consistent with recent cognitive models of GAD and depression that posit cognitive control as a key mechanism in anxiety (Eugène, Joormann, Cooney, Atlas, & Gotlib, 2010; Hirsch and Mathews 2012; Joormann & Gotlib, 2008; Joormann & Gotlib, 2010). Individuals with OCD have also been found to exhibit cognitive impairments in executive function and memory (Martinez-Gonzalez & Piqueras-Rodriguez, 2008; Olley, Malhi, & Sachdev, 2007), although authors tend to note problems with diagnostic heterogeneity for OCD as a limitation.

Interestingly, among a sample of veterans seeking treatment for PTSD at a VA hospital, self-reported cognitive problems are almost universal, and objectively-assessed cognitive problems are found in almost 50% of this sample (O’Neil et al., 2017). However, complicating matters are findings that objectively-measured cognitive symptoms based on the CVLT and portions of the Wechsler Adult Intelligence Scale (WAIS-IV) do not significantly correlate with self-reported cognitive impairment (Jak et al., 2015; O’Neil et al., 2017). This suggests that self-assessment of cognitive deficits may be unreliable. However, a limitation of these studies is that they did not use a validated measure of self-reported cognitive problems, but instead simply asked directly whether or not participants had experienced symptoms like concentration difficulties or forgetfulness.
Cognitive Problems and Treatment Response

In addition to the previously discussed factors that may influence treatment failures, cognitive factors can also play a role in treatment response in cognitive based psychotherapies. For example, Haaland et al. (2016) found that pre-treatment learning and memory predicted treatment outcomes for veterans participating in PTSD group therapy with exposure, cognitive restructuring, or skills training components. Importantly, successful treatment was associated with higher levels of executive control over purposeful attention switching processes and increased ability to inhibit automatic attention switching to irrelevant stimuli. In this study, it was unclear whether improved executive control led to reduced PTSS symptoms, or whether reduced PTSS led to improved executive control.

Research specifically in the domain of cognitive behavioral therapy supports that cognitive ability may be positively related to treatment response for PTSD (Nijdam, de Vries, Gersons, & Olff, 2015; Wild & Gur, 2008). Nijdam et al. (2015) found that treatment response, as measured by the PTSD Checklist for the DSM-5 (PCL-5) change scores, were positively associated with pre-treatment encoding, short-term retrieval, long-term retrieval, and recognition performance as measured by the California Verbal Learning Test (CVLT-II) and the Rivermead Behavioral Memory Test (RBMT-3). The association between cognitive performance, particularly verbal memory ability, and treatment response to cognitive behavioral or exposure therapies has been found in the literatures relating to other disorders such as OCD (Vandborg, Hartmann, Bennedsen, Pedersen, & Thomsen, 2016; D'Alcante et al., 2012) and MDD (Douglas, Porter, Knight, & Maruff, 2011; Story, Potter, Attix, Welsh-Bohmer, & Steffens, 2008). Perhaps unsurprisingly, these findings suggest that individuals with greater cognitive ability have an
advantage over individuals with lower cognitive ability when it comes to successfully completing cognitive psychotherapy.

Alternatives to First-Line Treatments

Taken together, these findings illustrate some of the factors that may contribute to an individual responding poorly to treatments in RCTs. As a result of treatment failures, treatment barriers, or residual symptoms, people may seek out adjunctive therapies or alternative therapies in order to address the entirety of their mental health symptoms. These alternatives are commonly referred to as Complementary and Alternative Medicine (CAM). Individuals suffering from PTSD commonly seek out alternatives to first-line treatment to manage their symptoms regardless of evidence that would support them doing so (Libby, Pilver, & Desai, 2013). This is also the case for other common psychological disorders (Jorm, Christensen, Griffiths, & Rodgers, 2002; Shapiro et al., 2007; Wang, Berglund, & Kessler, 2000). Possibly as a result of the evidence-less behaviors of nonresponding patients, research has recently begun to study commonly utilized CAMs. A systematic review found that yoga is helpful as a CAM for patients being treated for PTSD, GAD, depression, and substance abuse (CADTH Rapid Response Reports, 2015a). The strongest evidence was for yoga as a CAM for depression. However, the quality of the current evidence is somewhat poor, and yoga interventions are difficult to study in the context of a systematic review due to the large degree of variance between yoga intervention protocols. Additionally, the same systematic review found that yoga was not acceptable as a monotherapy. On the other hand, mindfulness-based interventions (MBI) have been shown to be possibly effective as a monotherapy or adjunctive CAM (CADTH Rapid
Response Reports, 2015b). In particular, MBIs have the strongest support for preventing relapse in common psychological disorders such as depression and substance use.

Due to some of the shortfalls found within traditional psychotherapies for common psychological disorders, researchers have begun to focus on alternative standalone treatments with somewhat different theoretical underpinnings. For example, ACT has been studied for the treatment of a wide variety of mental illness (Öst, 2014). Initially, ACT appeared to be superior to standard CBT protocols for treating anxiety and depression (Ruiz, 2012). However, more recently a greater level of methodological scrutiny has been applied to ACT and no difference in treatment outcomes between high-quality traditional CBT and high-quality ACT studies was observed (Öst, 2014). A similar sequence of events can be observed for behavioral activation therapy for depression (Ekers et al., 2014). In fact, a recent meta-analysis found that most common treatments for depression were largely equivalent to each other, with most patients improving after 8-12 weeks but a sizeable minority remaining symptomatic (Barth et al., 2013). A similar finding was present in a meta-analysis of treatments for OCD which found that exposure and response prevention (ERP) was equivalent to standard cognitive therapy, and group treatment was not significantly different from individual treatment (Öst, Havnen, Hansen, & Kvale, 2015). Because multiple seemingly equivalent independent treatment options are available for most psychological disorders, but CAMs have been found to enhance treatment response when attached to a variety of treatment modalities, it is valuable to identify and further study adjunctive treatments. Furthermore, it is likely best to focus on treatments with the lowest possible patient burden as well as CAMs or adjunctive treatments with high degrees of additive effectiveness.
Many modern psychological interventions, whether they target cognitions, conscious behaviors, or unconscious behaviors, attempt to restructure an individual’s neurobiology via some therapeutic process and researchers commonly seek to establish a translational connection between neuroscience and clinical psychology (e.g., Brewin, Gregory, Lipton, & Burgess, 2010; Briscione, Jovanovic, & Norrholm, 2014; Flanagan et al., 2018; Zhu et al., 2018). An important differentiator between these different forms of psychotherapy are the level of awareness at which these neurobehavioral changes are implemented. Neurobehavioral interventions are a promising class of empirically supported treatments that do not focus on cognitive/behavioral patterns surrounding traumatic events (Fergus & Bardeen, 2016). Neurobehavioral therapies instead focus on the underlying biological mechanisms that are implicated in the maintenance of psychological dysfunction (Siegle, Ghinassi, & Thase, 2007). While it has been found that cognitive-behavioral treatments result in physical changes in the brain, these physical changes are not the direct focus of the therapies (Fergus & Bardeen, 2016). Neurobehavioral therapies may be indicated, therefore, for individuals who struggle with the purposeful behavioral or cognitive restructuring found in cognitive behavioral therapies and some exposure-based therapies due to their cognitive difficulties. Furthermore, many neurobehavioral therapies fall into the previously discussed category of minimally-burdensome CAMs that can be used as adjunctive treatments.

The Attention Training Technique

The attention training technique (ATT) is one such neurobehavioral therapy that has been shown to be effective in the treatment of anxiety and depressive disorders. ATT involves daily auditory monitoring tasks that are attentionally demanding and are designed to strengthen attentional control (Wells, 2009). A single session of ATT occurs over no more than 15 minutes.
each day. A meta-analysis of 10 ATT effectiveness studies found large effect sizes for the reduction of clinical symptoms across mood disorders (Knowles et al., 2016). Specifically, this review found that, within the pooled sample of 10 RCTs, ATT is associated with greater treatment gains than reference groups (which included waitlist controls as well as more active treatment conditions like progressive muscle relaxation) across a majority of outcomes (e.g., anxiety and depression symptoms; adjusted Cohen’s d range: 0.40–1.23). Despite increasingly strong evidence indicating that ATT reduces symptoms of emotional disorders, even as a standalone intervention, it remains underutilized (Fergus & Bardeen, 2016). Given the previous discussion on effect sizes of newly developed psychological treatments, ATT also remains understudied.

The current literature suggests that ATT has several advantages. Although originally developed for use in depression, preliminary evidence suggests it has broad and nonspecific applicability across many psychological disorders. It is based on a neurobehavioral theory, rather than a cognitive-behavioral theory, which might differentiate it enough to help those who would otherwise not respond to a cognitive behavioral treatment. ATT is also inexpensive to implement and time-effective, so it is unlikely to contribute to increasing common factors that may lead to dropout. In addition, ATT directly addresses cognitive problems that are present in many mental disorders but are not the direct focus of treatment.

### Theoretical Basis of ATT

ATT is theoretically grounded in the self-regulatory executive function (S-REF) model, which posits that psychological dysfunction arises from poor self-regulatory strategies that lead to distress, culminating in mood disorder (Wells, 2009). The S-REF model of psychological
function is rooted in metacognitive theory. Metacognitions are internal cognitive factors that “control, monitor, and appraise thinking” (Wells, 2009). Metacognitive theory assumes that maladaptive metacognitive beliefs and cognitive control strategies disrupt adaptive self-regulation strategies leading to the persistence of mental disorder. For example, the proclivity to worry and ruminate, attend to imagined or unlikely threatening possibilities, and cope with unpleasantness with avoidance strategies are all maladaptive metacognitive strategies that perpetuate mental disorder and sustained thoughts about danger.

Several studies have been performed that support metacognitive theory. Metacognitive beliefs about memory are associated with trauma symptoms more strongly than features of memory (Bennett & Wells, 2010). Positive outcomes in CBT for obsessive-compulsive disorder and alcohol addiction are associated with metacognitive beliefs (Solem, Haland, Vogel, Hansen, & Wells, 2009). The range of applications of the metacognitive model is widening to include chronic fatigue syndrome (Maher-Edwards, Fernie, Murphy, Wells, & Spada, 2011) and emotional susceptibility to life stress (Yilmaz, Gencoz, & Wells, 2011).

In the S-REF model, the problematic pattern of metacognitive thinking that arises from poorly regulated attention is referred to as the cognitive attentional syndrome (CAS) and is generally responsible for psychological disturbance in some anxious individuals (Wells, 2007). The S-REF model describes maladaptive executive functioning as inflexible, which leads to recurrent styles of thinking when faced with negative emotions or beliefs (Wells 2009, 2013). The sustained processing of these negative emotions or beliefs, rather than flexibly switching attentional resources to more productive processing, leads to anxiety and distress. This also leads to observed cognitive deficit related to lower levels of accessible attentional resources (Wells, 2007). Importantly, and most differently from the theoretical bases of most cognitive behavioral
therapies, ATT seeks to modify negative beliefs about oneself not by questioning their validity, but by encouraging alternative thinking strategies (Wells, 2007). Many other psychological theories focus on inflexible patterns of thoughts, (e.g., mindset theory; Dweck, 1999), but metacognitive theory uniquely emphasizes the resulting impact of that inflexibility on cognitive functioning and resources (Wells, 2009).

ATT also aligns well with attentional control theory (Eysenck, Derakshan, Santos, & Calvo, 2007), which posits that high levels of anxiety impair attentional switching and inhibition by overloading the cognitive resources required to properly utilize top-down attentional control processes. Attentional control theory states that anxiety increases the allocation of attention to a threat-related stimulus and decreases attention to the current task, unless that task is dealing with the threatening stimuli. There are other scenarios wherein an anxious individual feels more abstractly threatened in the absence of a directly threatening stimulus. In this case, attentional control theory states that the anxious individual will engage in excessive threat scanning and will divide attentional resources to many areas of the environment, reducing the ability to focus on a single task. ATT seeks to reduce maladaptive forms of attentional focus, such as inflexibly and repetitively processing threat (Wells, 2013), while simultaneously increasing or strengthening the use of top-down attentional control ability (Bar-Haim, 2010).

The S-REF model and attentional control theory are well-suited to explain previous findings that individuals with anxiety disorders allocate more attentional resources towards threatening stimuli than healthy controls (Buckley, Blanchard, & Neill, 2000). Furthermore, the S-REF model is supported by findings that worry is attentionally demanding and is a key feature in many psychological disorders (Hazlett-Stephens, 1997; Mellings & Alden, 2000). In people with PTSD, several studies using the Stroop task found that processing disruptions were common
during attention to trauma-related words (Dalgleish et al., 2003; Foa, Feske, Murdock, Kozak, & McCarthy, 1991). Similar findings from dot probe tasks have also been reported (Elsesser, Sartory, & Tackenberg, 2004, 2005; Fani, Bradley, Ressler, & Tone, 2011). Attention to trauma-related stimuli in the absence of threat activates the CAS (Fergus, Bardeen, & Orcutt, 2011), a finding which supports the validity of the overall S-REF model of psychopathology. ATT seeks to directly address these maladaptive disruptions in normal attention processes.

