Mindfulness and Acceptance Approaches to Athletic Performance

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Statement of Authorship

This thesis contains no material published elsewhere or extracted in whole or in part from a thesis by which I have qualified for or been awarded another degree or diploma. No parts of this thesis have been submitted towards the award of any other degree or diploma in any other tertiary institution. No other person's work has been used without due acknowledgment in the main text of the thesis. All research procedures reported in the thesis received the approval of the relevant Ethics/Safety Committees.

I was involved in all aspects of the presented research and responsible for coordinating the projects under the supervision of Professor Joseph Ciarrochi and Professor Chris Lonsdale. The introduction, literature review, and discussion of this thesis are solely my work. The peer-reviewed publications included in this thesis are the work of the acknowledged authors on these publications. I am the principal author of each of these publications and led all aspects of the investigations. I was responsible for all aspects of the recruitment process, data collection and analyses, and preparing the drafts of all publications.

Michael Noetel

Statement of Appreciation

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Abstract

Performance enhancement strategies in sport have frequently attempted to help athletes gain control over their thoughts and their emotions (Vealey, 1994). These are 'content-focused' approaches try to change the content of the athlete's internal experience. Recently, increasing attention has been directed toward interventions that try to change an athlete's *relationship* with those internal experiences, instead of changing the content. These 'context-focused' approaches—including mindfulness and acceptance-based interventions—aim to help athletes perform well *with* anxiety, rather than trying to remove the anxiety (Gardner & Moore, 2012b). These approaches promote similar acceptance of other unhelpful cognitions and emotions, such as anger or self-doubt. In this thesis, I aimed to explore the effectiveness of these approaches for promoting performance in sport.

Chapter 1 identified the theories underlying context-focused approaches and outlined how they might reduce the likelihood of performance problems. For example, due to decreased self-focused attention, context-focused approaches may reduce the likelihood of choking due to explicit control of otherwise automatic skills. To see how well these theories held up to empirical exploration, Chapter 2 systematically reviewed the literature on context-focused approaches in sport. This review included prototypical context-focused approaches, like mindfulness, and also the wider range of approaches that operate by a similar mechanism, such as self-compassion. It found consistent trends in the research that these approaches improve athletic present-moment awareness, flow, performance, and help to reduce anxiety. It also revealed preliminary evidence for other outcomes like improved confidence and reduced rates of injury. However, none of the 66 studies met the Cochrane

Collaboration criteria for low risk of bias (Higgins, Altman, Gotzsche, et al., 2011).

In Chapter 3, I aimed to test a brief context-focused intervention using a study design that met these Cochrane criteria. Golfers were randomised into either a acceptance-based intervention or a control condition. The study was double-blinded, randomised, prospectively registered with putting performance as the primary outcome. This study found few benefits of the acceptance-based intervention for performance, anxiety, or state mindfulness. It found a significant improvement on a secondary outcome: swing mechanics as measured by a SAM PuttLab (Science & Motion, 2016). While brief interventions are well-established for testing context-focused interventions (Levin, Hildebrandt, Lillis, & Hayes, 2012), Chapter 3 did not find strong evidence that a brief context-focused intervention leads to short-term improvements in sport performance.

One barrier to testing interventions in sport is the questionnaire response burden placed on athletes. This is not unique to context-focused literature, because half of athletes in high-performance environments complete questionnaires every day (Taylor, Chapman, Cronin, Newton, & Gill, 2012). However, in mindfulness and acceptance literature, reducing response burden would help researchers in many ways. Shorter measures allow researchers to assess more constructs in parallel, or assess the same construct more regularly (Basarkod, Sahdra, & Ciarrochi, 2018). Many short measures fail to meet psychometric criteria (Smith, McCarthy, & Anderson, 2000) because the process of optimally shortening a questionnaire requires complex evaluations of many factors (Marsh, Ellis, Parada, Richards, & Heubeck, 2005). As an alternative method, Chapter 4 demonstrated that an advanced machine learning algorithm can shorten athletic questionnaires without compromising reliability or validity. As an example of this process, the substantive-methodological synthesis presents multiple versions of the Mindfulness Inventory for sport. Although reliability was compromised when measures were very brief, shorter measures showed equivalent validity to the full measure. The resulting measures offer future researchers some alternate methods of measuring mindfulness that can be adapted to their needs. The paper also outlined ways for other researchers to efficiently shorten their own measures.

Chapter 5 describes limitations of the previous chapters, but also identifies some ways in which researchers might assess the utility of context-focused approaches in the future. Many of these approaches require that athletes make significant investments in time. The thesis concludes that, although there is evidence these approaches might have widespread benefits inside and outside sport, it is not yet clear whether they are an effective use of time for optimising athletic performance.

Chapter 1—Introduction and Overview

“I was in a constant battle to control my nerves—but I fought them down, and the one thought that occupied my brain was that today I’d rise to the occasion.” (Rafael Nadal; Nadal & Carlin, 2012, p. 7)

“I acknowledge the negative thoughts and let them slide by. This lets me focus on what is really important” (Djokovic, 2013, p. 88).

For more than 10 years, there has been conflict between two greats of sport, Rafael Nadal and Novak Djokovic. The conflict exists between these two players on the tennis court, but also in the different ways they manage nerves and negative thoughts. Do athletes perform better when changing their negative thoughts, or is it better for them to accept those thoughts while maintaining focus on the present moment? Researchers have put forward theories and evidence to support both sides of this debate (e.g., Birrer, Rothlin, & Morgan, 2012; Gröpel & Mesagno, 2017; Sappington & Longshore, 2015), with some authors claiming that the two approaches are diametrically opposed (Gardner & Moore, 2012b). This chapter will first explore Nadal’s approach to negative thoughts and feelings through change and control. It will then address some concerns about that approach, and explore arguments for an alternative approach: one that involves managing those thoughts and feelings through awareness and acceptance. Finally, it will outline the research from this thesis, which is designed to shed additional light on the latter approach.

As will be discussed, the latter approach include mindfulness and acceptance based approaches, which this thesis subsumes under the broader label of ‘context-focused interventions.’ The research in this thesis explored context-focused approaches through three studies. In the first, I aimed to conduct a comprehensive synthesis of the literature on these approaches to establish the state of the evidence and to identify key gaps. To do this, Chapter 2 is a systematic review of the context-focused literature in sport. Two of the key gaps from that review related to the internal validity of the existing literature. No studies in the review assessed context-focused approaches using designs that met the Cochrane criteria for low risk of bias. Without meeting these criteria, studies are open to criticism that findings are attributable to experimental design rather than intervention effects. Chapter 3 presents a study using an experimental design that aims to meet these criteria. A second limitation of the existing literature is dearth of studies that explore the causal models underlying context-focused interventions. Previous authors have usually assumed that interventions change context-focused variables, and changes in those variables mediate improvements in performance. However, assessing this hypothesis requires that researchers repeatedly administer assessments of context-focused variables, which can impose a significant response burden. To facilitate this process, Chapter 4 demonstrates how researchers can use machine learning algorithms to abbreviate questionnaires to reduce this burden. It uses these algorithms to present and validate shorter measures of context-focused variables, with the intention of facilitating assessment of the causal models. Finally, Chapter 5 identifies strengths and weaknesses of these three studies, and identifies opportunities for future research.

Content-Focused Interventions to Performance Enhancement

Athletes tend to perform better when feeling good, confident, and relatively calm. Meta-analyses have established that performance has small to moderate correlations with positive moods (Beedie, Terry, & Lane, 2000) and high self-confidence (Craft, Magyar, Becker, & Feltz, 2003; Moritz, Feltz, Fahrbach, & Mack, 2000; Woodman & Hardy, 2003). These meta-analyses tend to find small or non-significant associations between performance and anxiety (Craft et al., 2003; Kleine, 1990; Woodman & Hardy, 2003); however, these data appear to be explained by a nonlinear relationship, where performance is best when athletes are in their zone of optimal functioning compared with being under- or over-aroused (d = 0.44, 95% CI [0.32, .55]; Jokela & Hanin, 1999).

When athletes are outside their optimal zone, lacking confidence, or in negative moods, they often try to control their thoughts and feeling. To achieve this goal in sport, one common suite of techniques is psychological skills training (Vealey, 1994). These techniques are largely drawn from the cognitive behavioural tradition, where changing thoughts and feelings is a key approach to changing behaviour and improving wellbeing (Johnsen & Friborg, 2015). Psychological skills training generally aims to change the *content* of internal experiences, for example by swapping a negative thought ("You cannot do it") for a positive one ("You can do it"; Brewer et al., 1995). These approaches are broader than just positive thinking. The four psychological techniques that have have been most extensively researched are goal setting, imagery, self-talk, and relaxation (Birrer & Morgan, 2010; Vealey, 1994). Almost every US olympian uses at least one of these strategies and those who win medals tend to use more of them (Taylor, Gould, & Rolo, 2008).

Use does not necessarily mean effectiveness, but Greenspan and Feltz (1989) reviewed the literature on these techniques and concluded that, on the whole, the interventions were effective at improving performance. Effects appeared stronger for interventions focusing on relaxation and cognitive restructuring than other skills like imagery (Greenspan & Feltz, 1989). A subsequent review by Vealey (1994) found the strongest performance benefits were from multimodal, cognitive-behavioural interventions: those that used some combination of goal setting, imagery, relaxation and self-talk. While these interventions appear to work better when combined, causal claims about psychological interventions are easier when addressing one component at a time (Rosen & Davison, 2003).

Goal setting. Goal setting is not strictly a content-focused intervention because it may function by shifting attention to important metrics or parts of a skill (Locke & Latham, 2002). It is sometimes considered a content-focused intervention because people use it to increase motivation (Kyllo & Landers, 1995). It is often grouped with the other psychological skills (Greenspan & Feltz, 1989). The most recent goal-setting meta-analysis (Kyllo & Landers, 1995) found that setting moderately difficult goals led to better athletic performance (*ES* = 0.53, 95% CI [0.45, 0.61]). The effect size for goal setting was significantly lower where the task was familiar than when it was novel (*ESs* = 0.28 and 0.51, respectively). It was larger for adolescents than it was for older, college athletes (*ESs* = 0.51 and 0.34 respectively), possibly because the intervention is more likely to be novel. It is unlikely to be novel for experienced athletes, because it is the most commonly used psychological skill (Taylor et al., 2008).

Weinberg (1994) identified “several methodological problems that have plagued goal setting research in sport and exercise” (p. 474). These problems include confounds like spontaneous goal setting and competition. People set goals—such as to win or beat a goal inside their mind—whether or not you ask them to. Studies that do control for these confounds found goal setting to be significantly more effective than setting no goals (Lerner & Locke, 1995). Most studies, like Lerner and Locke's, involved undergraduates across a single session of training. Weinberg's review described a dearth of goal setting studies that looked at long term effects in externally valid contexts. It may be a useful technique for improving performance, but evidence is stronger for younger athletes doing unfamiliar skills, and long-term effects remain unclear. A recent systematic review of literature looked at all strategies that might stop athletes performing poorly under pressure ('choking'; Gröpel & Mesagno, 2017). This review did not find evidence that goal setting reduces the likelihood of an athlete choking under pressure.

Imagery. For a long time, imagery has been another popular technique, with up to 99% of Olympic athletes using it (Orlick & Partington, 1988). Imagery and mental practice are similar but distinct skills that are often combined in surveys that measure levels of use (Murphy, 1994). Imagery is the internal creation or re-creation of any experience (Weinberg, 2008) often used to prepare for the execution of a physical skill by helping the athlete 'psych-up' (Murphy, 1994). Conversely, mental practice refers to "cognitive rehearsal of a task in the absence of overt physical movement" (Driskell, Copper, & Moran, 1994, p. 481). Driskell and colleagues (1994) conducted a meta-analysis on mental practice across a variety of skills in both sport and non-sport contexts. Their analysis aggregated a variety of tasks, from lab-based cognitive tasks to the execution of competitive, athletic skills, so the generalizability of their findings to sporting contexts should be used with caution. Nevertheless, the overall findings suggested mental practice significantly improves performance (d = 0.53), particularly for skills that require a higher cognitive input (r = 0.38), but it was not as effective as physical practice (d = 0.78). Mental practice does not aim to change the content of internal experiences during competition; instead, it is a tool used in training to help accelerate gains made from physical practice.

Motivational imagery is a prototypical content-focused intervention. It is designed to change emotional states by generating new thoughts. Athletes use imagery before and during competition to help them control their confidence, motivation, or arousal (Murphy, 1994). This type of imagery has been less rigorously studied (Murphy, 1994), and research has often relied upon single-case designs or laboratory experiments (Weinberg, 2008). While some studies have found significant improvements in confidence and reductions in pre-competitive anxiety (Mellalieu, Hanton, & Thomas, 2009), when studies have directly compared mental practice with imagery, mental practice was shown to be more effective for improving golf putting performance (Short et al., 2002) and weightlifting (Wright & Smith, 2009).

Self-talk. Motivational self-talk is another technique that has been used as a typical, content focused intervention to improve confidence and motivation (e.g., ‘you can do it!’; Hatzigeorgiadis, Zourbanos, Galanis, & Theodorakis, 2011). Instructional self-talk is subtly different: it is the term used to describe cues that help athletes remember where to direct their attention in the present moment (e.g., "watch the ball"; Hatzigeorgiadis et al., 2004). When aggregating across various research designs (multiple baseline; post vs. control; pre-post, with and without control groups), a recent meta-analysis found strong effects for instructional self-talk in sports that were classified as fine motor skills (e.g., basketball free-throws, golf putting; d = 0.83, 95% CI [0.64, 1.02]; Hatzigeorgiadis et al., 2011). Instructional self-talk showed a more modest aggregated effect size for gross motor skills (e.g., cycling, running; *d* = 0.22, 95% CI [0.14, 0.31]), and motivational self-talk displayed a small-moderate effect size for both fine (*d* = 0.41, 95% CI [0.22, 0.59]) and gross skills (*d* = 0.33, 95% CI [0.19, 0.48]).

When we look more closely at self-talk interventions, we see they are not all created equal. Subtle differences in the cues selected by athletes can transform the effects of instructional self-talk (Gucciardi & Dimmock, 2008). Athletic performance can be stifled by using self-talk as it may break an automatic skill into component parts (e.g., throwing underarm by using cues “back, forth, release”; Gucciardi & Dimmock, 2008) that are then performed in an inefficient and uncoordinated manner. Instructional self-talk that helped the athlete focus on the whole movement led to an increase in performance under pressure (holistic cue-words like “smooth”; Gucciardi & Dimmock, 2008). Hatzigeorgiadis et al. (2011) tried to account for differences in cue words by exploring the influence of assigned versus self-selected cues; however, they did not find this to be a significant moderator. Instead, the methodology of the included studies was important. Specifically, Hatzigeorgiadis and colleagues (2011) found the effect sizes for the six multiple-baseline studies (*d* = 1.31, 95% CI [.75, 1.88]) to be significantly higher than those with a control condition. While these less rigorous research designs may be artificially inflating the effect sizes in pooled analyses (Shadish, 2006). it appears that self-talk that directs *attention* to the whole movement can have positive influences on fine motor skills. Self-talk that aims to control the *content* of internal experience (e.g., motivation, confidence, anxiety) is less effective.

Relaxation. Relaxation is an umbrella term for a variety of techniques aimed at reducing anxiety and arousal, such as deep breathing, stretching, and progressive muscle relaxation (Kudlackova, Eccles, & Dieffenbach, 2013). These techniques are commonly used amongst athletes to enhance performance (Taylor et al., 2008), particularly amongst professional athletes who are faced with higher levels of competitive pressure (Kudlackova et al., 2013). These interventions presume relaxation increases the probability that athletes will self-regulate into their zone of optimal functioning (Jokela & Hanin, 1999). Relaxation may be more context dependent than other techniques: for example, athletes in high intensity sports may require higher levels of arousal (Birrer & Morgan, 2010) but the same athletes might find it useful to use relaxation for sleep or recovery. The empirical research for relaxation is less developed compared with the other psychological techniques discussed so far (Kudlackova et al., 2013). Reviews of the literature in sport have been equivocal (Gould & Udry, 1994; Pelka et al., 2016); some arousal regulation strategies (e.g., biofeedback) may help performance, but methodological limitations continue to hamper the strength of conclusions (Gould & Udry, 1994; Pelka et al., 2016).

In non-sport contexts, a recent meta-analysis of anxiety reduction techniques found meditation and ‘applied relaxation’ (the label used for a combination of relaxation and exposure therapy) were both found to be the most effective anxiety-reduction techniques (d = 0.86 & 0.95 respectively; Manzoni, Pagnini, Castelnuovo, & Molinari, 2008). The arousal regulation techniques that have traditionally been used in sport psychology showed systematically lower effect sizes (*d*s: progressive relaxation = 0.55; autogenic training =

0.42; multi-modal relaxation = 0.43; Manzoni et al., 2008). As will be explored later, these relaxation strategies appear to operate by different causal pathways compared with mindfulness meditation. Progressive relaxation, for example, is designed to "increase sensations of physical relaxation" (Feldman, Greeson, & Senville, 2010, p. 1003). In contrast, mindfulness "is not designed to actively change physical or emotional states, but instead to observe and accept current internal experiences as they are" (Feldman et al., 2010, p. 1003).

Relaxation strategies that seek to reduce anxiety have been directly compared with acceptance strategies like mindfulness. For example, one study directly compared relaxation training with mindfulness meditation (Jain et al., 2007). Both produced a relaxation response; however, meditation also demonstrated larger effects on cognitive processes like rumination, distractive thoughts, and positive states of mind (Jain et al., 2007). When people have negative thoughts while using progressive muscle relaxation, they still experienced a high level of distress (Feldman et al., 2010). Those using mindfulness reported higher levels of detachment from those thoughts, and while negative thoughts persisted, they do not lead to commensurate distress (Feldman et al., 2010). So while relaxation techniques can reduce anxiety, research has seldom shown the techniques improve performance. Relaxation strategies may reduce some negative emotional content. However, this effort to control anxiety may shift attention to task-irrelevant stimuli, may reduce arousal outside of the zone of optimal function, or may lead to less functional methods of managing other content, like negative thoughts.

Criticisms of Content-Focused Approaches

"As much as I tried to banish the thought entirely from my mind, there it lurked, inhibiting me." (Rafael Nadal; Nadal & Carlin, 2012, p. 281)

A number of authors have raised questions about the empirical support of these content focused interventions for improving sport performance (Gardner & Moore, 2006; Morgan, 1997). Morgan (1997) has been particularly critical, arguing that "most of the interventions in applied sport psychology are based upon unverified hypotheses and unsubstantiated pedagogical principles, rather than on scientific evidence" (p. 5). Even supporters of these interventions have been critical of their evidence base. Many reviews of psychological skills training have been critical of the research methods (Greenspan & Feltz, 1989; Pelka et al., 2016; Schweizer & Furley, 2016; Tamminen & Poucher, 2018; Vealey, 1994). Researchers have often ignored manipulation checks or follow-up assessment (Greenspan & Feltz, 1989; Vealey, 1994). They often use small sample sizes with poor power to detect intervention effects (Schweizer & Furley, 2016). They have seldom reported clear methods of randomisation or blinding (Pelka et al., 2016) and none have followed open science principles (Tamminen & Poucher, 2018).

Some of the most vocal critics of Psychological Skills Training are Gardner and Moore (2006, 2007, 2012). Gardner and Moore (2006) conducted a systematic review of the randomised controlled trials on these content focused approaches. They critiqued the available research on the basis of specific criteria for empirically supported treatments (Chambless & Hollon, 1998): the treatment has demonstrated superiority to control or equivalence to established treatment in a randomised controlled trial, controlled single-case experiment or time-series design, and the efficacy studies included treatment manuals, externally valid populations, reliable outcome measures and appropriate data analyses. Gardner and Moore (2006) concluded that each of the four psychological skills (i.e., goal setting, relaxation, imagery, and self-talk) should be considered ‘experimental’ for improving performance in experienced athletes, meaning that there were no published studies meeting the above criteria that included a sample size of three participants or more. They concluded the same was true for interventions that included a combination of the four psychological skills.

The Gardner and Moore (2006) analysis was not peer-reviewed, which is problematic because they leverage their review to propose their own approach to performance enhancement (i.e., Gardner & Moore, 2007), so may have had a conflict of interest. Also, the criteria used to establish empirical support is now antiquated, with best-practice now involving systematic reviews that assess risk of bias (Green et al., 2011). These criticisms are not exclusively problems with psychological skills training literature. Recent reviews have failed to find any studies in sport psychology that meet some risk of bias criteria, such as the criteria around prospective registration (Tamminen & Poucher, 2018).

The criticisms levelled against content-focused approaches in sport have been theoretical as well as methodological. Psychological skills like motivational imagery, motivational self-talk, and relaxation all required that athletes direct attention toward changing an internal experience (Birrer et al., 2012). Instead of negative pre-competitive thoughts, motivational self talk may reframe or replace those thoughts with more optimistic expectations. While increased confidence and reduced anxiety are often therapeutic goals of athletes, they may be neither necessary nor sufficient for performance enhancement. Hayes and colleagues (1999, 2011) suggest that these efforts to control or avoid internal experiences can make it harder to engage in valued actions. Similarly, Gardner and Moore (2007, 2012b) suggest directing attention in this way makes it harder to maintain task-focused attention. These theoretical criticisms have been used as a justification for 'third-wave', context-focused, mindfulness and acceptance approaches.

Theory Behind Context Focused Approaches

"I discovered that I was far more effective when I became completely immersed in the action, rather than trying to control it and fill my mind with unrealistic expectations" (Phil Jackson; P. Jackson & Delehanty, 2006, p. 51).

In contrast with approaches that change the *content* of internal experience, context focused approach involves changing the *relationship* athletes have with those experiences (Gardner & Moore, 2012). The intention is to help athletes respond to negative internal experiences with acceptance, self-compassion, and non-judgmental awareness of the present moment (Mosewich, Crocker, Kowalski, & Delongis, 2013). In other words, cognitive-behavioural strategies, like most psychological skills, aim to help athletes by reducing the amount of anxiety they experience, and increasing the number of positive thoughts and feelings (Vealey, 1994). Context focused approaches, on the other hand, focus on learning to accept anxiety as a normal part of the athletic experience, and focus on the present moment regardless of those internal processes (Birrer et al., 2012). This theme of present-moment awareness alongside acceptance underlies a number of related approaches including, but not limited to, mindfulness (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006; Kabat-Zinn, Beall, & Rippe, 1985), ACT (Hayes et al., 1999; 2012), Self-Compassion (Neff, 2003), and the Mindfulness-Acceptance-Commitment approach (MAC; Gardner & Moore, 2007).

There were a number of pieces of basic research that informed this shift in focus, including the ironic processes of mental control and the theory of reinvestment (Birrer et al., 2012). In Ironic Process Theory, deliberate attempts to control internal experiences are fraught because exercising control over these experiences initiates two conflicting processes: an operating processes that attempts to control the experience, and a monitoring process that checks to see whether control has been successful (Wegner, 1994). Wegner (1994) presented research demonstrating this effect with respect to thought suppression, concentration, mood control, arousal regulation, pain control, and various types of beliefs. To use a metaphor adapted from Hayes, Strosahl, and Wilson (2011), if an athlete was connected to a polygraph and told he or she would lose if their heart rate rose, the fear of an increasing heart rate may have made it particularly difficult to focus on the task at hand. These authors proposed that the more effort people invest into keeping calm, the stronger the monitoring of their level of arousal, leading them to be more sensitive to normal fluctuations in anxiety.

These ironic effects have been demonstrated in a sporting context. One study compared golfers who were only told to ‘get the ball in the hole’ with those who were also told to ‘avoid putting too short’ (Binsch, Oudejans, Bakker, & Savelsbergh, 2009). A large proportion of athletes who were told to avoid putting short spent more time gazing in front of the hole. Those athletes often proceeded to putt where they were looking, contrary to instructions. This was not the case for all athletes, as some who were provided with this instruction overcompensated and hit too hard (de la Pena, Murray, & Janelle, 2008). Nevertheless, similar ironic gaze behaviour has been found in soccer penalty kicks (Binsch, Oudejans, Bakker, & Savelsbergh, 2010), and these ironic processes have been shown to be particularly salient for athletes who typically try to repress their competitive anxiety (Woodman & Davis, 2008).

Another ironic effect in sport performance is the debilitating effect of excessive task-focused attention (Baumeister, 1984; Gucciardi & Dimmock, 2008; Masters & Maxwell, 2008). Reinvestment Theory describes the process where self-regulatory attention leads to poorer performance because this attention disrupts the automaticity of movement (Masters & Maxwell, 2008). It turns non-verbal, procedural processes into verbal, declarative ones. If a soccer player is trying to relax before a penalty kick, she may shift her attention away from the target. In a similar vein, deliberate attention to the execution of a skill is argued to be responsible for the tendency of athletes to perform less well with anxiety (Baumeister, 1984). If the soccer player focuses on how she is going to kick the ball, this focus may disrupt the execution of a kick that would otherwise be automatic. As discussed earlier, self-talk that directs attention to the execution of a physical task can exacerbate the detrimental influence of pressure on performance (Gucciardi & Dimmock, 2008), consistent with Baumeister's (1984) hypothesis. She will kick less well if she uses cues that break the components of the skill apart: "step, lock, impact". This may be useful in practice and skill acquisition, but is unhelpful for skill execution once expertise is obtained.

Both ironic processing and reinvestment theory highlight the problems surrounding the use of self-regulatory strategies when trying to execute a well learned task (Birrer et al., 2012). Content focused interventions may, therefore, be counterproductive to optimal performance (Moore, 2009). Gardner and Moore (2006, 2007, 2012) argued that the alternate approach of focusing on acceptance and present moment awareness facilitates the automatic execution of performance in experienced athletes. Some EEG studies have supported this assertion, showing experienced athletes use fewer attentional resources and less cognitive control when performing motor skills (Hatfield, Haufler, Hung, & Spalding, 2004). Marks (2008) explored some other neurological associations between mindfulness and successful athletic performance. He presented a number of studies showing differences between meditators and non-meditators on cortical thickness, and suggested those changes could be responsible for improvements in memory, perception, and attention (Marks, 2008). For example, moderately experienced meditators have been shown to demonstrate better concentration on functional magnetic resonance imaging (Brefczynski-Lewis, Lutz, Schaefer, Levinson, & Davidson, 2007). They show higher levels of activation in brain regions responsible for attention control (Brefczynski-Lewis et al., 2007). The most experienced meditators appeared to be able to maintain high levels of concentration with less effort (Brefczynski-Lewis et al., 2007). Meta-analyses of research have shown meditation positively influences both brain structure (Fox et al., 2014) and brain function (Fox et al., 2016). It alters brain structure by increasing areas responsible for awareness, memory consolidation, and emotional regulation (Fox et al., 2014). Some scholars have argued that inferences about causality drawn from fMRI studies are problematic (Ramsey et al., 2010). Nevertheless, a series of context-focused approaches have demonstrated significant associations with mediators of athletic performance, as described in the next section.

Context-Focused Approaches in Sport

As discussed earlier, context-focused approaches are a family of approaches connected by a similar mechanism of action: a changed *relationship* with internal thoughts and feelings, rather than a desire to *change or remove* those thoughts and feelings. The most frequently used context-focused approaches are mindfulness, acceptance-based approaches, and self-compassion, but other approaches with similar mechanisms have also been studied.

Mindfulness. Of the context focused approaches, mindfulness has been one of the most widely studied within the sporting domain (Sappington & Longshore, 2015). The construct has been criticised for the heterogeneous definitions found in the literature (Van Dam et al., 2018). Seminal texts describe it as intentional awareness of the present moment with open acceptance (Kabat-Zinn, 1994). This can include a state cultivated through practice, or a trait that endures over time (Goleman & Davidson, 2017). Debates regarding the definition generally concern whether phenomena like decentring—the process of seeing thoughts as internal events rather than literal truths (Feldman et al., 2010)—are the fuel or the exhaust of mindfulness; some have argued the phenomena are essential components of how mindfulness works but others say they are just frequently experienced side-effects (Van Dam et al., 2018). Mindfulness training also varies regarding the features of the practice, such as the focus of attention and the intensity of that focus (Van Dam et al., 2018). In this thesis, I will refer to mindfulness in the general sense as the state or traits of non-judgmental awareness of the present moment, and identify specific aspects of mindfulness where necessary.

Correlation between mindfulness and desirable traits. The tendency to be mindful is associated with a series of benefits for athletes. For example, mindfulness is associated with experiences of flow. Flow is a cluster of experiences associated with peak performance including a sense of control, a loss of self-consciousness, concentration on the task, and a merging of action and awareness (Csikzentmihalyi, 1990). Jackson and Eklund (2002) constructed measures of both state and trait flow in athletes, both of which have been proposed to relate to mindfulness due to the shared emphasis on contact with the present moment (Kee & Wang, 2008). Significant correlations between mindfulness and flow have been observed in elite athletes (Cathcart, McGregor, & Groundwater, 2014; Thienot et al., 2014), and rowers of various abilities (Pineau, Glass, Kaufman, & Bernal, 2014). The relationship between mindfulness and flow appears to be stronger for individual sports compared with team ones, and stronger for race sports than non-race sports (Cathcart et al., 2014). Mindfulness was also associated with lower levels of disrupted concentration (Thienot et al., 2014), consistent with the impact mechanisms suggested by Birrer and colleagues (2012).

In the study of rowers by Pineau and colleagues (2014), the association between mindfulness and flow was entirely mediated by sport self-confidence. Mindfulness and self-confidence are not regularly linked from a theoretical perspective. Most authors suggest mindfulness helps athletes perform *despite* low levels of confidence (Birrer et al., 2012). Nevertheless, there have been a number of empirical links between mindfulness, self-confidence, and performance. A longitudinal study of experienced soccer players found the relationship between mindfulness and performance was mediated by self-efficacy (Blecharz et al., 2014). Mindfulness has been found to be associated with higher self-esteem amongst student athletes (Denny & Steiner, 2009), female equestrian athletes (Diaz, 2010), and rowers of various abilities (Pineau et al., 2014). Due to the positive relationship between confidence and performance (Woodman & Hardy, 2003), it is plausible that these associations with confidence may lead to indirect effects on performance.

Direct correlations with performance are less prevalent. In one study of elite basketball players, higher mindfulness was associated with a smaller difference between performance in practice versus competition (Gooding & Gardner, 2009). Mindfulness also predicted coach ratings of performance and whether an athlete was selected for the A or B team in adolescent girls playing lacrosse (Sarnell, 2012). Most other observational research on mindfulness in athletes does not focus on performance, but on variables that are argued to lead to improved performance. Higher levels of mindfulness were associated with lower ego-goal orientation (Diaz, 2010), higher task orientation (McCarthy, 2011), less worry (Thienot et al., 2014), higher positive affect and lower levels of negative affect (Steinberg, 2012). It was associated with an internal locus of control, higher happiness, more self-restraint and more satisfaction with life (Denny & Steiner, 2009). Exploring similar hypotheses, Kee and Wang (2008) conducted a cluster analysis amongst Singaporean university athletes, and found that athletes in the high mindfulness cluster showed more goal setting than all other groups, used more self-talk than the lower mindfulness group, and more imagery than the average mindfulness group. Overall, dispositional mindfulness has been found to be associated with a series of desirable qualities in athletes but the direction of the causality is unclear. It is possible that athletes with higher skill have more automatic execution of performance, so have fewer doubts or worries that take them out of the present moment. Similarly, some effects may be due to biases like common-method variance (Podsakoff, MacKenzie, Lee, & Podsakoff, 2003).

