Abstract

Objectives. Using an experimental design, we tested whether a short mindfulness intervention led to improvements in state mindfulness and golf putting performance.

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

Method. Experienced golfers ($N = 116$) were randomly allocated to receive either a 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 state mindfulness, competitive anxiety, swing mechanics, and performance.

Results. We found no significant effects of the intervention for state mindfulness, performance, or cognitive anxiety. We significant effect for swing mechanics and somatic anxiety; participants who received mindfulness noticed more nervousness and showed better mechanics during the competition.

Conclusions. This study is the first prospectively-registered, double-blind, randomised trial assessing the effects of mindfulness in athletes. A brief intervention did not influence state mindfulness or performance.

Can a Brief Intervention Improve Mindfulness and Performance in Sport?

A Prospectively Registered, Double-Blind Randomised Controlled Trial

“We talk about mindfulness... so we can focus, get one-pointed attention, and be in concert with one another in times of need.” (NBA Championship coach, Phil Jackson; Oprah, 2013).

Mainstream media have begun to draw attention to mindfulness—non-judgmental awareness of the present moment (Kabat-Zinn, 1994)—as a method of improving many outcomes, including athletic performance. 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 (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 compared with non-meditators (Fox et al., 2014). Systematic review of studies that specifically focused on mindfulness showed similar results (Chiesa & Serretti, 2010).

In sport, mindfulness has been described as “a holistic intervention fostering the development of several personal, sport, and performance-relevant psychological skills”

(Birrer, Rothlin, & Morgan, 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., 2016). None of these trial registrations specified a clear outcome variable (Coronado-Montoya et al., 2016). 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., 2016). 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 for athletes (Noetel et al., 2017).

None of these studies on mindfulness in athletes described methods of double-blinding or 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 biased selection from the person enrolling participants, who could exclude 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 mindfulness 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, Hildebrandt, Lillis, & Hayes, 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, Carr, MacMahon, & Starks, 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 mindfulness, anxiety, swing mechanics, and performance 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 state mindfulness, swing mechanics, and performance. That is, we expected these variables to increase when an athlete received mindfulness, but not when the group received the control condition. 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.

Methods

Trial Design and Randomisation

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 reversed. 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 ($\eta^2 = .22$, Beilock et al., 2006; $\eta^2 = .30$, Gucciardi & Dimmock, 2008). To achieve 80% power using

the smallest of these previous effect sizes ($\eta^2 = .22$, mixed ANOVA design, $r_{\text{pre-post}} = 0.5$) the required total sample size was 8 participants. To achieve 95% power using a moderate effect size ($\eta^2 = .06$) the required sample size was 42 participants. To allow for mediation and moderation analyses, and to complement a 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 adults who did not have a handicap but had over 5 years golfing experience. We felt this group would be comparable to those 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

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. The experimental intervention was designed to increase mindful

165 awareness. The attention control was designed to entertain a golfing audience using no
166 mindfulness-related content.

167 **Mindfulness intervention.** The mindfulness intervention was developed by two sport
168 psychologists and two experts in mindfulness. The sport psychologists had been working in
169 golf and performance enhancement for over 10 years each. The mindfulness experts had
170 published research, academic texts, popular books, and organised mindfulness conferences.
171 The intervention was based on a meta-analysis of brief studies on mindfulness and
172 acceptance, which showed experiential exercises are the most useful way of promoting
173 change (Levin et al., 2012). As a result, we used Gardner and Moore's (2007) first
174 experiential mindfulness exercise—the Brief Centering Exercise. Gardner and Moore's
175 (2007) protocol is the most widely used mindfulness and acceptance protocol that is specially
176 designed for athletes (Hasker, 2011; Lutkenhouse, 2007; Schwanhausser, 2009; Wolanin &
177 Schwanhausser, 2010; Zhang et al., 2016).