The S-REF model of psychopathology is based firmly in clinical models but is paralleled and perhaps incidentally supported by behavioral neuroscience studies (Fan, McCandliss, Fossella, Flombaum, Posner, 2005; Fan, McCandliss, Sommer, Raz, Posner, 2002; Posner & Petersen, 1990; Posner & Rothbart, 2007). These studies indicate that the human attentional system is made up of three separate networks that encompass alerting, orienting, and executive control (Fan et al., 2009; Fan et al., 2002). In this model – commonly referred to as the Posner model – the alerting network is responsible for initiating and maintaining vigilence and alertness, the orienting network allows attention to move through space based on relevant sensory input, and the executive control network is responsible for monitoring the attentional system overall and to resolve any conflicts that arise between expectations, stimuli, and responses (Macleod et al., 2010). One review of this theory found evidence that the executive functioning system is impaired in individuals with borderline personality disorder, schizophrenia, and PTSD, but not the alerting or orienting networks, which seem to be impaired by physical injury such as concussions (Macleod et al., 2010). This finding is important to the field of clinical psychology, since studies seeking to measure variables of attention in clinical populations must use a measure of attention that is capable of distinguishing between attentional systems. These results also
suggest that broad measures of executive attention that combine all three attentional systems into one are not sufficient for clinical outcomes studies.

In order to establish the underlying mechanisms of change in mindfulness-based interventions (MBIs), researchers have successfully applied the Posner model of attention to clinical outcomes data (e.g., Jha, Krompinger, & Baime, 2007; Malinowski, 2013; Tang, Holzel, & Posner, 2015; van den Hurk, Giommi, Gielen, Speckens, & Barendregt, 2010). Specifically, Tang et al. (2007) found that a short-term mindfulness meditation training administered in 20-minute sessions over a period of five days significantly increased the efficiency of the executive control network in resolving conflicts between expectations and visual stimuli in a behavioral task meant to assess the three attention networks referred to as the attention network task (ANT; Fan et al., 2002). Importantly, only the executive control network was significantly affected by the mindfulness intervention. The alerting and orienting networks were unaffected. The study also found that, compared with the control group, the mindfulness intervention group reported lower anxiety, depression, anger, and fatigue. This suggests that one of the mechanisms of change in mindfulness is through improving the functioning of the executive control network. Reviews of MBI studies have found that, along with body awareness, emotion regulation, and change in perspective on the self, attention regulation is a clear mechanism of therapeutic change in MBIs (Holzel et al., 2011). Furthermore, a cross-sectional study using a Stroop task found that meditators exhibited less Stroop interference than nonmeditators (Moore & Malinowski, 2009) and another study found that a mindfulness meditation intervention improved within-subjects sustained attention on the continuous performance task (CPT; Rosvold, Mirsky Sarason, Bransome, & Beck, 1956), a measure frequently used in assessing broad attentional impairment.
Overall, the literature makes it evident that MBIs improve clinical symptoms of participants at least in part through improving attentional control. However, little is known about the mechanisms of therapeutic change in metacognitive therapies as they relate to attention and other executive functions. This is in spite of an inclusion in many metacognitive therapies of a neurobehavioral intervention component meant to directly address attentional concerns (ATT; Wells, 2009). To date, only one very recent study has evaluated the direct effect of ATT on networks of attention (Barth et al., 2019). This study includes several measures of attention but did not include the ANT which is able to specifically distinguish between relevant components of the human attentional system. Furthermore, this study did not measure any relevant psychological symptoms of common disorders such as symptoms of depression or anxiety, and therefore is an insufficient investigation into actual mechanisms of therapeutic change, since no therapeutic change was measured.

The Therapeutic Process of Change in ATT

ATT involves daily auditory monitoring tasks that are attentionally demanding and are designed to strengthen attentional control (Wells, 2009). The first session of ATT is typically conducted in-person and involves psychoeducation on the theoretical basis for ATT, including how unnecessary sustained processing of threat stimuli is unhelpful (Fergus & Bardeen, 2016). An audio recording is played and the client is instructed to attend to particular auditory stimuli within the recording. A single session of ATT takes about 12 minutes. Subsequent sessions of ATT beyond the first in person session can be performed at home, and a handful of studies have demonstrated the effectiveness of home-based ATT (Callinan, Johnson, & Wells, 2015; Fergus, Wheless, & Wright, 2014; Siegle et al., 2007; Siegle et al., 2014). Clients spend approximately
five minutes on a selective attention task wherein they attend to specific sounds in the recording and exclude other sounds (Fergus & Bardeen, 2016). The next set of instructions tell clients to perform an approximately 5-minute attention switching task in which they rapidly switch their attention from one sound to another. The final two minutes are spent on a divided attention task in which clients attempt to pay attention to multiple sounds at once. Positive outcomes for ATT are increased external focus of attention (S-REF theory indicates that excessive internal foci of attention are related to worry and rumination) and increased attentional control. Attentional control is the use of top-down executive attentional processes to regulate bottom-up emotional responses (Derryberry & Reed, 2002). The construct is of particular interest to research related to mood disorders, PTSD, and other forms of emotion dysregulation that fall within the scope of the S-REF and attentional control theories.

While ATT is founded upon a compelling theoretical framework and has been shown to be clinically effective, there is a considerable lack of research regarding the process of therapeutic change (Fergus & Bardeen, 2016). ATT is a process-focused therapy – a category that also includes mindfulness-based interventions (Hayes, Villatte, Levin, & Hildebrandt, 2011). ATT and mindfulness-based interventions both focus on regulating the focus on attention (Wells, 2009). The primary theoretical difference between the two is that mindfulness-based interventions seeks to increase adaptive attention patterns, while ATT seeks to reduce maladaptive attention patterns (Fergus & Bardeen, 2016). Many current cognitive therapies are partially based on the mindfulness construct. These therapies apply the concept of mindfulness as a skill that clients can learn through diligent guided practice in order to reduce symptoms and increase quality of life more generally (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006).
More work is necessary to solidify the distinction between mindfulness and ATT based processes of therapeutic change. A recent study on the therapeutic processes found that both ATT and mindfulness interventions were associated with changes in a variety of cognitive factors of interest including present-focused attention and metacognitive beliefs (McEvoy, Graville, Hayes, Kane, & Foster, 2017). The two interventions differed in that the mindfulness-based intervention was associated with internal shifts in attention, but ATT was not associated with significant shifts in attention in either direction. This contrasts with a previous study on ATT mechanisms which found that ATT was associated with a significant outward shift of attentional focus (Fergus et al., 2014), which would place the two interventions at opposing ends of a spectrum of attentional focus. Importantly, McEvoy et al. (2017) found that performance on a Stroop task was improved in both a mindfulness-based intervention and ATT, and both interventions increased Stroop performance relative to a thought wandering control group. The similar results between ATT and the mindfulness-based intervention suggest that the two psychotherapies may share similar therapeutic processes of change. However, a regression analysis based on survey data collected from a different study that measured mindfulness and CAS levels (measured as negative metacognitions) found that the two constructs differentially predict levels of psychopathology in a community sample (Solem, Thunes, Hjemdal, Hagen, & Wells, 2015). This study found no correlation between mindfulness and psychopathology, which is a highly uncommon result within the mindfulness literature. This aberrant finding requires further research. Overall, there is limited evidence regarding the processes of therapeutic change in ATT and whether it is truly distinct from other therapeutic approaches.
Ecological Momentary Assessment of ATT

Ecological momentary assessment (EMA), also referred to as ambulatory assessment (AA), is an excellent way to study therapeutic changes over time (Carpenter, Wycoff, & Trull, 2016), particularly if the constructs being studied are prone to frequent fluctuation. EMA is a general term for any assessment method that are used to study people in their natural environment. Individuals who may have trouble focusing their attention in their everyday life may not demonstrate this in a laboratory setting. Similarly, people with threat biases related to their anxiety may not perceive threats in a laboratory setting as they would in their natural environment.

EMA also has the advantage of providing multiple data points for a measure over time. Constructs measured over two or three points in time risk confounding variables having a large effect on outcome variables. For example, attention has been found to have a diurnal pattern (Marek et al., 2010) and is moderated by caffeine intake (Kahathuduwa et al., 2018). It is unknown whether the constructs of interest to the current research study, mindfulness and attentional control, fluctuate dramatically from day to day or whether they are relatively stable. In theory, both constructs should be stable, but both measures have been used as tracking measures in intervention studies which would suggest they are not. While this study collected data one time per day, it included both weekends and weekdays in its protocol. This may have helped to control for day-to-day fluctuations in the study’s outcome variables that were not attributable to experimental manipulation, whereas simple pre-post designs would be unable to control for these fluctuations.
Another advantage of EMA is that it avoids the potential biasing effects of responding to survey questions in a lab setting which is not as private as at home. Methodological research on retrospective responding (such as questionnaires that ask “in the past week…”) is consistent with findings that memory is fallible and therefore may produce unintentionally inaccurate responding on the part of research participants (Shiffman et al., 2008). Additional advantages to collecting data outside of a lab setting have been observed. Self-report survey items are unintentionally sensitive to the mood of study participants (Chun, 2016). EMA studies are able to be administered via smartphone technologies and can allow participants to conveniently live their lives as they would normally without having to come in to a lab to answer questions. As a result, inconvenient and rigidly scheduled data collection does not have to occur and does not run the risk of negatively affecting a participant’s mood. On the other hand, this study’s methodology cannot assure that participants will be at home while completing the study. As a result, participants may not be carrying out the ATT or PMR activities in optimal conditions.

However, although this methodology maximizes the convenience to participants and significantly reduces potential barriers to treatment fidelity and adherence, it is not without limitations. For instance, it has been established that mood tends to decrease throughout the day (Golder & Macy, 2011). Furthermore, in clinical populations such as in individuals suffering from MDD, mood has been found have a more pronounced diurnal pattern than compared with healthy controls (Peeters, Berkhof, Delespaul, Rottenberg, & Nicolson, 2006). While this may introduce a potentially confounding variable to this study, the study methods allowed for potential statistical control.
This Study

The limitations of the extant literature reviewed above indicate that there is a need for further research into broadly applicable, neurobehavioral adjunctive therapies that are aimed at enhancing treatment response and reducing treatment dropout. Current psychotherapies, while sufficient for many, are not effective for all. Efforts to address this have perhaps mistakenly focused on developing new systems of psychotherapy meant to address very specific problems. And yet, the current psychological nosology is plagued with problems rooted in heterogeneity and nonspecificity, which may explain why newly developed systems of psychotherapy that seek to target a specific disorder are no more effective than previously established psychotherapies. The use of additional components to treatment that are broadly applicable across psychopathologies is a promising alternative, and some adjunctive therapies have already been shown to reduce dropout and noncompliance. Cognitive factors can also play a role in treatment response to cognitive-based psychotherapies, yet these areas that contribute to treatment nonresponse are understudied. One promising adjunctive therapy that may reduce dropout and noncompliance as well as address some cognitive factors that attenuate treatment response is the attention training technique. Determining the mechanisms of therapeutic change is a fundamental and vital part of studying adjunctive treatments that ensures they are as effective and broadly applicable as they purport to be. However, researchers have yet to undertake this important scientific work regarding the mechanisms behind ATT. This study seeks to fill this gap in the literature by testing two proposed mechanisms by which ATT functions: mindfulness and attentional control.
Hypotheses

Hypotheses are presented following a brief overview of the study design. The study employed a randomized controlled trial (RCT) EMA design. Participants in the experimental condition listened to a 12-minute ATT recording once per day for a week from their smartphone. Participants in the control condition listened to a progressive muscle relaxation (PMR) exercise for 12 minutes daily for a week from their smartphone. PMR was selected for the control condition in order to maximize the direct applicability of the study results to previous studies that frequently have used PMR in a similar manner (see Fergus & Bardeen, 2016 for a list of control conditions). After listening to their respective recordings, participants received short mindfulness and attentional control questionnaires. This study sought to further expand the literature by examining the therapeutic change processes of ATT. Furthermore, this study may inform whether ATT is a helpful adjunctive therapy for individuals who are not well-suited for therapies that demand higher levels of cognitive abilities.

Measures Check Hypothesis

Hypothesis 0: Scores on the ANT, particularly the executive control network score administered at Time 1 will be significantly positively associated with Time 1 scores on the Attentional Control Scale (ACS). If this is not the case, the ACS scores will be conceptualized as perceived attentional control rather than a measure of attentional control. Initial ratings of Time 1 distress (depression, anxiety, stress as measured by the DASS-21) will predict levels of distress at time 7. Initial ratings of mindfulness (as measured by the FFMQ-SF) will predict levels of mindfulness at time 7.
**A Priori Hypotheses**

*Hypothesis 1:* Consistent with previous findings in the literature, ATT will reduce the presence and severity of anxiety and other mood symptoms in the sample relative to controls.

*Hypothesis 2:* ATT will improve attentional control as measured by the Attentional Control Scale (ACS) and the executive attention score on an in-person ANT.

*Hypothesis 3:* ATT will increase levels of mindfulness as measured by the Five Facet Mindfulness Questionnaire - Short Form (FFMQ-SF).

*Hypothesis 4:* Attentional control and mindfulness will vary together over time in both the ATT and control groups.

*Hypothesis 5:* ATT will reduce self-reported problems of attention and concentration in individuals compared to controls as measured by the Metacognitions Questionnaire (MCQ) and Thought Control Questionnaire (TCQ).

**Exploratory Hypotheses**

*Hypothesis 6:* Higher scores on the ANT executive control measure will attenuate the effect of ATT on increases in ACS scores. In other words, ATT will be more effective for individuals with lower baseline ANT executive control, and will not be able to increase attentional control beyond a certain ceiling.