Case studies and non-randomised designs. It is easier to infer causal associations from intervention research than correlational designs (Kunz, Vist, & Oxman, 2007). A substantial proportion of the intervention research into mindfulness in sport has used small-sample designs, none of which meet the guidelines for a carefully controlled single case experiment (Chambless and Hollon, 1998). Nevertheless, Hrycaiko and Martin (1996) advocated the use of single-subject designs when looking at interventions in sport settings due to the increased amount of information a researcher is able to draw about the progression of a treatment over time. They also identified the difficulty in finding a large number of competitive athletes with a similar problem who are willing to volunteer for randomised designs.

Mindfulness has been used to target burnout, where athletes lose motivation and feel physically and psychologically exhausted (Jouper & Gustafsson, 2013). Jouper and Gustafsson used a series of three face-to-face sessions with weekly phone consultations to help an athlete learn mindfulness and Qigong over a period of 20 weeks. Qigong, like mindfulness, teaches body awareness, concentration on internal processes and can induce feelings of relaxation. The athlete said they felt recovered after 8 weeks and continued to use the technique due to improvements in self-awareness, concentration, present moment awareness, and the ability to stop rumination (Jouper & Gustafsson, 2013). The athlete displayed good engagement in the self-directed activities and the positive effects were maintained at 50 weeks follow-up (Jouper & Gustafsson, 2013). Similar results have been found in golfers and archers, where 12 hours of mindfulness training led to pre-post increases in flow, reductions in worry, task-irrelevant thoughts, sport anxiety and trait anxiety (Kaufman, Glass, & Arnkoff, 2009).

One advantage of case studies is that they often allow for a qualitative analyses of the athlete's experience. These qualitative components allow for researchers to tap into barriers and facilitators to using mindfulness and acceptance (Baltzell, Caraballo, Chipman, & Hayden, 2014). For example, qualitative data collected on basketball players showed athletes appreciate experiential activities and believe more of these activities would have promoted higher engagement in the mindfulness program (Goodman, Kashdan, Mallard, & Schumann, 2014). Baltzell and colleagues (2014) interviewed seven Division I college soccer players about their experiences of a 12-session mindfulness program. All of those who volunteered said they found it helpful in some way, that it helped change their relationships with their internal processes, but that it was difficult to connect to sport in the early stages. Other themes that emerged were the struggle to stay committed to practice, particularly at home without someone to facilitate. Many suggested the mindfulness practice improved their performance on the pitch, helped foster team cohesion, and generalised to off-field challenges (Baltzell et al., 2014).

Similar experiences have been found in equestrian athletes, who reported an improvement in performance, contact with the present, frequency of flow experiences, and reduced levels of distress inside and outside of competitions (Wicks, 2013). Bernier, Thienot, Pelosse, and Fournier (2014) compared the experiences of two young, elite figure skaters who completed an 11-month mindfulness based program: one who found the experience helpful, and another who struggled to engage. The figure-skater who showed good understanding of the principles was disciplined with her mindfulness practice throughout the study, adapted metaphors in the program to her own experience, and showed an improvement in performance. On the other hand, the skater who struggled to engage showed misunderstandings about the purpose of mindfulness, found it boring to practice, and appeared to have trouble linking it to skating.

While these studies help researchers and practitioners identify some barriers and facilitators to mindfulness interventions, they are not a rigorous way for researchers to establish the empirical support for an intervention (Chambless & Hollon, 1998). To make those conclusions, we need to look at designs that compare athletes receiving these interventions to those who do not. The Cochrane Review Handbook for Systematic Reviews of Interventions recommends examining non-randomised controlled trials where those trials may be used as justification to conduct a more internally valid trial (Reeves, Deeks, Higgins, & Wells, 2008). Many non-randomised trials show greater improvements in performance for groups exposed to context focused interventions compared with controls (Bernier, Thienot, Codron, & Fournier, 2009). They have also demonstrated significant improvements in mindfulness and anxiety (Goodman et al., 2014), anxiety, worry, task irrelevant thoughts, and sport specific anxiety (De Petrillo, Kaufman, Glass, & Arnkoff, 2009). Thus, more rigorous evaluation through randomised trials appears warranted.

Randomised controlled trials of mindfulness in sport. Randomised controlled trials are generally recommended as the primary sources of information regarding the efficacy of interventions (Higgins, 2008). There have been a number of randomised controlled trials on mindfulness in sport, and most generally compared the training with no-treatment control conditions (Aherne, Moran, & Lonsdale, 2011; John, Kumar, & Lal, 2012; Ojaghi, Gholizade, & Mirheidari, 2013). These interventions were heterogeneous, varying in content and dose from a self-help mindfulness tape with instructions (Aherne et al., 2011) to a ten-hour group training program (John et al., 2012). The evidence from randomised trials was equivocal. Compared to the no-contact control conditions, context focused interventions have shown significant beneficial effects for flow (Aherne et al., 2011), trait mindfulness (Aherne et al., 2011; Ojaghi et al., 2013), competitive anxiety (Ojaghi et al., 2013), performance over a series of competitions (Ojaghi et al., 2013), and performance on a standardised sporting assessment (John et al., 2012). However, each of these studies contains significant limitations: Aherne and colleagues assessed only 13 participants; Ojaghi and colleagues presented no description of their intervention; and John and colleagues found mindfulness was less effective than a condition where athletes merely listened to music.

Overall, correlational evidence and non-randomised designs generally have supported the effectiveness of mindfulness for a range of outcomes. Randomised trials show some benefits of the intervention, but few have shown benefits above control interventions.

Acceptance and commitment approaches. Some authors have argued that mindfulness approaches are best augmented with a series of complementary, context-focused interventions like explicit training in acceptance, values identification, or goal setting (Gardner & Moore, 2007; Hayes et al., 2011). Both Acceptance and Commitment Therapy and the Mindfulness-Acceptance-Commitment approach integrate these skills with mindfulness approaches. Both focus on the benefits of acceptance for managing difficult thoughts and feelings, and both have been studied in sporting domains.

Two case studies have looked at the implementation of the seven-week Mindfulness-Acceptance-Commitment protocol described by Gardner and Moore (2007). Lutkenhouse (2007) helped a lacrosse player improve her self-reported scores for motivation, fitness, endurance, quickness, and overall performance. Schwanhausser (2009) found the protocol helped a young springboard diver reduce anxiety and improve his mindfulness, emotional acceptance, flow, and performance on both one and three-meter dives. One case series using Acceptance and Commitment Therapy with four injured athletes found no systematic results of the intervention on mindfulness, acceptance, and sport-injury anxiety (Mahoney & Hanrahan, 2011). The authors attributed this result to the educational nature of their intervention, acknowledging that Acceptance and Commitment Therapy interventions are significantly more effective when using metaphors or experiential exercises (Levin et al., 2012).

Non-randomised trials of these acceptance-based approaches provide evidence for hypothesised increases in acceptance and mindfulness (Hasker, 2011). These approaches also appear to lead to reductions in the distress caused by negative thoughts (Ruiz & Luciano, 2012). The cognitive benefits appear to translate to improvements in performance (Bernier et al., 2009; Ruiz & Luciano, 2012) and coach ratings of performance (Wolanin & Schwanhausser, 2010). There has also been one randomised controlled trial using the Mindfulness-Acceptance-Commitment approach, which demonstrated improvements in performance, mindfulness, and flow for beginner dart throwers (Zhang, Si, et al., 2016). Few studies with high internal validity have been conducted on experienced athletes; nevertheless, combining mindfulness with other acceptance-based strategies (e.g., value clarification, committed action, acceptance, and defusion from Acceptance and Commitment Therapy; Hayes et al., 2011) may be beneficial for promoting a series of positive athletic outcomes.

Other context-focused approaches. There has been a series of other context-focused approaches that have demonstrated a wide range of positive outcomes for athletes. For example, self-compassion is the process of directing care and compassion toward oneself during difficult times (Neff, 2003). It generally focuses on three core strategies: mindfulness, treating oneself kindly, and acknowledgment of the connection between oneself and the rest of humanity (Mosewich et al., 2013). In female athletes, self-compassion was associated with lower levels of many harmful psychological variables, such as body shame, fear of failure, shame proneness, and fear of negative evaluation, even when controlling for self-esteem (Mosewich, Kowalski, Sabiston, Sedgwick, & Tracy, 2011). It was associated with significantly higher self-esteem amongst women from different athletic backgrounds (Mosewich et al., 2011). Following an intervention designed to increase self-compassion, athletes showed significantly lower rumination, self-criticism, and concern over mistakes (Mosewich et al., 2013).

Other approaches that help athletes with negative thoughts and feeling through contact with the present moment include different types of meditation. For example, Muangnapoe (1998) compared Anapanasati Meditation to a classic content-focused technique, Progressive Muscle Relaxation. Anapanasati Meditation is like mindfulness with exclusive focus on the breath. Both Anapanasati Meditation and Progressive Muscle Relaxation were better than stretching at increasing confidence, but the meditation strategy led to less anxiety than the relaxation approach. Solberg and colleagues (1996) found some evidence for Acem meditation improving the performance of experience shooters. Acem meditation following a similar process of returning attention to a stimulus in the present; however, the reported goal of the intervention is to help athletes regulate arousal (Solberg et al., 1996, 2000). Meditative techniques appear to have some positive effects for athletes.

These meditative approaches are rarely compared, and most appear to share a similar mechanism of action. Context-focused meditations—like Anapanasati Meditation (Muangnapoe, 1998), Transcendental Meditation (Hall & Hardy, 1991), and Mindfulness Meditation (Aherne et al., 2011; John et al., 2012)—describe present moment awareness as the key focus. They share many core components, such as the observation of internal processes (Baer et al., 2006). Nevertheless, some authors have attempted to conduct explicit comparisons between similar meditative techniques. Buscombe et al. (2014) asked athletes to compare the experiences of three different meditative techniques: breath counting;

Transcendental Meditation where the focus repeatedly returns to a mantra; and Zazen Meditation where focus is brought back to the breath when the mind wanders. The experiences of the different techniques show considerable overlap. All were useful for reducing anxiety and inducing a sense of calm. Most athletes reported a preference for breath counting; however, preferences do not necessarily translate into positive athletic outcomes.

Validity of Studies on Context-Focused Approaches

To determine positive outcomes from context-focused approaches, both the internal and external validity of the studies are important to consider. With higher internal validity, researchers can make stronger causal claims about the effects being observed (Higgins & Altman, 2008). With higher external validity, researchers are more certain that effects are likely to generalise to real world athletes (Schünemann et al., 2008). In this section, I will discuss a number of important validity considerations that are central to the rest of the thesis.

Exploring causal models. As mentioned earlier, context-focused approaches are heterogeneous interventions that operate via a similar hypothesized mechanism of action. This interpretation, however, assumes that the causal model underlying these approaches is accurate. Mindfulness has been known to reduce anxiety, but most theories of these interventions suggest it does so as a consequence of psychological flexibility (Hayes et al., 2011). If mindfulness did not change context-focused variables like awareness, acceptance, or refocusing, but instead worked by reducing anxiety, then there may be a problem with the causal model. In this case, it might not be operating as a *context*-focused intervention because its primary function would be to change the *content* of internal experience. Testing the causal models, and ruling out alternative explanations, is an important process in establishing causality (Pearl, 2009).

There is evidence that some *content*-focused interventions may work by *context* focused mechanisms (Arch & Craske, 2008). Cognitive-behavioural interventions teach athletes to reduce their anxiety and change negative thoughts. These have the intention of reducing the form and frequency of these internal processes. Arch and Craske argued that these interventions may instead work by reducing the athlete's fear of anxiety or the believability of negative self-talk, which is a context-focused change in relationships. They base these conclusions on a small number of mediational analyses but reiterate the call from others (e.g., Kazdin & Nock, 2003) for an increase in the number of studies that look at mediators which explore the causal mechanisms of change in psychological interventions.

Similar interventions are often tested using different names, which diverts research away from interpretable conclusions (Rosen & Davison, 2003). This problem is prevalent in the context-focused literature in sport, with an A-to-Z of approaches: Acceptance and Commitment Therapy, Anapanasati Meditation, breath counting, Mindfulness-Acceptance-Commitment, mindfulness meditation, self-compassion, Transcendental Meditation, and Zazen Meditation, amongst others. Causal inferences are easier if researchers focus less on treatment packages and branded interventions, and focus more on causal models and empirically supported principles of change (Ciarrochi, Atkins, Hayes, Sahdra, & Parker, 2016; Rosen & Davison, 2003). For this reason, my thesis tends toward integrating literature from a variety of context-focused approaches, rather than focusing on one treatment exclusively. In this thesis, the systematic review and randomised trial aim to explore the causal models of context-focused interventions, and the questionnaire paper aims to help other researchers do the same.

Study design. Another internal validity factor that will be the focus of this thesis is study design. The studies on context-focused approaches in elite performance sometimes use designs with a variety of confounds. For example, Ruiz and Luciano (2012) made post-hoc selections of players for the control group from an online database of player results. Coincidentally perhaps, the rankings of players in the control group got worse over the period of the study, accentuating the difference between groups.

Randomisation addresses many of these concerns (Kunz et al., 2007); however, the risk of bias from the aforementioned randomised trials raises doubts about the internal validity of those studies too. Using the Cochrane Risk of Bias tool (Higgins & Altman, 2008; Higgins et al., 2011), none of the studies identified so far would be judged to represent a low risk of bias. For example, the first author delivered the intervention and assessed the outcome measures in a series of studies (Mosewich et al., 2013; Papanikolaou, 2011). Because of expectancy effects and biased measurement, studies can be judged to have low risk of bias if key personnel are blinded. If participants and the people conducting the intervention are aware of hypotheses, they may respond differently to the intervention based purely on expectations (Higgins & Altman, 2008). The outcome measure can be unbiased if collected by a blinded experimenter, or it can be an objective measure that is unlikely to be biased (e.g., standardised shooting tests; Hall & Hardy, 1991; Solberg et al., 1996). Recent reviews of mindfulness and acceptance (e.g., Birrer et al., 2012; Gardner &

Moore, 2012) have not assessed the quality of the literature using an established tool like the Cochrane Risk of Bias tool. The systematic review in this thesis assessed the quality of the studies using such a tool, and the randomised trial meets the criteria for low risk of bias.

External validity. The tension between reducing bias and maintaining external validity is apparent throughout science. Weinberg (2008) argues that there has been a tendency to choose either internally valid laboratory experiments *or* externally valid case designs in sport psychology. Elite athletes are, by definition, a very small segment of the population, so may be hard to recruit. Their livelihoods can depend on miniscule changes in performance, so they may be wary of being allocated to an ineffective control condition. However, at the other end of the spectrum, principles established in laboratory experiments often do not generalise to more naturalistic environments (Wulf & Shea, 2002). Some interventions have the opposite effect when studied in the lab and when studied with experienced athletes (Wulf & Shea, 2002). In this thesis, the systematic review assessed the external validity of context-focused interventions, and the questionnaire paper offered strategies for maintaining internal validity in externally valid contexts.

Research Objectives

There are a number of gaps in the evidence to date that this I sought to address with this research. Two reviews of context focused approaches in sport have exclusively included confirmatory evidence (Birrer et al., 2012; Gardner & Moore, 2012). A more recent review was systematic, but narrowly explored mindfulness meditation instead of the wider context-focused literature (Sappington & Longshore, 2015). The first aim of this thesis was to obtain a more comprehensive assessment of the current evidence base for context focused approaches in sport settings. Study 1 addressed this aim with a systematic review.

Next, given the preponderance of studies with a high risk of bias, the second aim of this thesis was to test the causal model of context-focused approaches in sport using a study with high internal validity. Brief, randomised trials have been used to test the causal model models behind context focused interventions outside of sport (Levin et al., 2012). Study 2 was a randomised controlled trial that met the Cochrane criteria for low risk of bias. In doing so, I tested the assumption that a brief mindfulness intervention would improve performance, particularly when compared with an inert control condition. I also measured mindfulness, anxiety, and biomechanics to test the hypothesised mediating influence of these variables.

Finally, compared with general measures of psychological principles, sport-specific questionnaires tend to better access the relationships between variables of interest in athletes (Moritz et al., 2000). With that in mind, the only validated measure of mindfulness in sport is 15 items long (Thienot et al., 2014). The burden from questionnaires like these can be a barrier that prevents researchers from assessing mediating relationships in intervention research. Recently, advanced methodologies have enabled efficient methods of shortening questionnaires to reduce questionnaire burden (Eisenbarth, Lilienfeld, & Yarkoni, 2015; Sandy, Gosling, & Koelkebeck, 2014). The third aim of this project was to develop a more concise version of the Mindfulness Inventory for Sport. I shortened the measure using genetic algorithms, then tested it for reliability and validity in two samples.

Chapter 2—Mindfulness and Acceptance Approaches to Sporting Performance Enhancement: A Systematic Review

Abstract

Background. Mindfulness and experiential acceptance approaches have been suggested as a method of promoting athletic performance by optimally managing the interplay among attention, cognition, and emotion. Our aim was to systematically review the evidence for these approaches in the sporting domain.

Methods. Studies of any design exploring mindfulness and acceptance in athletic populations were eligible for inclusion. We completed searches of PsycINFO, Scopus, MEDLINE, and SPORTDiscus in May, 2016. Two authors independently assessed risk of bias using the Cochrane Risk of Bias tool, and we synthesised the evidence using the GRADE criteria.

Results. Sixty-six studies (n = 3,908) met inclusion criteria. None of the included studies were rated as having a low risk of bias. Compared to no treatment in randomised trials, large effect sizes were found for improving mindfulness, flow, performance, and lower competitive anxiety. Evidence was graded to be low quality, meaning further research is very likely to have an important impact on confidence in these effects.

Conclusions. A number of studies found positive effects for mindfulness and acceptance interventions; however, with limited internal validity across studies, it is difficult to make strong causal claims about the benefits these strategies offer for athletes.

Introduction

Optimizing performance is considered one of most important goals in the field of sport and exercise psychology (American Psychological Association Division 47, 2016). Strategies to improve performance are typically directed toward either controlling the content of internal experiences or managing attention (Birrer et al., 2012). Meta-analyses have consistently established that optimal performance is associated with internal experiences like mood (Beedie et al., 2000), self-confidence (Craft et al., 2003; Moritz et al., 2000; Woodman & Hardy, 2003) and anxiety (Jokela & Hanin, 1999). Content-focused interventions teach strategies that seek to directly alter the form or frequency of inner experience. For example, athletes may use progressive muscle relaxation to reduce what is seen as problematic anxiety (Greenspan & Feltz, 1989), or positive self-talk to improve their confidence (e.g., "I can do it", Hatzigeorgiadis et al., 2011). To our knowledge, only one meta-analysis has been conducted on such content-focused interventions for performance, in which Hatzigeorgiadis and colleagues (2011) found a small-moderate pooled effect size for motivational self-talk, designed to influence arousal, confidence or mood ($d = .37$, 95% CI [.25, .49]). For other content-focused approaches like imagery and relaxation, studies have shown improvements in confidence and emotional control (Birrer & Morgan, 2010; Kudlackova et al., 2013; Mellalieu et al., 2009; Vealey, 1994); few such studies have demonstrated significant effects on performance (e.g., $d = .24$, n.s.; Short et al., 2002).

Where these interventions attempt to deliberately change the content of thoughts and feelings, other approaches shift attention to the important components of skill execution. Meta-analyses on these interventions appear to have demonstrated stronger pooled effect sizes on performance. Where Hatzigeorgiadis and colleagues (2011) found small-moderate effect sizes of motivational self-talk, they found strong effects for instructional self-talk ("cues aiming at focusing or directing attention", p. 349) for fine motor skills (e.g., basketball free-throws, golf putting; $d = .83$, 95% CI [.64, 1.02]). Goal setting, which is argued to "direct attention and effort toward goal-relevant activities and away from goal irrelevant activities" (Locke & Latham, 2002, p. 706), has shown promise in sport and exercise settings. A meta-analysis of 36 goal-setting interventions found moderately difficult goals were associated with the largest improvements in performance ($ES = .53$, 95% CI [.45, .61]; Kyllo & Landers, 1995). Finally, Driskell and colleagues (1994) completed a meta-analysis on mental practice, which involves the cognitive rehearsal of a skill prior to physical execution. When looking at the skill execution that involved muscular strength, endurance or coordination, they found a strong, significant effect size ($d = .78$). All three interventions appear more focused on shifting attention to useful cues, rather than controlling emotional states; however, the exact mechanism of action for these interventions is still debated (Locke & Latham, 2002; Wakefield, Smith, Moran, & Holmes, 2013). While these meta-analyses paid limited attention to the methodological rigor of the included randomised trials, the large effect sizes provide some support for the use of these interventions in athletic populations.

More recently, another class of interventions has been reported to also help athletes sustain task-focused attention, in this case by training open, non-reactive, present-moment awareness (Birrer et al., 2012). Mindfulness and acceptance interventions aim "to promote a modified *relationship* with internal experiences (i.e., cognitions, emotions, and physiological sensations), rather than seeking to change their form or frequency" (Gardner & Moore, 2012, p. 309). They often emphasize the acceptance of internal processes as a typical part of the athletic experience, and focus on the present moment regardless of those internal processes (Baltzell et al., 2014; Birrer et al., 2012; Gardner & Moore, 2007, 2012b; Mosewich et al., 2013). These interventions have largely drawn from psychotherapeutic approaches like mindfulness meditation (Kabat-Zinn et al., 1992), Acceptance and Commitment Therapy (ACT; Hayes et al., 1999), and self-compassion interventions (Gilbert, 2009; Neff, 2003). Meta-analyses in the clinical domain have found these approaches to have a positive effect for various psychological conditions (Brown, Glendenning, Hoon, & John, 2016; Khoury et al., 2013; Ost, 2014). More generally, meditative approaches have been shown to reduce anxiety, stress, and neurobiological markers such as cortisol, epinephrine and norepinephrine (Chen et al., 2012; Chiesa & Serretti, 2010).

In the sporting domain, authors have argued that focusing on the present moment with acceptance facilitates the automatic execution of performance (Gardner & Moore, 2006, 2007, 2012). Birrer and colleagues (2012) suggested that athletes perform at their peak when executing skills with automaticity, and with open awareness to the context so they can make goal-directed adjustments. To use the case of a golfer, she performs best when open to environmental stimuli such as the wind, the lie of the ball, and the target, but executing her swing without conscious control. Theoretically, mindfulness and acceptance promote these characteristics because they reduce ironic rebound effects (Wegner, 1994) and reinvestment (Baumeister, 1984).

Ironic rebound effects refer to the process by which the desire to suppress thoughts and feelings lead to an increase in their presence and the attention paid to them (Wegner, 1994). Efforts to suppress cognitions, emotions, pain and fatigue have been shown to lead to *increases* in the disruption caused by those processes (Wegner, 1994). Coming back to our golfer, a randomised crossover study found that telling her to “not putt short” sometimes leads to increased gaze in front of the hole, which in turn led to shorter putts (Binsch et al., 2009). Mindfulness and acceptance approaches theoretically overcome ironic processes by fostering acceptance rather than suppression of the thought or feeling, allowing attention to be directed to more useful cues (Birrer et al., 2012).

Reinvestment is another process by which performance decrements can be accounted for by unhelpful shifts in attention (Masters & Maxwell, 2008). Reinvestment Theory proposes that athletes perform less well under pressure when they direct conscious attention to the execution of the skill, rather than allowing the skill to be executed automatically (Baumeister, 1984; Beilock, Carr, MacMahon, & Starkes, 2002; Masters & Maxwell, 2008). Again, performance decrements could be induced in our golfer by asking her to dedicate attention to the steps required to make her putt (e.g., using cues ‘arms, weight, head’) rather than the characteristic of the putt as a whole (e.g., 'smooth'; Gucciardi & Dimmock, 2008). Mindfulness and acceptance approaches are proposed as an antidote to this process by noticing unhelpful shifts in attention to thoughts, feelings, or attentional foci, and instead redirecting attention to more useful, task-relevant cues (Birrer et al., 2012).

One systematic review has explored the effectiveness of mindfulness approaches in the sport and exercise domain (Sappington & Longshore, 2015). The review found preliminary support for the effectiveness of mindfulness interventions, but highlighted the need for interventions with greater internal validity. The quality assessment tool used by this review was not sensitive or specific for detecting biases in study quality, according to the Cochrane Review Handbook for Systematic Reviews of Interventions (Higgins & Altman, 2008). The review only included studies that explored mindfulness in isolation, and excluded the broader range of acceptance-based approaches (e.g., self-compassion; Mosewich et al., 2011) that may facilitate performance via similar mechanisms of action (Birrer et al., 2012). As mentioned earlier, interventions under the mindfulness and acceptance umbrella operate by increasing contact with the present moment while accepting internal thoughts and feelings; however, interventions differ on the degree to which they focus on acceptance versus present moment awareness, and the processes have been shown to differentially influence outcomes (Levin et al., 2012). In addition, some mindfulness and acceptance interventions also focus on commitment to value-driven action (Moore, 2009) where others forgo this process entirely (e.g., Kaufman et al., 2009). Similarly, there is discord regarding the measurement of mindfulness, such as whether it is unidimensional or multidimensional, and if multidimensional, which dimensions are important (Chiesa, 2012). While it is important to avoid grouping these interventions and outcomes as equivalent, reviews with broader eligibility criteria can assess the generalisability of findings for interventions that operate via similar mechanisms, and they provide a more comprehensive summary of the evidence base (O’Connor, Green, &

Higgins, 2008).

Extending the work of Sappington and Longshore (2015), our review aimed to synthesise and critique the research on mindfulness and acceptance approaches in athletic populations. In order to evaluate the quality of the evidence, we chose the Cochrane Risk of Bias tool (Higgins & Altman, 2008) and the GRADE method of interpreting results (Schünemann et al., 2008). We included studies on athletes using any design to allow for a comprehensive review of the available research. Our primary outcome of interest was athletic performance; evidence regarding proposed mediators of performance (e.g., competitive anxiety) was also collected to explore the other benefits that these interventions may afford athletes.

Method

Eligibility Criteria

The studies included in this review sampled participants competing in a sport, classified by SportsAccord (2015) as an activity that includes an element of competition, does not rely on luck, does not put animals or competitors at undue risk, and does not rely on proprietary equipment. We used a broad approach when selecting interventions because mindfulness and acceptance variables are conceptualised under a variety of titles. Studies needed to include mindfulness or acceptance as an independent variable, as defined above: one which aims "to promote a modified *relationship* with internal experiences (i.e., cognitions, emotions, and physiological sensations)" (Gardner & Moore, 2012, p. 309). This definition includes concepts like self-compassion (Neff, 2003), the processes described in ACT (e.g., cognitive fusion/defusion, experiential avoidance/acceptance;

Hayes et al., 1999), mindfulness, and various forms of meditation (e.g., transcendental meditation).

Rather than restrict the search to randomised controlled trials (RCTs), we included all study designs because other designs, such as non-randomised controlled trials and before-after designs, are recommend in systematic reviews when it would be beneficial to explore unexpected benefits, harms, and qualitative information that RCTs often neglect (Reeves et al., 2008). We included both published and unpublished studies to reduce the influence of publication bias. For logistical reasons, the search was restricted to studies that were written in English. We included studies if they were published or completed (but unpublished) at any time before the date of the search.

Information Sources

A search of titles, abstracts, and keywords was conducted on 9 May 2016 for the following four databases: PsycINFO (database coverage: 16th century-present), Scopus (1970-present), MEDLINE (1946-present), and SPORTDiscus (1930-present). These databases were chosen due to their comprehensive date coverage and their use in related meta-analyses (Hatzigeorgiadis et al., 2011; Levin et al., 2012; Manzoni et al., 2008). Reference lists were searched for any additional studies that would be eligible for inclusion. Additionally, authors of each included study were asked for any published or unpublished works on the topic. Finally, posts were placed on three list-serves (APA Div. 47, SPORTPSY, Association for Contextual Behaviour Science) to request any additional published or unpublished research.

Search Strategy

The review team formulated search terms using the titles, abstracts, and keywords of existing meta-analyses (Hatzigeorgiadis et al., 2011; Kyllo & Landers, 1995; Levin et al., 2012), reviews (Birrer et al., 2012; Gardner & Moore, 2012; Sappington & Longshore, 2015), and empirical articles (e.g., Aherne et al., 2011; Mosewich et al., 2013; Ruiz & Luciano, 2012). Additionally, MEDLINE's Medical Subject Headings (MeSH) were used to identify synonyms for the included search terms.

Using the criteria above, two groups of keywords were developed to identify relevant populations and interventions, respectively: a) Athlet* OR Sport* OR Players OR Exercise OR Performance OR "Physical activity" OR "Physical education" AND b) Mindful* OR Meditation OR "Present moment" OR "Acceptance-based" OR "MAC approach" OR "Contemplative science" OR "Acceptance and Commitment Therapy" OR "Psychological flexibility" OR "Experiential acceptance" OR "Experiential avoidance" OR "Cognitive fusion" OR Defusion

Study Selection

Results of the search were imported into Endnote (X7; Thomson Reuters, 2015) where duplicates were removed. Titles and abstracts were screened by two independent reviewers, and where discrepancies existed, the paper was included for full-text screening. Where full-texts were not available, we requested the paper from the author via email. Two authors independently screened all full-text articles (Brooke Van Zanden and I). Discrepancies were resolved through discussion with a third author (Chris Lonsdale) consulted in cases where agreement could not be made.

Data Collection Process

After initial piloting of data-extraction forms, the first author extracted the data from each study and sent the extracted data to the primary author of that study for confirmation. As per the Cochrane Handbook, these authors were also asked open-ended questions about their methodology where the risk of bias was unclear (Higgins & Altman, 2008). Of the 58 authors for whom email addresses could be identified, 26 responded, and three reported minor inaccuracies which were corrected by the first author. Another author also checked the data extraction.

Data Items

We extracted the age, gender, sport, and sporting experience of the athletes in each study. Where an intervention was conducted, we extracted the study design, intervention content, intervention dose, and details about comparison group, as recommended in Higgins and Deeks (2008). We extracted effect sizes with confidence intervals (CIs) when reported on primary outcomes, because they allow for more useful comparisons across studies (Thompson, 2002), and significance test where CIs were not available. To allow for more parsimonious conclusions, we extracted only composite scale results (e.g., dispositional mindfulness) rather than each subscale within measures (e.g., the Five Facet Mindfulness Questionnaire contains five subscales). Where two measures of a construct were reported (e.g., two measures of dispositional mindfulness), we calculated a mean of the two effect sizes for parsimony.