178 As an introduction to this experiential exercise, we provided some brief
179 psychoeducation, as recommended by both Gardner and Moore's (2007) protocol and
180 Acceptance and Commitment Therapy (Hayes, Strosahl, & Wilson, 2011). The
181 psychoeducational video provided information about how efforts to control unwanted
182 thoughts or feelings can lead to unhelpful, self-focused attention (Gardner & Moore, 2007). It
183 also described how acceptance of thoughts and feelings allows athletes to more easily control
184 their attention (Gardner & Moore), and it identified the benefits of focusing attention on an
185 external stimulus (Beilock et al., 2002). In sum, the intervention aimed to target the three core
186 components of mindfulness promulgated to influence performance: non-judgment,
187 awareness, and refocusing (Thienot et al., 2014). The full transcript is available via the Open
188 Science Framework at https://osf.io/natj4/?view_only=5d51cc89c87b4c3eb8ba3a91fa4ca6d3

Attention control. Participants allocated to the control condition watched 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. While the control condition may have held the attention of the golfers, it was designed to avoid any of the mindfulness components from the other condition (non-judgement, awareness, or refocusing). This transcript of the video is also available via the Open Science Framework at https://osf.io/natj4/?view_only=5d51cc89c87b4c3eb8ba3a91fa4ca6d3

Competition-based pressure manipulation. After the first two rounds, participants were told the final two rounds of putting were part of a competition designed to increase perceived pressure. They were told that their swing would be evaluated by an expert, and that their performance would be compared with other golfers at their club. Additionally, participants were led to believe that a \$50 prize money would be allocated for winning the competition, \$20 for coming second, and \$10 for third place. This pressure induction, using competition, evaluation, and financial incentives, has been shown to increase competitive anxiety in multiple studies of golf putting (Gucciardi & Dimmock, 2008; Mullen & Hardy, 2000; Richard Mullen, Hardy, & Tattersall, 2005).

Pre-treatment Variables

Demographics. Before any putting trials, golfers answered demographic questions assessing their age, gender, handicap, golfing experience, education, ethnicity, and meditation experience.

Mindfulness Inventory for Sport. They also responded on a validated measure of dispositional mindfulness for athletes (Thienot et al., 2014). This 15-item questionnaire is

intended to measure three core components of athletic mindfulness: present moment awareness (e.g., “I am aware of the thoughts that are passing through my mind.”); non-judgementality (e.g., “When I become aware that I am not focussing on my own performance, I blame myself for being distracted.”); and the ability to refocus (e.g., “When I become aware that I am tense, I am able to quickly bring my attention back to what I should focus on.”). Participants respond on a six-point likert scale from 1 = “*Not at all like me*” to 6 = “*Very much like me.*” In both undergraduate students and athletes, it has shown good internal consistency and factorial validity (Thienot et al., 2014; Noetel, Ciarrochi, Sahdra, & Lonsdale, 2019). In our sample, each subscale also showed good internal consistency ($\alpha_{\text{awareness}} = 0.87$; $\alpha_{\text{nonjudgment}} = 0.88$; $\alpha_{\text{refocusing}} = .85$) and reliability indicated by McDonald’s Omega ($\omega_{\text{awareness}} = 0.89$; $\omega_{\text{nonjudgment}} = 0.90$; $\omega_{\text{refocusing}} = .89$).

Outcomes

We assessed whether our intervention led to improvements in state mindfulness and reductions in competitive anxiety. We also measured mechanics to see whether there were any differences in the consistency or movement patterns of the putts. Our pre-registered primary outcome was putting performance, where better putts were those that finished closer to a target.

State mindfulness. To test the hypothesis that the mindfulness and acceptance intervention increases 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. Participants completed this questionnaire after the interventions but before the putting trials. The items were reverse scored to operationalise mindfulness. 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 explained 80-85% of the variance in the longer measure, which involved 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 ($\alpha_{\text{somatic}} = 0.68$; $\alpha_{\text{cognitive}} = 0.75$).

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. It 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.

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. 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. Rather than using a typical golf hole, targets were painted on synthetic turf because they needed to be in a straight line for the SAM PuttLab data to be valid. 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).