*Hypothesis 7:* If FFMQ scores increase as expected, the Observing factor of the FFMQ will increase more than the other factors. Improving attentional capacity may directly improve observational abilities. ATT would be unlikely to increase nonjudgement.
CHAPTER 2

METHODS

Participants

Participants were 67 Northern Illinois University (NIU) undergraduate students enrolled in an introductory psychology course. However, one participant did not meet full study inclusion criteria and was excluded from the analyses. Additionally, two participants exhibited patterns of responding that were not effortful (completing Time 1 surveys in five minutes and seven minutes instead of approximately 25 minutes, performing the ANT task with an unacceptably low accuracy rate indicative of random responding, and performing no follow-up study timepoints) and were removed from the analysis. In total, the final sample was two groups of 32 participants, for a total of 64 participants ($M_{age} = 20.13, SD = 3.65$; 42.2% Male; 64.1% non-Hispanic White; 23.4% Black; 9.4% Hispanic/Latino; 3.1% Other). There were no significant demographic differences between groups (Tables 1 and 2). Furthermore, there were no significant baseline differences between groups in terms of ANT performance, FFMQ total scores, and ACS total scores. Participants received course credit as compensation for their participation. Due to the nature of electronic EMA, credit was granted in a piecemeal fashion after participants completed each time point. Inclusion criteria were students with an Android (version 12.0 or later) or Apple (iOS 8 or later) smartphone with internet access at home who previously reported some level of anxiety, stress, depression, PTSD, or obsessive-compulsive (OC) symptoms during a general psychological survey conducted at the beginning of the academic semester. Individuals met this
inclusion criteria if they endorsed any symptom on a questionnaire greater than the midpoint of
the scale’s response options (e.g., if an item on a symptom severity scale is scored from 1 to 5, a
response of 4 or 5 would qualify an individual for the study). Individuals also met inclusion
criteria if their responses reach a clinical cutoff of at least mild. People with psychosis (0 in this
sample), suicidal ideation (0 in this sample), or auditory processing deficits (1 in this sample)
were excluded from the study. Inclusion/exclusion criteria screening failures, as well as those
who do not pass a manipulation check did not count toward the total number of participants.

Table 1
Categorical Variable Differences Between ATT and PMR Groups ($\chi^2$) – Time 1

<table>
<thead>
<tr>
<th></th>
<th>$\chi^2$ Statistic</th>
<th>df</th>
<th>$p$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sex</td>
<td>.064</td>
<td>1</td>
<td>.800</td>
</tr>
<tr>
<td>Race</td>
<td>10.341</td>
<td>5</td>
<td>.066</td>
</tr>
</tbody>
</table>

Note: ATT = Attention Training Technique, PMR = Progressive Muscle Relaxation.

Sample Size

Hierarchical linear growth models were necessary to analyze the results of this study
because grouped data violate the assumption of independence of all observations that is required
for standard multivariate models (Maas & Hox, 2005). However, estimation of required sample
size in multilevel modeling (MLM), also referred to as hierarchical linear modeling (HLM), is
difficult due to the complexity of multilevel models (i.e., whether they utilize cross-level
interactions or simple fixed effects; Scherbaum, 2009). This study utilized a repeated measures
design over the course of one week with two groups – a control and an ATT group. Level 2 of
Table 2
Continuous Variable Differences Between ATT and PMR Groups (ANOVA) – Time 1

<table>
<thead>
<tr>
<th></th>
<th>ATT Group Mean (SD)</th>
<th>PMR Group Mean (SD)</th>
<th>F-Statistic</th>
<th>df</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>21.0 (4.80)</td>
<td>19.3 (1.55)</td>
<td>3.855</td>
<td>62(1)</td>
<td>.054</td>
</tr>
<tr>
<td>Years of Education</td>
<td>13.0 (1.33)</td>
<td>12.8 (1.11)</td>
<td>.374</td>
<td>62(1)</td>
<td>.543</td>
</tr>
<tr>
<td>Mother’s Education</td>
<td>14.8 (2.61)</td>
<td>13.9 (2.06)</td>
<td>2.057</td>
<td>62(1)</td>
<td>.157</td>
</tr>
<tr>
<td>Father’s Education</td>
<td>14.9 (3.08)</td>
<td>14.4 (3.15)</td>
<td>.522</td>
<td>62(1)</td>
<td>.473</td>
</tr>
<tr>
<td>ANT - Alerting</td>
<td>38.4 (25.27)</td>
<td>37.0 (24.02)</td>
<td>.052</td>
<td>62(1)</td>
<td>.820</td>
</tr>
<tr>
<td>ANT - Orienting</td>
<td>39.9 (25.04)</td>
<td>37.6 (18.42)</td>
<td>.182</td>
<td>62(1)</td>
<td>.671</td>
</tr>
<tr>
<td>ANT - Executive</td>
<td>134.8 (43.51)</td>
<td>132.5 (63.27)</td>
<td>.029</td>
<td>62(1)</td>
<td>.865</td>
</tr>
<tr>
<td>ANT - Overall RT</td>
<td>632.4 (79.18)</td>
<td>623.2 (64.18)</td>
<td>.262</td>
<td>62(1)</td>
<td>.611</td>
</tr>
<tr>
<td>ACS</td>
<td>52.0 (7.85)</td>
<td>50.3 (6.64)</td>
<td>.862</td>
<td>62(1)</td>
<td>.357</td>
</tr>
<tr>
<td>FFMQ-SF</td>
<td>59.2 (9.64)</td>
<td>58.22 (9.16)</td>
<td>.181</td>
<td>62(1)</td>
<td>.672</td>
</tr>
<tr>
<td>DASS-Stress</td>
<td>7.4 (4.20)</td>
<td>7.2 (4.94)</td>
<td>.045</td>
<td>62(1)</td>
<td>.833</td>
</tr>
<tr>
<td>DASS-Anxiety</td>
<td>4.8 (4.57)</td>
<td>4.0 (4.54)</td>
<td>.471</td>
<td>62(1)</td>
<td>.495</td>
</tr>
<tr>
<td>DASS-Depression</td>
<td>5.0 (5.31)</td>
<td>4.7 (4.38)</td>
<td>.099</td>
<td>62(1)</td>
<td>.754</td>
</tr>
</tbody>
</table>

Note. ATT = Attention Training Technique, PMR = Progressive Muscle Relaxation, SD = Standard Deviation, ANT = Attention Network Task, RT = Reaction Time, ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire – Short Form, DASS = Depression, Anxiety, and Stress Scale.
the data is the between-subjects grouping variable (ATT vs control), while Level 1 is the within-subjects repeated measures nested within Level 2. The average cluster sizes for the highest levels of analyses are the limiting factor for HLM analyses (Snijders, 2005). Sample sizes of less than 30 at the highest cluster level are associated with smaller standard errors for fixed effects (Maas & Hox, 2005) which risks reduces the ability of the analysis in finding smaller effect sizes. A sample size of 60 will allow the study to avoid inappropriately small standard errors for fixed effects while also allowing for the analysis of cross-level interactions. However, a sample of this size will likely not allow for post hoc exploratory analysis including covariates in cross-level interactions, which can more than quadruple some sample size requirements (Scherbaum & Ferreter, 2009). This study has a sample size of 64, meaning that, for each experimental group, the highest cluster level will have a sample size of 32 which is adequately powered. However, exploratory analyses would not be adequately powered, especially those that included cross-level interactions. Therefore, the planned analysis for Hypothesis 6 is underpowered, as it is exploratory in nature and includes multiple cross-level interactions between executive attention and time. All other planned HLM analyses are adequately powered.

Measures

Attention Network Test (ANT)

The ANT is a behavioral reaction time (RT) measure of attentional alerting, orienting, and executive control (Fan et al., 2002). The test is a combination of the commonly used flanker task (see Eriksen & Eriksen, 1974) and a simple cued reaction time task (see Posner, 1980). The ANT provides measures of both RT and error rates (ER), but ER results are rarely reported
There are several test conditions of interest in the ANT that produce scores that are indications of efficiency in the three separate attention networks. In all conditions, participants indicate the direction of an arrow presented in the center of the screen that is flanked by two additional arrows on both its right and its left. The first condition is the congruent condition in which the flanking arrows are pointing in the same direction as the central arrow. The second condition is the incongruent condition in which the flanking arrows are pointing in the opposite direction. The third condition is a neutral condition in which no lines flank the central arrow. These three conditions are further modified by cues. Three types of cues precede the stimulus arrow (center cue, double cue, and spatially informative cue), all of which are temporally informative. Alternatively, no cue may precede the stimulus arrow which is referred to as a temporally uninformative condition. The center and double cues prompt the participant to expect that the stimulus arrow will appear soon with 100% accuracy (meaning that the participant is never falsely prompted of an impending stimulus arrow). The spatially informative cue indicates to the participant what direction the arrow will be facing, and this is also with 100% accuracy.

Three scores are computed from the raw data – the alerting network score, the orienting network score, and the executive control score. Macleod et al. (2010) summarize the scoring procedures in their review of the psychometric properties of the test. To calculate the alerting network score:

RT in the temporally informative double cue condition is subtracted from RT in the temporally uninformative no cue condition (averaging across all flanker conditions). For the orienting network score, RT in the spatially informative cue condition is subtracted from RT in the spatially uninformative central cue condition (averaging across all flanker conditions). Finally, the executive control network score is calculated by subtracting RT in the congruent flanker condition...
from RT in the incongruent flanker condition (averaging across all cue conditions). (p. 638)

The ANT is the only behavioral measure of attention that separates the three parts of the full attention network that are theoretically vital to distinguish between for clinical outcomes research (Macleod et al., 2010). Macleod and colleagues performed a review of the measure and its psychometric properties across several studies. The discriminant validity of the three distinct attention networks measured by the ANT is established by findings from neuroimaging studies that indicate that the three attention network scores derived from the ANT are each associated with different neuroactivity and anatomical structures in the brain (Coull, Nobre, & Frith, 2001; Fan et al., 2005). While convergent validity has not been specifically examined, the measure’s components are the flanker task (Eriksen & Eriksen, 1974) and a cued RT task (Posner, 1980) that are currently accepted measures of attentional control.

As for reliability, Macleod and colleagues (2010) reported that the test has low-to-moderate test-retest reliability. Specifically, the alerting network score has a test-retest reliability ranging from .36 to .52, the orienting score has a test-retest reliability ranging from .41 to .61, and the executive control score has a test-retest reliability ranging from .77 to .81. For this study, test-retest reliability was moderately important. The measure was theoretically expected to change based on a variety of factors including intervention (Tang et al., 2007), so a high test-retest reliability was not expected. The “test-retest” coefficient in this study was .60 for the alerting score, .61 for the orienting score, and .44 for the executive control score. The low value for the executive control score was expected because the ATT intervention is meant to specifically change participants’ executive control score on the ANT.
A computerized version of the ANT was presented to participants in-person at Time 1 in an isolated room with minimal distraction. Participants were given the option to return to the lab upon completion of the experience sampling portion of the study to be assessed using the ANT an additional time (Time 8). However, due to practical constraints, this follow-up in-person study session was not required (clearly outlined as “optional” in the informed consent process), and no compensation was provided to participants who chose to volunteer for the additional in-person portion. Mean overall response times, alerting effect, orienting effect, and conflict effect were similar in this study sample to that of others using college student samples in the literature (e.g., Seitz et al., 2016; Yu et al., 2019).

**Five-Facet Mindfulness Questionnaire - Short Form (FFMQ-SF)**

The Five-Facet Mindfulness Questionnaire - Short Form (FFMQ-SF) was developed through a factor analytic approach of the original FFMQ, a measure that was derived from five different mindfulness questionnaires (Baer et al., 2006). Each mindfulness questionnaire was developed independently, and each was meant to measure the construct of mindfulness. Mindfulness is a skill derived from eastern meditation traditions and is broadly defined as maintaining complete attention on experiences occurring at the present time without judgment (Baer et al., 2006). The original FFMQ is comprised of 39 items, each of which corresponds to one of the “five facets” (i.e., factors) that were identified in Baer et al.’s (2006) original factor analysis work on the mindfulness construct. The five factors are Observing (8 items), Describing (8 items), Acting with Awareness (8 items), Nonjudging (8 items), and Nonreacting (7 items). Each item uses the same 5-point scale. A score of 1 indicates never or very rarely true; a score of 2 indicates rarely true; a score of 3 indicates sometimes true; a score of 4 is sometimes true; a
score of 5 indicates very often or always true. Reliability coefficient alphas for the each of the five subscales are nonreactivity = .75, observing = .83, acting with awareness = .87, describing = .91, and nonjudging = .87 (Baer et al., 2006).

Baer et al. (2008) found evidence for each factors’ discriminant validity by showing that the individual factors of the scale differentially predict variance among different common psychological symptoms and other theoretically relevant psychological constructs. Many other studies have provided support for the FFMQ’s construct validity, and the measure appears to perform in theoretically consistent ways when predicting a variety of relevant outcomes (Barnes & Lynn, 2010; Cash & Whittingham, 2010; Fisak & von Lehe, 2012; Lavender, Gratz, & Tull, 2011; Park, Reilly-Spong, Gross, 2013). Strongest support for the measure’s validity are findings from studies that show MBIs significantly increase scores on the FFMQ (e.g., Banks, Newman, & Saleem, 2015; Katterman, Kleinman, Hood, Nackers, & Corsica, 2014; Kiken, Garland, Bluth, Palsson, & Gaylord, 2015). This is a vital characteristic of the mindfulness measure implemented in this study. The FFMQ-SF is ideal because it 1) has high reliability when not the focus of therapeutic intervention, 2) changes significantly over time when it is the focus of therapeutic intervention, 3) captures the full construct of mindfulness, and 4) is short and time-efficient, a necessary condition for EMA measures.