Performance data was extracted separately for measures of competitive performance (e.g., match performance, season-long scores) and measures of skill execution involving a contrived assessment (e.g., standardised free-throw shooting, non-competitive darts accuracy). As per existing meta-analyses in sport psychology (Hatzigeorgiadis et al., 2011), we coded the skills on two dimensions: we rated the skill as either novel or well-learned based on the descriptions of the participants and the task; and we rated the skill as either fine (i.e., those requiring precision, accuracy, and dexterity such as shooting or darts) or gross (i.e., those requiring strength, endurance, and power such as cycling or running). For correlational studies, we extracted relationships between mindfulness or acceptance focused variables and any other full scales. Finally, for qualitative studies, we extracted major themes from the analyses.

Risk of Bias in Individual Studies

We chose the Cochrane Risk of Bias assessment because it has greater validity, sensitivity, and specificity than scales and checklists that measure bias (Higgins & Altman, 2008). While quantitative measures afford the reader a degree of parsimony, the weights placed on different domains are seldom justified, and many such measures confuse issues of validity with other methodological issues (e.g., whether authors report a power analysis, which relates more to precision than validity; Higgins & Altman, 2008).

The Cochrane Risk of Bias assessment is a domain-based evaluation that guides reviewers to evaluate studies on the factors that meta-meta-analyses have shown to bias results (Higgins & Altman, 2008): concealed sequence generation, allocation concealment, blinding of participants and personnel, incomplete outcome data, and selective outcome reporting. Concealed sequence generation and allocation concealment reduce selection bias, where participants are included or excluded to increase the size of the study's effect. Using a properly random sequence (sequence generation) that is concealed from the person allocating participants (allocation concealment) reduces this bias by preventing personnel from foreseeing a participant's allocation. Two authors then independently completed risk of bias judgments for the RCTs, because all non-randomised controlled trials and before-after designs included in this review had inherent biases and potential confounds. Again, disagreements were resolved through discussions between the two authors, and a third author was consulted to resolve disputes. This information was used in the synthesis to weight the findings with lower risk of bias, as per the GRADE method.

Synthesis of Results

Few studies included in this review used similar interventions, comparison groups or outcome measures, so quantitative syntheses of findings via meta-analyses were not likely to be meaningful (Deeks, Higgins, & Altman, 2008). Instead, as recommended in the Cochrane Handbook (Schünemann, Oxman, Higgins, et al., 2008), we created summary tables for each key outcome and compared the body of evidence with the GRADE criteria (Schünemann, Oxman, Vist, et al., 2008).

The GRADE approach allows reviewers to rate a body of evidence on the level of certainty surrounding the conclusions, from high quality (further research is very unlikely to change our confidence in the estimate of effect) to very low (any estimate of effect is very uncertain). These judgments are formed by evaluating the quality of the evidence (e.g., mostly randomised-controlled trials vs. mostly observational studies), then upgrading or downgrading the evidence on the basis of certain criteria (e.g., high risk of bias, imprecise results; Schünemann, Oxman, Vist, et al., 2008). To facilitate this process, standardised mean differences (*d*) were calculated using the conversion formula provided by Wilson (2001) to allow for some comparisons between studies. Calculations were performed by the first author and cross-checked by another author.

If possible, the dose for each study (in hours) was calculated using the information presented in the manuscript, and scatterplots were created to explore possible dose-response gradients. Two authors independently reviewed the tables, scatterplots, and risk of bias judgments, then collaboratively decided on the GRADE criteria for each outcome. Without enough studies of matching participants, interventions and outcomes, it was not possible to assess some of the GRADE criteria; for example, "unexplained heterogeneity in results" requires a series of sufficiently similar studies where differences in participants, interventions, comparisons or outcomes do not explain heterogeneity. Similarly, publication bias is best assessed using a funnel plot (Sterne, Egger, & Moher, 2008), which usually require more studies than were included for each outcome in our review.

Results

Study Selection

After duplicates were removed, 5,198 papers were screened by two authors at the title and abstract level (see Figure 2.1), 129 full-texts were reviewed and 66 met the criteria to be included in the qualitative synthesis. The inter-rater reliability of full-text screening was high ($\varkappa = .84$).

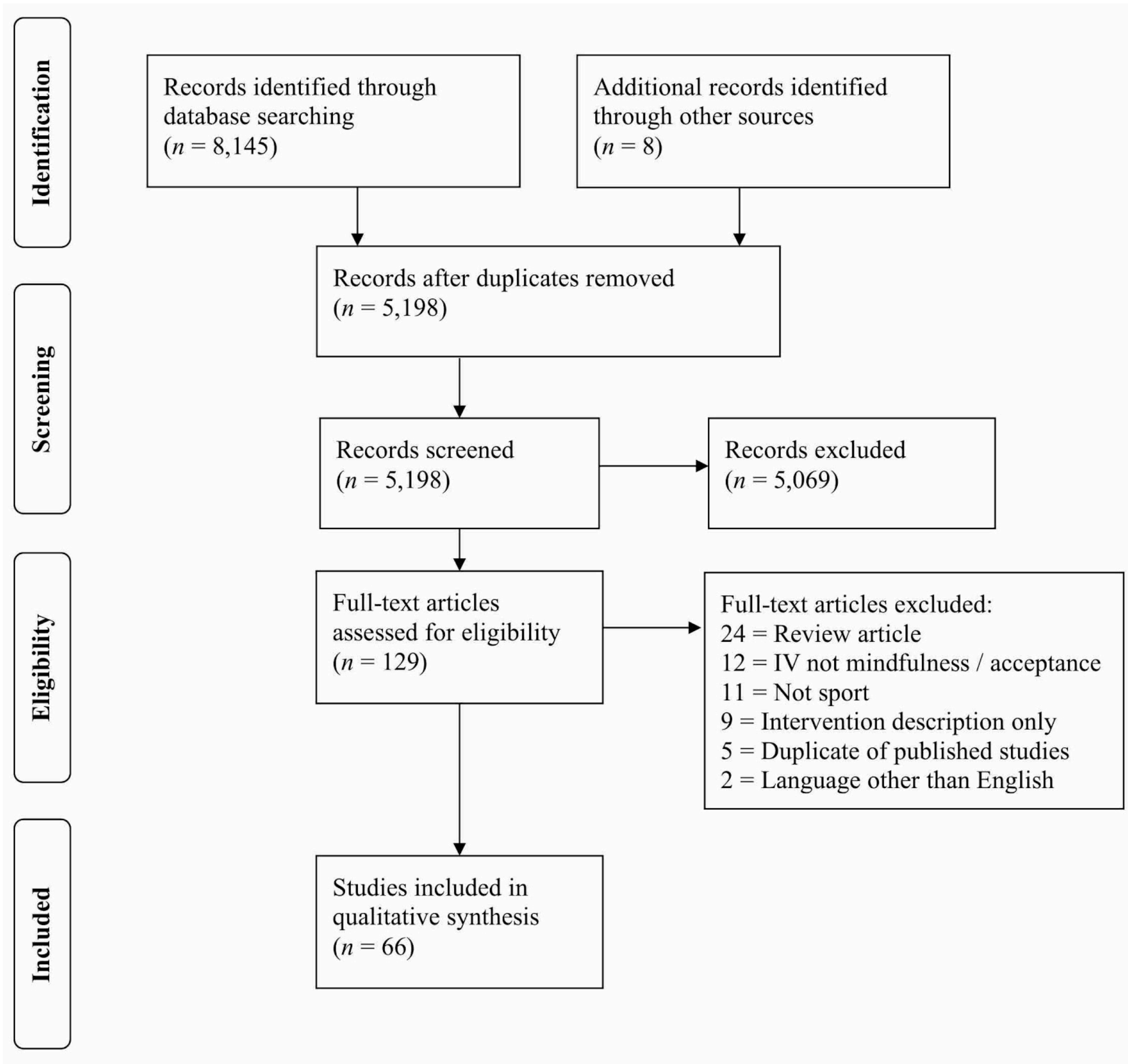

Figure 2.1. Flow diagram of search results

Study Characteristics

The studies included 3,908 athletes from a variety of sports and demographics (M_{age} = 22.89). There was also a range of athletic experience from beginner to elite international athletes, with most studies including athletes competing at university level or higher. Complete study characteristics are provided in Table 2.1. Forty-three studies evaluated an intervention. Of those, 17 were RCTs, 14 included a non-randomised control group, and 12 did not have a control. Finally, 21 studies used observational designs, usually correlational designs including mindfulness or acceptance variable along with a relevant outcome variable (e.g., performance). Effect sizes with CIs on primary outcomes were available for two of the 66 studies (Ivarsson, Johnson, Andersen, Fallby, & Altemyr, 2015; Zhang et al., 2016). Nine others reported CIs but on outcomes that were not included in this review: for example, subscale scores (Shaw, 2015), mediation models (Gustafsson, Davis, Skoog, Kenttä, & Haberl, 2015) or pre-post differences in between-group designs (Goodman, Kashdan, Mallard, & Schumann, 2014).

Table 2.1.

Characteristics of Included Studies

Citation	Participant Descriptions w/ Means (SDs)	Intervention	Comparison	Outcomes
Randomised Controlled Trials – 17				
Aherne et al., 2011	13 (4 female) athletes from various sports aged 21 (1.69) yrs. from Ireland; national or international level	Mindfulness, information sheet and mindfulness CD, postal only mins. contact, 110 mins. practice / wk. prescribed	NT	Dispositional mindfulness (CAMS-R); State flow (FSS-2)
Hall & Hardy, 1991	30 (15 female) beginner pistol shooters aged 18-23 yrs. from USA	TM, group with TM expert, 6 x 100 mins. contact, 280 mins. practice / wk. prescribed	NT, VMBR	Skill Execution (standardised marksmanship test)
Ivarsson et al., 2015	41 (10 female) soccer players aged 16.97 (0.79) yrs. from Sweden; junior elite recruited from one school	MAC, group with first author, 7 x 45 mins. contact, various different activities prescribed	Sport psych presentation	Exploratory outcomes (injuries recorded by physiotherapists)
Jha, 2015	105 American football athletes aged from USA; Div. I college	Mindfulness, group with a trainer, 4 x 45 mins. contact, 84 mins. practice / wk. prescribed	Relaxation & visualisation	Competitive anxiety (STAI); Exploratory outcomes (PSS, Sustained Attention Response Task)
John et al., 2012	165 male shooters aged 29.4 (4.3) yrs. from India; 3-5+ yrs. at national level	Mindfulness, group with certified meditation instructor, 24 x 20 mins. contact	NT, Music therapy	Skill Execution (standardised shooting test)
Moen & Wells, 2016	78 (26 female) athletes from various sports aged 18.5 yrs. from Norway; junior elite recruited from schools	ATT, n, 6 x 120-150 mins. contact, 60 mins. practice / wk. prescribed	NT	Dispositional mindfulness (MAAS); Exploratory outcomes (ABQ)
Moen et al., 2015	77 (38 female) athletes from various sports aged 18.5 (16-20) yrs. from Norway; junior elite recruited from schools	Mindfulness, group with experienced mindfulness coach, 4 x 120 mins. contact, 90-115 mins. practice / wk. prescribed	NT	Dispositional mindfulness (MAAS); Exploratory outcomes (ABQ, PSS, Athlete Satisfaction Questionnaire)
Mosewich et al., 2013	51 female athletes from various sports aged 20.28 (1.75) yrs. from Canada; current varsity athletes	SC, group with first author, 1 x 20 mins. contact, 50 mins. practice / wk. prescribed	Journalling	Exploratory outcomes (SCS, state rumination, state self-criticism, Concern over Mistakes)
Muangnapoe, 1998	48 (24 female) weightlifters aged 18-30	AM, group, no description of personnel, 30 x 30	PMR, Stretching	Competitive anxiety (SCAT-Thai, CSAI-2Thai); Exploratory outcomes

	yrs. from Thailand; elite & sub-elite	mins. contact, informal practice prescribed		(perceived uncertainty and importance of competition)
Ojaghi et al., 2013	40 table tennis athletes from Iran; professional athletes, premier league or first division	Mindfulness, group, no description of personnel, unclear dose	NT	Dispositional mindfulness (MAAS); Competitive anxiety (CSAI-2); Competitive performance (table-tennis match scores)
Papanikolaou, 2011	40 male soccer athletes aged 10.1 (1.1) yrs. from Greece	Various, group with first author, 24 x 30 mins. contact, various different prescribed	Video review	Exploratory outcomes (Test of Attentional and Interpersonal Style)
Quinones-Paredes, 2014	13 female soccer athletes aged 21.5 (19-24) yrs. from USA; 7-20 yrs. experience	Mindfulness, group, no description of personnel, 4 x 45 mins. contact, 135 mins. practice / wk. prescribed	Relaxation	Dispositional mindfulness (MIS, MAAS); Dispositional flow (DFS-2); Exploratory outcomes (WBSI)
Regan et al., 1998	28 runners aged 24.4 (4.8) yrs. from UK	Meditation, audio file, unclear dose, informal practice prescribed	NT	Competitive anxiety (STAI-Y1); Exploratory outcomes (body tension, perceived exertion, incredibly short Profile of Mood States, respiratory output)
Scott-Hamilton et al., 2016	47 (5 female) cyclists aged 39.93 (11.53) yrs. from Australia; competing at club level	MiCBT, group with first author, 8 x ~90 mins. contact, 210 mins. practice / wk. prescribed	NT	Dispositional mindfulness (FFMQ); Dispositional flow (DFS-2); Competitive anxiety (SAS-2); Exploratory outcomes (Sport Attributional Style Scale)
Solberg et al., 1996	25 (4 female) shooters aged Median 25 (18-46) yrs. from Norway; elite based on standardised test (NRAN > 236/250)	Acem, group, no description of personnel, unclear dose, 210 mins. practice / wk. prescribed	NT	Skill execution (standardised rifle shooting test); Competitive performance (competitive performance over season); Exploratory outcomes (tension visual analogue scale)
Solberg et al., 2000	31 male runners aged 39 (36-42) yrs. from Norway	Acem, group with experienced instructors, 7 x 150 mins. contact, informal practice prescribed	Autogenic training, Problem solving	Competitive anxiety (STAI); Exploratory outcomes (maximal and recovery oxygen uptake, stress-induced lactate, resting and recovery heart rate)
Zhang et al., 2016	43 (27 female) dart throwers aged 19.23 (1.27) yrs. from China; amateur with no meditation experience	MAC, group with sport psychology consultants, 7 x 90 mins. contact, various different activities prescribed	Sport psych lectures	Dispositional mindfulness (FFMQ); Dispositional flow (short DFS); Skill execution (standardised dart throwing accuracy); Exploratory outcomes (AAQ-II)
Crossover Randomised Controlled Trials – 1				
Buscombe et al., 2014	9 (2 female) athletes from various sports aged 31.56 (22-44) yrs. from UK; amateur	TM and Zazen, 1:1 with authors, experienced in all three approaches, 1 x U mins. contact, 140 mins. practice / wk. prescribed	Ratio breathing	Exploratory outcomes (Electro-encephalography Respiration rate, Electromyography, Blood volume pulse, Sense of coherence, Qualitative, open ended responses)

Non-Randomised Controlled Trials – 11				
Baltzell & Akhtar, 2014	42 (52 female) soccer and rowing athletes from USA; varsity Div. I	MMTS, group with expert insight meditation teacher, 12 x 30 mins. contact, 70 mins. practice / wk. prescribed	NT	Dispositional mindfulness (MAAS); Exploratory outcomes (Psychological Well-Being Scale, PANAS, SWLS)
Bernier et al., 2009	7 (2 female) golfers aged 15.67 (0.74) yrs. from France; junior-elite (4-10 yrs.)	ACT & MBCT + PST, group with researcher, 5 yrs. in PST, 5 x U mins. contact, ~20 mins. practice / wk. prescribed	PST alone	Exploratory outcomes (Ottawa Mental Skills Assessment Tool-3, Qualitative interviews)
Bernier et al., 2014	7 female figure skaters aged 12.57 (0.73, 12-14) yrs. from France; national top 3	ACT & MBCT, 1:1 with researcher, 6 yrs. as sport psychology consultant, ~16 x 40 mins. contact, 70 mins. practice / wk. prescribed	NT	Competitive performance (average performance at national competitions); Exploratory outcomes (customised awareness and acceptance scale)
Bortoli et al., 2012	15 (7 female) rifle & pistol shooters aged 27.9 (8.1, 20-47) yrs. from Italy; top level international	MAP, 1:1 with author, sport psychology consultant, 12 x 150 mins. contact	NT	Exploratory outcomes (self-reported behavioural indicators)
Goodman et al., 2014	26 male athletes from various sports aged 20.23 (1.53) yrs. from USA; NCAA Div I.	MAC + Hatha yoga, group with licensed clinical psychologist, 500hr yoga instructor, 8 x 90 + 8 x 60 (yoga) mins. contact, various different activities prescribed	NT	Dispositional mindfulness (MAAS); Exploratory outcomes (AAQ-II, Tolerance of Negative Affect, Adult Hope Scale, PSS, Valued Living Questionnaire, Short Grit Scale, Drexel Defusion Scale, DASS-21)
Hasker, 2010	19 (8 female) athletes from various sports aged 19.4 (18-23) yrs. from USA; NCAA Div II.	MAC, group with two clinical psychology doctoral students, 7 x 60 mins. contact	Mental Training	Dispositional mindfulness (FFMQ); State flow (FSS); Competitive performance (coach and athlete self-report); Exploratory outcomes (AAQ, WBSI, Mini-Markers of Big 5 Personality Traits)
Kettunen & Välimäki, 2014	49 female floorball players aged 21.79 (17-38) yrs. from Finland; 9.50 yrs. experience (SD = 3.1)	ACT, group with two psychology masters students, 6 x 60 mins. contact, various different activities prescribed	NT	Dispositional mindfulness (FFMQ); Competitive performance (coach and athlete self-report); Exploratory outcomes (AAQ-II, PSS, Mental Health Continuum Short Form, sport self-confidence measure, Group Environment Questionnaire)
Little & Simpson, 2000	7 female softball players aged 20 (18-24) yrs. from USA; >8 yrs., NCAA Div I.	Acceptance-based, 1:1 with sport psychology consultant, unclear dose, informal practice prescribed	NT	Competitive performance (Batting, pitching, fielding statistics); Exploratory outcomes (WBSI, Fear of Sadness Test, Frequency and

				Suppression of Thoughts During Competition Questionnaire)
Longshore & Sachs, 2015	20 (12 female) Div I. coaches from various sports aged 34.5 (9.87) yrs. from USA	Mindfulness, group with first author, 1 x 90 mins. contact, 140 mins. practice / wk. prescribed	NT	State and dispositional mindfulness (TMS: MAAS); Competitive anxiety (STAI); Exploratory outcomes (PANAS, Brunel Mood Scale, qualitative interviews)
Pineau, 2014	55 (29 female) cross country runners aged 19.35 yrs. from USA; Div I.	MSPE ± SC, group with author or licensed clinical psychologist, 6 x 90 mins. contact, daily practice encouraged	NT	State and dispositional mindfulness (TMS, PHLMS, FFMQ); State and dispositional flow (FSS-2, DFS-2); Competitive anxiety (SAS, CSAI-2R); Competitive performance (objective and self-reported race times); Exploratory outcomes (Eating Attitudes Test, Multidimensional Body-Self Relations Questionnaire, Body Image Coping Strategies Inventory, SCS, CSCI, Thoughts During Running Scale)
Ruiz & Luciano, 2012	5 male chess players aged 23-50 yrs. from Spain; grand master ranking	ACT, 1:1 with author, experienced chess player, 2 x 120 or 3 x 75 mins. contact	NT	Competitive performance (International Ranking [ELO]); Exploratory outcomes (AAQ-II, Chess Counterproductive Reactions Questionnaire, believability and interference questions)
Shaw, 2014	51 (14 female) taekwondo athletes aged U (18-70+) yrs. from USA; mostly beginners	ACT, group with licensed psychologist, 1 x 180 mins. contact	NT	Dispositional mindfulness (FFMQ); Exploratory outcomes (PSS, qualitative interviews)
Wolanin & Schwanhausser, 2010	20 female volleyball & field hockey players from USA; NCAA Div I.	MAC, group with 2 clinical psychology doctoral students, 7 x 40 mins. contact	NT	Competitive anxiety (SAS); Competitive performance (coach ratings); Exploratory outcomes (Metacognitions Questionnaire, Generalized Anxiety Disorder Scale, Quality of Athletic Life Inventory)
Cohort/Case Studies – 12				
De Petrillo et al., 2009	25 (15 female) runners aged 34.73 (18-55) yrs. from USA; 6.68 yrs. experience	MSPE, group with first author, 4 x 150-180 mins. contact, encouraged to listen to mindfulness CD		State and dispositional mindfulness (TMS, KIMS); Competitive anxiety (SAS); Competitive performance (self-reported best mile time); Exploratory outcomes (MPS, TOQS)
Furrer, 2014b	29 (14 female) athletes from various sports aged 18.5 (18-20) yrs. from Norway; junior elite recruited from schools	Mindfulness, group session with experienced mindfulness coach, 4 x 120 mins. contact, 210 mins. practice / wk. prescribed		Dispositional mindfulness (MAAS); Exploratory outcomes (PSS, Athlete Satisfaction Scale, ABQ)
Gardner & Moore, 2004	2 (1 female) athletes from various sports	MAC, 1:1 session with author of protocol, 12-16 x 60 mins.		Competitive anxiety (SAS); Exploratory outcomes (AAQ, PSWQ)

	aged 29.5 (22-39) yrs. from USA; elite	contact, mindfulness prescribed for home	
Haase et al., 2015	7 BMX riders aged 21.86 (3.67) yrs. from USA; national representatives	mPEAK, unclear mode of administration, 4 x 180 + 6 x 90 mins. contact, 210 mins. practice / wk. prescribed	Dispositional mindfulness (FFMQ); Exploratory outcomes (Multidimensional Assessment of Interoceptive Awareness, Toronto Alexithymia Scale, neural response to stress [fMRI Inspiratory Breathing Load])
Jouper & Gustafsson, 2013	1 female shooter from Sweden; 'top international athlete'	Mindfulness and Qigong, 1:1 with weekly phone or email, unclear dose, 190 mins. practice / wk. prescribed	Exploratory outcomes (ABQ, Stress Energy Scale, daily concentration rating)
Kaufman et al., 2009	32 (9 female) archers & golfers aged 52.19 (18-76) yrs. from USA; recreational	MSPE, manualised treatment with no description of presenter experience, 4 x 150-180 mins. contact, 165-270 mins. practice / wk. prescribed	State and dispositional mindfulness (TMS, KIMS); State and dispositional flow (FSS-2, DFS-2); Competitive anxiety (SAS); Competitive performance (best score for year, average score for week); Exploratory outcomes (MPS, TOQS, CSCI)
Kingma, 2014	5 male golfers aged 53.6 (10.7) yrs. from South Africa; handicaps <= 15	MSPE + Schema, delivered by principal researcher, counselling psychologist with >5 yrs. mindfulness experience, 4 x 90 mins. contact, 50-150 mins. practice / wk. prescribed	Dispositional mindfulness (MAAS); Exploratory outcomes (Self-Consciousness Scale Revised, psychological momentum)
Lutkenhouse, 2007	1 female lacrosse athlete aged 19 yrs. from USA; NCAA Div I.	MAC, 1:1 session with clinical and sport psychology doctoral student, 7 x U mins. contact, regular practice encouraged	Competitive anxiety (SAS); Competitive performance (self-reported lacrosse performance); Exploratory outcomes (AAQ-R, PSWQ)
Mahoney & Hanrahan, 2011	4 (2 female) athletes from various sports aged 18-49 yrs. from Australia	ACT, 1:1 session with masters student trained in ACT, 4 x U mins. contact	Dispositional mindfulness (MAAS); Exploratory outcomes (Sport Injury Anxiety Scale, AAQ-II)
Mosewich et al., 2016	1 female athlete from Australia; elite individual sport	SC + Mindfulness, 1:1 session, no description of personnel, 6 x U mins. contact, daily practice encouraged	Qualitative interviews
Perret, 2014	7 (4 female) athletes from various sports aged 18.86 (3.52) yrs. from USA	ACT, 1:1 session with 5 different clinical psychology PhD students, each with 2-years ACT experience, 6 x 90 mins. contact, various	Dispositional mindfulness (FFMQ); Exploratory outcomes (AAQ-II, Cognitive Fusion Questionnaire, Rehabilitation Adherence Measure for Athletic Training, Psychological Inflexibility in Pain Scale)

		different activities prescribed	
Schwanhausser, 2009	1 male diver aged 12 yrs. from USA; 'high level'	MAC, 1:1 session with sport psychology doctoral student, 7 x 45 mins. contact	Dispositional mindfulness (MAAS, PHMS); State and dispositional flow (FSS-2, DFS-2); Competitive anxiety (SAS); Competitive performance (Scores in diving competition); Exploratory outcomes (AAQ-II)

Observational Designs – 21		Outcomes
Baranoff et al., 2015	44 (17 female) athletes from various sports aged 27 (9.4) yrs. from Australia; athletes post-ACL reconstruction	Exploratory outcomes (AAQ, Pain Catastrophising Scale, Athletic Identity Measurement Scale, DASS-21, Brief Coping Orientations to the Problem Experience)
Blecharz et al., 2014	10 male soccer players aged 18.14 (1.56) yrs. from Poland; 9.33 yrs. Experience (SD = 2.64)	Dispositional mindfulness (Freiburg Mindfulness Inventory); Skill Execution (standardised shooting test) Exploratory outcomes (task-related self-efficacy, team, peer and leadership self-efficacy)
Cathcart et al., 2014	92 (36 female) athletes from various sports aged 18 (2.6) yrs. from Australia; elite athletes	Dispositional mindfulness (FFMQ); Dispositional flow (DFS-2)
Chang et al., 2015	76 (32 female) athletes from various sports aged 20 (1.4) yrs. from Taiwan; university athletes	Exploratory outcomes (AAQ-II-Taiwanese, Center for Epidemiological Studies Depression Scale)
Denny & Steiner, 2009	140 (61 female) athletes from various sports aged 19.4 (1.51, 16-24) yrs. from USA; university athletes	Dispositional mindfulness (MMS); Exploratory outcomes (Locus of Control, Weinberger Adjustment Inventory)
Diaz, 2009	79 female equestrian athletes aged U (18-66+) yrs. from USA; 28.5 yrs. experience (range = 1-62)	Dispositional mindfulness (CAMS-R); Exploratory outcomes (State and Trait Sport-Confidence Inventory, Assessment of Schema Polarity Profile, TEOSQ)
Furrer, 2014a	382 (116 female) athletes from various sports aged 18.5 (17-20) yrs. from Norway; junior elite	Dispositional mindfulness (MAAS); Exploratory outcomes (PSS, Athlete Satisfaction Questionnaire, ABQ)
Gooding & Gardner, 2009	43 male basketball players aged 19-24 yrs. from USA; NCAA Div. I	Dispositional mindfulness (MAAS); Competitive anxiety (SCAT); Skill Execution (non-competitive free-throw test); Exploratory outcomes (duration of in-game pre-shot routine)
Gustafsson et al., 2015	233 (107 female) athletes from various sports aged 17.50 (1.08) yrs. from Sweden; high school athletes in national talent program	Dispositional mindfulness (MAAS); Exploratory outcomes (ABQ, PSS, PANAS)

Hanneman, 2013	90 (32 female) runners aged 24.1 (3.49) yrs. from USA; healthy undergraduates	Dispositional mindfulness (FFMQ); Exploratory outcomes (Ratings of Perceived Exertion via treadmill test, Body Awareness Questionnaire, Exercise Self-Efficacy Scale)
Housley, 2009	146 (42 female) runners & divers aged 32.04 (16-68) yrs. from USA; 1-50 yrs. experience	Skill Execution (standardised diving test); Exploratory outcomes (AAQ, Eysenck Personality Inventory, self-efficacy measure)
Kee & Wang, 2008	182 (80 female) athletes from various sports aged 22.3 (1.98) yrs. from Singapore; interuniversity athletes	Dispositional mindfulness (MMS); Dispositional flow (DFS-2); Exploratory outcomes (Test of Performance Strategies)
McCarthy, 2011	52 (36 female) athletes from various sports aged 19.76 (1.3, 18-21) yrs. from USA; NCAA Div. III	Dispositional mindfulness (KIMS); Exploratory outcomes (TEOSQ)
Mosewich et al., 2011	151 female athletes from various sports aged 15.1 (1.2) yrs. from Canada; recreational - international	Exploratory outcomes (SCS, Rosenberg Self-Esteem Scale, Test of Self-Conscious Affect for Adolescents, Social Physique Anxiety Scale, Obligatory Exercise Questionnaire, Objectified Body Consciousness Scale for Youth, Performance Failure Appraisal Inventory, Fear of Negative Evaluation Scale)
Pineau et al., 2014	58 (41 female) rowers aged 28.43 (14-60) yrs. from USA; 3.58 yrs. experience(range = 0-10)	Dispositional mindfulness (FFMQ); Dispositional flow (DFS-2); Exploratory outcomes (CSCI, individual and team rowing efficacy)
Rafeeque & Sultana, 2016	323 (161 female) track & field athletes aged 18-22 yrs. from India; interuniversity athletes	Dispositional mindfulness (MMS); Competitive performance (coach and self-ratings); Exploratory outcomes (Mental Toughness Scale)
Röthlin et al., 2016	133 (72 female) athletes from various sports aged 23.68 (6.12) yrs. from Switzerland; national representatives	Dispositional mindfulness (Comprehensive Inventory of Mindfulness Experiences); Competitive anxiety (Competition Anxiety Inventory); Competitive performance (self-ratings)
Sarnell, 2012	197 female lacrosse athletes aged 14.42 (1.65, 11-18) yrs. from USA; 6.69 yrs. experience (SD = 2.16)	Dispositional mindfulness (Children's Acceptance and Mindfulness Measure); Competitive performance (coach ratings); Exploratory outcomes (Sport Commitment Scale, Sport Motivation Scale)
Steinberg, 2011	114 (42 female) rock climbers aged 29.9 (7.1, 19-61) yrs. from USA; 7.8 yrs. (sd = 7.16)	Dispositional mindfulness (MAAS); Exploratory outcomes (PANAS, SWLS)
Thienot et al., 2014	343 (165 female) athletes from various sports aged 23.14 (5.87) yrs. from Australia; elite & sub-elite	Dispositional mindfulness (MIS, MAAS); Dispositional flow (DFS-2); Competitive anxiety (SAS-2); Exploratory outcomes (Personal Standards Perfectionism, Evaluative Concern Perfectionism, Rumination from Emotional Control Questionnaire-2)
Wicks, 2012	5 female equestrian athletes aged 13-18 yrs.	Exploratory outcomes (qualitative interviews)

	from USA; 6.6 yrs. experience	

Note: U = Unclear from manuscript; Interventions: NT = No Treatment, ACT = Acceptance and Commitment Therapy, AM = Anapanasati Meditation, ATT = Attention Training Technique, MAC = Mindfulness-Acceptance-Commitment, MAP = Multi-Action Plan, MBCT = Mindfulness-based Cognitive Therapy, MBSR = Mindfulness-Based Stress Reduction, MiCBT = Mindfulness-integrated Cognitive Behavior Therapy, MMTS = Mindfulness meditation training for sport, mPEAK = Mindful Performance Enhancement, Awareness and Knowledge, MSPE = Mindful Sport Performance Enhancement, PST = Psychological Skills Training, SC = Self-Compassion, TM = Transcendental Meditation; Measures: AAQ = Acceptance and Action Questionnaire, ABQ = Athlete Burnout Questionnaire, CAMS = Cognitive and Affective Mindfulness Scale, CSAI = Competitive Sport Anxiety Inventory, CSCI = Carolina Sport Confidence Inventory, DASS = Depression Anxiety Stress Scale, DFS-2 = Dispositional Flow Scale, FFMQ = Five Facets of Mindfulness Questionnaire, FSS-2 = Flow State Scale, KIMS = Kentucky Inventory of Mindfulness Skills, MAAS = Mindful Attention Awareness Scale, MIS = Mindfulness Inventory for Sport, MMS = Mindfulness/Mindlessness Scale, MPS = Multidimensional Perfectionism Scale, PANAS = Positive and Negative Affect Scale, PHLMS = Philadelphia Mindfulness Scale, PSS = Perceived Stress Scale, PSWQ = Penn State Worry Questionnaire, SAS = Sport Anxiety Scale, SCAT = Sport Competition Anxiety Test, SCS = Self-Compassion Scale, STAI = State and Trait Anxiety Inventory, SWLS = Satisfaction with Life Scale, TEOSQ = Task and Ego Orientation in Sport Questionnaire, TMS = Toronto Mindfulness Scale, TOQS = Thought Occurrence Questionnaire for Sport, WBSI = White Bear Suppression Inventory

As mentioned earlier, no set of studies were sufficiently homogenous for a meaningful meta-analysis to be conducted. Of the RCTs: five studies tested mindfulness; two evaluated the Mindfulness, Acceptance and Commitment (MAC) protocol; two examined Transcendental Meditation (TM); two investigated Acem meditation; and six explored other types of mindfulness or acceptance interventions. Of the mindfulness studies, three included comparisons with no-treatment and three with other interventions. These studies could not be meaningfully aggregated because the reported outcomes varied between studies. This pattern of heterogeneity was consistent across other study designs. Instead of meta-analytic results, key findings are presented in Tables 2.3 through 2.7.