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

We used the lme4 package (Bates, Mächler, Bolker, & Walker, 2015) in R (R Core Team, 2016) to construct linear mixed models to test hypotheses, as described in our prospective registration (see https://osf.io/natj4/?view_only=5d51cc89c87b4c3eb8ba3a91fa4ca6d3). 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. 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. Finally, we conducted sensitivity analyses by exploring all models using three sub-samples of the participants. In these subgroups, we excluded participants without a registered handicap, participants who missed any part of the protocol, and participants with handicaps greater than 10.

Results

Recruitment and Descriptive Statistics

Between October 2016 and November 2017, we recruited 116 participants who met the modified inclusion criteria (i.e., golfers with registered handicap or at least 5 years of golfing experience). See Figure 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). Of golfers who met inclusion criteria, 75% were male ($n_{\text{male}} = 86$; $n_{\text{female}} = 29$; one did not disclose gender). There was a wide range of ages ($M_{\text{age}} = 51.84$; $SD_{\text{age}} = 15.86$) and golfing abilities ($M_{\text{handicap}} = 14.64$; $SD_{\text{handicap}} = 10.10$). Most participants had substantial experience in golf ($M_{\text{years playing}} = 22.15$; $SD_{\text{years playing}} = 14.92$), but only 23% currently practised meditation. Of those who had practiced meditation, the average number of years meditating was 6.70 ($SD_{\text{years meditating}} = 7.34$). A full list of descriptive statistics is available in Supplementary Table 1. Consistent with open science recommendations (Tamminen & Poucher, 2018), our cleaned data and analysis code is also available on the Open Science Framework at https://osf.io/natj4/?view_only=5d51cc89c87b4c3eb8ba3a91fa4ca6d3

Figure 1. CONSORT flow diagram indicating participant flow throughout the procedure

State Mindfulness

Means and standard errors for all outcome variables are available in Figure 2. 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 for both conditions (see Figure 2). These findings were robust to sensitivity analyses such that main-effects and interactions were consistent for all subgroups.

Figure 2. Each line represents the means of the two randomised groups. Some scales have been reversed so higher numbers are better for all variables. **Orange line** represents effect of **mindfulness intervention**. **Blue line** represents effect of **control intervention**. Error bars represent standard errors. For ease of interpretation, key results identified using grey signalling.

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 after the pressure induction ($M_{\text{difference}} = 0.52, SE_{\text{difference}} = .17$), but this difference was negated when the other group received the mindfulness intervention too ($M_{\text{difference}} = 0.23, SE_{\text{difference}} = .17$). 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.

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 score ($SE = 1.01$; 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; and 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 ($\chi^2(2) = 1.33$, $p = 0.51$).

Performance

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, and those who met the original inclusion criteria (See Supplementary Table 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.

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 ($b_{\text{awareness}} = 0.09, p = 0.48$; $b_{\text{nonjudgment}} = 0.04, p = 0.71$; $b_{\text{refocusing}} = -0.13, p = 0.41$). 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.

Discussion

Using a double-blind, randomised trial design, we tested the effects of a short mindfulness intervention on state mindfulness, anxiety, golf mechanics and performance. Our study had enough power to detect small effect sizes. Nevertheless, we found no significant influence of the intervention on self-reported mindfulness or putting performance. Although the intervention did not significantly improve accuracy, it did improve putting mechanics. Specifically, golfers in the intervention group had better putting mechanics: their putter was more likely to be square to the target at key moments (e.g., at impact). There was a significant effect of the intervention on somatic anxiety, where those receiving the mindfulness intervention felt more nervous following the pressure manipulation than those who received the control intervention.

We utilized Gardner and Moore's (2007) a brief mindfulness exercise, because experiential exercises have been shown to be most effective at inducing mindfulness and acceptance in experimental studies (Levin et al., 2012). Despite this careful design, our intervention did not influence state mindfulness in the predicted direction. Although there is widespread optimism about the benefits of mindfulness—possibly accelerated by authors file-drawing non-significant results (Coronado-Montoya et al., 2016)—a number of recent high-quality studies have failed to find significant results on their primary outcome. For example, 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). An intervention with one week's practice failed to improve working memory or mind wandering compared with relaxation training (Banks, Welhaf, & Srour, 2015). A brief mindfulness intervention had no significant effect on isometric plank performance compared with listening to an audiobook (Stocker, Englert, & Seiler, 2018). 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. The intervention 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 effect 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).