Using a Rasch analysis methodology, Medvedev, Titkova, Siegert, Hwang, and Krageloh (2018) empirically derived a modified short form of the FFMQ, the FFMQ-18, by directly comparing several available short-form versions of the original measure. The FFMQ-18 retains the five-factor structure of the original measure. The Rasch analysis was performed on data from 400 participants, half of whom were university students and half of whom were adult community members. Participants filled out the full English FFMQ and the following previously developed
short-forms were included in the analysis: A 24-item Dutch version (Bohlmeijer, ten Klooster, Fledderus, Veehof, & Baer, 2011), a 20-item German version (Tran, Glück, & Nader, 2013), a 20-item Chinese version (Hou et al., 2014), and a 15-item English version (Gu et al., 2016). Items with insufficient loadings were excluded, which left an 18-item Chinese version, an 18-item Dutch version, an 18-item German version, and a 15-item English version. Evidence for unidimensionality was strongest for the 18-item Dutch version which also displayed high Pearson correlations of its factors (ranging from .86 for the Observe factor to .95 for the total scale), suggesting a high degree of convergent validity.

The FFMQ-18 was administered at all timepoints throughout the study via the smartphone data collection app. The measure displayed adequate to good internal consistencies across all time points (α = .733 - .807).

**Attentional Control Scale (ACS)**

The ACS is a 20-item self-report measure (Derryberry & Reed, 2002). Responses are scored on a 1-4 Likert-type scale. The qualitative anchors to each point on the scale are 1, almost never; 2 sometimes; 3 often; and 4 always. Eleven of the items are reverse scored so that the higher the total score, the higher an individual’s level of attentional control. The ACS was developed based on the findings that anxious people’s attentional biases favor threatening information over nonthreatening information. There was a need for researchers to distinguish between nonanxious people, anxious people with highly developed, top-down coping abilities, and anxious people without well-developed coping methods. The ACS was formed by combining attentional focusing and shifting scales (Derryberry & Reed, 2002). Full details of exploratory and confirmatory factor analyses were not published. However, Derryberry and Reed
(2002) report that factor analyses indicate that the scale measures a single factor made up of three subfactors of attention focusing, attention shifting, and flexibility of control. The ACS was found to be internally consistent with an alpha of .88. However, subsequent published factor analyses have not supported the 3-factor structure and instead support a two-factor structure (Judah et al., 2014; Olafsson et al., 2011; Quigley, Wright, Dobson, & Sears, 2017).

At this time, very few studies have explored the validity of the ACS, and the evidence is mixed (Judah et al., 2014; Quigley et al., 2017). For this reason, this study used the ANT at Time 1 both in an effort to show validity for the ACS and, if the results do not support the ACS as a valid measure, to substitute as a measure of attentional control in some analyses. However, due to the nature of the ANT and other behavioral measures of attention and the practical limitations of this study, the ANT results in this study were not used in the primary longitudinal analyses. For example, it is not possible for participants to come into a lab and perform an ANT every day for seven days in a row both for practical reasons and because potential practice effects would confound data analyses. Therefore, a self-report measure such as the ACS is necessary to be able to test some of the hypotheses of this study. To the author’s knowledge, the ACS is the only currently peer-reviewed measure of self-reported attentional control. Due to the design of the study resulting from resource limitations, a self-report measure of this construct is required.

The ACS was presented at all timepoints throughout the study via the smartphone data-collection app. The measure displayed good internal consistencies across all time points (α = .839 - .860).
Metacognitions Questionnaires (MCQ-30)

The MCQ-30 is a 30-item measure of several metacognitive variables, many of which are conceptualized by Wells (2009) to be central to the metacognitive model of psychological disorder. The measure has good psychometric properties (Wells & Cartwright-Hatton, 2004) and measures five factors: positive beliefs and worry, negative beliefs about worry concerning uncontrollability and danger, low cognitive confidence, need to control thoughts, and cognitive self-consciousness. The MCQ-30 has the advantage of allowing the data obtained through this study to be applicable to a broad area of clinical literature, as it is a fundamental measure to many metacognitive studies. Internal consistency for the subscales ranges from .72-.89 (Wells, 2009). Participants rate their agreement with statements on a 4-item Likert scale (1 = Do not agree, 2 = Agree slightly, 3 = Agree moderately, 4 = Agree very much).

Construct validity of the measure has been assessed in several studies which have found the MCQ-30 to be positively correlated (coefficients ranging from .32 to .61) with trait-anxiety, pathological worry, depressive symptoms, and OCD symptoms (Cartwright-Hatton & Wells, 1997; Gwilliam, Wells, & Cartwright-Hatton, 2004; Myers & Wells, 2005; Wells & Papageorgiou, 1998). The different subscales of the measure have also been shown to be able to differentially predict a variety of outcomes. For example, the uncontrollability and danger subscale was able to discriminate between GAD and OCD from patients with panic disorder or social phobia (Cartwright-Hatton & Wells, 1997). The need for control subscale also tends to be higher in patients with GAD as opposed to panic or social phobia (Wells & Carter, 2001).

Construct validity is highly important to this measure since it was used to relate this study’s results to the broader metacognitive theories. This is important to providing context for the
results and elucidating important theoretical implications for the study as a whole. The aforementioned discriminant validity is also a positive quality for this study because the MCQ-30 should be able to discriminate between metacognitive-theory based interventions (e.g., ATT) and mindfulness-based interventions if there is indeed a difference.

The MCQ-30 was administered at Time 1 and Time 7 of the study via the smartphone data collection app. The measure displayed excellent internal consistency at Time 1 ($\alpha = .902$) and good internal consistency at Time 7 ($\alpha = .890$).

**Thought Control Questionnaire (TCQ)**

The TCQ (Wells & Davies, 1994) was developed to assess the use of strategies for the control of unpleasant or intrusive thoughts. It is a 30-item measure and has shown to have good psychometric properties (Reynolds & Wells, 1999). A factor analysis of the measure found that it had a reliable five-factor scale (Wells & Davies, 1994). The scale’s factors are Distraction, Social Control, Worry, Punishment, and Reappraisal. Cronbach alphas for the subscales are adequate and range from .64 to .79. Wells and Davies (1994) analyzed the individual factors’ test-retest reliability, which was also found to be adequate, coincidentally also ranging from .64 to .79.

The TCQ’s construct validity has been shown in studies that have found relationships between the worry and punishment subscales and a variety of different emotional disorders (Wells, 2009). Social control has been found to be significantly negatively associated with trauma symptoms (Holeva, Tarrier, & Wells, 2001). Additionally, individuals with OCD scored higher on the punishment, worry, reappraisal, and social control subscales than healthy controls (Amir, Cashman, & Foa, 1997). Similar to the MCQ-30, the TCQ will be used to apply this
study’s results to the broader metacognitive theories. As a result, construct and discriminant validity are vital to this measure.

This measure was administered at Time 1 and Time 7 via the smartphone data collection app. The TCQ displayed good internal consistency at Time 1 (α = .825) and good internal consistency at Time 7 (α = .829).

**Depression Anxiety Stress Scale-21 (DASS-21).**

The DASS-21 is a 21-item measure of negative emotionality. This construct is divided into three subscales – anxiety, depression, and stress – which are supported in factor analyses that report evidence for a three-factor model (Willemsen, Markey, Declercq, & Vanheule, 2011). Each subscale on the DASS-21 has seven items and reliability coefficients ranging from .74 to .81. Participants respond by choosing one of four ordinal response options (0 = did not apply to me at all to 3 = applied to me very much or most of the time). A higher score is indicative of greater symptoms. The DASS-21 has been found to be a valid measure of depression and anxiety in clinical settings (Ng et al., 2007). The reliability and validity for the DASS-21 in a nonclinical sample was later established (Sinclair et al., 2012). The measure has also been shown to have good convergent validity when compared to well-established measures of depression, anxiety, and stress (Henry & Crawford, 2005; Osman et al., 2012). The DASS-21 will serve several functions within the context of this study. Convergent validity is required because the DASS-21 will be used to track symptom changes over time, and these observed changes must be generalizable to the broader fields of depression, anxiety, and stress research. The DASS-21 will also serve as a measure that evaluates the ability of ATT to reduce psychological symptoms, which requires construct validity.
This measure was administered at Time 1 and Time 7 via the smartphone data collection app. At Time 1, the DASS-Anxiety scale displayed adequate internal consistency ($\alpha = .799$), the DASS-Depression scale displayed excellent internal consistency ($\alpha = .905$), and the DASS-Stress scale displayed good internal consistency ($\alpha = .800$). At Time 7, the DASS-Anxiety scale displayed adequate internal consistency ($\alpha = .778$), the DASS-Depression scale displayed good internal consistency ($\alpha = .859$), and the DASS-Stress scale displayed adequate internal consistency ($\alpha = .750$).

Procedure

Based on the results of the initial screener, individuals who appeared to have met study inclusion criteria were invited to participate in the study via email.

Time 1

The first study session was in-person and run by the study PI. Each in-person study session was administered to study participants individually. Participants received a copy of the informed consent document to read through. After the participant affirmed that they have read the document and indicated verbally and via a signature that they consented to continue with the study, a copy of the consent form was provided to them for their records and later reference. In order to avoid coercion, if informed consent was not obtained, the participant received course credit as if they had finished all Time 1 activities and were not penalized.

Once informed consent was obtained, a brief paper questionnaire screened for the inclusion and exclusion criteria as well as record demographic variables such as age, race, sex, and years of education. Once inclusion and exclusion criteria were met, participants were
randomized using a block randomization scheme that ensured that neither condition had an unbalanced level of endorsed psychological symptoms. Groups were not blocked based on sex or age, and the two groups did not significantly differ in either respect ($\chi^2_{sex} = .064, p = .800; \chi^2_{age} = 8.70, p = .464$). Random assignment after study criteria are met prevents potentially imbalanced groups due to study ineligibility.

After random assignment, all participants began the remainder of their study activities by downloading the custom smartphone application where all data but the ANT are recorded (the Study App). The Study App utilized the ExpiWell platform on both Android and iOS platforms (ExpiWell, 2020). During the download period, participants were administered the ANT on a computer, as is standard for the test. Once the Study App was downloaded, participants were shown how to use the app, which then prompted them to fill out questionnaires including the ACS, MCQ, FFMQ-SF, DASS-21 and TCQ. Participants in the control condition were then prompted by the Study App to listen to a 12-minute audio file that contains instructions on PMR, similar to other control conditions in the literature (Wells, 2009). The research assistant or PI was present during this first recording to answer any questions the participant may have.

The control participants were then asked to set a time where they would have 20 minutes every day for the next seven days, where they were able to repeat the PMR intervention and answer a few questions through the Study App. The Study App automatically prompted participants when it was time to begin the study procedures for each day based on their previously entered preferred time. In the ATT condition, the ATT audio recording was played instead of the PMR audio recording and the participant was instructed to attend to particular auditory stimuli within the recording. Each session of ATT took approximately 12 minutes. Subsequent sessions of ATT/PMR beyond the first in person session were performed at home,
and several studies have demonstrated the viability and feasibility of at home practice (Callinan et al., 2015; Fergus et al., 2014; Siegle et al., 2007; Siegle et al., 2014). The instructor in the ATT audio file directed participants to spend five minutes on a selective attention task wherein they attend to specific sounds in the recording and exclude other sounds (Fergus & Bardeen, 2016). In the subsequent five minutes, participants were instructed to perform an attention switching task in which they rapidly switched their attention from one sound to another. The final two minutes were spent on a divided attention task in which participants attempted to pay attention to multiple sounds at once. As with the control participants, ATT participants were able to set times when they would receive reminders to complete study procedures at home over the next week. Study App notifications were customized to each participant’s schedule to maximize the likelihood that participants were in a quiet, relatively distraction-free environment when completing the study tasks and measures.

**Time 2 Through Time 6**

The Study App automatically prompted participants to listen to the PMR or ATT audio files depending on assigned condition once per day for the next five days. Following the audio file, the Study App prompted participants to respond to the FFMQ-SF and ACS questionnaires immediately. During Time 3, a manipulation check was inserted between the ACS and FFMQ-SF asking participants to select response option 4. All but one (98.2%, n = 54) of participants who completed Time 3 responded appropriately to this question. The participant who responded incorrectly to this item was excluded from data analyses for this and other reasons described in the analysis section. If study participants indicated that they were not able to perform the study procedures at their designated time, the Study App reminded them through a system notification
every half hour until participants completed that day’s procedure. Participants were reminded no more than six times per missed session, after which that day’s data were considered missing.

**Time 7**

Following completion of the final session of ATT/PMR, participants were then prompted to complete all study measures filled out during Time 1, including the ACS, FFMQ-SF, TCQ, DASS-21, and MCQ. After completing the last survey, participants were given a message thanking them for their participation. They were told that their participation was recorded and their course credit would not be affected by their response to the following question, “Did you actually pay attention to the audio file’s instructions each day?” All participants who completed Time 7 responded affirmatively ($n = 42$).