Risk of Bias Within Studies

The non-randomised controlled trials we found were all judged to be high risk because the comparison groups varied systematically from the intervention group. For example, comparison groups were selected from: (i) a different training environment

(Bernier, Thienot, Codron, & Fournier, 2009; Bernier, Thienot, Pelosse, & Fournier, 2014; Kettunen & Välimäki, 2014); (ii) a different sport (Baltzell & Akhtar, 2014); (iii) a different level of competition (Goodman et al., 2014); (iv) an online database (Ruiz & Luciano, 2012); (v) or because of their lower self-reported dysfunction (Bortoli, Bertollo, Hanin, & Robazza, 2012; Little & Simpson, 2000). Similarly, none of the before-after comparisons included sufficient controls to be considered low risk of bias. As a result, Table 2.2 contains the risk of bias assessment for the RCTs, with all other studies considered high risk.

Table 2.2

Consensus Risk of Bias for Randomised Controlled Trials

Citation	**Overall Risk of Bias**	**Sequence Generation**	**Allocation Conceal-ment**	**Blind-ing**	**Incomplete Data**	**Selective Reporting**	**Other Bias**
Aherne et al., 2011	?	? [a]	? [a]	? [a]	+	? [e]	+
Hall & Hardy, 1991	?	? [a]	? [a]	? [a]	+	? [e]	+
Ivarsson et al., 2015	?	? [a]	? [a]	- [c]	? [a]	? [e]	- [g]
Jha, 2015	-	? [a]	? [a]	? [a]	- [d]	- [f]	? [g]
John et al., 2012	?	? [a]	? [a]	? [a]	- [d]	? [e]	+
Moen & Wells, 2016	-	? [a]	? [a]	? [a]	- [d]	? [e]	- [g]
Moen et al., 2015	?	? [a]	? [a]	? [a]	- [d]	? [e]	? [g]
Mosewich et al., 2013	-	+	? [a]	- [c]	+	? [e]	+
Muangnapoe, 1998	-	? [a]	? [a]	- [c]	? [a]	- [f]	+
Ojaghi et al., 2013	-	? [a]	? [a]	? [a]	? [a]	? [e]	- [h]
Papanikolaou, 2011	-	? [a]	? [a]	- [c]	? [a]	- [f]	+
Quinones-Paredes, 2014	?	? [a]	? [a]	? [a]	- [d]	? [e]	? [g]
Regan et al., 1998	-	? [a]	? [a]	? [a]	? [a]	? [e]	- [h]
Scott-Hamilton et al., 2016	-	+	? [a]	- [c]	- [d]	? [e]	+
Solberg et al., 1996	-	? [a]	? [a]	- [c]	? [a]	? [e]	- [h]
Solberg et al., 2000	-	? [a]	? [a]	- [c]	- [d]	? [e]	- [g]
Zhang et al., 2016	-	+	- [b]	- [c]	+	? [e]	+

Note: + = low risk of bias; ? = unclear risk; - = high risk of bias; a = unclear description in manuscript or from author's response; b = transparent allocation sequence; c = authors appeared to provide intervention and control; d = significant dropout with inadequate analyses; e = no protocol; f = measures collected but not adequately reported; g = risk of baseline discrepancies; h = inadequate reporting of methods

Quality of Evidence for Improving Mindfulness

As outlined in Table 2.3, seven RCTs have explored the influence of mindfulness and acceptance interventions for promoting mindfulness as a presumed facilitator of performance (Aherne et al., 2011; Moen, Abrahamsen, & Furrer, 2015; Moen & Wells, 2016; Ojaghi et al.,, 2013; Quinones-Paredes, 2014; Scott-Hamilton, Schutte, & Brown, 2016; Zhang et al., 2016). Risk of bias was judged to be low in none of these studies. Effect sizes ranged from very low (Moen et al., 2015; Quinones-Paredes, 2014) to very high (Aherne et al., 2011; Moen & Wells, 2016; Zhang et al., 2016). Sample sizes were generally small (n_{mean}= 44, range = 13-78) and the only reported confidence interval was very wide (95% CI [.79, 2.14], Zhang et al., 2016). All effect sizes for non-randomised controlled trials were all positive. All before-after comparisons showed positive effect sizes except one (Kingma, 2014), with no evidence of a dose-response relationship.

Overall, there was a consistent pattern that mindfulness and acceptance interventions increase self-reported mindfulness. The large strength of these effect sizes was tempered by the high risk of bias in the studies and the imprecision of results. Using the GRADE criteria, the quality of the evidence was judged to be low using the GRADE criteria, meaning further research is very likely to have an important impact on our confidence in effect (Schünemann, Oxman, Vist, et al., 2008).

Table 2.3

Effects of Mindfulness and Acceptance on Athlete Reports of Mindfulness

Citation	**ROB**	**N**	**Skill Level**	**Type of Task**	**Intervention**	**Prescribed Dose (hrs.)**	**Comparison**	**Mindfulness ES**
Randomised Controlled Trials								
Aherne et al., 2011	?	13	W	V	Mindfulness	11	NT	**1.02**
Moen & Wells, 2016	-	78	W	V	ATT	26	NT	**1.23**
Moen et al., 2015	?	77	W	V	Mindfulness	29	NT	**0.17**
Ojaghi et al., 2013	-	40	W	F	Mindfulness	N	NT	**0.69**
Quinones-Paredes, 2014	?	13	W	G	Mindfulness	12	Relaxation	0.1
Scott-Hamilton et al., 2016	-	47	W	G	MiCBT	40	NT	**0.71**
Zhang et al., 2016	-	43	N	F	MAC	11	Sport psych lectures	**1.47** 95% CI [.79, 2.14]
Non-Randomised Controlled Trials								
Baltzell & Akhtar, 2014	-	42	W	G	MMTS	13	NT	**0.99**
Goodman et al., 2014	-	26	W	V	MAC + Hatha yoga	20	NT	**0.68**
Hasker, 2010	-	19	W	V	MAC	7	Mental Training	0.24
Kettunen & Välimäki, 2014	-	49	W	G	ACT	6	NT	0.17
Longshore & Sachs, 2015	-	20	W	V	Mindfulness	16	NT	0.37; State: U
Pineau, 2014	-	55	W	G	MSPE ± SC	9	NT	0.07
Shaw, 2014	-	51	N	G	ACT	3	NT	U
Cohort/Case Studies								
De Petrillo et al., 2009	-	25	W	G	MSPE	11		0.32; State: 1.15
Furrer, 2014b	-	29	W	V	Mindfulness	50		U
Haase et al., 2015	-	7	W	G	mPEAK	46		0.41
Kaufman et al., 2009	-	32	V	F	MSPE	8		0.87; State: 0.49
Kingma, 2014	-	5	W	F	MSPE + Schema	13		-0.61
Mahoney & Hanrahan, 2011	-	4	U	V	ACT	~4		U
Perret, 2014	-	7	V	V	ACT	9		U
Schwanhausser, 2009	-	1	W	G	MAC	5		U

GRADE: Low – further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate. Randomised trials and correlational data support the use of these interventions and

RCT effect sizes are large; however, effect sizes are imprecise and no studies reported adequate concealment, blinding, or protocols.

Note: Refers to between-group differences in dispositional mindfulness for RCT and NRCT designs, or pre-post differences for cohort designs, unless otherwise specified; significant effects in bold ($p < 0.05$); N = Novel skill; W = Well-learned skill; F = Fine motor tasks; G = Gross motor task; V = Various; U = Unclear from manuscript; NT = No Treatment; ACT = Acceptance and Commitment Therapy; AM = Anapanasati Meditation; MAC = Mindfulness-Acceptance-Commitment; MAP = Multi-Action Plan; MBSR = Mindfulness-Based Stress Reduction; MiCBT = Mindfulness-integrated Cognitive Behavior Therapy; MMTS = Mindfulness meditation training for sport; mPEAK = Mindful Performance Enhancement, Awareness and Knowledge; MSPE = Mindful Sport Performance Enhancement; SC = Self-Compassion

Quality of Evidence for Increasing Flow

In sport, flow is defined as an intense, rewarding, undistracted absorption in the activity, which has been found to be a mediator of success in performance (Swann, Keegan, Piggott, & Crust, 2012). It can reflect a moment-to-moment experience (state flow) or the tendency of an athlete experience these states (dispositional flow; Jackson & Eklund, 2002). As outlined in Table 2.4, four of the seven RCTs that explored mindfulness also examined the influence of the intervention on dispositional flow (Aherne et al., 2011; Quinones-Paredes, 2014; Scott-Hamilton et al., 2016; Zhang et al., 2016). All effect sizes were positive, ranging from small ($d = .22$; Quinones-Paredes, 2014) to very large ($d = 1.66$; Aherne et al., 2011). The pattern was less consistent for other designs. Both non-randomised controlled trials reported lower flow as a result of the intervention (Hasker, 2011; Pineau, 2014). Kaufman and colleagues (2009) found a large effect size for state flow in their before and after study.

Correlational data supported the relationship between mindfulness and flow; effect sizes in all five studies were positive and significant, ranging from 0.15 ($p < 0.01$; Thienot et al., 2014) to 0.79 ($p < 0.001$; Kaufman et al., 2009).

Overall, the evidence from interventions and observational designs generally supported the relationship between mindfulness and acceptance interventions and the promotion of flow states, with strong effect sizes. Again, the potential bias in the evidence and imprecise results, meaning the overall quality of evidence was judged to be low

Table 2.4

Effects of Mindfulness and Acceptance on Athlete Reports of Flow

Citation	**ROB**	**N**	**Skill Level**	**Type of Task**	**IV**	**Prescribed Dose (hrs.)**	**Comparison**	**Flow ES**
Randomised Controlled Trials								
Aherne et al., 2011	?	13	W	V	Mindfulness	11	NT	**1.66**
Quinones-Paredes, 2014	?	13	W	G	Mindfulness	12	Relaxation	0.22
Scott-Hamilton et al., 2016	-	47	W	G	MiCBT	40	NT	0.64
Zhang et al., 2016	-	43	N	F	MAC	11	Sport psych lectures	**1.50 (95% CI = .81-2.17)**
Non-Randomised Controlled Trials								
Hasker, 2010	-	19	W	V	MAC	7	Mental Training	State: **-1.06**
Pineau, 2014	-	55	W	G	MSPE ± SC	9	NT	**-0.79**; State: -0.23
Cohort/Case Studies								
Kaufman et al., 2009	-	32	V	F	MSPE	8		0.49; State: **0.93**
Schwanhausser, 2009	-	1	W	G	MAC	5		U; State: U
Observational Designs						**Correlation with Dispositional Flow**		
Cathcart et al., 2014		92	W	V	Mindfulness	**0.33**		
Kaufman et al., 2009		32	V	F	Mindfulness	**0.79**		
Kee & Wang, 2008		182	W	V	Mindfulness	**0.28**		
Pineau et al., 2014		58	V	G	Mindfulness	**0.41**		
Thienot et al., 2014		343	W	V	Mindfulness	**0.15**		

GRADE: Low – further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate. Randomised trials and correlational data support the use of these interventions and RCT effect sizes are large; however, effect sizes are imprecise and no studies reported adequate concealment, blinding, or protocols.

Note: Refers to between-group differences in dispositional flow for RCT and NRCT designs, or pre-post differences for cohort designs, unless otherwise specified; significant effects in bold ($p < 0.05$); N = Novel skill; W = Well-learned skill; F = Fine motor tasks; G = Gross motor task; V = Various; U = Unclear from manuscript; NT = No Treatment; MAC = Mindfulness-Acceptance-Commitment; MiCBT = Mindfulness-integrated Cognitive Behavior Therapy; MSPE = Mindful Sport Performance Enhancement; SC = Self-Compassion

Quality of Evidence for Reducing Anxiety

Six comparisons from four RCTs explored the relationship between mindfulness and acceptance interventions and competitive anxiety (see Table 2.5; Muangnapoe, 1998; Ojaghi et al., 2013; Scott-Hamilton et al., 2016; Solberg et al., 2000). While all studies were judged to have high risk of bias, each comparison showed greater reductions in anxiety compared with the control condition, most with moderate or large effect sizes. Conclusions may not be representative of all mindfulness and acceptance approaches because while all appeared to promote present-moment awareness, only one explicitly included an acceptance component (Scott-Hamilton et al., 2016). Also, all RCTs were conducted on experienced athletes, with none testing novel skill acquisition.

Anxiety reductions were less consistent amongst the non-randomised controlled trials and before-after designs, with two studies finding reduced anxiety (Kaufman et al., 2009; Longshore & Sachs, 2015) and three finding higher anxiety (De Petrillo, Kaufman, Glass, & Arnkoff, 2009; Kingma, 2014; Pineau, 2014). Three correlational studies have explored the relationship between mindfulness and anxiety: Gooding and Gardner (2009) found a positive, non-significant relationship, and both other studies found mindfulness was associated with significantly lower anxiety (Röthlin, Horvath, Birrer, & Holtforth, 2016; Thienot et al., 2014). Overall, with the high risk of bias amongst the included studies, large but imprecise effect sizes, the quality of the evidence reviewed here was judged to be low.

Table 2.5

Effects of Mindfulness and Acceptance on Athlete Reports of Competitive Anxiety

Citation	**ROB**	**N**	**Skill Level**	**Type of Task**	**Intervention**	**Prescribed Dose (hrs.)**	**Comparison**	**Anxiety ES**
Randomised Controlled Trials								
Muangnapoe, 1998	-	48	W	G	AM	15	PMR	**-0.78**
							Stretching	**-1.38**
Ojaghi et al., 2013	-	40	W	F	Mindfulness	N	NT	-0.74
Scott-Hamilton et al., 2016	-	47	W	G	MiCBT	40	NT	-0.43
Solberg et al., 2000	-	31	W	G	Acem	18	Autogenic training	-0.43
							Problem solving	-0.21
Non-Randomised Controlled Trials								
Longshore & Sachs, 2015	-	20	W	V	Mindfulness	16	NT	-0.44
Pineau, 2014	-	55	W	G	MSPE ± SC	9	NT	-0.13
Wolanin & Schwanhausser, 2010	-	20	W	G	MAC	5	NT	U
Cohort/Case Studies								
De Petrillo et al., 2009	-	25	W	G	MSPE	11		0.62
Gardner & Moore, 2004	-	2	W	V	MAC	14		U
Kaufman et al., 2009	-	32	V	F	MSPE	8		0.14
Kingma, 2014	-	5	W	F	MSPE + Schema	13		0.85
Lutkenhouse, 2007	-	1	W	G	MAC	~7		U
Schwanhausser, 2009	-	1	W	G	MAC	5		U
Observational Designs						**Correlation with Competitive Anxiety**		
Gooding & Gardner, 2009		43	W	F	Mindfulness	0.26		
Röthlin et al., 2016		133	W	V	Mindfulness	**-0.45** (cognitive); **-.29** (somatic)		
Thienot et al., 2014		343	W	V	Mindfulness	**-0.43**		

GRADE: Low – further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate. Randomised trials and correlational data support the use of these interventions and RCT effect sizes are large; however, effect sizes are imprecise and no studies reported adequate concealment, blinding, or protocols. Findings only generalisable to experienced athletes.

Note: Refers to between-group differences in competitive anxiety for RCT and NRCT designs, or pre-post differences for cohort designs, unless otherwise specified; significant effects in bold ($p < 0.05$); N = Novel skill; W = Well-learned skill; F = Fine motor tasks; G = Gross motor task; V = Various; U = Unclear from manuscript; NT = No Treatment; AM = Anapanasati Meditation; MAC = Mindfulness-Acceptance-Commitment; MiCBT = Mindfulness-integrated Cognitive Behavior Therapy; MSPE = Mindful Sport Performance Enhancement; SC = Self-Compassion

Quality of Evidence for Performance Enhancement

As outlined in Table 2.6, five RCTs explored the influence of mindfulness and acceptance interventions toward athletic performance enhancement (Hall & Hardy, 1991; John, Kumar, & Lal, 2012; Ojaghi et al., 2013; Solberg, Berglund, Engen, Ekeberg, & Loeb, 1996; Zhang et al., 2016). Two studies comparing these approaches to active treatments found effect sizes favouring the other treatment (visuomotor behaviour rehearsal and music therapy respectively; Hall & Hardy, 1991; John et al., 2012). Of those that compared mindfulness and acceptance approaches to placebo or waitlist control conditions (*k* = 5), effect sizes were imprecise, with conflicting results from the same participants (Solberg et al., 1996) to large effects with wide confidence intervals large (95% CI [1.12, 2.55]; John et al., 2012; Zhang et al., 2016). None of these RCTs reported sufficient detail to be judged as low risk of bias.

Table 2.6

Effects of Mindfulness and Acceptance on Athletic Performance

Citation	ROB	N	Skill Level	Type of Task	Intervention	Prescribed Dose (hrs.)	Comparison	Performance ES
Randomised Controlled Trials								
Hall & Hardy, 1991	?	30	N	F	TM	38	NT	Skill: 0.17
							VMBR	Skill: **-0.54**
John et al., 2012	?	165	W	F	Mindfulness	8	NT	Skill: **0.87**
							Music therapy	Skill: -0.11
Ojaghi et al., 2013	-	40	W	F	Mindfulness	N	NT	**0.41**
Solberg et al., 1996	-	25	W	F	Acem	25	NT	**0.26** Skill: -0.28
Zhang et al., 2016	-	43	N	F	MAC	11	Sport psych lectures	Skill: **1.84** 95% CI [1.12, 2.55]
Non-Randomised Controlled Trials								

Bernier et al., 2014	-	7	W	G	ACT & MBCT	66	NT	U
Hasker, 2010	-	19	W	V	MAC	7	Mental Training	0.16
Kettunen & Välimäki, 2014	-	49	W	G	ACT	6	NT	0.06
Little & Simpson, 2000	-	7	W	F	Acceptance-based	N	NT	U
Pineau, 2014	-	55	W	G	MSPE ± SC	9	NT	0.08
Ruiz & Luciano, 2012	-	5	W	F	ACT	4	NT	**1.22**
Wolanin & Schwanhausser, 2010	-	20	W	G	MAC	5	NT	U
Cohort/Case Studies								
De Petrillo et al., 2009	-	25	W	G	MSPE	11		U
Kaufman et al., 2009	-	32	V	F	MSPE	8		U
Kingma, 2014	-	5	W	F	MSPE + Schema	13		0.41
Lutkenhouse, 2007	-	1	W	G	MAC	~7		U
Schwanhausser, 2009	-	1	W	G	MAC	5		U
Observational Designs						**Correlation with Performance**		
Blecharz et al., 2014		101	W	G	Mindfulness	**Skill: 0.17**		
Gooding & Gardner, 2009		43	W	F	Mindfulness	Skill: 0.14		
Röthlin et al., 2016		133	W	V	Mindfulness	**0.33**		
Sarnell, 2012		197	V	G	Mindfulness	**0.19**		

GRADE: Low – further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate. Randomised trials and correlational data support the use of these interventions and RCT effect sizes are large; however, effect sizes are imprecise and no studies reported adequate concealment, blinding, or protocols. Performance effects generalisable to fine motor skills only.

Note: Refers to between-group differences in competitive performance for RCT and NRCT designs, or pre-post differences for cohort designs, unless otherwise specified as skill execution in a non-competitive environment; significant effects in bold ($p < 0.05$); N = Novel skill; W = Well-learned skill; F = Fine motor tasks; G = Gross motor task; V = Various; U = Unclear from manuscript; NT = No Treatment; ACT = Acceptance and Commitment Therapy; MAC = Mindfulness-Acceptance-Commitment; MBCT = Mindfulness-based Cognitive Therapy; MSPE = Mindful Sport Performance Enhancement; SC = Self-Compassion; TM = Transcendental Meditation

Four papers explored the performance benefits of the MAC protocol: one RCT (Zhang et al., 2016), one non-randomised controlled trial (Hasker, 2011), and two before-after comparisons (Gardner & Moore, 2004; Lutkenhouse, 2007). Only Zhang and colleagues (2016) demonstrated statistically significant increases in performance. Two other interventions were also used in non-randomised controlled trials and before-after designs (ACT; Kettunen & Välimäki, 2014; Ruiz & Luciano, 2012; MSPE; Kingma, 2014; Pineau, 2015). Only one of these studies showed a significant improvement in performance (Ruiz & Luciano, 2012). From the observational data, there were small to moderate correlations between mindfulness and performance in three studies (Blecharz et al., 2014; Gooding & Gardner, 2009; Sarnell, 2012).

Overall, there is a dearth of high-quality studies and some inconsistent findings in support of mindfulness and acceptance approaches for performance enhancement. Due to the apparent bias in evidence base, the quality of evidence for these approaches was judged to be low.

Other Exploratory Outcomes

There are a number of outcomes that were explored by few studies with high internal validity. We present the available evidence on these outcomes here as possible avenues for future research.

Firstly, two RCTs showed significant reductions in burnout as a result of a mindfulness intervention (Moen et al., 2015; Moen & Wells, 2016). This result may be associated with changes in affect, where mindfulness was found to be correlated with higher positive affect and lower negative affect (Diaz, 2010; Gustafsson et al., 2015; Steinberg, 2012).

Secondly, a number of studies have explored physiological or psychophysiological effects of these interventions (Buscombe et al., 2014; Haase et al., 2015; John et al., 2012;

Solberg et al., 2000). Preliminary findings suggest that mindfulness may lead to increased anterior cingulate cortex and insula activation (Haase et al., 2015) and reduced salivary cortisol (John et al., 2012), but no differences have been found for lactate response, heart rate, or oxygen intake (Buscombe et al., 2014; Solberg et al., 2000).

Finally, there is some preliminary evidence for mindfulness and acceptance approaches toward the prevention and management of injuries. Ivarsson and colleagues (2015) found a reduced injury rate from a seven-week MAC intervention. While Mahoney and Hanrahan (2011) found inconsistent results using ACT with injured athletes over four sessions, Perret (2014) found increased rehabilitation adherence from a six-session ACT intervention.

Table 2.7

Effects of Mindfulness and Acceptance on Other Outcomes

Citation	N	Skill Level	Type of Task	Intervention	Dose (hrs.)	Comparison	Exploratory and Qualitative Outcomes
Randomised Controlled Trials							
Ivarsson et al., 2015	41	W	G	MAC	5	Sport psych presentation	Lower injuries: d = -0.59 [80%CI: −0.37, −0.74]
Jha, 2015	105	W	G	Mindfulness	9	Relaxation & visualisation	Among those who practiced, higher sustained attention for mindfulness
John et al., 2012	165	W	F	Mindfulness	8	NT, Music therapy	Reduced salivary cortisol vs. no treatment; no diff. vs. music
Moen & Wells, 2016	78	W	V	ATT	26	NT	Reduced burnout
Moen et al., 2015	77	W	V	Mindfulness	29	NT	Reduced burnout
Mosewich et al., 2013	51	W	V	SC	1	Journalling	Higher self-compassion (.79), lower rumination (-.66), self-criticism (-.89), concern over mistakes (-.63), all maintained at 1-month
Muangnapoe, 1998	48	W	G	AM	15	PMR, Stretching	For confidence, no diff vs. PMR (d = -.03), sig. increased vs. stretching (d = **.56**)
Papanikolaou, 2011	40	U	G	Various	12	Video review	Increased use of different attentional styles
Quinones-Paredes, 2014	13	W	G	Mindfulness	12	Relaxation	No diff. for thought suppression, qual. data found increased focus, but mindfulness practice was challenging
Regan et al., 1998	28	U	G	Meditation	N	NT	No diff. for RPE, mood, anxiety, efficiency
Scott-Hamilton et al., 2016	47	W	G	MiCBT	40	NT	Less pessimism
Solberg et al., 2000	31	W	G	Acem	18	Autogenic training, Problem solving	No diff. vs. either condition for lactate response, oxygen intake, heart rate

Non-Randomised Controlled Trials							
Baltzell & Akhtar, 2014	42	W	G	MMTS	13	NT	Lower negative affect, no diff. for wellbeing, positive affect, life satisfaction; qual. data found increased focus, generalised benefits, challenging to practice, and requested more experiential exercises
Bernier et al., 2009	7	W	F	ACT & MBCT + PST	11	PST alone	Higher percentage improved national ranking, all improved adherence to routines, higher activation
Bernier et al., 2014	7	W	G	ACT & MBCT	66	NT	Increased acceptance and awareness in action, qual. reported increased focus, generalised benefits, links between practice and improvement, and challenging to practice
Buscombe et al., 2014	9	N	V	TM, Zazen	2	Ratio breathing	TM: Higher respiration rate, no diff. on biofeedback, qual. data found increased focus, generalised benefits Zazen: No diff. on biofeedback, qual. data found increased focus, generalised benefits
Goodman et al., 2014	26	W	V	MAC + Hatha yoga	20	NT	Higher goal directed energy, qual. data found increased focus, generalised benefits, requested more experiential exercises
Hasker, 2010	19	W	V	MAC	7	Mental Training	No diff. on experiential avoidance, suppression
Kettunen & Välimäki, 2014	49	W	G	ACT	6	NT	No diff. on wellbeing, cohesion, confidence (d = .30)
Little & Simpson, 2000	7	W	F	Acceptance-based	N	NT	No sig. diff. on thought suppression or experiential avoidance
Longshore & Sachs, 2015	20	W	V	Mindfulness	16	NT	Lower negative affect
Pineau, 2014	55	W	G	MSPE ± SC	9	NT	No diff. on body image, self-compassion, confidence (d = -0.04)

Ruiz & Luciano, 2012	5	W	F	ACT	4	NT	No diff. on experiential avoidance
Shaw, 2014	51	N	G	ACT	3	NT	Lower stress for treatment, not control, some mindfulness facets improved, others worse
Wolanin & Schwanhausser 2010	20	W	G	MAC	5	NT	No diff. on anxiety, quality of life, performance, metacognition
Cohort/Case Studies							
De Petrillo et al., 2009	25	W	G	MSPE	11	No differences for performance (means not reported; improved at follow-up), perfectionism, or thought disruption	
Furrer, 2014b	29	W	V	Mindfulness	50	Qual. data found increased focus, generalised benefits, higher perceived performance	
Gardner & Moore, 2004	2	W	V	MAC	14	Increased psychological flexibility, perceived performance	
Haase et al., 2015	7	W	G	mPEAK	46	Increased anterior cingulate cortex and insula activation, lower alexithymia	
Jouper & Gustafsson, 2013	1	W	F	Mindfulness and Qigong	158	Increased concentration, reduced burnout	
Kingma, 2014	5	W	F	MSPE + Schema	13	Qual. data found increased awareness and acceptance	
Lutkenhouse, 2007	1	W	G	MAC	~7	Increased motivation, fitness, performance, team relationships	
Mahoney & Hanrahan, 2011	4	U	V	ACT	~4	Inconsistent effects on psychological flexibility, mindfulness, and anxiety; qual. data found practice was challenging but positive link between practice and improvement, benefits from experiential/metaphorical exercises	
Mosewich et al., 2016	1	W	U	SC + Mindfulness	~6	Increase emotional regulation, some difficulty with practice	
Perret, 2014	7	V	V	ACT	9	Increased psychological flexibility and rehabilitation adherence	
Schwanhausser 2009	1	W	G	MAC	5	Increased mindfulness, flow, psychological flexibility, performance, decreased anxiety, qual. data found increased focus	
Observational Designs							
Baranoff et al., 2015	44	U	V	Experiential Avoidance	Higher depression (r = **.47**) and alcohol use (r = .33) @ 6 months		
Blecharz et al., 2014	101	W	G	Mindfulness	Higher self-efficacy (r = **.29**) and performance (r = .17) at 7-month follow-up		
Chang et al., 2015	76	W	V	Experiential Avoidance	Higher depression (r = **.70**) and negative affect (r = **.66**); lower autonomy support (r = -.23), positive affect (**-.37**), life satisfaction (-.21)		

Diaz, 2009	79	V	F	Mindfulness	Higher confidence (r = **.35**), positive affect (r = **.34**), locus of control (r = **.22**), happiness (r = **.34**), satisfaction with life (r = **.36**) and self (r = **.28**) and denial of distress (r = **.27**); lower negative affect (r = **-.18**)
Furrer, 2014a	382	W	V	Mindfulness	Lower stress (beta = **-.19**), indirect relationships with burnout, performance in sport and school
Hanneman, 2013	90	U	G	Mindfulness	Lower perceived exertion on treadmill test (r = **-.25**)
Housley, 2009	146	V	G	Experiential Avoidance	Predicted diving performance over and above physical discomfort tolerance ($R^2\Delta$ = **.13**)
Kee & Wang, 2008	182	W	V	Mindfulness	"High Mindfulness" cluster used more goal-setting than all other clusters
McCarthy, 2011	52	W	V	Mindfulness	No significant relationships with gender (r = .02), playing time (r = .10), task (r = -.05) or ego orientation (r = -.08)
Mosewich et al., 2011	151	V	V	Self-Compassion	Higher self-confidence (r = **.6**); lower physique anxiety (r = **.37**), fear of failure (r = **-.57**), shame (r= **-.39**) and self-consciousness (r = **-.50**)
Rafeeque & Sultana, 2016	323	W	G	Mindfulness	Higher mental toughness (MT; r = **.44**), higher performance controlling for MT (beta = **.08**)
Röthlin et al., 2016	133	W	V	Mindfulness	Lower trait cognitive anxiety (r = **-.45**) and trait somatic anxiety (r = **-.29**)
Sarnell, 2012	197	V	G	Mindfulness	Higher self-determined motivation (r = **.18**)
Thienot et al., 2014	343	W	V	Mindfulness	Lower worry (r = **-.48**), concentration disruption (r = **-.38**), evaluative concern (r = **-.51**) and rumination (r = **-.18**)
Wicks, 2012	5	W	F	Mindfulness	Qual. data found increased focused, generalised benefits of practice

Note: Refers to between-group differences for RCT and NRCT designs, or pre-post differences for cohort designs, unless otherwise specified; significant effects in bold ($p < 0.05$); N = Novel skill; W = Well-learned skill; F = Fine motor tasks; G = Gross motor task; V = Various; U = Unclear from manuscript; NT = No Treatment; ACT = Acceptance and Commitment Therapy; AM = Anapanasati Meditation; ATT = Attention Training Technique; MAC = Mindfulness-Acceptance-Commitment; MBCT = Mindfulness-based Cognitive Therapy; MiCBT = Mindfulness-integrated Cognitive Behavior Therapy; MMTS = Mindfulness Meditation Training Sport; mPEAK = Mindful Performance Enhancement, Awareness and Knowledge; MSPE = Mindful Sport Performance Enhancement; PST = Psychological Skills Training; SC = Self-Compassion

Qualitative themes

Some qualitative themes from the included studies help extend upon the quantitative data presented thus far. Themes emerged around other benefits of these mindfulness and acceptance interventions. In most studies that reported qualitative data, participants described a direct link between the intervention and the ability to maintain task-focused attention (Baltzell et al., 2014; Bernier et al., 2014; Buscombe et al., 2014; Goodman et al., 2014; Longshore & Sachs, 2015; Quinones-Paredes, 2014; Wicks, 2013). In six studies, participants described how the perceived benefits of mindfulness and acceptance interventions generalised beyond the sporting arena (e.g., via increased concentration or reduced anxiety; Baltzell et al., 2014; Bernier et al., 2014; Buscombe et al., 2014; Goodman et al., 2014; Hickman, Murphy, & Spino, 1977; Wicks, 2013).