Limitations and Future Directions

The limited authenticity of the putting task may mean findings do not generalise to more externally valid environments. We utilized a highly controlled measure of performance, which might have 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 mindfulness and acceptance approaches. 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). Putting performance has been shown to respond to very brief interventions like distraction (Beilock et al., 2002) and cue words (Gucciardi & Dimmock, 2008). As a result, mindfulness may have smaller acute effects than these alternate interventions, or may require a higher intervention dose to achieve significant benefits.

Mindfulness studies that have found performance benefits lasted eight weeks or more (e.g., Zhang 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., $\beta = .01$, 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). It is possible that there is a minimum effective dose of mindfulness training, and while some brief interventions meet this dose, ours might have been too brief. Insufficient dose is a plausible limitation of our design, so longer trials of mindfulness and acceptance interventions would allow researchers to assess the effects in a more externally valid design. These trials, however, would benefit from maintaining controls on internal validity (e.g., pre-registration, blinding, allocation concealment) because few such controls have been implemented in athletic mindfulness research (Noetel et al., 2017).

As mentioned earlier, blinding is one such control (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. Future research could implement sham meditation conditions that remove these possibilities while still providing an attentional control.

Similar brief interventions have found short-term changes in mindfulness in sport skills (Stocker et al., 2018). Stocker and colleagues used a positively worded mindfulness measure, where the Mindful Attention Awareness Scale assesses an *absence* of mindlessness (Chiesa, 2012). Heterogeneous definitions of the assessed construct may explain variability in effects (Chiesa, 2012). Similarly, the content of our 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 a meta-analysis of experimental studies (Levin et al., 2012) and two seminal texts on mindfulness and acceptance approaches (Gardner & Moore, 2007; Hayes et al., 2011). We chose to focus on components that were widely believed to improve performance for this kind of skill (Colzato & Kibele, 2017; Thienot et al., 2014). Nevertheless, selecting components for a short intervention requires some experimenter judgement. We followed guidelines in those texts to prioritise acceptance and focused mindful attention; 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; R. Mullen & Hardy, 2000; Richard Mullen et al., 2005), 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.

We also found an interaction between group and time, showing increased somatic anxiety when participants received mindfulness after the pressure manipulation but 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, the intervention may have primed participants to notice genuine signs of anxiety in their body. Alternatively, the

increased somatic anxiety 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 no 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 our intervention significantly improved golfer’s aim but had no immediate influence on mindfulness or putting performance. While we cannot rule out the benefits of long-term mindfulness training, our mixed findings raise doubts about the 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): see https://osf.io/natj4/?view_only=5d51cc89c87b4c3eb8ba3a91fa4ca6d3

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Supplementary Table 1*Descriptive Statistics of Sample, Broken Down by Randomised Group*

	Total	Group A	Group B
<i>Age</i>			
Mean	51.8	53.1	50.8
Standard Deviation	15.9	15.2	16.5
<i>Gender</i>			
Male	86	45	41
Female	29	17	12
Did not report	1	1	0
<i>Handicap</i>			
Mean	14.6	14.2	15.0
Standard Deviation	10.1	9.7	10.5
<i>Years Playing Golf</i>			
Mean	21.9	23.8	20.8
Standard Deviation	15	14.9	14.9
<i>Education</i>			
Masters degree or higher	18	5	13
Bachelor degree	33	19	14
Diploma	17	7	10
High school certificate	30	13	16
School certificate or lower	20	9	10
<i>Country Born</i>			
Australia	91	40	50
Germany	2	2	0
India	1	0	1
Japan	1	0	1
Malaysia	1	0	1
New Zealand	5	4	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
<i>Meditators</i>			
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	0	0	0
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