Data were automatically collected and stored in a server, so no additional contact with participants was necessary unless participants indicated they wished to participate in an option Time 8 in-person study session in which they completed the ANT one additional time. Debriefing materials were made available via the Study App. After Time 7 of the study, a link to a website that contained the debriefing materials was shared with participants.
CHAPTER 3
DATA ANALYSIS

Tests of HLM Assumptions, Normality, and Missing Data

Prior to analyzing the hypotheses using the study data, multilevel modeling assumptions were examined. Multiple assumptions were not met, so robust standard errors were used in all study models used for the analysis. Additionally, a t-test determined that mindfulness did not differ significantly by randomly assigned group (PMR and ATT). Little’s Missing Completely At Random (MCAR) Test (Little, 1988) determined if data is missing at random. Missing data exceeded 10% (per Little, 1988), so multiple imputation by ordinary least squares was used to fill in the gaps. The procedure outlined in Cohen, Cohen, West, and Aiken (2007) was followed for missing data imputation. If time of day is significantly related to any measure, it will be introduced as a Level 1 control variable in the relevant HLM analyses. It was significantly related to MCQ scores ($r = .270, p = .037$) but no other variable. Therefore, it was only included in the analysis of Hypothesis 5. Finally, the level of clustering within the dataset was assessed by calculating the interclass correlation (ICC) and Design Effect (DEFF) by comparing a null model to each model produced in HLM analysis.

All analyses were performed using HLM unless otherwise specified.
Testing the Hypotheses

**Hypothesis 0**

Ordinary least-squares regression models were used for these analyses. ACS at Time 1 will be positively associated with ANT executive control scores at Time 1 (0.1). In other words, $\beta_1$ will be positive and significant. Initial ratings of Time 1 distress (depression, anxiety, stress) will predict final levels of distress (0.2). In other words, $\beta_1$ will be positive and significant. Initial ratings of mindfulness will predict final levels of mindfulness (0.3). In other words, $\beta_1$ will be positive and significant.

- **Hypothesis 0.1:**
  \[ AC_i = \beta_0 + \beta_1 \text{ANT} + e_i \]

- **Hypothesis 0.2:**
  \[ T1_{DASS_i} = \beta_0 + \beta_1 T7_{DASS} + e_i \]

- **Hypothesis 0.3:**
  \[ T1_{Mindfulness_i} = \beta_0 + \beta_1 T7_{Mindfulness} + e_i \]

**Hypothesis 1**

ATT will reduce the presence and severity of depression (1.1), anxiety (1.2), and stress (1.3) symptoms in the sample relative to controls. In other words, $\beta_1$ will be significantly different from zero and positive. The 1-week change score will be calculated for the three subscales of the DASS-21 and each will be entered as Level 1 outcome variables.

- **Hypothesis 1.1**
  \[ DEP_{ij} = \beta_{0j} + \beta_{1j} ATT_{ij} + r_{ij} \]

- **Level 2:**
  \[ \beta_{0j} = \gamma_{00} + \mu_{0j} \]
  \[ \beta_{1j} = \gamma_{10} \]
Hypothesis 1.2

Level 1: \(\text{ANX}_{ij} = \beta_0 + \beta_1 \text{ATT}_{ij} + r_{ij}\)

Level 2: \(\beta_0 = \gamma_0 + \mu_0\)
\(\beta_1 = \gamma_{10}\)

Hypothesis 1.3

Level 1: \(\text{STR}_{ij} = \beta_0 + \beta_1 \text{ATT}_{ij} + r_{ij}\)

Level 2: \(\beta_0 = \gamma_0 + \mu_0\)
\(\beta_1 = \gamma_{10}\)

Hypothesis 2

Attentional control as measured by the Attentional Control Scale will significantly increase over time in the ATT group (2.1). In other words, \(\beta_{10}\) will be significant and positive. Attentional control will remain stable in the control group (2.2). In other words, \(\beta_{10}\) will not be significantly different from zero. Increases in attentional control will be positively and significantly associated with the ATT experimental group. In other words, \(\beta_{01}\) will be positive and significantly differ from zero. Attentional control will be entered as an outcome variable and group assignment will be entered as a Level 2 predictor.

Hypothesis 2.1

Level 1: \(\text{AC} = \pi_{0i} + \pi_{1i}(time) + e_{ti}\)

Level 2: \(\pi_{0i} = \beta_{00} + \tau_{0i}\)
\(\pi_{1i} = \beta_{10}\)
Hypothesis 2.2

Level 1: \[ AC = \pi_{0l} + \pi_{1l}(time) + e_{tl} \]

Level 2: \[ \pi_{0l} = \beta_{00} + r_{0l} \]
\[ \pi_{1l} = \beta_{10} \]

Hypothesis 2.3

Level 1: \[ AC = \pi_{0l} + \pi_{1l}(time) + e_{tl} \]

Level 2: \[ \pi_{0l} = \beta_{00} + \beta_{01}(ATT) + r_{0l} \]
\[ \pi_{1l} = \beta_{10} + \beta_{11}(ATT) \]

Hypothesis 3

Mindfulness as measured by the Five-Facet Mindfulness Questionnaire - Short Form will significantly increase over time in the ATT group (3.1). In other words, \( \beta_{10} \) will be significant and positive. Mindfulness will remain stable in the control group (3.2). In other words, \( \beta_{10} \) will not be significantly different from zero. Increases in mindfulness will be positively and significantly associated with the ATT experimental group. In other words, \( \beta_{01} \) will be positive and significantly differ from zero. Mindfulness will be entered as an outcome variable and group assignment will be entered as a Level 2 predictor.

Hypothesis 3.1

Level 1: \[ Mindfulness = \pi_{0l} + \pi_{1l}(time) + e_{tl} \]

Level 2: \[ \pi_{0l} = \beta_{00} + r_{0l} \]
\[ \pi_{1l} = \beta_{10} \]
Hypothesis 3.2

Level 1: \( Mindfulness = \pi_{0i} + \pi_{1i}(time) + e_{ti} \)

Level 2:
\( \pi_{0i} = \beta_{00} + r_{0i} \)
\( \pi_{1i} = \beta_{10} \)

Hypothesis 3.3

Level 1: \( Mindfulness = \pi_{0i} + \pi_{1i}(time) + e_{ti} \)

Level 2:
\( \pi_{0i} = \beta_{00} + \beta_{01}(ATT) + r_{0i} \)
\( \pi_{1i} = \beta_{10} + \beta_{01}(ATT) \)

Hypothesis 4

Attentional control and mindfulness scores will vary together over time. In other words, \( \beta_{10} \) will be positive and significant. Attentional control will be entered as a Level 1 predictor of mindfulness, controlling for time.

Hypothesis 4:

Level 1: \( Mindfulness = \pi_{0i} + \pi_{1i}(AC) + \pi_{2i}(time) + e_{ti} \)

Level 2:
\( \pi_{0i} = \beta_{00} + r_{0i} \)
\( \pi_{1i} = \beta_{10} + r_{1i} \)
\( \pi_{2i} = \beta_{20} + r_{2i} \)

Hypothesis 5

ATT will reduce self-reported problems of attention and concentration in individuals as measured by the Metacognitions Questionnaire (MCQ; 5.1) and Thought Control Questionnaire
(TCQ; 5.2). In other words, $\beta_1$ will be significantly different from zero and negative. Group assignment will be entered as Level 1 predictors, while MCQ and TCQ scores will be entered as Level 1 outcome variables.

**Hypothesis 5.1:**

Level 1: \[ MCQ_{ij} = \beta_0j + \beta_1jATT_{ij} + r_{ij} \]

Level 2: \[ \beta_0j = \gamma_{00} + \mu_{0j} \]
\[ \beta_1j = \gamma_{10} \]

**Hypothesis 5.2:**

Level 1: \[ TCQ_{ij} = \beta_0j + \beta_1jATT_{ij} + r_{ij} \]

Level 2: \[ \beta_0j = \gamma_{00} + \mu_{0j} \]
\[ \beta_1j = \gamma_{10} \]

**Hypothesis 6**

Higher scores on the ANT executive control measure will attenuate the effect of ATT on increases in ACS scores. In other words, $\beta_{21}$ will be significant and negative.

**Hypothesis 6:**

Level 1: \[ AC = \pi_{0i} + \pi_{1i}(timeMin1) + \pi_{2i}(timeMin1)^2 + e_{ti} \]

Level 2: \[ \pi_{0i} = \beta_{00} + \beta_{01}(ANT) + r_{0i} \]
\[ \pi_{1i} = \beta_{10} + \beta_{11}(ANT) + r_{1i} \]
\[ \pi_{2i} = \beta_{20} + \beta_{21}(ANT) + r_{2i} \]
Hypothesis 7

If FFMQ scores increase as expected, the Observing factor of the FFMQ will increase more than the other factors. In other words, the $R^2$ for the FFMQ Observing model will be greater than the models that include the other factors.

Hypothesis 7: Repeat the following for all five FFMQ factors:

Level 1: \[ Mindfulness = \pi_{0i} + \pi_{1i}(time) + e_{ti} \]

Level 2: \[ \pi_{0i} = \beta_{00} + \beta_{01}(Observe)_{0i} \]
\[ \pi_{1i} = \beta_{10} \]

\[ R^2 = \frac{\hat{\sigma}^2(Null \ model) - \hat{\sigma}^2(Linear \ growth \ model)}{\hat{\sigma}^2(Null \ model)} \]
CHAPTER 4

RESULTS

Data were compiled from the Study App database and were placed into two separate data files. One data file was a “long” data file meant for growth models, while the other was a “short” data file meant for linear regression, correlation, and ANOVA analyses. Measures were computed first in Excel and later in SPSS in order to ensure scoring accuracy. Variables were not screened for normality, as this is not an assumption of HLM, but were screened for extreme outliers. The means and standard deviations of the combined sample and of each study group (see Table 2) of the study variables were produced and examined for errors and unexpected results. Internal consistencies of the study measures were then calculated, followed by correlation tables. After missing data analyses were performed, ten imputations using ordinary least squares regression of the SPSS “short” and “long” data files were then produced using the missing data SPSS extension. These were then converted to multivariate data matrix (MDM) files for input into HLM 7.03 software in order to run growth models.

The mean age of the combined sample was 20.01 (SD = 3.65), the mean level of education was 12.94 years (SD = 1.22), the mean level of mother’s education was 14.36 years (SD = 2.37), while the mean level of father’s education was 14.66 years (SD = 3.10). In general, psychological symptoms were present in the sample, but fell below any recommended cutoff score in the literature for the DASS ($M_{Anx} = 4.42$, SD = 4.54; $M_{Dep} = 4.84$, SD = 4.84; $M_{Stress} = 7.29$, SD = 4.54). See Tables 3, 4, and 5 for detailed descriptive statistics and correlations of
psychological constructs. This was consistent with the inclusion criteria of the study. Mean ACS scores at Time 1 were 51.19 (SD = 7.26; similar to other studies of comparable samples, e.g., Buckman, Saunders, Fearon, Leibowitz, & Pilling, 2018; Klumpp et al, 2018) and Mean FFMQ-SF scores were 58.72 (SD = 9.34; similar to other studies of comparable samples using the full FFMQ, e.g., Aguado et al., 2016; Lemay, Hoolahan, & Buchanan, 2019). As for the metacognitive measures, mean MCQ scores at Time 1 were 26.84 (SD = 14.62; somewhat lower than studies of comparable samples, e.g., Van Camp, Sabbe, & Oldenburg, 2019) and mean TCQ scores at Time 1 were 34.45 (SD = 10.55; no nonclinical sample means of the TCQ in young adults are reported in the literature). See Tables 6 and 7 for detailed descriptive statistics and correlations of cognitive constructs.

Participants in the study appeared to be compliant with the study requirements. The percentage of ATT interventions completed was 84.82%, while the percentage of PMR interventions completed was 82.6%. The amount of time spent on each study session was within expectations and appropriate (M minutes = 19.45, SD = 1.49). Two participants were less than 70% accurate in the ANT, a strong indication of random responding. Furthermore, both of these participants completed the Time 1 questionnaires in an extremely small amount of time. One of these participants failed an accuracy check at Time 3, while the other participant completed no additional study sessions beyond Time 1. For these reasons, these participants were excluded from analysis. Total accuracy in the ANT across the sample with these two participants removed was 98.2%, indicating effortful responding.