Themes also emerged about experience of participating in mindfulness and acceptance interventions. Participants in four studies discussed the difficulty they experienced in learning and practicing the skills, particularly with respect to mindfulness (Baltzell et al., 2014; Bernier et al., 2014; Mahoney & Hanrahan, 2011; Quinones-Paredes, 2014). In two of these studies, participants also described a positive association between the amount of practice they completed and the benefits they received (Bernier et al., 2014; Mahoney & Hanrahan, 2011). In three papers, participants reported that the interventions would have been more helpful if they included a greater number of experiential exercises (Baltzell et al., 2014; Goodman et al., 2014; Mahoney & Hanrahan, 2011).

Discussion

While there are a number of studies showing positive effects for mindfulness and acceptance-based interventions for athletes, this systematic review indicates that the evidence is, at present, of low quality. Some studies have found large effect sizes for mindfulness and acceptance interventions for promoting present moment awareness, flow, performance, and for reducing competitive anxiety. For all outcomes, the findings were tempered by the risk of bias in included studies and imprecision in the effect sizes. Our review also found research showing preliminary support for the use of these interventions to prevent injuries, reduce burnout, and increase confidence. Observational studies suggest athletes differ in the degree to which they are mindful, and that a tendency toward mindfulness may be associated with higher mental toughness, self-determined motivation, self-efficacy, lower stress and lower ratings of perceived exertion.

These findings are largely consistent with previous reviews on mindfulness in sport (Birrer et al., 2012; Gardner & Moore, 2012; Sappington & Longshore, 2015). Our review synthesised the results from a larger number of studies ($k = 66$) compared with Sappington and Longshore's (2015) systematic review ($k = 19$). Despite the larger pool of evidence, we were not able to make any stronger conclusions about the effectiveness of mindfulness and acceptance approaches for performance enhancement. The need for well-designed RCTs described by previous reviewers (Birrer et al., 2012; Gardner & Moore, 2012; Sappington & Longshore, 2015) appears to still be unmet for this group of interventions.

Other attention management strategies (e.g., mental practice, instructional self-talk, goal setting) also demonstrate large effect sizes for performance enhancement (Driskell, Copper, & Moran, 1994; Hatzigeorgiadis et al., 2011; Kyllo & Landers, 1995). These meta-analyses did not systematically explore the risk of bias in the included studies, so conclusions based on those papers should also be tempered by the uncertainty regarding internal validity. Comparing the effect sizes here with those in previous meta-analyses, the incremental benefit of acceptance over-and-above the attentional management processes may be small. Theoretically, this incremental benefit may still be practically meaningful because effect sizes as small as 0.3 have been hypothesised to increase an athlete's chance of receiving an Olympic medal by 10% (Hopkins, Hawley, & Burke, 1999); however, the evidence found here comparing mindfulness and acceptance to other treatments is weak. No studies found significant performance benefits in favour of mindfulness (Hasker, 2011; John et al., 2012; Quinones-Paredes, 2014) and one found the alternate treatment to be significantly better (VMBR; Hall & Hardy, 1991). These findings suggest that mindfulness and acceptance approaches may offer some benefit compared to no treatment, but further research is required to rigorously compare these approaches with established interventions that control the content of internal experiences or manage attention.

Strengths and Limitations of Included Studies

Any benefits from mindfulness compared to placebo or wait-list controls ought to be considered in the context of internal validity. As described in previous reviews of mindfulness in sport, research to date has a number of limitations that undermine our ability to determine causality (Sappington & Longshore, 2015). While Sappington and Longshore (2015) judged two studies to be ‘very good quality’ (Aherne et al., 2011; John et al., 2011), no studies included in our review were judged to have a low risk of bias using the Cochrane Risk of Bias tool. No study clearly described a system where random allocation was concealed to the experimenter, and we were not able to find any papers that had registered a study protocol. No studies used designs in which all key personnel were blinded, and only six described *a priori* power analyses to determine sufficient sample sizes. Meeting pre-specified sample sizes reduce the likelihood of false-positive results and inflated effect sizes (Schweizer & Furley, 2016).

These internal validity criticisms are neither new nor uncharacteristic of literature exploring other interventions in sport psychology (Greenspan & Feltz, 1989; Martin, Vause,

& Schwartzman, 2005; Schweizer & Furley, 2016; Vealey, 1994). In sporting contexts, the desire to establish high levels of external validity can compromise the ability for studies to establish causality due to reduced control and precision (Greenspan & Feltz, 1989; Vealey, 1994). Coaches and athletes can be resistant to experimental designs in which they are given placebos or control conditions (Martin et al., 2005), and smaller pools of potential participants and funding can lead to inadequate sample sizes (Schweizer & Furley, 2016) or less well-controlled studies (Martin et al., 2005).

As a result of these influences, we acknowledge the challenge of meeting the internal validity standards set in other areas such as medicine and clinical psychology. However, meeting those standards would increase the strength of the causal conclusions that researchers could make (Higgins & Altman, 2008). For example, while blinding can be onerous for researchers, a review of meta-analyses found un-blinded studies were more likely to find significant treatment effects (Pildal et al., 2007) and placebo effects have demonstrated dose-response relationships even in objectively measured cycling performance (Beedie, Stuart, Coleman, & Foad, 2006). In a review of mindfulness-based interventions in clinical domains, a number of studies used double-blind designs, but those studies with higher internal validity demonstrated lower effect sizes, suggesting possible expectancy effects (Khoury et al., 2013).

One internal-validity standard that could be met regardless of sample size, funding, or context is protocol registration. Protocol registration can significantly increase the internal validity of studies because doing so usually requires that researchers declare power calculations, *a priori* outcomes of interest, blinding and randomisation processes

(Chambers, Feredoes, Muthukumaraswamy, & Etchells, 2014). Most top quality journals in medicine (De Angelis et al., 2004) and some in psychology (Chambers et al., 2014) are no longer accepting research without a registered protocol, and many others are requiring that authors follow reporting checklists like TIDieR (Hoffmann et al., 2014) and CONSORT (Schulz, Altman, & Moher, 2010) to ensure transparent reporting. Requiring the same standards in the sport psychology literature would encourage a higher level of transparency from authors regarding their methods, giving readers greater confidence in the performance benefits found from interventions.

The performance benefits from the mindfulness and acceptance interventions included in this review varied greatly (Cohen's *d* ranged from -.54 to 1.84) with no clear dose-response relationship. It is possible that this heterogeneity may be explained by the different interventions that were grouped under the mindfulness and acceptance umbrella. There were at least 10 different labels for interventions that appear to help athletes via similar mechanisms: all appeared to involve training to bring attention back to the present moment, and most explicitly described an attitude of experiential acceptance. Where Sappington and Longshore (2015) argued for increased manualisation of treatments, others have described a range of scientific advantages from exploring empirically supported principles of change instead of 'branded' interventions (Ciarrochi, Atkins, Hayes, Sahdra, & Parker, 2016; Ciarrochi, Bilich, & Godsell, 2010; Rosen & Davison, 2003). For example, clinical and experimental studies often report the specific ACT process that they are targeting (i.e., defusion, acceptance, present-moment awareness, self-as-context, values or committed action; Hayes, Luoma, Bond, Masuda, & Lillis, 2006). Doing so has allowed reviewers to conduct moderation analyses that explore the relative impact of targeting the different processes (Levin et al., 2012). In our review, it was not possible to explore these potential moderators because reporting of interventions was inconsistent. For example, it was not possible to discern the degree to which each included study focused on present moment awareness, acceptance, or both. If future interventions report the specific process being targeted (e.g., via the ACT model) then it would be possible to discern which components are having the biggest influence for athletes. Also, experimental designs could explicitly compare these components (e.g., acceptance vs. present-moment awareness), because each has a theoretical relationship with performance (Birrer et al., 2012). Nevertheless, it is currently unclear whether interventions are best with present-moment awareness, acceptance or both.

Another approach for looking at processes of change is to explore the mediators through which an intervention has an effect (Ciarrochi et al., 2010). In this review, few studies explored mediators of the intervention effects; however, there were large effect sizes for these interventions to promote mindfulness. The authors often presumed that increasing mindfulness in this way would lead to increases in performance; however, without designing interventions with mediation in mind (e.g., by measuring mindfulness sometime before performance measures) it is difficult to determine the causal nature of these relationships. Designing studies in this way would also allow for more rigorous exploration of the presumed causal chain involved in mindfulness and acceptance-focused performance enhancement.

A number of studies explored changes in anxiety and flow as potential links between mindfulness and acceptance interventions and performance, and this review found low-quality evidence that mindfulness and acceptance approaches help reduce anxiety and increase flow. The hypothesis that targeting these variables will cause performance improvements has yet to be tested. Designing an intervention that targets anxiety-reduction may symbolise a theoretical disconnect from the mindfulness and acceptance approaches, since most promote acceptance rather than reduction of anxiety. Some have proposed that both flow (Bortoli et al., 2012) and relaxation (Hayes, Strosahl, & Wilson, 2011) may be 'exhaust from the engine': serendipitous by-products of mindful awareness, without necessarily being mechanisms of action. Again, studies designed with mediation in mind (e.g., explicitly comparing relaxation vs. acceptance) would allow for additional evidence to be collected to explore these proposals.

Strengths and Limitations of this Review

Including studies reporting any outcome (e.g., performance, mindfulness, flow) was both a strength and limitation of this review. While it allowed us to discover effects of mindfulness and acceptance approaches on a range of metrics from neurological activation (Haase et al., 2015) to qualitative reports, it was one factor that precluded a meaningful meta-analysis since we could not aggregate across the different outcomes reported by the included studies.

Similarly, by including a diverse range of interventions under the mindfulness and acceptance umbrella, we could not conduct a meta-analysis because a pooled effect size was unlikely to be meaningful (Deeks et al., 2008). Including both mindfulness and acceptance interventions allowed us to synthesise a larger number of conceptually related approaches compared with reviews that focused exclusively on mindfulness (Sappington & Longshore, 2015). Nevertheless, despite the broad scope of this review, the small number of studies for each intervention and outcome was another factor that precluded meta-analysis. While the GRADE method used here is methodologically transparent and objective compared with other methods of narrative review (Schünemann, Oxman, Vist, et al., 2008), future reviews in this area would benefit from a quantitative synthesis of findings, perhaps by coding the interventions on the processes of change described earlier.

A related limitation with our methodology is that we could not create funnel plots to assess publication bias. We did search for and include unpublished research, many of which did not find significant effects (Hasker, 2011; Pineau, 2014; Quinones-Paredes, 2014), which may be an indicator of either publication bias or lower methodological rigor. Coronado-Montoya and colleagues (2016) found data consistent with this bias regarding mindfulness literature in the clinical domain. They discovered a disproportionately high number of published studies with significant findings, and found that 62% of registered protocols were still unpublished 2.5 years after trial-completion. These data contribute to the argument for protocols described earlier, because it allows for a systematic exploration of publication bias. Future reviews on this topic would benefit from exploring publication bias more methodically.

One other potential bias in our review comes from the pragmatic decision to only include papers published in English. Nevertheless, our broad inclusion criteria meant we sourced papers from various cultures, including Taiwan, China, India, Iran, Western Europe, North America, and Australasia. We did not examine the effect of culture or gender on the effectiveness of these approaches, so future quantitative syntheses may consider controlling for gender and culture as potential moderators.

Conclusions

Despite these limitations, our systematic review extends the findings of previous research on mindfulness and acceptance in sport by synthesising the results from a large number of studies. The included studies displayed poor internal validity, so future research would benefit from protocol registration, blinding, and reporting via standardised checklists (e.g., CONSORT). The causal processes underlying these interventions could be better explored by examining the empirically supported processes of change and theoretical mediators of performance improvements, rather than branded or trademarked interventions as a whole. Currently, it appears that these approaches may have benefits for improving performance, but higher quality studies are required to make causal claims about the efficacy of mindfulness and acceptance approaches for athletes.

Funding

No funding was associated with this review

Linking Chapter—From Systematic Review to Randomised Controlled Trial

The systematic review in Chapter 2 uncovered a number of potential benefits of context-focused interventions for athletes. Athletes may be able to use these interventions to become more focused, relaxed, and aware. As a result, perhaps, they may perform better too. The systematic review findings were tempered by limitations to the internal validity of the included studies. I failed to find any studies that met the Cochrane criteria for low risk of bias. These criteria are designed to ensure effects of a treatment are due to the treatment itself, as opposed to effects of the research design. These criteria include transparent reporting of randomisation procedure, blinding of participants and key personnelle, prospective registration of methods, and good handling of missing data. In the next study of this thesis, I aimed to meet these criteria set by the Cochrane collaboration. This study was designed to assess the effects of a brief mindfulness and acceptance intervention for athletes, while removing biases that were present in previous implementations.

Chapter 3—Can a Brief Mindfulness Intervention Improve Sports Performance?

A Double-Blind Randomised Controlled Trial

Abstract

Objectives. Mindfulness offers a range of possible benefits for athletes, but typically requires they spend hours each week practicing the skill. Short mindfulness interventions may offer benefits with lower time costs. Using an experimental design, we tested whether a short mindfulness intervention leads to performance improvements.

Design. This study was a double-blind, randomised crossover design.

Method. Experienced golfers (N = 118) were randomly allocated to receive either a brief mindfulness or attention-control intervention before competing in a putting competition. They then received the opposite intervention before another round of competition. At baseline and in each round of competition, we assessed performance, swing mechanics, state mindfulness, and competitive anxiety.

Results. We found no significant group by time interactions for performance, state mindfulness, or cognitive anxiety. These findings showed no effects of the intervention on these outcomes compared with the control condition. We found a significant effect on swing mechanics, with the mindfulness condition showing better movement patterns than control. There was also a significant interaction for somatic anxiety, suggesting participants who received mindfulness reported greater bodily feelings of nervousness during the competition.

Conclusions. This study is the first prospectively-registered, double-blind randomised trial assessing mindfulness in athletes. Brief mindfulness training may lead to small improvements in mechanics, but does not appear to be an effective method for improving golf putting performance over a brief time period. Studies with high internal

validity could test the effects of longer mindfulness interventions before athletes invest significant amounts of time developing these skills.

Keywords

Mindfulness; acceptance; sport performance enhancement; choking

Highlights

- This study was a double-blind, randomised trial of a brief mindfulness intervention
- Golfers were assessed on performance, swing mechanics, and competitive anxiety
- We found no significant effects of mindfulness on performance
- We found a benefit of mindfulness for swing mechanics, specifically tendency
- Findings underscore the importance of internal validity for strong causal claims

Introduction

Systematic reviews and meta-analyses have found that mindfulness training leads to improvements in stress, anxiety, depression, and quality of life in clinical (Goyal et al., 2014) and healthy populations (Bassam Khoury, Sharma, Rush, & Fournier, 2015). In athletes, systematic reviews have found a range of benefits, including more frequent experiences of flow, lower anxiety, and increased performance (Noetel, Ciarrochi, Van Zanden, & Lonsdale, 2017; Sappington & Longshore, 2015).

The benefits of mindfulness have also been studied at the neurological level. Mindfulness and meditation, which share similar non-judgemental awareness qualities, appear to have significant positive influences on both brain structure and function (Chiesa & Serretti, 2010; Fox et al., 2016, 2014). For example, a recent meta-analysis found increases in activity for the parts of the brain responsible for focused attention, and decreases in the areas responsible for mind wandering (Fox et al., 2016). Structural analyses of brains of experienced meditators found changes in brain structures responsible for memory, awareness, and emotional regulation (Fox et al., 2014). Systematic review of studies that specifically focused on mindfulness showed similar results (Chiesa & Serretti, 2010).

Mindfulness has been described as "a holistic intervention fostering the development of several personal, sport, and performance-relevant psychological skills" (Birrer et al., 2012, p. 243). However, both inside and outside sport, mindfulness research has come under scrutiny for demonstrating questionable internal validity (Farias & Wikholm, 2016; Noetel et al., 2017; Sappington & Longshore, 2015; Van Dam et al., 2018).

Internal validity is important because mindfulness practice involves a substantial opportunity cost (Van Dam et al., 2018). Traditional methods of practicing mindfulness involve sitting for long-stretches of time with focused attention. Practitioners and teachers may want to know whether or not the practice is likely to causally influence outcomes. Only studies with high internal validity can allow researchers to make such causal claims about these effects.

Research on mindfulness has been plagued by a series of problems that may overestimate its effects (Coronado-Montoya et al., 2016). In an ominous symptom of the 'file-drawer problem', 62% of the registered studies on mindfulness remained unpublished two-and-a-half years after completion of the trial (Coronado-Montoya et al.). None of these trial registrations specified any single clear outcome variable (Coronado-Montoya et al.). Instead, published reports frequently emphasised whichever outcome was statistically significant, leading to likely overestimations of the positive effects of the interventions (Coronado-Montoya et al.). Prospective registration can guard against post-hoc manipulations that lead to significant findings. For example, in health studies funded by the National Heart Lung, and Blood Institute, the number of significant published findings dropped from 57% to 8% after prospective registration standards were established in the year 2000 (Kaplan & Irvin, 2015). Prospective registration was not reported in any sport psychology journals in the last seven years (Tamminen & Poucher, 2018), or in any of the 66 studies included in a recent systematic review of mindfulness in sport (Noetel et al., 2017).

Similarly, none of these studies on mindfulness in sport described methods of double-blinding or methods of allocation concealment (i.e., concealing the allocation process to avoid experimenter manipulation, Noetel et al., 2017). Meta-meta-analyses have shown unblinded studies and those without allocation concealment overestimate effect sizes by 9% and 18% respectively (Pildal et al., 2007). Blinding can reduce the chance of expectation effects, of differential behaviours between groups, and of biased measurement of outcomes in favour of hypotheses (Higgins & Altman, 2008). Allocation concealment reduces the chance of the person enrolling participants biasing selection, by excluding a participant from the intervention group who appears unlikely to respond to treatment (Pildal et al., 2007). Poor control conditions are also a problem in mindfulness literature. While mindfulness may perform better than receiving nothing, there is insufficient evidence to suggest that mindfulness performs well compared to other treatments or active control conditions (Farias & Wikholm, 2016). For these reasons, review authors in this domain have reiterated the importance of internal validity: “studies with appropriately randomized design and proper active control groups will be absolutely crucial” (Van Dam et al., 2018, p. 47).

One model for testing mindfulness while maintaining internal validity comes from the brief, highly controlled mindfulness induction studies (Levin et al., 2012). Brief studies allow researchers to test theoretical underpinnings while mitigating logistical challenges associated with longer trials (Levin et al., 2012).These studies often use single-session designs to maintain experimental control over extraneous variables (Levin et al., 2012). For example, to test the effects of mindfulness on anxiety management, one study used a 9-minute audio file to induce either mindfulness, thought suppression, or unfocused attention (Hooper, Davies, Davies, & McHugh, 2011). The mindfulness intervention led to lower anxiety, and lower behavioural avoidance of spiders, compared with thought suppression. In a similar study, women in a control condition performed worse on a maths test when told "males are better than females in maths", but the effect was abated in those who received a five-minute mindfulness intervention (Weger, Hooper, Meier, & Hopthrow, 2012). Brief mindfulness activities as short as 8 minutes have been shown to lead to a better ability to control attention too (Mrazek, Smallwood, & Schooler, 2012). A meta-analysis found that brief mindfulness and acceptance interventions can have significant benefits such as increased task persistence or decreased intensity of negative thoughts and feelings (Levin et al., 2012). Our study used a brief intervention to test the effect of mindfulness training on sport performance.

Increased ability to control attention is one of the core mechanisms by which mindfulness has been hypothesised to improve athletic performance (Birrer et al., 2012; Gardner & Moore, 2007; Noetel et al., 2017; Sappington & Longshore, 2015). Mindfulness training is presumed to help athletes maintain their focus on useful objects of attention. Focus is improved through better awareness of the present moment and acceptance of distracting thoughts and feelings. Athletes have been shown to perform poorly when attention was excessively focused on task execution (Beilock et al., 2002; Gucciardi & Dimmock, 2008) or on negative thoughts about themselves (Beilock, Jellison, Rydell, McConnell, & Carr, 2006). Instead, they performed better when focused on automatic execution of the skill without conscious thought (Beilock et al., 2002; Gucciardi & Dimmock, 2008). Sport performance often fails in the presence of anxiety due to self-focused attention (Beilock et al., 2002; Gucciardi & Dimmock, 2008). Mindfulness may help athletes stay focused, leading to automatic execution of skills and more consistent performance (Birrer et al., 2012; Gardner & Moore, 2007; Noetel et al., 2017; Sappington & Longshore, 2015).

Our study aimed to test these causal mechanisms by assessing whether instruction and practice in mindfulness and acceptance lead to more mindful awareness of the present moment, and whether that awareness translated into improvements in performance and swing mechanics. We measured performance, swing mechanics, mindfulness, and anxiety at three time points: at baseline and during two rounds of a putting competition. Before the first round of the competition, we randomised participants into either a mindfulness intervention or an attention control. Groups then crossed over before the second competition.

We hypothesised a group by time interaction for performance, swing mechanics, and state mindfulness. That is, we expected increases in these variables when each group received the mindfulness intervention, but not when that group received the attention control. Because mindfulness explicitly promotes the acceptance of anxiety, we did not predict an interaction effect between group and time for anxiety, but an increase from baseline for both groups during both competition rounds. Finally, we hypothesised the effect of the intervention on the primary outcome would be mediated by increases in state mindfulness. That is, we expected increases in state mindfulness would explain the relationship between the mindfulness intervention and performance.

Methods

Trial Design

This study used a randomised crossover design over three rounds of a putting task. After familiarisation and the baseline round, participants were individually randomised into either Group A or Group B using a computer generated sequence: the block randomizer in Qualtrics (http://www.qualtrics.com). Group A completed a mindfulness intervention before a competition round. That group then completed the control intervention before another competition round. For Group B, the order of mindfulness and control were reverse. Allocation was concealed; it could not be foreseen by the experimenters because it was done on the iPad immediately before exposure to either intervention.

Participants

Sample size. We determined sample size a priori by conducting a power analysis using G*Power 3 (Faul, 2007). Golf putting studies with similar measurement methods and brief psychological interventions have shown moderate to large effect sizes (partial eta squared = .22, Beilock et al., 2006; partial eta squared = .30, Gucciardi & Dimmock, 2008). To achieve 80% power using conservative assumptions (partial eta squared = .22, no pre-post correlation, mixed ANOVA design) the required sample size was 40 participants. To allow for mediation and moderation analyses, and to complement study using the same participants, we used a substantially larger sample size. This study assumed 80% and a moderate effect size (d = .5; independent groups design), requiring 128 golfers to be recruited (64 in each of two conditions).

Eligibility criteria. The pre-specified eligibility criteria were adults with a golf handicap registered with a club or with Golf Australia. After registering the trial protocol, we made a small change to these eligibility criteria in order to recruit a sufficient size sample within a reasonable time period. We included adolescents (age 13-17) with registered golf handicaps and adults who did not have a handicap, but had over 5 years golfing experience. We felt these groups would be comparable to adults with registered handicaps, but conducted sensitivity analyses for all outcomes to determine if results differed between participants fulfilling the original and modified inclusion criteria.

Recruitment and testing setting. We contacted golf professionals at clubs in Australia to recruit for the study. We offered players a mechanical analysis of their putting swing using the SAM PuttLab. Pros introduced the project to their players, and most testing was conducted in a quiet area at their club (e.g., a room in the clubhouse). Some players were recruited through word of mouth and were tested in a quiet room at our university campus.

Interventions

The two conditions contained different content. The experimental intervention was designed to increase mindful awareness. The attention control was designed to entertain a general audience using no mindfulness-related content. To eliminate the mechanism of delivery as a confounding variable, intervention and control conditions followed the same format. Both conditions involved a three-minute video, two paragraphs of text, and a three-minute audio file. Conditions were both delivered by an iPad through Qualtrics.

Mindfulness intervention. The mindfulness and acceptance intervention was based on Acceptance and Commitment Therapy (Hayes et al., 2011) and

Mindfulness-Acceptance-Commitment protocol for sport performance enhancement (Gardner & Moore, 2007). Both approaches emphasise psychoeducation as a first step. The video provided information about how efforts to control unwanted thoughts or feelings can lead to unhelpful, self-focused attention (Gardner & Moore, 2007). It also described how acceptance of thoughts and feelings allows athletes to more easily control their attention (Gardner & Moore), and it identified the benefits of focusing attention on an external stimulus (Beilock et al., 2002). Golfers then listened to a guided mindfulness meditation: The Brief Centering Exercise from Gardner and Moore (2007). The full transcript is available via the Open Science Framework at osf.io/natj4.

Attention control. The control condition was a segment from a documentary about an Australian golfer. The documentary included interviews about the personal character of the golfer without providing any technical advice. We chose this attention control because it followed a similar interview format to the mindfulness intervention video and it was likely to be interesting to golfers without targeting their putting technique. This transcript of the video is also available via the Open Science Framework at osf.io/natj4.

Competition-based pressure manipulation. Participants were told the final two rounds of putting – those after the intervention and control conditions – were part of a competition. They were told their performance would be compared with other golfers, including those from their club, and they would get $50 for winning the competition. This procedure has been shown to increase anxiety and cause pressure-induced performance decrements in previous studies of golf putting (Gucciardi & Dimmock, 2008).

Outcomes

The primary outcome was putting performance where better putts are those that finish closer to a target. We also measured mechanics to see whether there were any differences in the consistency or movement patterns of the putts. Finally, we examined state mindfulness as a presumed mediator of outcomes, and anxiety as a manipulation check.

Putting performance. To create a challenging task in a controlled environment, we based our performance measure on a series of other studies that asked golfers to putt a ball as close as possible to targets on synthetic turf (Beilock et al., 2002, 2006). In our study, the targets were at 1-foot intervals between 8 and 12 feet. Rather than using a typical golf hole, targets were painted because they needed to be in a straight line for the SAM PuttLab data to be valid. For each round, we asked golfers to aim at the five targets in a quasi random order, two putts per target, that was consistent for each participant. Over the centre target, we suspended a camera used to record footage of each putt. This footage was imported into Kinovea software (Version 0.8.15; 2016) by an external research assistant, blinded to the experimental hypotheses. This assistant used the software to measure the radial distance between each target and the final resting place of each putt to the nearest centimetre. We used these distances as a measure of performance, with lower overall error indicating better performance. These measurements were very reliable, with almost perfect agreement between scores from two independent coders ($r > .99$). They were also valid, with unique variance explained by both handicap ($b = .31$, $p < 0.001$) and swing mechanics ($b = -.18$, $p = 0.02$). These findings mean that better golfers (quantified by lower handicap) and those with better swing mechanics (quantified by the SAM PuttLab) performed better on our task (i.e., had lower error).

Swing mechanics. To assess whether the intervention enhanced swing mechanics, we used the data from the SAM PuttLab (Science & Motion, 2016). The SAM PuttLab uses ultrasound from a lightweight sensor to determine the position and rotation of the club throughout the swing. As a result, measurement is very precise; when analysing a robot putter, assumed to be the gold standard of consistency, the standard deviations of PuttLab measurements were less than 0.1° (Marquardt, 2007). The device has been used in previous studies to detect the influence of conscious control on putting kinematics (Toner, Moran, & Jackson, 2013). The software uses this data to compare the golfer's performance with mechanics from a sample of Professional Golf Association tour players (Marquardt, 2007). An overall score is computed for the series of ten putts, with a set of 10 perfect putts receiving a score of 100. The overall score is comprised of three components:

- timing – the degree to which the athlete emulates a professional golfer's timing;
- tendency – the degree to which the putter faces the target at key moments; and
- consistency – the degree to which the player reproduced a consistent stroke.

State mindfulness. To test the hypothesis that the mindfulness and acceptance intervention increases performance by changing non-judgmental awareness of the present moment, we used the state Mindful Attention Awareness Scale (Brown & Ryan, 2003). The six items on this scale (e.g., in the last 15 minutes "I found myself preoccupied with the future or the past") provide an indication about the degree to which people are lost in thought or acting mindlessly. The items are reverse scored to operationalise mindfulness. In other athletic samples, this measure showed good internal consistency ($\alpha = 0.87$) and concurrent validity with significant associations in predicted directions for anxiety, flow, and sport-specific mindfulness subscales (Thienot et al., 2014). In our sample the questionnaire showed good internal consistency ($\alpha = 0.87$).