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Supplementary Table 2

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
Performance	1.29	0.55	0.56	0.53	0.55	0.65	1.20	0.84
State mindfulness	54.39***	4.62	4.62	4.64	4.62	5.19	3.22	4.46
Overall mechanics	0.16	6.98*	6.91*	7.01*	6.98*	3.87	6.43*	7.32*
<i>Timing mechanics</i>	0.79	2.46	2.47	2.46	2.46	0.19	2.89	1.74
<i>Tendency mechanics</i>	1.34	10.06**	10.05**	10.11**	10.06**	1.33	9.28*	11.51**
<i>Consistency mechanics</i>	0.22	2.96	2.82	2.96	2.96	3.42	2.68	3.18
Cognitive anxiety	2.13	0.18	0.18	0.17	0.18	0.73	0.13	0.07
Somatic anxiety	13.05**	9.22*	9.22*	9.18*	9.22*	3.55	9.96**	7.63*

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 those without a registered handicap.

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CONSORT 2010 checklist of information to include when reporting a randomised trial*

Section/Topic	Item No	Checklist item	Reported on page No
Title and abstract			
	1a	Identification as a randomised trial in the title	1
	1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	2
Introduction			
Background and objectives	2a	Scientific background and explanation of rationale	4-7
	2b	Specific objectives or hypotheses	8
Methods			
Trial design	3a	Description of trial design (such as parallel, factorial) including allocation ratio	8
	3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	9
Participants	4a	Eligibility criteria for participants	9
	4b	Settings and locations where the data were collected	9-10
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	10
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	11-12
	6b	Any changes to trial outcomes after the trial commenced, with reasons	No changes to outcomes
Sample size	7a	How sample size was determined	9
	7b	When applicable, explanation of any interim analyses and stopping guidelines	None

			<u>performed</u>
Randomisation:			
Sequence	8a	Method used to generate the random allocation sequence	<u>10</u>
generation	8b	Type of randomisation; details of any restriction (such as blocking and block size)	<u>10</u>
Allocation	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered	<u>8-9</u>
concealment		containers), describing any steps taken to conceal the sequence until interventions were	
mechanism		assigned	
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned	<u>10</u>
		participants to interventions	
Blinding	11a	If done, who was blinded after assignment to interventions (for example, participants, care	<u>13</u>
		providers, those assessing outcomes) and how	
	11b	If relevant, description of the similarity of interventions	<u>10-11</u>
Statistical methods	12a	Statistical methods used to compare groups for primary and secondary outcomes	<u>13</u>
	12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses	<u>13</u>
Results			
Participant flow (a	13a	For each group, the numbers of participants who were randomly assigned, received intended	<u>14</u>
diagram is strongly		treatment, and were analysed for the primary outcome	
recommended)	13b	For each group, losses and exclusions after randomisation, together with reasons	<u>Flow</u>
			<u>diagram</u>
Recruitment	14a	Dates defining the periods of recruitment and follow-up	<u>14</u>
	14b	Why the trial ended or was stopped	<u>14</u>
Baseline data	15	A table showing baseline demographic and clinical characteristics for each group	<u>Supp. 1</u>
Numbers analysed	16	For each group, number of participants (denominator) included in each analysis and whether the	<u>14</u>
		analysis was by original assigned groups	
Outcomes and	17a	For each primary and secondary outcome, results for each group, and the estimated effect size	<u>Table 1</u>
estimation		and its precision (such as 95% confidence interval)	
	17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	<u>No binary</u>
			<u>outcomes</u>
Ancillary analyses	18	Results of any other analyses performed, including subgroup analyses and adjusted analyses,	<u>14-17 and</u>

		distinguishing pre-specified from exploratory	Supp. 2
Harms	19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	None reported
Discussion			
Limitations	20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	19-22
Generalisability	21	Generalisability (external validity, applicability) of the trial findings	19-22
Interpretation	22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	19-22
Other information			
Registration	23	Registration number and name of trial registry	23
Protocol	24	Where the full trial protocol can be accessed, if available	23
Funding	25	Sources of funding and other support (such as supply of drugs), role of funders	23