Data were marginally MCAR (p = .07), but the percentage of missing data was 26.6%, so multiple imputation (MI) by ordinary least squares regression was employed, stratified across 10 MI datasets, the maximum number supported in HLM 7.03 student. When using HLM 7.03
Table 3
Correlations Between Attention, Mindfulness, and Psychological Symptoms Constructs, Time 1 and Times 7/8 – Combined Groups

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| N                | 64  | 64  | 64  | 64  | 64  | 64  | 64  | 64  | 64  | 64   | 64   | 64   | 64   | 64   | 64   |
| Minimum          | -5  | -18 | 45  | 507 | 37  | 34  | 0   | 0   | 0   | 0    | 0    | 0    | 0    | 0    | 0    |
| Maximum          | 100 | 86  | 407 | 793 | 72  | 78  | 21  | 19  | 19  | 19   | 19   | 19   | 19   | 19   | 19   |
| Mean             | 37.76 | 38.77 | 133.66 | 627.77 | 51.19 | 58.72 | 4.42 | 4.84 | 7.29 | 34.42 | 30.00 | 103.04 | 574.19 | 53.67 | 62.00 |
| SD               | 24.46 | 21.84 | 53.87 | 71.64 | 7.26 | 9.34 | 4.54 | 4.84 | 4.54 | 19.65 | 20.83 | 24.94 | 62.90 | 6.99 | 10.90 |

(Continued on following page)
Table 3 (continued)

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<td>10. T8 ANT Alerting</td>
<td>11. T8 ANT Orienting</td>
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Note: *p < .05, **p < .01. ANT = Attention Network Task, RT = Reaction Time, ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire Short Form, DASS = Depression, Anxiety, and Stress Scale, SD = Standard Deviation. ANT scores were multiplied by -1 to aid with interpretation such that a positive correlation between a construct and an ANT measure indicates in improvement in attention network functioning.
Table 4
Correlations Between Attention, Mindfulness, and Psychological Symptoms Constructs, Time 1 and Times 7/8 – ATT Group

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| Minimum| -2   | -18  | 64   | 507  | 37   | 34   | 0    | 0    | 0    | 0    | -10  | -18  | 56   | 460  | 43   |
| Maximum| 100  | 86   | 240  | 772  | 72   | 76   | 19   | 19   | 17   | 79   | 77   | 166  | 722  | 74   | 88   |
| Mean   | 38.38| 39.94| 134.81| 632.38| 52.03| 59.22| 4.81| 5.03| 7.41| 31.13| 30.47| 104.47| 584.60| 55.17| 63.78|
| SD     | 25.26| 25.04| 43.51| 79.18| 7.85 | 9.64 | 4.57| 5.31| 4.20| 23.98| 24.24| 30.13| 73.96| 7.99 | 11.50|

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Maximum 19 10 12
Mean 2.96 2.35 5.09
SD 3.90 2.48 2.84

Note: *p < .05, **p < .01. ATT = Attention Training Technique, ANT = Attention Network Task, RT = Reaction Time, ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire Short Form, DASS = Depression, Anxiety, and Stress Scale. ANT scores were multiplied by -1 to aid with interpretation such that a positive correlation between a construct and an ANT measure indicates in improvement in attention network functioning.
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| N                | 32   | 32   | 32   | 32   | 32   | 32   | 32   | 31   | 31   | 31   | 11   | 11   | 11   | 11   | 19   |
| Minimum          | -5   | 10   | 45   | 523  | 37   | 39   | 37   | 32   | 32   | 31   | 19   | 8    | 68   | 503  | 43   |
| Maximum          | 84   | 82   | 407  | 793  | 68   | 78   | 21   | 18   | 19   | 54   | 52   | 132  | 660  | 62   | 77   |
| Mean             | 36.97| 37.59| 132.50| 623.16| 50.34| 58.22| 4.03 | 4.65 | 7.16 | 38.91| 29.36| 101.09| 560.00| 51.84| 59.84|
| SD               | 24.02| 18.42| 63.27| 64.18| 6.64 | 9.16 | 4.54 | 4.38 | 4.94 | 11.02| 16.16| 16.63| 42.99| 5.17 | 10.00|

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Table 5 (continued)

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Maximum 8 18 19
Mean 2.83 3.28 4.89
SD 2.50 4.30 4.43

Note: *p < .05, **p < .01. PMR = Progressive Muscle Relaxation, ANT = Attention Network Task, RT = Reaction Time, ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire Short Form, DASS = Depression, Anxiety, and Stress Scale. ANT scores were multiplied by -1 to aid with interpretation such that a positive correlation between a construct and an ANT measure indicates improvement in attention network functioning.
Table 6

Correlations Between Self-Report Attention and Cognitive Constructs, Time 1

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<td>0.456**</td>
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<tr>
<td>13. T1 TCQ Reappraise</td>
<td>0.031</td>
<td>0.109</td>
<td>-0.091</td>
<td>-0.251*</td>
<td>-0.504**</td>
<td>0.074</td>
<td>0.323**</td>
<td>0.315*</td>
<td>0.416**</td>
<td>0.580*</td>
<td>0.145</td>
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<td>14. T1 TCQ Total</td>
<td>0.160</td>
<td>-0.090</td>
<td>-0.041</td>
<td>0.480**</td>
<td>0.585**</td>
<td>0.229</td>
<td>0.659**</td>
<td>0.558**</td>
<td>0.607**</td>
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<td>0.548**</td>
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N 64   64   64   64   64   64   64   64   64   64   64   64   64   64   64   64
Minimum 37   34   0    0    0    1    0    0    1    4    4    4    0    0    0    12
Maximum 72   78   16   18   17   18   17   74   18   16   16   16   13   18   65
Mean 51.19 58.72 4.37 4.34 8.11 4.97 5.05 26.84 10.09 10.22 3.42 2.86 7.86 34.45
SD 7.26 9.34 4.03 4.35 4.66 4.68 3.69 14.62 3.43 2.80 3.15 2.73 4.22 10.55

Note: *p < .05, **p < .01. ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire Short Form, MCQ = Metacognitions Questionnaire, TCQ = Thought Control Questionnaire, SD = Standard Deviation.
Table 7

Correlations Between Self-Report Attention and Cognitive Constructs, Time 7

<table>
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<tr>
<th></th>
<th>1.</th>
<th>2.</th>
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<td>1. T7 ACS</td>
<td>-</td>
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<td>2. T7 FFMQ-SF</td>
<td>.360*</td>
<td>-</td>
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<td>3. T7 MCQ Conf</td>
<td>-.351*</td>
<td>-.356*</td>
<td>-</td>
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<tr>
<td>4. T7 MCQ Worry</td>
<td>.235</td>
<td>.013</td>
<td>.447**</td>
<td>-</td>
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<tr>
<td>5. T7 MCQ Self</td>
<td>.099</td>
<td>-.229</td>
<td>.179</td>
<td>.353*</td>
<td>-</td>
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<tr>
<td>6. T7 MCQ Danger</td>
<td>.162</td>
<td>-.333*</td>
<td>.737**</td>
<td>.442**</td>
<td>.200</td>
<td>-</td>
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<tr>
<td>7. T7 MCQ Control</td>
<td>.284</td>
<td>-.159</td>
<td>.498**</td>
<td>.599**</td>
<td>.463**</td>
<td>.681**</td>
<td>-</td>
<td></td>
<td></td>
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<tr>
<td>8. T7 MCQ Total</td>
<td>.192</td>
<td>-.331*</td>
<td>.815**</td>
<td>.646**</td>
<td>.451**</td>
<td>.928**</td>
<td>.812**</td>
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<td>9. T7 TCQ Distract</td>
<td>-.072</td>
<td>.077</td>
<td>-.124</td>
<td>.273</td>
<td>.437**</td>
<td>-.109</td>
<td>-.034</td>
<td>.022</td>
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<tr>
<td>10. T7 TCQ Social</td>
<td>.228</td>
<td>.018</td>
<td>-.395*</td>
<td>.030</td>
<td>.595**</td>
<td>-.008</td>
<td>.318*</td>
<td>.059</td>
<td>.283</td>
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<td>11. T7 TCQ Worry</td>
<td>.440**</td>
<td>-.002</td>
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<td>12. T7 TCQ Punish</td>
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<td>.362*</td>
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<td>.423**</td>
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<td>.408**</td>
<td>.439**</td>
<td>.244</td>
<td>-</td>
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<td>14. T7 TCQ Total</td>
<td>.373*</td>
<td>.036</td>
<td>.528**</td>
<td>.528**</td>
<td>.404**</td>
<td>.409**</td>
<td>.616**</td>
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<td>.498**</td>
<td>.587**</td>
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<td>Maximum</td>
<td>74</td>
<td>88</td>
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<td>31</td>
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<td>11</td>
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<tr>
<td>Mean</td>
<td>53.67</td>
<td>62.00</td>
<td>6.68</td>
<td>4.10</td>
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<td>4.78</td>
<td>3.98</td>
<td>7.63</td>
<td>32.24</td>
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</table>

Note: *p < .05, **p < .01. ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire Short Form, MCQ = Metacognitions Questionnaire, TCQ = Thought Control Questionnaire, SD = Standard Deviation.
student, results derived from the MI data sets were automatically combined to produce a single set of regression weights, test statistics, and $p$-values. The intraclass correlation coefficient (ICC) was determined for each of the relevant null models for the analyses performed in this study. The ICC is an index of the degree to which cases within clusters are similar to each other and distinct from cases in other clusters. In this study, high ICC indicates that each timepoint is distinct from other timepoints in the study (Castro, 2002). The ICC for the null model with the ACS as an outcome was .87, indicating that taking the clusters into account is important for the data. The ICC for the null model with the FFMQ as an outcome was .88, which also indicates that taking the clusters into account is important for the data. In order to quantitatively determine the importance of clustering to the data, the design effect (DEFF) was computed. The DEFF indicates how much the squared standard error increases due to clustering (Osborne, 2000). The DEFF for models with the ACS as an outcome was 45.21, while the DEFF for models with the FFMQ as an outcome was 45.71, indicating that it was important for analyses using this data to account for clustering (which HLM does account for).

For the main HLM models, all residuals were found to be not normally distributed using the Shapiro-Wilk test ($p < .001$ for all analyses), which violates an assumption of HLM analysis. However, visual inspection of the residuals indicated that they were not far off from normally distributed. It may be the case that, with a large sample size (each datapoint in the longitudinal dataset adds one degree of freedom to the parametric test), the Shapiro-Wilk test is overly sensitive to minor deviations in normality (Noughabi & Arghami, 2011). Regardless, due to the violation of this HLM assumption, robust standard errors were used in all HLM analyses. Furthermore, visual inspections of plots of Level 1 residuals over time indicated that the Level 1 residuals were homogenous across clusters (in this case, across time). However, a parametric test
of this estimation revealed that Level 1 residuals were heterogeneous \( (\chi^2 = 189.64, p < .001) \),
again indicating the need for robust standard errors.

An additional assumption of HLM was examined and found be satisfactory. In analyses
for which there were Level 2 predictors (Hypotheses 2.3, 3.3, and 6), Level 2 predictors were
found to be independent of Level 2 errors \( (r < .001 \text{ for all cases}) \).

In summary, numerous HLM assumptions were not met, so robust standard errors were
used in all analyses. Data were MCAR, but missing at a high enough proportion that multiple
imputation by ordinary least squares was implemented.

Hypothesis 0

It was predicted that ACS at Time 1 would be positively associated with ANT executive
control scores at Time 1 (0.1). It was also predicted that initial ratings of Time 1 distress
(depression, anxiety, stress) would predict final levels of distress (0.2). Finally, it was predicted
that initial ratings of mindfulness would predict final levels of mindfulness (0.3). Hypotheses 0.1
through 0.3 were tested using ordinary least-squares regression models. The ACS score at Time
1 across both groups did not significantly predict the executive attention score on the ANT at
Time 1, \( \beta = -.055, t(63) = - .431, p = .668 \) (Hypothesis 0.1). \textbf{The hypothesis was not supported.}

Exploratory analyses were performed, entering each measure of the ANT as an outcome
variable predicted by ACS score. Time 1 ACS significantly predicted Time 1 Alerting, \( \beta = .339, t(63) = 2.840, p = .006 \). Time 1 ACS did not significantly predict Time 1 Orienting, \( \beta = .161, t(63) = 1.288, p = .203 \), or overall Time 1 ANT reaction time (RT), \( \beta = -.081, t(63) = - .638, p = .526 \). Exploratory analyses were also performed on Time 8 ANT and ACS scores. As in
Time 1, Time 8 ACS significantly predicted Time 8 Alerting, $\beta = .254$, $t(63) = 2.031$, $p = .043$, while Time 8 ACS did not significantly predict Time 8 Orienting, $\beta = .194$, $t(63) = 1.437$, $p = .153$, Time 8 Executive Attention, $\beta = -.035$, $t(63) = -.216$, $p = .829$, and Time 8 overall RT, $\beta = -.131$, $t(63) = -1.017$, $p = .309$. Further exploratory analyses were performed at Time 1 by examining the two subfactors of the ACS found by Olafsson et al. (2011) in a factor analysis. The Focusing factor and the Switching factor appeared to perform similarly to each other and to the broader ACS total score across several linear regressions. Time 1 ACS Focusing, $\beta = .350$, $t(63) = 2.943$, $p = .004$, and Time 1 ACS Switching, $\beta = .321$, $t(63) = 2.622$, $p = .009$, both significantly predicted Time 1 ANT Alerting. Time 1 ACS Focusing, $\beta = .121$, $t(63) = 1.008$, $p = .313$, and Time 1 ACS Switching, $\beta = .192$, $t(63) = 1.429$, $p = .174$, did not predict Time 1 ANT Orienting. Finally, Time 1 ACS Focusing, $\beta = -.066$, $t(63) = -.401$, $p = .646$, and Time 1 ACS Switching, $\beta = -.044$, $t(63) = -.333$, $p = .700$, did not significantly predict Time 1 ANT Executive Attention. Time 7 DASS Anxiety, $\beta = .571$, $t(63) = 5.477$, $p < .001$, DASS Depression, $\beta = .385$, $t(63) = 3.286$, $p = .001$, and DASS Stress, $\beta = .325$, $t(63) = 2.706$, $p = .007$, were significantly predicted by their Time 1 counterparts (Hypothesis 0.2). **This hypothesis was supported.**

Time 7 Mindfulness was significantly predicted by Time 1 Mindfulness (Hypothesis 0.3), $\beta = .571$, $t(63) = 5.275$, $p < .001$. **This hypothesis was supported.**

Hypothesis 1

It was predicted that ATT would reduce the presence and severity of depression (1.1), anxiety (1.2), and stress (1.3) symptoms in the sample relative to controls. The Group variable
was coded as 0 = ATT and 1 = PMR and was treated as if it were a continuous, linear variable. ATT and PMR groups did not significantly differ in reductions in depression from Time 1 to Time 7, $\beta_{10} = .131$, $t(63) = .975$, $p = .331$. ATT and PMR groups did not significantly differ in reductions in anxiety from Time 1 to Time 7, $\beta_{10} = .084$, $t(63) = .639$, $p = .523$. ATT and PMR groups did not significantly differ in reductions in anxiety from Time 1 to Time 7, $\beta_{10} = .025$, $t(63) = .193$, $p = .847$. These hypotheses were not supported. An exploratory analysis showed that, collapsing across groups, DASS total scores significantly decreased from Time 1 to Time 7, $\beta = -.723$, $t(63) = -3.394$, $p = .011$. Table 8 shows group mean comparisons over time.