State anxiety. To check whether the competition-based pressure manipulation induced feelings of competitive anxiety, we used the short form of the Competitive State Anxiety Inventory-2 from Cox, Russell, and Robb (1998). This short form contains three items for each of two subscales: somatic (e.g., "I feel nervous") and cognitive anxiety (e.g., "I am concerned about this performance"). For each subscale, the short measure explains 80-85% of the variance in the longer measure, which involves nine items per subscale (Cox et al., 1998). The Competitive State Anxiety Inventory-2 has demonstrated good internal consistency and construct validity demonstrated by to increased scores under pressure and decreased scores following meditation (Muangnapoe, 1998). In our sample the short measures demonstrated acceptable internal consistency ($\alpha_{somatic} = 0.68$; $\alpha_{cognitive} = 0.75$).

Blinding

Participants, coaches (i.e., golf professionals), and outcome assessors were blinded to the hypothesis of the study. They were told the study examined 'the influence of pressure on putting biomechanics'. While the personnel responsible for developing the intervention were aware of the hypotheses, the intervention was delivered online, so participants were unlikely to be affected by demand characteristics.

Procedure

After participants were briefed about the purported purpose of the study, they provided written consent. They then completed baseline psychological measures and a practice putting round. The researcher then answered any questions before participants completed baseline putting without pressure. Participants were then randomly allocated to either the mindfulness or control condition. After completing the intervention or viewing control media, they completed post-test measures and the putting task under pressure. They then crossed over to the other condition, followed by post-test measures and putting for the final time. Participants completed the other study on the iPad before being debriefed.

Statistical Methods

As described in our registered protocol (Noetel, 2016), we used the lme4 package (Bates, Mächler, Bolker, & Walker, 2015) in R (R Core Team, 2016) to construct linear mixed models to test hypotheses. To test group by time interactions, we compared models with and without the interaction term, then used likelihood ratio tests to compare model fit (Bates et al., 2015). To look for main effects, we compared models with and without the predictor variable (e.g., time), and used likelihood ratio tests to compare model fit. For models with putting performance as the dependent variable, we nested putting attempts within people over time. For the models with stroke mechanics, mindfulness, or anxiety as the dependent variable, we nested scores within people over time. We controlled for the family-wise error rate by adjusting the alpha for each set of tests by the number of comparisons using the Bonferroni correction ($\alpha_c = 0.05/n_{comparisons}$).

To control for skill, we entered handicap (where available) into models and retained it in the model if fit indices improved. We did the same for mindfulness practice and for dispositional mindfulness. When the intervention had a significant effect on a hypothesised mediator and an outcome, we used the R mediation package (Tingley, Yamamoto, Hirose, Keele, & Imai, 2014) to test the significance of mediation effects. Finally, we conducted sensitivity analyses by exploring all models using five sub-samples of the participants. In these subgroups: we separately analysed those who currently meditated and those who did not, we excluded participants without a registered handicap, excluded participants who missed any part of the protocol, and excluded participants with handicaps greater than 10.

Results

Recruitment and Descriptive Statistics

Between October 2016 and November 2017, we recruited 118 participants who met the modified inclusion criteria (i.e., golfers with registered handicap or adult golfers with at least 5 years of golfing experience). See Figure 3.1 for full participant flow. Using the parameters from our power calculation, this sample size makes our study sensitive enough to detect small effects (partial eta squared = .03). It may not have sufficient power to assess mediation and moderation effects. Of golfers who met inclusion criteria, 75% were male (n_{male} = 88; n_{female} = 29; one did not disclose gender). There was a wide range of ages (M_{age} = 51.19; SD_{age} = 16.48) and golfing abilities ($M_{handicap}$ = 14.48; $SD_{handicap}$ = 10.09). Most participants had substantial experience in golf ($M_{years\ playing}$ = 21.86; $SD_{years\ plaing}$ = 14.96), but only 23% currently practised meditation. Of those practicing meditation, the average number of years meditating was 6.46 ($SD_{years\ meditating}$ = 7.29). A full list of descriptive statistics are available in Supplementary Table 3.1. Consistent with open science recommendations (Tamminen & Poucher, 2018), our cleaned data and analysis code is also available on the Open Science Framework at osf.io/natj4.

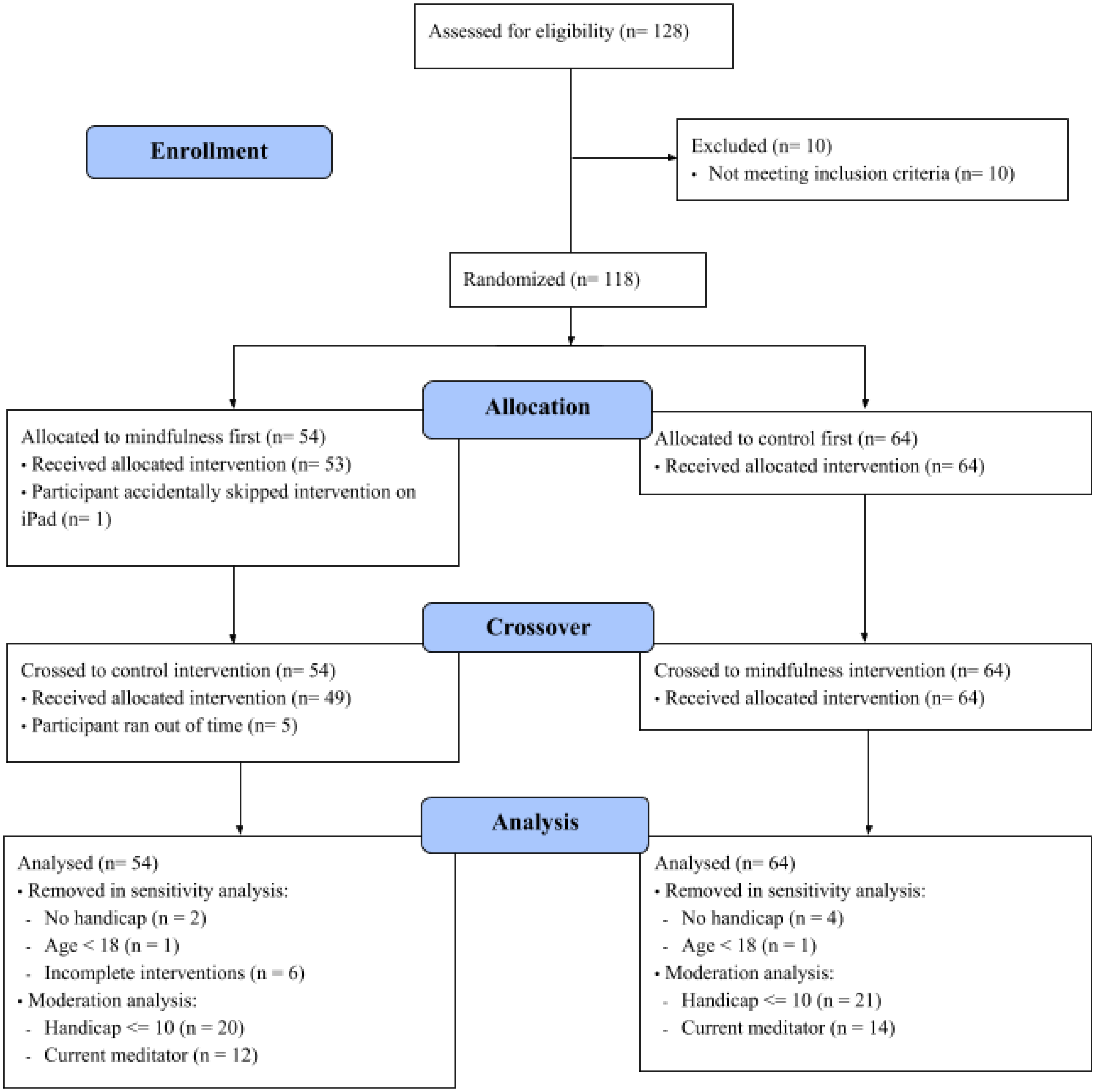

Figure 3.1. CONSORT flow diagram indicating participant flow throughout the procedure

Performance

Means and standard deviations for all outcome variables are available in Table 3.1. There was no significant group by time interaction for performance ($\chi^2(2) = 0.55$, $p = 0.76$), indicating the mindfulness intervention had no significant effect on putting accuracy. These findings were robust to sensitivity analyses, where we only included experienced golfers, participants who completed all aspects of the protocol, meditators, non-meditators, and those who met the original inclusion criteria (See Supplementary Table 3.2). Adding handicap to the model increased model fit ($\chi^2(1) = 12.92$, $p < 0.001$), but doing so did not reveal a significant group by time interaction ($\chi^2(2) = 0.56$, $p = 0.75$). Neither trait mindfulness nor years meditating explained significant variance in performance.

Table 3.1
Means and standard deviations of outcome variables for each randomised group at baseline, after first intervention, then after crossover

Outcome Variable	Group A						Group B					
	Baseline		After Mindfulness		After Control		Baseline		After Control		After Mindfulness	
	M	SD	M	SD	M	SD	M	SD	M	SD	M	SD
Putting Error (radial error in cm)	24.86	6.67	24.80	7.59	24.43	6.81	26.03	8.74	26.92	7.44	25.81	7.25
SAM PuttLab Overall %	61.80	9.74	62.61	9.76	61.10	9.99	60.47	8.62	59.53	10.86	61.46	9.38
PuttLab Tendency %	63.41	15.10	65.44	14.89	63.26	14.50	62.88	13.01	61.21	14.99	64.90	12.47
PuttLab Timing %	71.15	11.29	71.63	10.97	70.88	11.67	71.24	12.16	71.13	11.83	71.62	11.28
PuttLab Consistency %	56.32	11.86	56.69	12.90	55.13	13.71	53.87	12.00	52.88	14.78	54.64	13.20
State Mindfulness / 30	13.20	6.67	11.62	4.84	10.38	4.90	14.53	6.05	11.75	6.41	9.52	4.87
State Cognitive Anxiety / 9	4.65	1.81	4.74	1.76	4.63	1.94	4.64	1.93	4.66	2.00	4.47	1.83
State Somatic Anxiety / 9	3.82	1.15	4.09	1.31	3.67	1.02	3.92	1.19	3.67	1.01	3.53	0.87

There was a significant association between better performance on the putting task (lower error) and better overall swing mechanics ($b = -.31$, $p < 0.001$). Follow-up analyses revealed significant associations for swing tendency ($b = -.13$, $p = 0.01$) and consistency ($b = -.27$, $p < 0.001$), but not timing ($b = .02$, $p = 0.80$). Golfers were more accurate when their mechanics were on target and consistent. There was also a relationship between lower putting error and lower handicap ($b = .36$, $p < 0.001$), indicating that better golfers tended to do better at our putting task. Multiple regression indicated that both handicap ($b = .31$, $p < 0.001$) and swing mechanics ($b = -.18$, $p = 0.02$) explained unique variance in our putting task. There was no association between putting performance and either state mindfulness ($b = -0.06$, $p = 0.60$) or any of trait variables from the Mindfulness Inventory for Sport ($b_{awareness} = 0.09$, $p = 0.48$; $b_{nonjudgment} = 0.04$, $p = 0.71$; $b_{awareness} = -0.13$, $p = 0.41$). Golfers who were more mindful at the time of measurement, or more mindful in their usual sporting performance, did not perform better than those who were less mindful.

Swing Mechanics

There was a significant group by time interaction for swing mechanics ($\chi^2(2) = 6.77$, $p = 0.03$). Controlling for time and baseline mechanics, mindfulness led to 1.87 point increase on the SAM PuttLab (*SE* = 1.01; points scored out of 100) compared with the control condition. These findings were consistent in the subsample of participants who met original inclusion criteria and those who completed the intervention as per the protocol. However, the interaction was not significant for skilled golfers. For skilled golfers, mean differences were smaller but in the same direction (e.g., difference between mindfulness and control at first competition: $M = 0.72$, $SE = 1.43$). Adding handicap to the model increased model fit ($\chi^2(1) = 10.62$, $p = 0.001$), but did not influence the group by time interaction ($\chi^2(2) = 6.76$, $p = 0.03$). Neither trait mindfulness nor years meditating explained significant variance in swing mechanics.

Exploratory analysis of swing mechanics. We had no a-priori hypotheses regarding which component of the SAM PuttLab analysis would respond to the intervention. However, following the significant effects on overall swing mechanics described previously, we analysed data separately for the three metrics that make up the overall rating (timing, tendency, consistency). We found no significant group by time interaction for timing ($\chi^2(2) = 2.46$, $p = 0.29$) or consistency ($\chi^2(2) = 2.96$, $p = 0.23$). We found a significant group by time interaction for tendency ($\chi^2(2) = 10.06$, $p = 0.006$), such that players receiving the intervention were more likely to keep their putter square with the target at key moments (e.g., at address, at impact). This pattern was not apparent in the skilled golfers ($\chi^2(2) = 1.33$, $p = 0.51$).

State Mindfulness

There was no significant group by time interaction for state mindfulness ($\chi^2(2) = 4.25$, $p = 0.12$). None of the proposed moderators (handicap, trait mindfulness, or meditation experience) explained significant variance in state mindfulness. There was, however, a significant main effect for time ($\chi^2(2) = 55.60$, $p < 0.001$) such that mindfulness decreased over time (see Table 1). These findings were robust to sensitivity analyses such that main-effects and interactions were consistent for all subgroups. We did not conduct mediation analyses because there was no significant effect of the intervention on either mindfulness or performance.

Cognitive Anxiety

There was no significant main effect of time for cognitive anxiety ($\chi^2(2) = 2.22$, $p = 0.33$), indicating the competition had no effect on anxious thinking. There was also no group by time interaction ($\chi^2(2) = 0.16$, $p = 0.92$), suggesting no difference between the mindfulness and control interventions. None of the proposed moderators explained significant variance in cognitive anxiety. These findings were robust to sensitivity analyses.

Somatic Anxiety

There was a significant main effect of time for somatic anxiety ($\chi^2(2) = 13.45$, $p = 0.001$), such that somatic anxiety decreased for both groups. There was also a significant group by time interaction ($\chi^2(2) = 9.04$, $p = 0.01$). The group allocated to the mindfulness intervention reported higher anxiety before the first competition ($M_{difference} = 0.52$, $SE_{differerence} = .17$), but this difference was negated when the other group received the mindfulness intervention too ($M_{difference} = 0.23$, $SE_{differerence} = .17$). These findings were consistent in the subsample of participants who met original inclusion criteria and those who completed the intervention as per the protocol. However, neither main effect nor interaction were significant for skilled golfers. For skilled golfers, mean differences were in the same direction and similar in scale (e.g., difference between mindfulness and control at first competition: $M = 0.57$, $SE = .30$), suggesting lack of significance may be due to the reduced sample size. None of the proposed moderators explained significant variance in somatic anxiety.

Discussion

Using a double-blind, randomised trial design, we tested the effects of a short mindfulness intervention on golf mechanics and performance. Our study had enough power to detect small effect sizes. Nevertheless, we found no significant influences of the mindfulness and acceptance intervention on performance. The intervention improved swing mechanics. Specifically, golfers in the intervention group had better tendency, meaning their putter was more likely to be square to the target at key moments (e.g., at impact). We found no effect of the intervention on state mindfulness or cognitive anxiety. There was a significant effect on somatic anxiety, where those receiving the mindfulness intervention felt more nervous during the competition than those who received the control intervention.

While there is widespread optimism about the benefits of mindfulness (Coronado-Montoya et al., 2016), a number of recent high-quality studies have failed to find significant results on their primary outcome. A brief mindfulness intervention had no significant effect on isometric plank performance compared with listening to an audiobook (Stocker, Englert, & Seiler, 2018). An intervention with one week's practice failed to improve working memory or mind wandering compared with relaxation training (Banks, Welhaf, & Srour, 2015). A pre-registered study with six weeks of mindfulness found equivalent gains in mindfulness from the Headspace app and a sham meditation program (Noone & Hogan, 2018). An eight-week, double-blinded randomised trial found no differences on sustained attention performance between eight weeks of Mindfulness-Based Stress Reduction and the attentional control, which contained music therapy, nutrition education, physical activity, and a 'spa day' (MacCoon, MacLean, Davidson, Saron, & Lutz, 2014). Our study adds to a growing list of studies with high internal validity, but no significant effects of mindfulness on their primary outcome.

While golfers did not become more accurate with their putting, they did improve their swing mechanics. Mindfulness did not improve consistency or timing of their stroke, but it did lead to better tendency (i.e., more accurate aim at key points of the stroke). This may be due to the explicit cue in the intervention to direct attention toward the target. Alternatively, it may be that acceptance allows for increased task focused attention due to reduced self-focused attention. Reinvestment theory proposes that decreased self-focused attention would lead to increases in performance (Masters & Maxwell, 2008). While this was not the case for our primary outcome, positive effects on golfers' tendency may be consequential for long-term performance, assuming effects are maintained with continued mindfulness practice. Even small amounts of explained variance in technique can lead to big differences in performance over a sporting season (Abelson, 1985), like decreases in handicap, would be better than our short-term putting task for assessing whether changes in mechanics lead to ongoing improvements in performance. Nevertheless, given the brief nature of the intervention, we chose our method of operationalising performance for a number of reasons. It was sensitive to both aim and distance-control, it was precise to the nearest centimetre, and other manipulations have demonstrated acute changes in performance outcomes (Beilock et al., 2002, 2006).

Mindfulness interventions may not be a panacea, and may instead have effects on a narrow set of variables. As noted earlier, brief mindfulness studies have shown significant effects on a range of variables (Levin et al., 2012), including reduced anxiety (Hooper et al., 2011), reduced stereotype threat (Weger et al., 2012), and better attentional control (Mrazek et al., 2012). It is possible that these variables, like swing mechanics, are more amenable to change from short mindfulness interventions compared with sporting performance. Performance at golf putting has been shown to respond to interventions, with similar study designs finding benefits from brief interventions like distraction (Beilock et al., 2002) and cue words (Gucciardi & Dimmock, 2008). As a result, mindfulness may have smaller effects than these alternate interventions, or may require a higher intervention dose to achieve equivalent benefits.

Mindfulness studies that have found performance benefits lasted eight weeks or more (e.g., Zhang, Si, et al., 2016). While a dose response relationship is often presumed (Goyal et al., 2014), moderation effects in meta-analyses show the influence of intervention length is negligible (e.g., $\beta = .01$, Bassam Khoury et al., 2015; r = .05, Sedlmeier et al., 2012). For example, some high-quality studies that last eight weeks do not find effects (MacCoon et al., 2014), and some studies that last five minutes do (Weger et al., 2012). While insufficient dose is a plausible limitation of our design, trials of longer interventions need study designs that are less susceptible to bias than have previously been conducted (Noetel et al., 2017).

As mentioned earlier, blinding is a strategy that reduces such bias (Pildal et al., 2007). Without blinding or active control conditions, positive expectations may be a confounding factor that could explain results (Banks et al., 2015). These expectancy effects may have played a role in previous studies that showed widespread benefits from mindfulness (Goyal et al., 2014; Khoury et al., 2015; Noetel et al., 2017). While our control condition was not designed to have a therapeutic effect, the documentary may have provided a distraction, relaxation, or vicarious experiences of success by hearing about a talented Australian golfer. Regardless, if such control conditions show equivalent effects on mindfulness compared with an intervention designed to target the construct, then it raises doubts about the unique value of mindfulness interventions over those with innocuous content.

The content of a mindfulness intervention may explain heterogeneity in effects because different interventions have different defining features (e.g., narrow vs. diffuse attention; for other defining features, see Van Dam et al., 2018). The content of our intervention was drawn from two seminal texts on mindfulness and acceptance approaches (Gardner & Moore, 2007; Hayes et al., 2011). Nevertheless, using components of those texts for a short intervention requires some experimenter judgement. We followed guidelines in those texts to prioritise acceptance and mindfulness; however, focusing on those components for all participants may not have been as effective as tailoring content to the participant, or using a more diffuse intervention that included a wider range of components.

While our pressure manipulation followed a procedure previously used in golf putting (Gucciardi & Dimmock, 2008), we found lower anxiety for our competition rounds than the baseline round. We also found lower mindfulness in both competition rounds. These findings may be due to the high level of baseline perceived evaluation when being assessed via the SAM PuttLab. Measurement via the PuttLab was novel for most players and they may have been keenly aware of the evaluative nature of the procedure, even before the competition began. Despite our attempt to mitigate this effect with a familiarisation round before baseline, ongoing familiarisation with the apparatus may explain the decreases in anxiety over time. Our findings may not be generalisable to more authentic pressure situations.

The authenticity of the putting task may also mean findings do not generalise to more externally valid environments. We sacrificed external validity in order to make the measurement and intervention more tightly controlled. While the internal validity of this study is a strength, particularly given the limitations of previous literature, the findings may not generalise to more authentic implementations of both the training and assessment. For example, a more typical, extended mindfulness intervention with performance assessed across multiple, competitive games of golf would allow for more externally valid assessment of the intervention.

We also found an interaction between group and time, showing increased somatic anxiety when participants received mindfulness before their first competition. Mindfulness approaches often promote the acceptance of anxiety as a normal human experience (Gardner & Moore, 2007; Hayes et al., 2011). Given our intervention emphasised the importance of accepting anxiety, it is plausible the increased somatic anxiety reported following mindfulness may represent an increased willingness to acknowledge or report the emotion. Increased willingness to experience emotions is often considered a beneficial therapeutic goal (Hayes et al., 2011; Levin et al., 2012); however, to confirm this hypothesis, future research could explore the influence of mindfulness interventions on acceptance and willingness more specifically, and the mediating effects of willingness on behavioural outcomes like performance.

Conclusions

"More rigorous studies are more likely to yield results that are closer to the truth." (Higgins & Altman, 2008, p. 188). For decades, reviewers have lamented the rigor of intervention research in sport and exercise psychology (Vealey, 1994). Rigor can be increased at little or not cost via transparent reporting of methods and preregistration (Tamminen & Poucher, 2018). Yet, few researchers have used these strategies in the mindfulness or sport psychology literature thus far (Farias & Wikholm, 2016; Noetel et al., 2017; Tamminen & Poucher, 2018; Van Dam et al., 2018). As far as we are aware, ours is the first prospectively registered, double-blind randomised trial assessing mindfulness in sport. We found mindfulness significantly improved golfer's aim, but had no immediate influence on putting performance. While we cannot rule out the benefits of long-term mindfulness training, our mixed findings raise doubts about the performance benefits of brief mindfulness interventions for athletes.

Protocol and Registration

The protocol for this study was prospectively registered at the Australia and New Zealand Clinical Trials Registry (www.anzctr.org.au): ACTRN12616001252404.

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Linking Chapter—From a Randomised Controlled Trial to a Substantive-Methodological Synthesis

One of the core limitations of the randomised trial in Chapter 3 was the brevity of the intervention. Short context-focused interventions have been shown to change mindfulness and wellbeing in the past. Similarly short psychological interventions have shown significant improvement in putting performance. Nevertheless, it is possible that mindfulness and acceptance approaches are more likely to be beneficial once the skill has been practiced over time.

In athletic populations, one of the barriers to longer studies is the burden associated with repeated assessment (Saw, Main, & Gastin, 2015). Athletes are already saturated by questionnaires. More than half of high-performance athletes in Australia and New Zealand complete questionnaires every day (Taylor et al., 2012). This questionnaire burden can preclude athletes from participating in additional research, particularly when it requires completing assessments repeatedly.

Repeated assessments are important because they allow researchers to test the causal model underlying a psychological intervention. As mentioned earlier, the model underlying mindfulness interventions is that training improves trait mindfulness, and that trait mindfulness improves performance (Birrer et al., 2012). As identified in the systematic review, this model has yet to be thoroughly explored amongst athletes.

Our substantive-methodological synthesis aims to facilitate this exploration. The methodological aim is to demonstrate the use of machine learning techniques for abbreviating athletic questionnaires. These methods may help alleviate the heavy response burden shouldered by athletes (Taylor et al., 2012). The methods may facilitate flexible, efficient, and cheap data collection (Basarkod et al., 2018) amongst athletes while maintaining reliability and validity of those measurements (Sahdra, Ciarrochi, Parker, & Scrucca, 2016). The substantive aims involved taking the shortest questionnaire of mindfulness in athletes and making it even shorter with minimal loss in reliability and validity. Doing so is likely to make it easier for researchers to assess in various context. It also aimed to briefly assess the usefulness of three components of mindfulness to predict objective measures of performance.

Chapter 4—Using Genetic Algorithms to Abbreviate the Mindfulness Inventory for Sport: A Substantive-Methodological Synthesis

Abstract

Objectives. To demonstrate the use of machine-learning for reducing questionnaire response burden, we created multiple, shorter versions of the Mindfulness Inventory for Sport. We then tested the reliability and validity of scores derived from these shorter versions in athletic populations.

Design. We used genetic algorithms to shorten the measure, and both cross-sectional and longitudinal data to test psychometric properties.

Method. We collected data from 859 undergraduate exercise science students and 118 golfers. We used 75% of the student sample to shorten the measure, and the rest of the data to test the internal consistency, test-retest reliability, content validity, and factorial validity. For criterion validity, we explored relationships between the subscales and other measures of mindfulness, golf handicaps, and an objective measure of putting accuracy.

Results. Genetic algorithms efficiently generated stable solutions to shortening the measure. Reliability decreased as the measure become shorter—especially between three and two items per subscale—but remained acceptable. Validity metrics for shorter versions were as good, and sometimes better, than the full questionnaire. Awareness and refocusing subscales demonstrated weak associations with golf handicap for long and short versions. Non-judgment showed no significant associations, and no subscales significantly predicted putting performance.

Conclusions. Genetic algorithms provide efficient solutions to reducing questionnaire response burden for athletes.

Introduction

Self-report measures are the most common form of assessment in athletic populations (Halson, 2014). They are simple and inexpensive ways of measuring many constructs (Saw et al., 2015). As a result, athletes complete many questionnaires. For example, of 55 high-performance sport programs in Australia and New Zealand, 55% administered questionnaires on a daily basis, with 97% doing it at least once per week (Taylor et al., 2012). However, when coaches, athletes, and sport scientists were surveyed about faithful completion of questionnaires, one of the most common barriers was questionnaire length (Saw et al., 2015).

Participants are more likely to complete questionnaires, and complete them faithfully, when measures are brief. In a meta-analysis of studies that assessed response burden, shorter questionnaires had higher response rates (Rolstad, Adler, & Rydén, 2011). In another systematic review of survey design factors, Fan and Yan (2010) reported an inverse relationship between questionnaire length and completion rate. Incentives can increase response rates, but longer questionnaires often require commensurate incentives (Sahdra et al., 2016). Shorter measures, therefore, can make data collection more affordable. In a randomised controlled trial comparing incentives, reminders, and shortened questionnaires, the only cost-effective strategy was an abridged questionnaire following a reminder (Glidewell et al., 2012). In clinical settings, short measures can reduce the in-session time required for clients to complete measures, and they can increase compliance with measures assigned between sessions (Basarkod et al., 2018). Overall, short measures can allow for more flexible, efficient, affordable, and accurate data collection.

Short measures also facilitate many core aims of science, including description, prediction, and causal inference. Short questionnaires allow for more constructs to be assed in the same battery (Basarkod et al., 2018). This process has helped to describe patterns across nations through international surveys like Programme for International Student Assessment (Kaplan & Su, 2016) and the European Social Survey (Davidov, Schmidt, & Schwartz, 2008). It has helped with prediction, where outcomes are often predicted more accurately by including a large number of heterogeneous constructs than a small number from within one theory (Parker, Jerrim, Chmielewski, & Marsh, 2017). Short measures also help determine causality. Causal models can be strengthened if researchers assess the gamut of variables that influence an outcome (Pearl, 2009). In intervention research, for example, brief measures allow researchers to more easily assess a larger number of baseline characteristics, mediators, and outcomes. So, short measures are not just pragmatic, but helpful for many of the core goals of sport and exercise psychology research.

Although they are useful tools for achieving important goals, many short measures fail to meet established psychometric standards (Smith et al., 2000). They “have frequently been developed without careful, thorough examination of the new form's validity” (Smith et al., 2000, p. 102). Often, the final size of the measure is arbitrary, rather than being based on theoretical or empirical grounds. One problem is that traditional approaches to shortening measures require a delicate balance of competing factors. These approaches find the items that balance high item-total correlations, low cross-loadings, low correlated uniqueness, low chance of missingness, high face validity of construct coverage, and high internal consistency of the resulting scale (Marsh et al., 2005). This process asks researchers to consider seven different elements simultaneously, meaning that the possible combinations are beyond human calculation, even for relatively small measures. When reducing a 15-item scale to six items, there are over 2,500 possible combinations to consider. The complexity means researchers have to rely on unwritten idiosyncratic heuristics, frequently leading to poor measures (Smith et al., 2000). Even when using Item Response Theory to shorten questionnaires, researchers must make a series of judgments on the basis of a large number of parameters (Fletcher & Hattie, 2004).

Genetic Algorithm Approach

An efficient alternative to these approaches is to use machine learning to find the items that explain the most variability in the full measure (Sandy et al., 2014). Genetic algorithms were first introduced to solve problems related to pattern recognition (Holland, 1975). More recently, machine-learning techniques based on genetic algorithms have been used to create reliable and valid shortened measures of personality (Yarkoni, 2010), values (Sandy et al., 2014), psychopathy (Eisenbarth et al., 2015), experiential avoidance (Sahdra et al., 2016), and body image-acceptance (Basarkod et al., 2018). These algorithms function via principles of evolution and natural selection (Yarkoni, 2010). Items are analogous to the genes, and a series of items that form a scale are analogous to the chromosomes. The algorithm first generates a sample of chromosomes: it creates a random set of subscales from the whole measure. These chromosomes then compete for fitness. For shortening a questionnaire, the fitter chromosomes are those that explain more variability in the full measure. As in natural selection, the least fit chromosomes are removed from the gene pool, and the fittest items—usually the top 5%— reproduce to create a new pool of chromosomes (Yarkoni, 2010).

During reproduction, chromosomes mutate and genes crossover. In mutation, items are randomly substituted into the chromosome. If a 'fit' short version from a 10-item scale were items 1, 2, and 3, then mutation would randomly substitute good items for new ones (e.g., swap item 1 for 7) to see if the measure is improved. In crossover, genes from one fit measure are swapped with genes from another fit measure. If another 'fit' short version contained items 4, 5, and 6, then crossover would test combinations of the two fit measures (e.g., it may try items 1, 4, and 6). Following mutation and crossover, the new pool of chromosomes is tested for fitness, and the process repeats until the algorithm reaches a stable solution. That is, short measures are created, mutated, and recombined until the algorithm finds the short measure that explains the most variability in the full measure (Yarkoni, 2010). The shortened measures from this efficient process often produce scores that are as valid as those derived from traditional psychometric approaches (Sandy et al., 2014).

In software packages that use genetic algorithms to shorten questionnaires, the length of these shortened measures are usually determined in two key ways (Scrucca & Sahdra, 2016). Researchers can either specify the desired length of the short measures, or they can determine an item-cost parameter. In the first method, the algorithm will constrain the length of each chromosome to that provided by the researcher. This process will aim for the measure of that length that best explains the variability in the full measure (e.g., the best 3-item measure). Alternatively, researchers can specify the item-cost parameter. This parameter determines the weight a researcher places on having a shorter measure versus one that explains more variance. These two factors are in competition with each other, because longer measures will naturally explain more variance in the full measure, but shorter measures are desirable for the reasons described earlier. By setting the item-cost parameter, researchers can specify how much variance they are willing to lose by removing an item from the questionnaire. This parameter can be pre-specified, or it can be 'tuned' during pilot testing. Tuning occurs when researchers pilot-test machine learning parameters until the algorithm starts generating a desired solution. Tuning parameters in this way allows researchers to aim for a shortened measure, but set a limit on the amount of variance they are willing to sacrifice in the proces.

To our knowledge, no machine learning approach to questionnaire abbreviation has been demonstrated in sport and exercise psychology. Given the aforementioned response burden placed upon athletes, this study aimed to test the viability of this approach for generating abbreviated athletic questionnaires that meet reliability and validity standards. We chose to apply this method to the assessment of mindfulness in sport for two key reasons. There has been growing interest in the potential of mindfulness for performance enhancement, and there have been nascent explorations of causal relationships between mindfulness and positive athletic outcomes performance. Both of these may be facilitated by shorter measures of the construct.

Mindfulness in Athletes

"I acknowledge the negative thoughts and let them slide by. This lets me focus on what is really important." (Djokovic, 2013, p. 88)

Novak Djokovic's approach to thoughts is characteristic of many mindfulness and acceptance approaches: notice thoughts and feelings, accept those feelings non-judgmentally, and refocus on the task at hand. The definition of mindfulness is a controversial topic (Chiesa, 2012), but for this paper we will use the most common, modern interpretation of the term: non-judgemental awareness of the present moment. This definition covers both the dispositional trait and the state induced through practice (Birrer et al., 2012; Chiesa, 2012). These practices have become increasingly popular among coaches, athletes, and sport psychology researchers. In the last ten years, authors have published over fifty papers on mindfulness in the sporting domain (Noetel et al., 2017). Both experimental and observational research has found mindfulness may help athletes perform better, suffer less anxiety, and experience flow more frequently (Bühlmayer, Birrer, Röthlin, Faude, & Donath, 2017; Noetel et al., 2017; Sappington & Longshore, 2015).

More mindful athletes tended to perform better (Blecharz et al., 2014), reported higher performance (Röthlin, Horvath, Birrer, & Holtforth, 2016), lower anxiety (Röthlin et al., 2016), higher flow (Cathcart et al., 2014; Kaufman et al., 2009; Kee & Wang, 2008; Pineau et al., 2014), higher self-efficacy (Blecharz et al., 2014), fewer intrusive thoughts (Thienot et al., 2014), and less attachment to those thoughts (Zhang, Chung, Si, & Gucciardi, 2016). These correlational studies could not demonstrate that mindfulness was causing these benefits, but favourable results from intervention research have helped to make stronger causal claims. Randomised trials of mindfulness interventions have found increases in performance (Gross et al., 2018; Ojaghi et al., 2013; Zhang, Si, et al., 2016), flow (Aherne et al., 2011; Zhang, Si, et al., 2016), and reductions in burnout (Moen & Wells, 2016). But, other randomised trials have failed to find significant effects of mindfulness on strength (Stocker et al., 2018) and shooting performance (John et al., 2012; Solberg et al., 1996).

Other concerns that may temper enthusiasm are the methodological limitations of the research thus far. Noetel and colleagues (2017) found no studies that met the Cochrane Collaboration's criteria for low risk of bias (Higgins, Altman, Sterne, & on behalf of the Cochrane Statistical Methods Group and the Cochrane Bias Methods Group, 2011). Similar methodological problems have been identified by two other systematic reviews (Bühlmayer et al., 2017; Sappington & Longshore, 2015). These criticisms raise doubts about the ability of existing research to make causal inferences about mindfulness interventions. The causal model of mindfulness would be strengthened by assessing mediators of the intervention and by controlling for confounding variables (Pearl, 2009). Although high-quality randomised trials control for most confounds (Green, 2008), few have controlled for these confounds in mindfulness interventions (Bühlmayer et al., 2017; Noetel et al., 2017; Sappington & Longshore, 2015). Similarly, few studies have looked at mediators between mindfulness and positive outcomes. Josefsson and colleagues (2017) found mindfulness was associated with decreased rumination and increased self-regulation, leading to better coping. Gustafsson and colleagues (2015) tested a different model, measuring trait mindfulness, exhaustion, perceived stress, and affect. They found mindfulness was associated with lower exhaustion, which was partially mediated by reduced negative affect and increased positive affect.

One problem with the latter model, as an example, is that it contradicts a common model proposed by mindfulness and acceptance theories. Mindfulness is not designed to target positive outcomes by changing the frequency and intensity of negative experiences (e.g., affect). Instead, it aims “to promote a modified *relationship* with internal experiences” (Gardner & Moore, 2012, p. 309). As outlined in Figure 4.1, this approach usually manifests in at least one of three ways (Thienot et al., 2014): increased ability to become aware of thoughts and feelings before they influence behaviour (acceptance); increased tendency to accept oneself despite negative thoughts and feelings (non-judgment); and increased ability to refocus attention where it is important (refocusing). Others have hypothesised additional mechanisms by which mindfulness improves performance (Birrer et al., 2012), but for the sake of parsimony we have focused on some core theoretical assumptions (Thienot et al., 2014). If athletes who perform well are more aware, accepting, and nonjudgmental, then it is possible that the three factors are having an influence on performance. Stronger claims about causality could be made if those factors predicted performance controlling for established associations like confidence and anxiety (Woodman & Hardy, 2011). Even stronger claims could be made if mindfulness interventions increased these three variables, and changes in those variables explained improvements in performance.

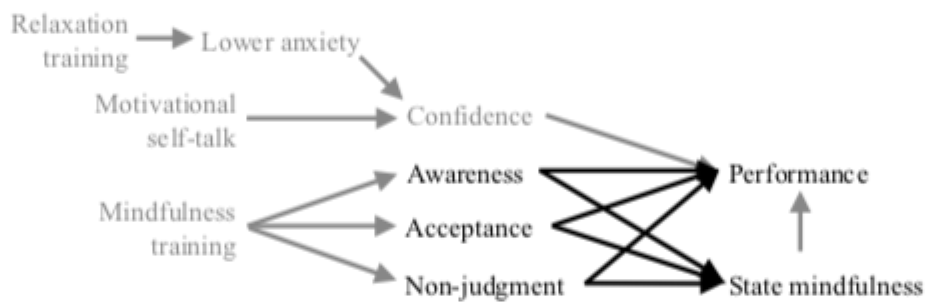


Figure 4.1. A causal model of mindfulness, relaxation, and motivational self-talk. Measured variables presented in black, with unmeasured variables in grey. Black arrows indicate hypothesised correlations tested in this paper. Untested associations in grey.

Rigorously testing causal models in this way would require repeated assessment of multiple constructs. Often, researchers in sport use measures of trait mindfulness from non-sport domains (Five Facet Mindfulness Questionnaire; Baer et al., 2006; e.g., Mindful Attention Awareness Scale; K. W. Brown & Ryan, 2003). Compared with general measures of psychological principles, sport-specific questionnaires tend to better assess psychological constructs in athletes (Moritz et al., 2000). Two measures of athletic mindfulness are the Mindfulness Inventory for Sport (15 items; Thienot et al., 2014) and the Athlete Mindfulness Questionnaire (16 items; Zhang, Chung, & Si, 2017). Both questionnaires assess three constructs, and while each measure is not overly burdensome on its own, reducing the length would decrease response burden and increase the number of constructs that could be measured in parallel, thereby enhancing the quality of research that could be conducted.

As suggested by Smith and colleagues (2000), we started with one of the few mindfulness measures specifically validated for athletic populations: the Mindfulness

Inventory for Sport (Thienot et al., 2014). We aimed to outline the trade-off between brevity, validity, and reliability for measures of various lengths, so researchers may choose the best set of items for their needs. In doing so, the methodological aim of this research was to determine whether genetic algorithms might be a useful tool for reducing response burden in sport and exercise psychology. Finally, a supplemental aim was to assess whether mindfulness constructs fit the assumptions of the causal model proposed by previous researchers (Gardner & Moore, 2012a; Thienot et al., 2014).

Method

We conducted this study in three steps. First, we collected data from two samples of athletic populations, one studying exercise science and one playing golf regularly. Second, we used data from 75% of the exercise science students to shorten the Mindfulness Inventory for Sport using a genetic algorithm. Finally, we tested these shortened measures on the golfers and the other 25% of the exercise science students to establish reliability and validity. Ethics approval was provided by our university's Human Research Ethics Committee.

Participants

Sample 1: Undergraduate Exercise Science students. First-year students (N = 859, M_{age} = 19.57 years; SD_{age} = 3.84 years) studying "Psychology of Sport" in Australia completed the questionnaire battery as a learning exercise, using the cohort's data to write a research report. They provided informed consent online and were allowed to opt out of the study without consequence. Students reported a range of abilities in both athletic (15% elite, 36% sub-elite, 38% club, 11% social, <1% inactive) and academic domains (university entry score out of 100: $M = 65.25$; $SD = 10.9$). Three months later, students were invited to complete the questionnaire battery again. Both administrations were completed in English, during a scheduled tutorial.

Sample 2: Golf sample. We recruited 118 Australian golfers ($M_{age} = 51.19$ years, $SD_{age} = 16.48$ years; 89 male, 29 female) of whom 112 had a golf handicap ($M_{handicap} = 14.48$; $SD_{handicap} = 10.09$). Six golfers did not have a handicap but had more than three years golfing experience**.** Golfers voluntarily signed up for the study in exchange for free biomechanical analysis of their technique using a Science and Motion PuttLab (Science & Motion, 2016), which a research assistant provided upon completion. Measures were administered in English and in a quiet area at each participant's golf club.

Measures

Demographics. Students were asked their age, level of competitive performance, the degree in which they were enrolled, and their university entry score. The golfer demographic questionnaire included age, gender, golf handicap, and years of playing experience.

Mindfulness Inventory for Sport. Our primary measure of interest was the Mindfulness Inventory for Sport (Thienot et al., 2014). This 15-item questionnaire is intended to measure three components of athletic mindfulness: present moment awareness; non-judgementality; and the ability to refocus (see items in Table 4.1). Participants respond on a six-point likert scale from *1 = "Not at all like me"* to *6 = "Very much like me."* It was initially validated with undergraduate students and elite athletes (Thienot et al., 2014).

Table 4.1
Percentage of Runs in which Item Was Selected by Genetic Algorithm Procedure

	Number of items per subscale			
Item	1	2	3	4
Awareness subscale				
I am able to notice the intensity of nervousness in my body.	0%	0%	3%	**100%**
I pay attention to the type of emotions I am feeling.	**100%**	**100%**	**100%**	**100%**
I am aware of the thoughts that are passing through my mind.	0%	0%	**97%**	**99%**
I am able to notice the sensations of excitement in my body.	0%	**100%**	3%	1%
I am able to notice the location of physical discomfort when I experience it.	0%	0%	**97%**	**100%**
Non-judgementallity subscale (reverse scored)				
When I become aware that I am thinking of the final result, I blame myself for not being focused on relevant cues for my performance.	0%	0.0%	2%	**100%**
When I become aware that I am really upset because I am losing, I criticise myself for reacting this way.	0%	**100%**	**99%**	**98%**
When I become aware that I am not focussing on my own performance, I blame myself for being distracted.	**100%**	0.3%	**98%**	2%
When I become aware that I am thinking about a past performance, I criticise myself for not being focused on my current performance.	0%	**100%**	**99%**	**100%**
When I become aware that I am angry at myself for making a mistake, I criticise myself for having this reaction.	0%	0.3%	1%	**100%**
Refocusing subscale				
When I become aware that I am tense, I am able to quickly bring my attention back to what I should focus on.	0%	0%	**98%**	**99%**
When I become aware that I am thinking about how tired I am, I quickly bring my attention back to what I should focus on.	**100%**	**100%**	2%	**100%**
When I become aware that I am not focussing on my own performance, I am able to quickly refocus my attention on things that help me to perform well.	0%	0.2%	**100%**	1%
When I become aware that some of my muscles are sore, I quickly refocus on what I have to do.	0%	**100%**	**100%**	**100%**
When I become aware that I am really excited because I am winning, I stay focused on what I have to do.	0%	0%	0%	**100%**

Note. selected item indicated in **bold**. Choosing emboldened items for one item per subscale gives the recommended 3-item measure. The items under two items per subscale gives the 6-item measure, and so on.

Child and Adolescent Mindfulness Measure. Eighty-five percent of undergraduates were adolescents (21 years or younger), so to test concurrent validity we asked them to complete the Child and Adolescent Mindfulness Measure (CAMM; Greco, Baer, & Smith, 2011). This 10-item, unidimensional measure asks participants about their daily mindfulness (e.g., “I keep myself busy so I don’t notice my thoughts or feelings” [reverse scored]). Participants respond on a five-point likert scale from 0 = “*Never true”* to 4 = “*Always true”*. The Child and Adolescent Mindfulness Measure demonstrated good internal consistency in previous studies (α = 0.81; Greco et al., 2011) and in our undergraduate sample (*α* = .83).

State Mindful Attention Awareness Scale. As an indicator of concurrent validity in the golf sample, we used the 5-item state Mindful Attention Awareness Scale (Brown & Ryan, 2003). Golfers reported their current levels of mindful awareness (e.g., “I found myself doing things without paying attention” [reverse-scored]) on a seven-point likert scale (from *0 = “Not at all”* to *6 = “Very much”*). This measure has shown significant positive associations with trait mindfulness, pleasant affect, perceived autonomy, and negative correlations with unpleasant affect (Brown & Ryan, 2003), and had good internal consistency in our golf sample (*α* = .86).

Putting performance. We measured acute putting performance by asking golfers to hit balls as close as possible to specified targets at various distances (Beilock et al., 2006). After a familiarisation round of 10 putts and completion of the questionnaires, golfers completed 10 putts aiming at five targets in a quasi-random order. Targets were at one-foot intervals from 8 to 12 feet away. A camera was suspended above the targets and accuracy was calculated by importing the footage into Kinovea software (Kinovea, 2016). Performance was operationalised as mean radial error across the 10 putts.

Analysis

Missing data. For cases where there was more than 20% missing data, we excluded the participant under the assumption that he or she abandoned the protocol. For cases with less than 20% missing data, we created 25 multiple imputations to fill the missing data using the Expectation-Maximisation procedure with bootstrapping in Amelia II software (Honaker, King, & Blackwell, 2012).

Genetic algorithm procedure. We used the GAabbreviate package (Version 1.3; Scrucca & Sahdra, 2016) in R software (R Core Team, 2018) to implement the genetic algorithm procedure. As described earlier, the algorithm first generated a population of shorter scales by randomly sampling items from the full scale. We chose a population size of 200 short scales based on recommendations from other implementations of genetic algorithms (Basarkod et al., 2018; Sahdra et al., 2016; Yarkoni, 2010).

Each short scale was then tested for fitness by assessing the percentage of variance it explained in the full scale. The most fit measures were then subjected to crossover and mutation so a new pool of 200 short scales was developed. Following previous authors, we set the probability of crossover and mutation to the defaults of the GA package (Basarkod et al., 2018; Sahdra et al., 2016; Scrucca, 2013).

To choose the best short-measure of a certain length (e.g., nine item measure with three items for each of three subscales), we ran the algorithm 25 times for each of the 25 imputed data sets. Being a stochastic approach, not all runs resulted in identical subscales, so we tallied a vote each time an item was selected at the end of a run. Items were included when they were selected the most frequently at the end of the 625 runs. To get short measures of various lengths (i.e., 5-items per subscale, 4-items, 3-items, 2-items, 1-item), we completed the 625 runs four times, setting the maximum length of the subscale each time. Instead of using cross-validation, we separated the undergraduate population into a training sample (75%) and a testing sample (25%) to mitigate the chance of overfitting the models to our data (James, Witten, Hastie, & Tibshirani, 2013). Item-selection was performed on the training sample, but reliability and validity measures were examined in the testing sample and the sample of golfers.

Reliability and validity. Smith and colleagues (2000) suggested a series of steps to avoid the 'sins of short form development.' The key steps involve: establishing the reliability of each subscale of the short form; looking for overlapping variance with the long form; and confirming the factor structure in the short form. We also aimed to extend some of the validity checks conducted by Thienot and colleagues (2014) by assessing test-retest reliability over three months and predictive validity using objective putting performance.

Reliability. Despite the limitations of Cronbach's α (see Sijtsma, 2009), we calculated it for all subscales because it is the legacy measure of internal consistency. For a more definitive assessment of reliability, we relied on McDonald's omega because it requires fewer assumptions of the data and is more generalisable (Zinbarg, Revelle, Yovel, & Li, 2005). We used test-retest reliability to test temporal stability.

Content validity. For each subscale, we calculated a correlation between the participants' scores on the full scale and their scores on the shorter versions.

Factorial validity. For each short measure, we conducted a confirmatory factor analysis using the *lavaan* package (Rosseel, 2012) in R. We used robust maximum likelihood estimation because it is less sensitive to non-normality and non-independence of observations (Basarkod et al., 2018). To evaluate the fit of the models, we used the χ^2 goodness-of-fit statistics, Tucker-Lewis index (TLI), Comparative Fit index (CFI), root mean square error of approximation (RMSEA), standardised root mean square residual (SRMR) and Aitken's Information Criteria (AIC). We used the criteria from Marsh and colleagues (2005) for close (RMSEA ≤ 0.04; SRMR ≤ 0.08; TLI ≥ 0.99; CFI ≥ 0.99) and acceptable fit (RMSEA ≤ 0.08; SRMR ≤ 0.05; TLI ≥ 0.90; CFI ≥ 0.90).

Criterion-related validity. In the adolescent undergraduate sample, we conducted correlations between the short measures and the Child and Adolescent Mindfulness measure. In the golf sample, we examined correlations between the short measures and the state Mindful Attention Awareness Scale, and examined correlations between the golfers' responses on the measures of mindfulness, their handicaps, and their putting performance.

Results

Consistent with open science principles (Tamminen & Poucher, 2018), we uploaded the raw data and analysis code to the Open Science Framework: https://osf.io/hv5dy/?view_only=31376b1ba4bb48a093c491598efe4002

Missing Data

Of students (Sample 1) who completed the initial battery of questionnaires (i.e., Time 1), 34% (*n* = 296) voluntarily completed the battery three months later (i.e., Time 2). Fewer may have completed Time 2 because tutorial attendance is notoriously lower at the end of the semester than at the beginning. Also, where students used Time 1 data for their own research reports, Time 2 data was used for research purposes only, so they had a less compelling rationale for participation. We explored differences in Time 1 responses between those who did and did not complete the questionnaire at Time 2. We found no significant differences on any questionnaires, on university entry scores, on their enrolled degree, or their level of athletic competition (ps > 0.05). Those who did complete the follow-up questionnaire were about six-months younger than those who did not ($t_{771} = 2.01$, $p = 0.04$). Ten percent of participants had missing data on more than 20% of the items. These responses were excluded under the presumption that students abandoned the procedure. Of those remaining, less than 1% of data was missing.

Genetic Algorithm-Derived Short Measures

We ran the genetic algorithm procedure using the training subsample of undergraduate students ($N = 644$, 75% of undergraduate sample). The selection process for an example run through the algorithm is depicted in Figure 4.2. The results from the 2,500 runs of the genetic algorithm (625 runs to find short measures of each length) are presented in Table 4.1. The items selected for each short measure appear to demonstrate face validity and coverage of the construct. For example, “I pay attention to the type of emotions I am feeling” was consistently selected for the Awareness subscale and appears to cover the essential components of the construct. To assess how well these short measures perform on quantitative measures of reliability and validity, we tested each measure in the undergraduate testing sample and the golfers, with results presented in Figure 4.3 and raw data in Supplementary Table 4.1.

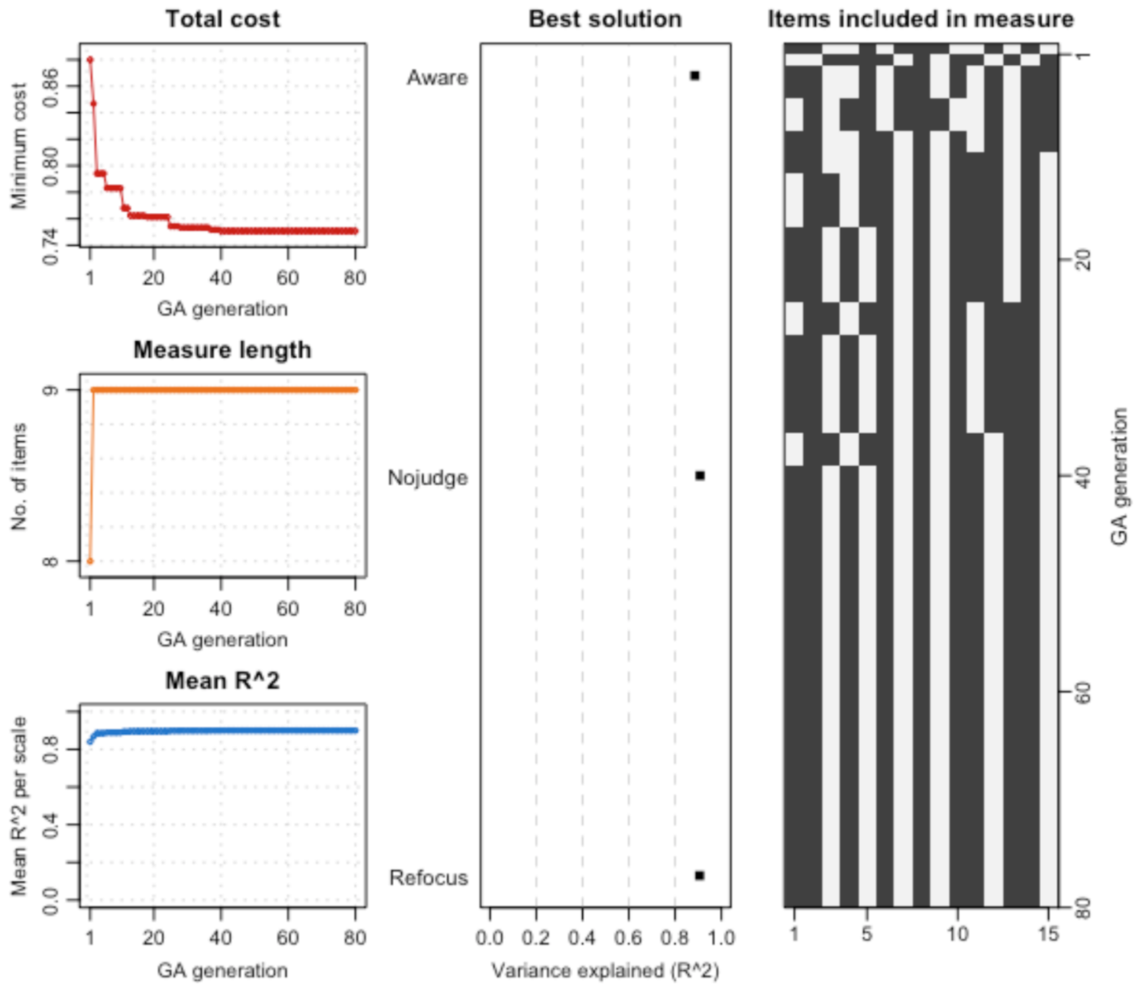
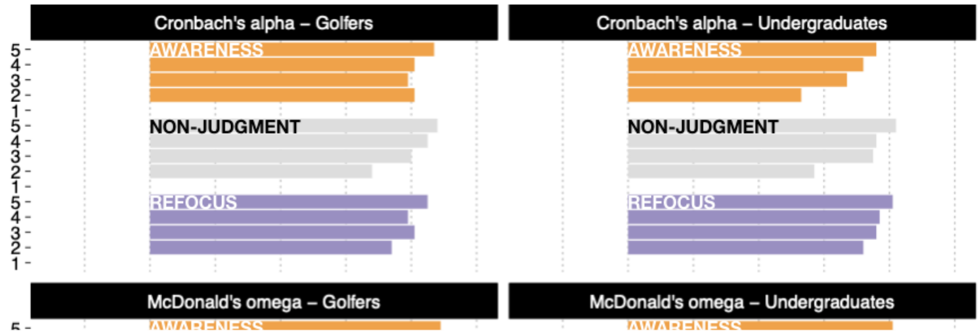


Figure 4.2. Plots demonstrating output from example genetic algorithm run. The left thee plots display improvements across generations for: cost of shortening measure (top), measure length (middle), and variance explained by short measure (bottom). The central plot demonstrates the variance explained by the best solution for each subscale. The right hand plot shows the selected items (in black), with high variation in the first runs (top of plot) and a stable solution emerging after 40 generations (bottom half of plot).


Cronbach's alpha – Golfers
Cronbach's alpha – Undergraduates
AWARENESS
NON-JUDGMENT
REFOCUS
McDonald's omega – Golfers
McDonald's omega – Undergraduates

Reliability and Validity

Reliability. In the undergraduate sample, omegas for the 5-, 4-, 3-item subscales showed acceptable reliability ($\omega \geq 0.7$) for all subscales. For the 2-item subscales, we found acceptable reliability for refocusing, but poor reliability for the other two subscales ($\omega_{awareness} = 0.55$, $\omega_{non\text{-}judgment} = 0.57$). Cronbach's alpha largely mirrored this pattern. In the golf sample, omega for the non-judgment subscale was borderline using 2-items ($\omega_{non\text{-}judgment} = .68$), but acceptable for all other subscales and all longer measures.

Test-retest reliability. In the undergraduate sample, correlations between mindfulness subscales at the start of the semester and those three months later were all strong ($.43 \leq r \leq .61$). While correlations were somewhat stronger when using 5-items per subscale ($.57 \leq r \leq .61$), correlations were acceptable even when using 1-item per subscale ($.43 \leq r \leq .47$).

Content validity. In the undergraduate sample, correlations between the full and short measures were stronger than .75 for all subscales. Correlations were almost perfect when using 4 items per subscale ($r = .98$), and were modestly weaker when using 3 items ($.93 \leq r \leq .96$), 2 items ($.88 \leq r \leq .91$), or one item per subscale ($.76 \leq r \leq .83$). In the golf sample, correlations between the full and short measures were stronger than .80 for all subscales. Correlations were almost perfect when using 4 or 3 items per subscale ($.96 \leq r \leq .99$), and were modestly weaker when using 2 items ($.89 \leq r \leq .94$) or one item per subscale ($.82 \leq r \leq .85$).

Factorial validity. The factor structure was generally maintained for all shorter measures (see Table 4.2). In the undergraduate sample, there was a close fit when using

2-items per subscale. The fit was less good, but still acceptable, when using 3-, 4-, or 5-items. In the golf sample, most indices described a close fit with the three-factor structure.

Table 4.2
Confirmatory Factor Analysis Fit Indices for Short Measures in Undergraduate and Golf Samples

	Maximum Likelihood							Robust Maximum Likelihood	
Items per subscale	Chi Squared	*p*	TLI	CFI	RMSEA	SRMR	AIC	Chi Squared	*p*
Undergraduate sample									
5	172.23	0.00	0.90	0.91	0.07	0.07	9020.69	153.12	0.00
4	95.19	0.00	0.91	0.93	0.06	0.07	7416.84	86.63	0.00
3	39.04	0.03	0.95	0.97	0.05	0.06	5620.96	37.38	0.04
2	8.91	0.18	0.96	0.98	0.05	0.04	3880.07	9.17	0.16
Golf sample									
5	93.35	0.30	0.99	0.99	0.02	0.06	5295.34	81.67	0.64
4	53.18	0.39	0.99	1.00	0.02	0.06	4400.26	46.61	0.65
3	27.53	0.28	0.99	0.99	0.04	0.06	3324.06	25.50	0.38
2	2.38	0.88	1.05	1.00	0.00	0.03	2252.60	2.14	0.91

Criterion-related validity: self-report. In the undergraduate sample, mindfulness in sport was associated with trait mindfulness regardless of the length of the sport measure. Trait mindfulness was associated with a tendency to avoid self-judgment in sport ($.29 \leq r \leq .39$) and an ability to refocus in sporting settings ($.11 \leq r \leq .19$). In the golf sample, correlations between trait mindfulness (via the Mindfulness Inventory for Sport) and state mindfulness (via the Mindful Attention Awareness Scale) were similar regardless of the length of the measure. State mindfulness showed weak positive correlations with a tendency to be aware ($.16 \leq r \leq .20$) and weak negative correlations with a tendency to avoid self-judgement ($-.14 \leq r \leq -.21$).

Criterion-related validity: behaviour. Performance, as measured by error on ten putts, was not associated with mindfulness subscales of any length ($-.11 \leq r \leq .07$; all $p >$ 0.38). This was possibly due to the short task (so no need to 'refocus') with no pressure (so no need for 'awareness') and no ability to compare against a standard, being a novel task with no competition (so small probability of 'self-judgment'). However, golfers with better (i.e., lower) handicaps tended to be better able to refocus their attention ($-.20 \leq r \leq -.23$; all $p < 0.02$). Lower handicaps were associated with higher awareness ($-.12 \leq r \leq -.16$), but correlations were only significant for one and two item versions (p = .03 and .049 respectively; others *p*s between .052 and .083). Handicap did not appear to strongly relate to a tendency for self-judgment ($.04 \leq r \leq .05$; all $p > 0.5$). As seen by the narrow range between the highest and lowest correlation, there were no meaningful differences in these associations for subscales of different lengths. The shorter versions of the Mindfulness Inventory for Sport appeared equivalent to longer versions for both types of criterion validity.

Discussion

The purpose of this study was to use the genetic algorithm procedure to abbreviate the Mindfulness Inventory for Sport and assess the utility of the resulting scales. Table 4.3 summarises the trade-offs that occur when using the measures shortened by the genetic algorithm. Reliability was proportional to the length of the measure, as would be expected (Tavakol & Dennick, 2011), though drop-off in reliability was particular steep between the 9- and 6-item measure. The 9- and 6-item versions may balance the needs of many researchers. The 9-item version of the measure demonstrated acceptable reliability, acceptable factor fit, equivalent criterion validity and 40% lower response burden than the 15-item measure. The 6-item version demonstrated poorer reliability, but better factor fit, equivalent criterion validity, and 60% lower response burden compared with the 15-item measure. Short measures demonstrated very high correlations with the long version of the questionnaire (*r*s between .88 and .97).