Hypothesis 2

It was predicted that attentional control as measured by the ACS would significantly increase over time in the ATT group (2.1). The effect of time on ACS scores within the ATT group was not significant, $\beta_{10} = 0.290$, $t(157) = 1.841$, $p = 0.067$. This hypothesis was not supported.

It was also predicted that attentional control would remain stable in the control group (2.2). The effect of time on ACS scores within the PMR group was not significant. $\beta_{10} = 0.115$, $t(152) = 1.304$, $p = 0.194$. This hypothesis was supported.

Finally, it was predicted that increases in attentional control would be positively and significantly associated with the ATT experimental group (2.3). The impact of group on the effect of time on ACS scores was not significant, $\beta_{11} = -0.174$, $t(309) = -0.903$, $p = 0.370$. This hypothesis was not supported. See Table 8 for group mean comparisons over time.
Table 8
Mean Scores Over Time Between Groups and Combined

<table>
<thead>
<tr>
<th></th>
<th>T1 ATT Group Mean (SD)</th>
<th>T1 PMR Group Mean (SD)</th>
<th>T1 Combined Group Mean (SD)</th>
<th>T7/T8 ATT Group Mean (SD)</th>
<th>T7/T8 PMR Group Mean (SD)</th>
<th>T7/T8 Combined Group Mean (SD)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ANT - Alerting</td>
<td>38.4 (25.27)</td>
<td>37.0 (24.02)</td>
<td>37.8 (24.46)</td>
<td>31.1 (23.98)</td>
<td>38.9 (11.02)</td>
<td>34.4 (19.65)</td>
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<tr>
<td>ANT - Orienting</td>
<td>39.9 (25.04)</td>
<td>37.6 (18.42)</td>
<td>38.8 (21.84)</td>
<td>30.5 (24.24)</td>
<td>29.4 (16.16)</td>
<td>30.0 (20.83)</td>
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<td>ANT - Executive</td>
<td>134.8 (43.51)</td>
<td>132.5 (63.27)</td>
<td>133.7 (53.9)</td>
<td>104.5 (30.13)</td>
<td>101.1 (16.63)</td>
<td>103.0 (24.94)</td>
</tr>
<tr>
<td>ANT - Overall RT</td>
<td>632.4 (79.18)</td>
<td>623.2 (64.18)</td>
<td>627.8 (71.64)</td>
<td>584.6 (73.96)</td>
<td>560.0 (42.99)</td>
<td>574.2 (62.90)</td>
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<td>ACS</td>
<td>52.0 (7.85)</td>
<td>50.3 (6.64)</td>
<td>51.2 (7.26)</td>
<td>55.2 (7.99)</td>
<td>51.8 (5.17)</td>
<td>53.7 (6.99)</td>
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<td>FFMQ-SF</td>
<td>59.2 (9.64)</td>
<td>58.22 (9.16)</td>
<td>58.7 (9.34)</td>
<td>63.8 (11.50)</td>
<td>59.8 (10.00)</td>
<td>62.0 (10.90)</td>
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<td>DASS-Stress</td>
<td>7.4 (4.20)</td>
<td>7.2 (4.94)</td>
<td>7.3 (4.54)</td>
<td>5.1 (2.84)</td>
<td>4.9 (4.43)</td>
<td>5.0 (3.58)</td>
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<td>DASS-Anxiety</td>
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<td>4.0 (4.54)</td>
<td>4.4 (4.54)</td>
<td>3.0 (3.90)</td>
<td>2.8 (2.50)</td>
<td>2.9 (3.32)</td>
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<td>DASS-Depression</td>
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<td>MCQ-30</td>
<td>25.8 (13.30)</td>
<td>27.9 (15.98)</td>
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<td>TCQ-30</td>
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<td>33.9 (10.63)</td>
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<td>31.1 (9.66)</td>
<td>33.3 (10.12)</td>
<td>32.2 (9.98)</td>
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</table>

Note. ATT = Attention Training Technique, PMR = Progressive Muscle Relaxation, SD = Standard Deviation, ANT = Attention Network Task, RT = Reaction Time, ACS = Attentional Control Scale, FFMQ-SF = Five Facet Mindfulness Questionnaire – Short Form, DASS = Depression, Anxiety, and Stress Scale, MCQ = Metacognitions Questionnaire, TCQ = Thought Control Questionnaire.
Hypothesis 3

It was predicted that mindfulness would significantly increase over time in the ATT group (3.1). Mindfulness in the ATT group did not significantly increase over time, $\beta_{10} = 0.169, t(157) = 1.354, p = 0.281$. This hypothesis was not supported.

It was predicted that mindfulness would remain stable in the control group (3.2). Mindfulness in the PMR group did not significantly increase over time, $\beta_{10} = 0.169, t(152) = 0.627, p = 0.350$. This hypothesis was supported.

Finally, it was predicted that increases in mindfulness will be positively and significantly associated with the ATT experimental group. The impact of group on the effect of time on FFMQ scores was not significant, $\beta_{11} = -0.372, t(309) = -1.309, p = 0.195$. This hypothesis was not supported. See Table 8 for group mean comparisons over time.

Hypothesis 4

It was predicted that attentional control and mindfulness scores would vary together over time. ACS scores over time did not significantly vary together with FFMQ scores, $\beta_{20} = .130, t(309) = 1.261, p = 0.212$. This hypothesis was not supported. See Table 8 for group differences over time.

Hypothesis 5

It was predicted that ATT would reduce self-reported problems of attention and concentration in individuals as measured by the Metacognitions Questionnaire (MCQ; 5.1) and Thought Control Questionnaire (TCQ; 5.2). Change in MCQ scores over time was significantly
associated with experimental group, such that individuals in the ATT group displayed greater
decreases in problematic metacognitions than individuals in the PMR group, $\beta = -2.023$, $t(63) = -2.755$, $p = .009$. This relationship held when entering time of day as a Level 1 control variable as per the data analysis plan, $\beta = -2.108$, $t(62) = -2.987$, $p = .005$. This hypothesis was supported.

However, TCQ scores were not significantly associated with experimental group, $\beta = -.061$, $t(63) = -0.471$, $p = .638$. This hypothesis was not supported.

Hypothesis 6

It was predicted that Higher scores on the ANT executive control measure would attenuate the effect of ATT on increases in ACS scores. In other words, it was predicted that individuals who already displayed efficient attention network efficiency would receive less benefit from the intervention than individuals with inefficient attention network functioning. However, better Time 1 executive attention scores on the ANT task did not significantly attenuate the effect of ATT on changes in the ACS measure, $\beta = .001$, $t(307) = -1.011$ $p = 0.313$, such that individuals with worse initial ANT task scores did not show greater change in ACS scores over time than individuals with higher initial ANT task scores. This hypothesis was not supported.

Hypothesis 7

It was predicted that, if FFMQ scores increase as expected, the Observing factor of the FFMQ will demonstrate a greater increase as compared to the other factors. In other words, the $R^2$ for the FFMQ Observing model will be greater than the models that include the other factors.
Although FFMQ scores did not increase as expected (see results for hypotheses 3.1 and 3.2), this analysis was performed regardless. Overall, 17.24% of the total individual variation in the FFMQ over time was explained by the variation in the Observing factor, 23.18% was explained by the Acting with Awareness factor, 55.16% was explained by the Describing factor, 31.35% by the Nonjudgement factor, and 15.1% was explained by the Nonreacting factor. This hypothesis was not supported.

Additional Post-Hoc Exploratory Analyses

Twenty-six optional Time 8 follow-up study visits occurred (41% of study sample; 15 ATT and 11 PMR), which was sufficient to perform additional analyses. Multiple one-sample t-tests were performed to determine whether Time 8 ANT scores were significantly improved from Time 1 ANT scores in both the PMR and ATT experimental groups, after which an ANOVA was performed to determine whether there were significant differences in ANT score improvement between the two experimental groups. The mean difference between Time 1 and Time 8 ANT conflict scores in the ATT group was significantly different from zero \((t(14) = 3.56, p = .003)\), as was the mean difference in overall mean RT, \((t(14) = 4.758, p < .001)\). Furthermore, mean difference scores for Alerting, \((t(14) = 1.52, p = .151)\), and Orienting, \((t(14) = .83, p = .420)\), in the ATT group were not significantly different from zero. These results were similar in the PMR group. The mean difference between Time 1 and Time 8 ANT conflict scores in the PMR group was significantly different from zero, \((t(10) = 3.51, p = .006)\), as was the mean difference in overall mean RT, \((t(10) = 2.909, p = .016)\). Furthermore, mean difference scores for Alerting, \((t(10) = -.918, p = .38)\), and Orienting, \((t(10) = .808, p = .438)\), in the ATT group were not significantly different from zero. Additionally, a one-way ANOVA was performed to determine whether there
were significant differences in ANT score improvement between the two experimental groups. There was not a significant difference between experimental groups in ANT score improvement from Time 1 to Time 8, $F(1,24) = .056, p = .814$. 
CHAPTER 5
DISCUSSION

The findings of this study are generally mixed. Although the majority of hypotheses were not supported by the data collected via the one-week study protocol, several hypotheses were supported and the feasibility of the innovative EMA RCT design was demonstrated. The primary focus of this discussion will be on an investigation of the potential cause of null results. Additionally, exploratory analyses uncovered promising avenues for further research.

Beginning with the first hypotheses, the ACS was unrelated to the primary outcome variable of interest for the study, executive control over attention. Several strong pieces of evidence suggest that a major issue with the study data, and a primary explanation for so many null results, is that the ACS did not perform as expected, displaying little variability over time within persons and not being associated with several objective measures of attention network function produced by the ANT. The mean unstandardized increase in within-persons ACS scores was 1.48 (SD = 5.01) points (the scale goes up to 80). Furthermore, the mean level of ACS improvement is not significantly different from zero, t(41) = .086, p = .932. The test-retest reliability using the interclass correlation of the ACS from Time 1 to Time 8 was .748 (p < .001), which is very high for an intervention target. Additionally, the results showed that the ACS was not significantly related to the ANT with the exception of the ANT Alerting score, even when examining the two subfactors of the measure separately (as noted in the measures section, the original authors of the measure suggested it had a 3-factor structure, while subsequent factor analyses indicated that the measure has a 2-factor structure, e.g., Olafsson et al., 2011). It may be
the case that individuals are better able to perceive their alerting network functioning than the functioning of their orienting or executive attention networks. Additionally, the ACS was moderately correlated with the FFMQ-SF, suggesting that the ACS has more to do with mindfulness than attentional control. More broadly, these results call into question the validity of the ACS. The findings indicate that the ACS may be better suited as a measure of perceived attentional control, rather than of actual attention network functioning, particularly in the case of higher-order attentional processes.

These results are in line with some of the findings of Quigley et al. (2017), who performed a psychometric and validation study of the ACS and found that it was not related to performance on an antisaccade task (a behavioral measure of executive attention) nor an operation span task (a measure of working memory; Turner & Engle, 1989). Quigley et al. (2017) also found that the ACS was related to measures of anxiety and depression, while their measures of attention and working memory were not. This study found no correlations between the ACS and DASS anxiety or depression, but also found no correlation between executive attention and DASS anxiety or depression. Furthermore, another study found that the ACS was unrelated to performance on the Continuous Performance Test – Second Edition (a task used to aid in clinical diagnosis of ADHD and generally thought to measure inattention, impulsivity, and vigilance), the WAIS-III Working Memory Index, and a DKEFS working memory composite (Williams, Rau, Suchy, Thorgusen, & Smith, 2017). However, one study did find marginally significant associations between the ACS and a minority of subscales of an antisaccade task (Judah et al., 2014). The results of this study add to the small base of literature that appears to show that the ACS is not sufficiently related to objective behavioral tasks of attention or working memory. The measure was selected for use in this study because it is the
only measure that is suitable for EMA methodologies. Behavioral measures of attentional control are time-intensive and require in-person laboratory visits. Administering objective tests of attention via a smartphone app is potential problematic due to the differences in latency between various devices (Kay et al., 2013) as well as the inability to control the participant’s environment during test-taking.