Table 4.3
Questionnaire Options with Benefits and Drawbacks

	Benefits	Drawbacks
15-item version	Highest reliability	Highest questionnaire burden Poor factor fit in some samples
12-item version	Near perfect correlation with full scale (*r*s = .98-.99) Good reliability Acceptable factor fit 20% lower response burden than full measure Equivalent criterion validity with full measure	Poorer factor fit than 6-item scale
9-item version	Near perfect correlation with full scale (*r*s = .93-.97) Acceptable reliability Acceptable factor fit 40% lower questionnaire burden than full measure Equivalent criterion validity with full measure	Poorer factor fit than 6-item scale
6-item version	High correlation with full scale (*r*s =.88-.93) Strongest factor fit across samples 60% lower response burden than full measure Equivalent criterion validity with full measure	Lowest reliability
3-item version	Good correlation with full scale (*r*s = 75-.83) 80% lower response burden than full measure Equivalent criterion validity with full measure	Cannot calculate reliability

Reliability and Validity of Shortened Measures

The psychometric properties of our shortened measures replicate those of the initial development paper. The reliability metrics for the 9-item measure were as good as those found in the original validation paper (Thienot et al., 2014). Thienot and colleagues found acceptable internal consistency in both their samples of undergraduates (.77 ≤ α ≤ .78) and elite athletes (.70 ≤ α ≤ .79). While, the 6-item measure showed poorer reliability, the fit indices were as good, if not better, than those presented in the initial validation study (.89 ≤ CFI ≤ .92 and .88 ≤ TLI ≤ .90 from Thienot et al., 2014). These data indicate that the factorial validity for the short measures is at least as strong as that of the full scale. Our data suggest that, in some samples, shorter measures may be preferable, suggesting that longer measures may assess latent variables outside the core hypothesised constructs.

Neither the criterion validity nor the test-retest reliability appeared to be influenced by the length of the questionnaire. Correlations with other measures of mindfulness were weak. The same was true for behavioural measures of performance. This was a consistent pattern for all short measures, including that with one item per subscale. These weak correlations are similar to those demonstrated in previous studies, which found weak correlations between non-judgment, refocusing and other trait mindfulness measures (Thienot et al., 2014). While these correlations were weak, we could not find evidence that the validity of the Mindfulness Inventory for Sport was compromised by shortening the measure.

Comparisons with Other Genetic Algorithm Implementations

Our findings are consistent with previous implementations of genetic algorithms for questionnaire abbreviation (Basarkod et al., 2018; Eisenbarth et al., 2015; Sahdra et al., 2016; Sandy et al., 2014). Those implementations have sometimes shortened long, multi-factor questionnaires while maintaining explained variance (e.g., *r*s between .90 and .97; Sahdra et al., 2016). Some have also abbreviated unidimensional questionnaires that were already quite short (e.g., 12-items to 5-items with $r = .98$; Basarkod et al., 2017). In all of these examples, genetic algorithms have produced shorter versions of questionnaires while maintaining high full-scale correlations. Reliability metrics are usually compromised to some degree, but other implementations have also found long and short versions perform comparably on criterion validity (Basarkod et al., 2018; Eisenbarth et al., 2015; Sahdra et al., 2016; Sandy et al., 2014). Our findings add to the evidence for using machine learning as an efficient tool for abbreviating questionnaires. Genetic algorithms appear to reliably produce short questionnaires that meet standards of reliability and validity. These algorithms still require some researcher input to tune parameters (e.g., the item-cost parameter). However, compared with traditional approaches to measure abbreviation (Marsh et al., 2005), these approaches require less subjectivity and judgment.

Other implementations of genetic algorithms have selected an item-cost parameter to find one shorter version of the questionnaire that fits the goals of the researchers (Basarkod et al., 2018; Eisenbarth et al., 2015; Sahdra et al., 2016). In contrast, our implementation was the first to offer end-users flexibility to choose their own level of abbreviation. Rather than determining scale length by tuning the item-cost parameter ourselves, we have provided future researchers with a transparent description of the costs and benefits of using questionnaires of various lengths. These shorter measures appear to be reliable and valid approaches to measuring mindfulness components in athletes. For example, the 9-item measure was 40% shorter than the full measures and demonstrated strong reliability and validity. It may allow for more flexible and accurate data collection while reducing cost and questionnaire burden (Basarkod et al., 2017). Using this measure may increase the ability for researchers to assess the causal models underlying mindfulness interventions in athletes.

Causal Models of Mindfulness

Assessing causal models is likely to strengthen the evidence surrounding mindfulness interventions because it will transparently assess the processes of change (Pearl, 2009; Rosen & Davison, 2003). Instead of comparing the effectiveness of mindfulness interventions with different names (e.g., Mindfulness-Acceptance-Commitment vs. Acceptance and Commitment Therapy), researchers can build upon each other's work by measuring the processes underlying those interventions (Rosen & Davison, 2003). While not a core goal of this paper, the data presented here provide limited support for the assumed model of mindfulness presented in Figure 1. As outlined in Figure 3, we found no meaningful associations between any mindfulness subscale and putting performance or state mindfulness. We found only weak associations between golf handicap and the mindfulness constructs of awareness and refocusing. Non-judgement demonstrated a negligible association with handicap. Causal models can be more rigorously explored using structural equation modelling (Pearl, 2009). With weak zero-order correlations, however, our sample size was insufficient to detect effects using structural equation models (Wolf, Harrington, Clark, & Miller, 2013). Regardless, the weak correlations raise doubts about the predictive utility of self-reported mindfulness.

These data are consistent with previous studies that found weak positive correlations of mindfulness with performance ($r = .17$, Blecharz et al., 2014; $r = .33$, Röthlin et al., 2016; $r = .19$, Sarnell, 2012). All three of these studies used general mindfulness measures rather than those specifically assessing mindfulness in sporting contexts. They also used different measures of performance (soccer skills, Blecharz et al., 2014; self-report, Röthlin et al., 2016; coach report, Sarnell, 2012). Nevertheless, the consistency of these associations suggest a small percentage of the variance in performance may be explained by an athlete's dispositional mindfulness. Our findings suggest that these associations may be better explained by an athlete's tendency to be aware of their thoughts and refocus, rather than their tendency to avoid self-judgement. Birrer and colleagues (2012) identified nine possible impact mechanisms by which mindfulness may help athletes, few of which have been explicitly assessed. Teasing apart these components allows researchers to understand why mindfulness may be helping athletes, allowing for more targeted interventions and a stronger causal model.

Limitations and Future Directions

This research was not designed to be a thorough assessment of a causal model. The cross-sectional nature of most associations means extraneous variables may explain many relationships. High-quality randomised trials would allow researchers to make stronger causal claims without the interference of confounding variables (e.g., Noetel et al., 2017). Alternatively, designs that explicitly test mediation or use structural equation models may allow for a more direct assessment of the causal model (Pearl, 2009). These short measures facilitate this research by enabling researchers to test more variables in the same questionnaire battery.

Our study did not assess the cross-cultural validity of these measures, so they are still primarily validated on western, educated, industrialized, wealthy, and democratic populations. They are also yet to be validated with children, and researchers may want to assess how a child's level of athletic mindfulness influences his or her sporting trajectory. Researchers may benefit from checking whether the psychometric properties hold in these populations before making strong claims based on the use of these measures.

The Mindfulness Inventory for Sport assesses distinct components of mindfulness, and these components vary in their association with unidimensional measures of mindfulness. Many previous studies have used scales that treat mindfulness as a unidimensional trait, although separating the construct into these components may have theoretical and practical benefits for future research. For example, if refocusing is the best predictor of performance and refocusing is amenable to change, then interventions may be more effective if they target refocusing, rather than awareness or non-judgementality. Mindfulness and acceptance interventions vary in the degree to which they address these components, and the focus of the intervention is sometimes poorly reported (Noetel et al., 2017). It is not yet clear which of these processes is the most important and which is the most amenable to change, but we hope that these shorter measures make it easier to explore those questions. Shorter measures likely make it more feasible to assess mindfulness before, during, and after an intervention. Doing so would allow the research community to identify whether mindfulness does mediate the relationship between interventions and performance outcomes, and if it does, which processes of change are most important.

Conclusions

More than half of high-performance athletes fill out questionnaires every day (Taylor et al., 2012) and questionnaire length is one of the biggest barriers to completion

(Saw et al., 2015). Traditional methods of shortening questionnaires require a difficult balance of multiple factors (Marsh et al., 2005), which often leads to poor measures (Smith et al., 2000). Genetic algorithms can identify robust shorter version of questionnaires while maintaining sound psychometric qualities. We demonstrated that these methods are useful in sport and exercise psychology by creating multiple short versions of the Mindfulness Inventory for Sport. While our data found weak associations between performance and some components of mindfulness, we hope the measures encourage more research into the construct. We also offer researchers a new tool for shortening questionnaires, facilitating flexible, efficient, affordable, and accurate data collection.

Chapter 5—Discussion and Conclusion

Review of the Overall Objectives

In this thesis, I aimed to assess the utility of context-focused approaches, like mindfulness and acceptance, that seek to change an athlete's *relationship* with internal experiences (e.g., thoughts and feelings), rather than the content of those experiences. The key objectives were three-fold. The first objective was to conduct a comprehensive review of the effectiveness of context focused approaches for athletes. Previous reviews of mindfulness have either focused on non-athletic populations or been narrow in scope as to exclude other interventions operating by a similar mechanism of action.

The second objective was to conduct a rigorous experimental trial of mindfulness and acceptance. Previous authors should be acknowledged for their efforts to design high-quality studies. However, the criteria for rigour in applied research have increased over the last few years, with greater focus on prospective registration and open-science principles (Tamminen & Poucher, 2018). Based on that review, the randomised trial in Chapter 3 was the first in the sport psychology literature to meet these new criteria.

The final objective of the thesis was to facilitate future research into mindfulness by reducing response burden. Doing so may make it easier to include measures of mindfulness in questionnaire batteries and facilitate the assessment of mindfulness across multiple time points. I used an advanced machine learning algorithm to create shortened versions of an established measure. Chapter 4 aimed to test the shortened versions of this measure for reliability and validity.

Significant Findings

Based on this thesis, context-focused approaches are yet to be an established way of improving performance. First, there have been more than sixty studies on mindfulness, acceptance, and meditative approaches in athletes; however, none met Cochrane standards for low risk of bias. When I conducted a low risk of bias study on mindfulness and acceptance with enough power to detect small effects, I found mixed results, with performance benefits in only one aspect of putting mechanics but no benefits in the primary outcome (putting performance). The findings did not support the effectiveness of brief mindfulness interventions for promoting immediate gains in performance. I also found few studies that explored the theoretical mediators of context-focused interventions. Mindfulness interventions often assume that interventions increase performance because athletes who practice are more likely to be mindful. Few studies have explored this assumption, which may be partially due to the response burden associated with measurement. So, in the final study of this thesis, I took the shortest measure of mindfulness with established psychometric properties in athlete samples and used advanced methodologies to shorten it further. In doing so, I helped researchers to assess the construct using as few as two items for each subscale while maintaining content validity, factorial validity, and criterion validity. I also explored associations between these subscales and performance measures, and found only weak correlations with golf handicap.

Strengths

This thesis had a number of strengths that increased the internal and external validity of the included studies. With the systematic review, I was able to consolidate current evidence on context-focused approaches in athletes. As far as I am aware, the systematic review in Chapter 2 was the most comprehensive reviews of these approaches in sport, because it included studies of all designs and a range of context-focused approaches. It was also among the first reviews in sport psychology to use the Cochrane Risk of Bias tool and the GRADE method of evaluating the body of evidence. The Cochrane Risk of Bias assessment is more valid than quantitative measures of study quality (Higgins & Altman, 2008). It also has higher sensitivity and specificity than quantitative approaches. The GRADE method uses these risk of bias judgements to transparently rate the certainty of conclusions from a systematic review (Schünemann, Oxman, Vist, et al., 2008). Both approaches reduce the potential for bias in the systematic review, with less subjectivity than other approaches to narrative synthesis.

With the randomised controlled trial, I could assess the role of a brief mindfulness intervention while controlling for many confounding variables. Selection bias is where a researcher could plausibly exclude undesirable participants who would have been allocated to the intervention condition. I negated the chance of this bias by concealing the allocation of athletes from the experimental personnel. I also concealed the hypotheses from participants and key personnel to reduce expectancy effects. These effects include detection and performance bias. Detection bias is where outcome assessors bias their assessment in favour of the hypothesis. Performance bias is where participants or key personnel change their behaviour in response to knowledge about the hypothesis (e.g., increased care and attention). Selective reporting is a bias where researchers may be tempted to report studies and outcomes that align with their hypotheses. I mitigated the chance of this bias by prospectively registering the methods. I used a short intervention and conducted a sensitivity analysis to reduce the chance of attrition bias. This bias is where participants who complete an intervention are systematically different from those who drop out (e.g., because of better outcomes). This study was the first to examine mindfulness in sport while mitigating the chance of all of these biases (Noetel et al., 2017). I powered the study to detect even small effect sizes, and published data and analysis code following open-science principles. As far as I am aware, this randomised trial was the first to meet open-science standards within the field of sport psychology (Noetel et al., 2017; Tamminen & Poucher, 2018).

Another standard that is developing within psychology is the movement toward exploring empirically supported principles of change, rather than assessing trademarked treatment packages (Ciarrochi et al., 2016; Rosen & Davison, 2003). This process can involve testing discrete components of an intervention (Levin et al., 2012) or assessing important mediators of change over the course of an intervention (Arch & Craske, 2008). Both of these approaches test the causal models underlying psychological interventions (Pearl, 2009), but both can be labour intensive. I reduced barriers to assessing the models underlying mindfulness interventions by demonstrating a new approach to shortening psychometrics and developing a brief mindfulness measure specific to sports. I used an advanced machine learning algorithm to reduce the subjectivity that is sometimes present when shortening questionnaires (Sandy et al., 2014). I followed the key steps that maintain psychometric properties of short measures (Short et al., 2000): testing reliability, overlapping variance, and factor structure. In doing each of these, I compared the results with established standards for psychometric validity (i.e., Marsh et al., 2005). By creating short measures that meet these standards, I reduced the questionnaire burden for participants, which can reduce cost (Sahdra et al., 2016), completion rate (Fan & Yan, 2010), and the ability to assess a construct over time (Sandy et al., 2014).

Each of the three studies used a broad inclusion criteria, and the studies tested for moderation when appropriate. Compared with using an narrow group of participants, this approach increased the generalisability of the results while being sensitive to differences between participants. In testing the reliability and validity of the questionnaire, I used a heterogeneous group of Australian sporting participants. Both the undergraduate and golfing sample included athletes with a wide range of abilities from a range of backgrounds. Similarly, the randomised controlled trial focused on skilled golfers but included golfers with a range of abilities, which allowed me to examine moderating effects of participants' level of skill. In the systematic review, the included studies were from Europe, North America, Asia, Australasia, and the Middle East. I also assessed whether the findings differed based upon the skill level of the athletes.

Limitations and Future Directions

While I made efforts to increase the generalisability of the results to a wide range of participants, those in the measurement study and randomised trial were still largely educated participants from western cultures. It is possible that the measurement of relevant constructs may be different in cultures with different understanding of the concepts. It is also possible that the effects of context-focused approaches are different in other cultures. For example, given the origins of mindfulness are from eastern cultures, interventions might be more effective due to cultural fit, or less effective due to the mindfulness practices being more widespread before the intervention. Regardless, future research into context-focused approaches could endeavour to target people from different cultures and demographics.

Online interventions may reduce barriers to these efforts. For example, online interventions could be available globally. They require care during translation, but can influence a greater number of participants with a lower marginal cost. Meta-analyses have found online psychological interventions to be effective for health and wellbeing (Andrews, Cuijpers, Craske, McEvoy, & Titov, 2010; Davies, Morriss, & Glazebrook, 2014; Richards & Richardson, 2012; Rooksby, Elouafkaoui, Humphris, Clarkson, & Freeman, 2015; Spek et al., 2007; Spijkerman, Pots, & Bohlmeijer, 2016). Using online interventions to test the effectiveness of context focused approaches in sport would eliminate some barriers to external validity. It would also make some components of internal validity less cumbersome. For example, blinding and random allocation of participants can be easier when conducted online, as I did with the randomised trial.

Where blinding and random allocation were a strength of the randomised trial, the tight controls in this study mean the findings may have limited external validity. Golfers were putting in a contrived environment with artificially induced pressure. Similarly, the measurement of performance was not authentic. Rather than sinking a ball into a hole, participants were asked to stop a ball close to a target. These differences mean that mindfulness may be a useful intervention when assessing the utility in a real game under authentic competitive pressures.

A similar limitation with the randomised trial was the short dose of the intervention (~7 minutes). Many studies use similar, short doses and have found significant results (e.g., Hooper et al., 2011; Weger et al., 2012; Mrazek et al., 2012; for a review, see Levin et al., 2012). The average dose for the longer, mindfulness-based stress reduction intervention was found to be about twenty hours (Bassam Khoury et al., 2015). Some studies in the systematic review lasted this long (e.g., Zhang, Si, et al., 2016), but those studies failed to meet internal validity criteria discussed earlier. For example, none were pre-registered, and participants and key personnel were usually aware of hypotheses. As such, an absence of rigorous long-term studies is a limitation of this thesis and the literature to date. In the wider context-focused literature, some long-term studies have met many internal validity standards. For example, Noone and Hogan (2018) used an online intervention to conduct a randomised trial of mindfulness versus a sham meditation. While this pre-registered, blinded study did not report significant effects on its primary outcome, future studies of this ilk could assess the influence of mindfulness on key athletic outcomes, like performance, flow, or wellbeing.

In the substantive-methodological synthesis, I did not compare the outcomes of the method with a more traditional method of shortening a questionnaire (e.g., Marsh et al., 2005). Explicitly comparing both the process (e.g., time required) and outcome (e.g., final scales, reliability and validity of those scales) of these two methods would allow researchers to better assess the costs and benefits of this novel methodology.

Future research on context-focused interventions could assess the causal model by comparing context-focused approaches with purportedly antithetical model: that is, those that focus on cognitive and emotional control. As mentioned earlier, many psychological skills aim to increase the ability for athletes to control their thoughts and feelings. Context-focused interventions explicitly caution against that approach, and instead promote acceptance of thoughts and feelings. Future research could explore these differing causal models by comparing two interventions (e.g., mindfulness vs. relaxation or positive self-talk) toward a common outcome (e.g., performance). By assessing hypothesised mediators—acceptance of emotions vs. frequency—research could provide evidence about the relative merits of the two propositions.

Mindfulness focuses on a relatively narrow set of processes within the broader context-focused literature. While this thesis has given particular attention to mindfulness as an emblematic technique within the context-focused literature, other components of the literature may be more effective within athletic settings. Acceptance and commitment therapy includes mindfulness alongside acceptance, defusion, self-as-context, values, and committed action. Some combination of these interventions may fit better within athletic populations. For example, committed action regularly includes goal setting, and goal setting has been shown to improve athletic performance in meta-analyses in sport (Kyllo & Landers, 1995). Researchers could assess the relative importance of these components by selectively deconstructing the intervention and measuring the effects (Coffey, Hartman, & Fredrickson, 2010). Similarly, as discussed in the questionnaire paper, the model could also be tested by assessing mediating variables using multi-factor questionnaires. Doing so would allow researchers to understand whether mindfulness improves performance by increasing awareness, non-judgment, refocusing, or all three.

Conclusions

Context-focused approaches to performance enhancement have garnered increasing attention over the last decade. These approaches have established themselves as comparable to other psychotherapies for mental health conditions like depression (Barth et al., 2013). However, the efficacy of the interventions for athletes is yet to be established. In the systematic review (Chapter 2), studies that found positive effects failed to meet current standards of internal validity. Many studies from non-sport disciplines that met current standards have failed to find positive effects of mindfulness training. This is a non-trivial problem, because the recommended dose for athletes engaging in mindfulness is between 10 and 45 minutes each day. This could present a significant opportunity cost, because the athlete could be using this time to engage in another sport-related activity. It is currently unclear whether the practice would offer commensurate benefits. Nevertheless, the possible benefits could be widespread, with mindfulness-based studies reporting a range of behavioural, cognitive, neurological, and athletic benefits. If these benefits occur for athletes, then studies with high internal validity will find them. In this thesis, I aimed to synthesise, generate, and facilitate rigorous research. As the standards for rigor increase throughout the scientific community, this thesis described applications of those standards to context-focused approaches in sport. It is not year clear whether time invested in context-focused approaches is worth the opportunity cost.

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Research Portfolio Appendix

Systematic Review

The systematic review in Chapter 2 has been published in the International Review of Sport and Exercise Psychology. The published copy is available here:

Noetel, M., Ciarrochi, J., Van Zanden, B., & Lonsdale, C. (2017). Mindfulness and acceptance approaches to sporting performance enhancement: A systematic review. *International Review of Sport and Exercise Psychology*, 1–37. doi: 10.1080/1750984X.2017.1387803

I acknowledge that my contribution to the above paper is 70 percent

Michael Noetel

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Brooke Van Zanden

I acknowledge that my contribution to the above paper is 10 percent

Chris Lonsdale

Randomised Controlled Trial

The trial in Chapter 3 is currently under review at Psychology of Sport and Exercise with reference number PSE_2018_413. Proof of submission is attached below.

Can a Brief Mindfulness Intervention Improve Sports Performance? A Double-Blind Randomised Controlled Trial

Current status: Under Review (15/Aug/2018)

PSE_2018_413

Associate Editor: Richard Keegan

Article Type: Research Paper

Initial submission : 07/Aug/2018

I acknowledge that my contribution to the above paper is 70 percent

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James Conigrave

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Chris Lonsdale

Substantive-Methodological Synergy

The paper in Chapter 4 is currently under review at Psychology of Sport and Exercise with reference number PSE_2018_630. Proof of submission is attached below.

Using Genetic Algorithms to Abbreviate the Mindfulness Inventory for Sport: A Substantive-Methodological Synthesis

Current status: With Editor (31/Oct/2018)

PSE_2018_630
Editor in Chief: Bernd Strauss

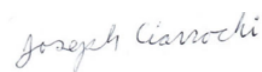
Article Type: Research Paper

Initial submission : 31/Oct/2018

I acknowledge that my contribution to the above paper is 70 percent

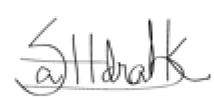
Michael Noetel

I acknowledge that my contribution to the above paper is 10 percent



Joseph Ciarrochi

I acknowledge that my contribution to the above paper is 10 percent



Baljinder Sahdra

I acknowledge that my contribution to the above paper is 10 percent

Chris Lonsdale

Appendices

Appendix A: Supplementary Tables

Supplementary Table 3.1

Descriptive Statistics of Sample, Broken Down by Randomised Group

	Total	Group A	Group B
Age			
Mean	51.2	52.4	50.2
Standard Deviation	16.5	15.9	17
Gender			
Male	88	46	42
Female	29	17	12
Did not report	1	1	0
Handicap			
Mean	14.5	14	14.9
Standard Deviation	10.1	9.7	10.5
Years Playing Golf			
Mean	21.9	23.4	20.5
Standard Deviation	15	15	14.9
Education			
Masters degree or higher	18	5	13
Bachelor degree	33	19	14
Diploma	17	7	10
High school certificate	30	14	16
School certificate or lower	20	9	11
Country Born			
Australia	91	41	50
Germany	2	2	0

India	1	0	1
Japan	1	0	1
Malaysia	1	0	1
New Zealand	5	4	1
Phillipenes	1	0	1
South Africa	3	1	2
South Korea	1	0	1
United Kingdom	8	3	5
USA	2	2	0
Vietnam	1	1	0
Zimbabwe	1	0	1
Meditators			
No	91	41	50
Yes, less than 1 hour per week	17	7	10
Yes, 1-2 hours per week	6	4	2
Yes, 2-3 hours per week	1	0	1
Yes, 4-5 hours per week	1	1	0
Yes, more than 5 hours per week	1	0	1
Years of Meditation Experience			
Mean	1.4	1.9	1
Standard Deviation	4.2	5.1	3.3

Supplementary Table 3.2

Results from Likelihood Ratio Tests Using Linear Mixed Models

Results from Likelihood Ratio Tests Using Linear Mixed Models

Outcome	Main effect of time	Group-by-time interaction	Interaction when moderator in model			Interaction using sensitivity samples				
			Handicap	Meditation experience	Trait mindfulness	Handicap <= 10	As per protocol	Original exclusion	Meditators	Non-Meditators
Performance	1.29	0.55	0.56	0.53	0.55	0.65	1.20	0.84	2.25	0.99
Overall mechanics	0.16	6.98*	6.91*	7.01*	6.98*	3.87	6.43*	7.32*	2.87	5.89
Timing mechanics	0.79	2.46	2.47	2.46	2.46	0.19	2.89	1.74	1.61	2.83
Tendency mechanics	1.34	10.06**	10.05**	10.11**	10.06**	1.33	9.28*	11.51**	3.33	6.58*
Consistency mechanics	0.22	2.96	2.82	2.96	2.96	3.42	2.68	3.18	2.95	2.45
State mindfulness	54.39***	4.62	4.62	4.64	4.62	5.19	3.22	4.46	0.49	5.67
Cognitive anxiety	2.13	0.18	0.18	0.17	0.18	0.73	0.13	0.07	1.73	0.08
Somatic anxiety	13.05**	9.22*	9.22*	9.18*	9.22*	3.55	9.96**	7.63*	1.37	8.98*

Note. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Interaction with moderator in model tested the effect of the interaction term while also statistically controlling for handicap, meditation experience and trait mindfulness respectively. Interaction using sensitivity samples tested significance of interaction term when using three subsamples of the data: handicap <=10 means the sensitivity analysis was conducted only on skilled golfers with low handicaps; as per protocol excludes the small number of participants who failed to complete crossover; and the original exclusion criteria eliminated adolescents and those with at least 5 years golfing experience but no registered handicap.

Supplementary Table 4.1

Raw Reliability and Validity Metrics for Subscales of Various Lengths

	Cronbach's Alpha		McDonald's Omega		Test-retest	Correlation with full scale		Correlation with Child and Adolescent Mindfulness Measure	Correlation with state mindfulness	Correlation with handicap	Correlation with putting performance
Items per subscale	Under-graduates	Golfers	Under-graduates	Golfers	Under-graduates	Under-graduates	Golfers	Under-graduates	Golfers	Golfers	Golfers
Awareness subscale											
5	0.87	0.76	0.89	0.81	0.61			-0.02	0.11	-0.10	0.07
4	0.81	0.72	0.83	0.77	0.59	0.99	0.98	-0.02	0.10	-0.09	0.07
3	0.82	0.66	0.79	0.70	0.58	0.95	0.93	-0.08	0.15	-0.11	0.03
2	0.81	0.53	0.81	0.55	0.53	0.93	0.88	0.00	0.15	-0.15	0.01
1					0.46	0.82	0.76	0.03	0.16	-0.17	-0.03
Non-judgementallity subscale (reverse scored)											
5	0.88	0.82	0.9	0.86	0.57			0.39	-0.16	0.06	-0.03
4	0.85	0.76	0.88	0.82	0.51	0.99	0.98	0.39	-0.17	0.05	-0.03
3	0.80	0.75	0.81	0.78	0.55	0.97	0.96	0.38	-0.16	0.06	-0.06
2	0.68	0.57	0.68	0.57	0.43	0.93	0.91	0.38	-0.18	0.06	-0.06
1					0.49	0.83	0.83	0.29	-0.10	0.07	-0.04
Refocusing subscale											
5	0.85	0.81	0.89	0.84	0.60			0.16	-0.07	-0.28	-0.08
4	0.79	0.77	0.84	0.79	0.57	0.99	0.98	0.14	-0.05	-0.28	-0.08
3	0.81	0.76	0.82	0.77	0.59	0.96	0.95	0.19	-0.03	-0.27	-0.07
2	0.74	0.72	0.74	0.72	0.51	0.88	0.90	0.11	-0.01	-0.26	-0.05
1					0.47	0.83	0.81	0.16	-0.07	-0.26	-0.11

Appendix B: Chapter 3 Psychometric Questionnaires

Appendix B.1 State Mindful Attention Awareness Scale (Brown & Ryan, 2003)

"Sometimes we go about our day without paying attention to what is going on. To establish your current level of distraction, please indicate to what degree were you having these experiences in the **past 15 minutes**."

Questions	Not at all 0	1	2	Somewhat 3	4	5	Very much 6
I found it difficult to stay focused on what was happening.							
I rushed through activities without being really attentive to them.							
I did jobs or tasks automatically, without being aware of what I was doing.							
I found myself preoccupied with the future or the past.							
I found myself doing things without paying attention.							

Appendix B.2 Competitive State Anxiety Inventory-2 Short Form (Cox et al., 1998)

"With respect to the putts you are about to make, please indicate to what degree were you having these experiences **right now**:"

Questions	Not at all	Somewhat	Moderately so	Very much so
I am concerned about this performance.				
I feel nervous.				
I feel tense in my stomach.				
I am concerned that others will be disappointed with my performance.				
I'm concerned about performing poorly.				
My body feels tight.				

Appendix C: Chapter 4 Psychometric Questionnaires

Appendix C.1 Child and Adolescent Mindfulness Measure (Greco et al., 2011)

Questions	Never true	Rarely true	Sometimes true	Very often true	Always True
I get upset with myself for having feelings that don't make sense.					
At school, I walk from class to class without noticing what I'm doing.					
I keep myself busy so I don't notice my thoughts or feelings.					
I tell myself that I shouldn't feel the way I'm feeling.					
I push away thoughts that I don't like.					
It's hard for me to pay attention to only one thing at a time.					
I think about things that happened in the past instead of thinking about things that are happening right now.					
I get upset with myself for having certain thoughts.					
I think that some of my feelings are bad and that I shouldn't have them.					
I stop myself from having feelings that I don't like.					

The Mindfulness Inventory for Sport is available in Table 4.1.

The State Mindful Attention Awareness Scale is available in Appendix B.1.

Appendix D: Human Research Ethics Committee Approval

Appendix D1. Ethics approval and confirmation of project completion for Chapter 3

Friday, November 2, 2018 at 12:08:17 PM Australian Eastern Standard Time

Subject: 2016-109H Final Report Approved
Date: Wednesday, 27 June 2018 at 10:06:14 am Australian Eastern Standard Time
From: Ms Pratigya Pozniak
To: Michael Noetel
CC: Pratigya Pozniak

Dear Michael

Ethics Register Number : 2016-109H
Project Title : The Influence of Mindfulness Trianing and Growth Mindsets on Golfers

Thank you for returning the Final Ethics Report for your project.

The Deputy Chair of the Human Research Ethics Committee has signed off on this project as completed. It has now been recorded on the Ethics Register as COMPLETED AND CLOSED.

We wish you well in future research projects.

Kind regards,
Ms Pratigya Pozniak

Research Ethics Officer | Office of the Deputy Vice-Chancellor (Research)
Australian Catholic University
T: 02 9739 2646 E: res.ethics@acu.edu.au

Appendix D2. Ethics approval and confirmation of project completion for Chapter 4

Friday, November 2, 2018 at 12:07:46 PM Australian Eastern Standard Time

Subject: 2016-15E Final Report Approved .
Date: Monday, 7 August 2017 at 3:38:40 pm Australian Eastern Standard Time
From: Ms Pratigya Pozniak
To: Mr Michael Noetel
CC: Pratigya Pozniak

Dear Michael,

Ethics Register Number : 2016-15E
Project Title : Exploring the Relationship between Mental Toughness and Mindfulness

Thank you for returning the Final Ethics Report for your project.

The Deputy Chair of the Human Research Ethics Committee has signed off on this project as completed. It has now been recorded on the Ethics Register as COMPLETED AND CLOSED.

We wish you well in future research projects.

Kind regards,
Ms Pratigya Pozniak

Research Ethics Officer | Office of the Deputy Vice-Chancellor (Research)
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