In spite of its apparent lack of construct validity, a number of studies have shown evidence of predictive validity of the ACS (Abasi, Mohammadkhani, Pourshahbaz, & Dolatshahi, 2017; Baskin-Sommers, Zeier, & Newman, 2009; Judah et al., 2014; Melendez, Bechor, Rey, Pettit, & Silverman, 2017; Ólafsson et al., 2011), although it is questionable whether a measure can have predictive validity if it does not have construct validity. Furthermore, the measure has been found to have a consistent 2-factor structure that converges into a single latent construct (Judah et al., 2014; Ólafsson et al., 2011; Quigley et al., 2017), and is a reliable measure of some construct as evidenced by the dozens of studies published using the scale in the literature. Therefore, in light of the current findings and the state of the extant literature, the ACS appears best conceptualized as a measure of perceived attentional control or “cognitive self-efficacy.” This conceptualization would better explain why the ACS is generally found to have a consistent and stable factor structure and is negatively predictive of psychological symptoms such as worry and rumination, but does not predict performance on behavioral measures of executive functions. Evidence for this interpretation of the ACS in this study is that the ACS total score is significantly associated with the cognitive confidence subscale of the MCQ at Time 1 \( (r = .544, p < .001) \) which measures the degree to which one distrusts their memory and other unhelpful beliefs about their cognitive ability (Wells &
Cartweight-Hatton, 2004). Therefore, negative correlations with this subscale would indicate higher levels of overall cognitive confidence.

Additionally, the trend in the literature of examining the predictive validity of the ACS in the absence of strong evidence for construct validity is premature, likely not supportable, and, at worst, potentially misleading. The distinction between predictive and construct validity must be clearly outlined in any psychometric validation study. Arguably, while more difficult to ascertain, construct validity is more important than predictive validity (Whitley & Kite, 2013), particularly in this transtheoretical context that is attempting to bridge together cognitive and clinical psychology. However, earlier psychometric studies of the ACS which establish predictive validity fail to acknowledge the lack of evidence for construct validity in the literature (e.g., Ólafsson et al., 2011). That said, S-REF and broader metacognitive theory does indicate a relationship between attentional control and stress or worry (Wells, 2009), so it would be expected that any measure of attentional control ought to negatively predict these psychological symptoms. However, the original authors of the ACS defined it as a measure of an individual’s “general capacity for attentional control, with correlated subfactors” (Derryberry & Reed, 2002), not a measure of metacognitive susceptibility to worry and rumination. Therefore, validation studies of the ACS must include cognitive data, even if coming from metacognitive theory perspective, and collections of studies that support a measure’s predictive validity are not a substitute for evidence of construct validity, no matter how numerous the former are in the literature. Because objective construct validation studies such as Williams et al. (2017) have not observed a relationship between psychological symptoms and attentional control, this may be a broader issue for S-REF theory, and it may be the case that some other confounding underlying process dictates the relationship between worry/rumination and executive control. If S-REF and
other metacognitive theories are correct, a measure of attentional control must be significantly related to behavioral measures of executive attention as well as psychological symptoms related to rumination and worry. At this time, there does not appear to be such a measure.

Neither the ATT nor the PMR intervention significantly reduced anxiety, depression, or stress in the sample. However, these symptoms were only assessed at two timepoints due to EMA design constraints, and the power for these analyses was reduced as a result. In a post-hoc analysis, when collapsing across groups to increase the power of the analyses, individuals in the study did experience a significant reduction in DASS total scores. This analysis must be interpreted with extreme caution, due to the lack of a comparison group and the psychological symptoms assessed by the DASS frequently being observed to spontaneously increase or decrease over time (e.g., Cuijpers et al., 2013; Goncalves & Byrne, 2012). However, this analysis indicates there may be an important avenue for future research to continue exploring brief psychological interventions via smartphone such as the two administered in this study for the purposes of symptom reduction.

In light of the poor construct validity demonstrated by the ACS, as well as the lack of variability within the measure over time, it is not surprising that the results of the study indicated that neither group significantly changed ACS scores over time. On the other hand, each group experienced a similar improvement in ANT scores over the one-week study period, with executive attention scores improving and other, more rudimentary attentional processes remaining stable. This again indicates that this area of research may warrant additional studies with more valid measures of outcome variables. There were no differences in attention network improvements between the two experimental groups. The pattern of general similarity between outcomes in the PMR and ATT groups is suggestive of a greater degree of overlap between the
two interventions than was initially anticipated. While several studies have found differences between ATT and PMR in emotional domains (Fergus et al., 2014; McEvoy et al., 2017), the two interventions share several components that may function along similar pathways, albeit explained differently across the theoretical bases from which the two interventions were produced. For example, both ATT and PMR require periods of sustained attention on a specific sensation (McCallie, Blum, & Hood, 2006; Wells, 2009). In the case of ATT, it is a particular sound, while in PMR it is a group of muscles. There is less attention switching in PMR than in ATT, but at the very least, both tasks require that if attention wanders away from the instructions it be switched back to the task at hand. There is also a particular order to ATT and PMR that does not change (McCallie et al., 2006; Wells, 2009) – the metronome is always attended to first in ATT, just as the muscle groups in the dominant hand and forearm are always attended to first in PMR\(^1\). This consistency may produce a meditative-like practice element to both ATT and PMR. That said, the results of Hypothesis 5 indicate that there is some difference between the two interventions. ATT decreased the presence of problematic metacognitions as predicted while the PMR intervention did not. This is consistent with the S-REF theory that psychological dysfunction arises from poor cognitive self-regulatory strategies that lead to distress, culminating in mood disorder (Wells, 2009). ATT is an intervention developed for the purpose of improving potentially maladaptive or poorly performing cognitive self-regulatory strategies (Fergus & Bardeen, 2016), while PMR and other mindfulness-based interventions seek to reduce judgement and increase awareness of these cognitive processes (Grossman, Niemann, Schmidt, & Walach, \(^1\)This is not to say that all PMR interventions begin this way, but that for the purposes of a controlled and repeated daily intervention, the particular method of PMR used in this study repeated the muscle groups in the same order at each timepoint.)
2004). This may explain why ATT was associated with reductions in MCQ scores but PMR was not.

Given the similarities between the two interventions, it would not be unreasonable to expect that they equivalently affected an individual’s overall level of mindfulness over time. Unfortunately, along with the ACS, the FFMQ-SF used in this study also did not perform as expected, displaying little within-persons variability. This means that it is difficult to evaluate hypotheses that are attempting to explain the small amount of variability within the measure. The mean unstandardized increase in within-persons FFMQ-SF scores was 3.28 (SD = 6.73) points (the scale goes up to 90). Furthermore, the mean level of FFMQ-SF improvement is not significantly different from zero, t(41) = .220, p = .827. The test-retest reliability of the FFMQ-SF from Time 1 to Time 8 was .791 (p < .001), which is again very high for an intervention target. The ability of the HLM models in Hypothesis 3 and 4 to detect change over time was limited by the lack of variance in the FFMQ-SF over time (Hypothesis 4 was also limited by the invariability of the ACS as well). As a result, neither of the experimental groups were associated with changes in mindfulness scores over time. There are three plausible explanations for this, one being that the FFMQ-SF measures a stable construct that is not amenable to change over such a short period of time. Another is that neither of the two interventions employed in this study affect mindfulness. A third is that participants simply did not adequately engage with the study intervention, although there was no observed evidence of this. In terms of stability, the FFMQ has been consistently shown to have high test-retest reliabilities in the absence of interventions (ranging from .6 to .85; Cebolla et al., 2012; de Bruin, Topper, Muskens, Bögels, & Kamphuis, 2012; Deng, Liu, Rodriguez, & Xia, 2011; Veehof, ten Klooster, Taal, Westerhof, & Bohlmeijer, 2011). Brief mindfulness interventions have successfully increased mindfulness in participants
(Chiesa & Serretti, 2011; Pelekasis, Matouka, & Koumarianou, 2017), although this is not a consistent finding (e.g., Mermelstein & Garske, 2015; Reynolds, Bissett, Porter, & Consedine, 2017), and effects are generally reported after several weeks of an intervention. Additionally, PMR has been found to be not as effective at increasing mindfulness as other directed mindful meditation practices (Semple, 2010). Because the evidence is mixed as to the speed with which an intervention can meaningfully change an individual’s mindfulness, the results of this study are in-line with the broader picture in the literature. Furthermore, ATT has been directly compared to PMR in a handful of previous studies, but was only assessed on its ability to reduce worry and self-focused attention (Fergus et al., 2014), chronic pain (Sharpe et al., 2010), and psychological symptoms (McEvoy et al., 2017). Prior to this study, PMR had not been used as a comparison group to ATT assessing executive functioning or broad measures of mindfulness.

The two a priori exploratory hypotheses, demarcated as such because they were based on deductive reasoning rather than derived from empirical evidence, were also not supported in this study. Hypothesis 6 described the logical idea that individuals who were already high in executive attention functioning would receive less benefit from a neurobiological intervention than those who were lower in functioning at Time 1. However, this supposition was not supported, although unfortunately it is difficult to determine whether this is the case because the outcome variable was the ACS or whether there is no difference between high- and low-functioning individuals in terms of response to the ATT intervention. Hypothesis 7 predicted that the Observing Factor of the FFMQ would be most affected by the ATT intervention, although this was also not supported. Furthermore, due to the lack of overall variability within the FFMQ data, it is difficult to definitively state that the Acting with Awareness FFMQ Factor is the most
important factor contributing to the change in mindfulness scores over time in response to either PMR or ATT.

To the author’s knowledge, this study was the first randomized controlled trial of its kind to demonstrate the feasibility of administering the ATT intervention via a smartphone. The percentage of ATT interventions completed was 84.82% on average which suggests the intervention was highly tolerable. Furthermore, out of all randomized controlled trials of ATT, this study administered a higher dose (7 sessions) of ATT than any previous study (see Fergus and Bardeen, 2016 for a review of RCTs as well as Haukaas, Gjerde, Verting, Hallan, & Solem, 2018; Murray, Scott, Connolly, & Wells, 2018) with the exception of Moritz et al. (2011) who did not use a standardized or controlled version of ATT. User data from the smartphone application indicated that participants spent a reasonable amount of time in the application during each session ($M_{\text{minutes}} = 19.45, \text{SD} = 1.49$) in order to complete the 12-minute intervention and to complete the two accompanying questionnaires, an indication of participant engagement. The study offers an excellent blueprint for how to administer full RCTs via EMA methodologies.

Limitations

There are several limitations to the study. The first of which has already been discussed at length – there is not an ideal measure of attentional control that is suitable for EMA methodologies. Therefore, investigating the mechanisms of change of brief or adjunctive neurobiological interventions administered via smartphones remains difficult. However, the additional inclusion of the ANT strengthened the study by validly measuring changes in attentional control over time. Unfortunately, the ANT was only administered at two timepoints. Additional administrations of the ANT would allow for its inclusion in HLM analyses and would
reduce or eliminate the need for self-report measures of attentional control. Another weakness to the study is that only 41% of participants volunteered to complete the Time 8 study visit that included the second administration of the ANT. This reduced the statistical power of any potential analysis, precluding the ability to introduce control variables into regression models, for example.

The study sample was homogeneous in a few different ways which may limit the generalizability of findings. Participants were, for the most part, between the ages of 18 and 22 and may be better educated than the general population. However, the student sample was representative in terms of race/ethnicity and may be more closely aligned with the income distribution of the US than typical university students.

Another limitation of the study design is that the active control group selected, PMR, was likely very close in terms of mechanisms of action to the intervention, ATT. Both PMR and ATT manipulate and exercise participants’ attention in qualitatively different ways. However, these differences may not sufficiently differentiate the two interventions for the purposes of comparing mechanisms of change.

Finally, it was not possible for the study design to control the environment in which the participants completed their daily tasks. It is possible that the sample was often distracted while listening to the interventions, limiting their overall effectiveness in terms of reducing psychological symptoms and increasing mindfulness/attentional control. It is also possible that participants were not fully engaged with the interventions, limiting their effectiveness. This is broadly a limitation of unsupervised interventions. Many cognitive-behavioral therapies that aim to promote unsupervised, at-home practice provide structured homework that is then discussed in
person upon completion (e.g., CPT; Resick et al., 2017). No such in-person accountability was required in this study protocol.

**Future Directions**

Because of the successful demonstration of study design feasibility as well as preliminary indications that objectively measured attentional control is improved in both individuals undergoing a PMR or an ATT intervention, further research into this area is warranted. It is recommended that future studies use alternative or additional control groups to ensure that they are isolating the unique components of ATT. For example, an additional thought-wandering control group could be introduced alongside ATT and PMR such as in McEvoy et al. (2017). Future research also may consider different mechanisms of change to explore that do not require the ACS or the FFMQ. Measuring less abstract variables for the purposes of self-report may be more feasible, such as the Cognitive Flexibility Scale (Martin & Rubin, 1995), the Focus of Attention Scale (Fergus et al., 2014), or the Awareness and Distancing Scale (McEvoy et al., 2017). Additionally, measuring components of mindfulness with, for example, the Cognitive Fusion Questionnaire (Gillanders et al., 2014) or the Uncontrollable Thoughts Scale (Wells & Cartwright-Hatton, 2004) may yield quicker changes over brief periods of time. Additionally, modifying the ACS and the FFMQ instruction sets to prompt for state-based responses as opposed to trait-based responses may produce additional variance within the constructs. Future research ought to retain this study’s procedure of intervention administration, as results indicated that the smartphone application intervention was well-tolerated and appropriately engaging. Additionally, future research may consider collecting data from a higher number of participants in order to introduce control variables into the analyses. This would be particularly useful in
studies investigating the validity of the ACS. In particular, it may be useful to control for self-esteem, self-confidence, or positive affect when attempting to establish predictive validity.
REFERENCES


