



School of Psychology

Ysgol Seicoleg

**A Systematic Review of Standalone Cognitive
Defusion Interventions, and an Empirical Study of
Psychological Flexibility, Burnout and Mental
Wellbeing in Elite Rugby Athletes**

Thesis submitted in partial fulfilment of the requirement for the degree of:

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Preface

Acceptance and Commitment Therapy (ACT) aims to improve an individuals' quality of life through the development of 'psychological flexibility'. This refers to a person's ability to remain connected to the present moment, without getting caught up in thoughts of the past or future, so that they can behave in a way that is in line with their personal values. ACT helps people develop skills in six key areas to increase their psychological flexibility; these are acceptance, contact with the present moment, self as context, values, committed action and cognitive defusion.

The first paper is a systematic review that explores that effectiveness of psychological interventions that use only cognitive defusion as a standalone intervention to improve mental health outcomes. Cognitive defusion involves recognising thoughts as simply words and images produced by the mind, rather than getting caught up in their content. The systematic search found 12 papers that satisfied inclusion criteria and were included in the review. The included studies used a variety of methodologies to provide cognitive defusion interventions, including single-sessions, single-techniques, traditional face-to-face therapy, therapeutic chatbots and mobile apps. The studies used outcome measures to assess a range of aspects of mental health and wellbeing, including anxiety, low mood and stress. The review found that cognitive defusion interventions can be effective in improving participants' mental health outcomes, when compared to participants in control conditions who did not receive any intervention. However, there was little evidence in the reviewed studies to suggest that cognitive defusion was more effective than other types of therapeutic intervention. There was also limited evidence that the improvements seen following interventions were due to changes in levels of cognitive fusion or psychological flexibility, as would be expected if the interventions were addressing the underlying ACT processes. The clinical implications of this

review should be considered with caution, as few studies involved participants who would be representative of people accessing mental health services.

The second paper evaluated how psychological flexibility influenced mental wellbeing and burnout in a sample of UK-based elite rugby union athletes. Previous research looking at mental wellbeing or burnout in elite athletes has focused on how these factors influence their sporting performance. However, more recent shifts in public awareness of mental health and sport have led to an increased interest in methods of protecting mental health in elite athletes. Rugby union players may be exposed to uniquely stressful demands and experiences linked to their sport participation, but there remains little research focused on the factors involved in their mental wellbeing. Psychological flexibility has been shown to be important in protecting mental wellbeing and preventing burnout in people who are not elite athletes. This paper aimed to explore how psychological flexibility influenced burnout and mental wellbeing in elite rugby union athletes. A total of 87 UK-based elite rugby union athletes completed three questionnaires on psychological flexibility, athlete burnout and mental wellbeing. The study found a significant association between psychological flexibility and mental wellbeing, indicating that psychological flexibility had a large positive effect on wellbeing. However, there was no significant association between athlete burnout and mental wellbeing, as was initially hypothesised. In all, this study provided preliminary support for a focus on psychological flexibility in the protection and maintenance of mental wellbeing in elite rugby athletes. However, further research is required to assess the wider implications for clinical psychology in this area.

Paper 1

Cognitive Defusion as a Standalone Intervention for Psychological Wellbeing and Mental Health: A Systematic Review

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3 This paper is prepared in accordance with the author guidelines for the Journal of Contextual Behavioural Sciences (see Appendix A). APA 7th formatting has been used throughout, in line with both South Wales Doctoral Programme in Clinical Psychology and journal guidelines. For the purposes of thesis submission, tables and figures have been embedded in the main body of the paper, however, will be placed on separate pages at the end of the paper for journal submission.

Abstract

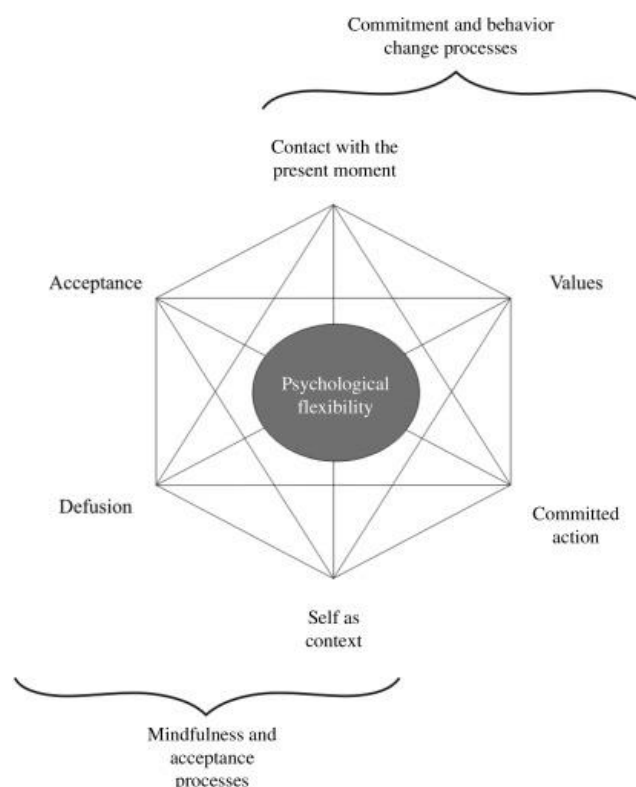
Acceptance Commitment Therapy (ACT) involves developing psychological flexibility through the six 'hexaflex' processes, these are cognitive defusion, contact with the present moment, self as context, values, committed action and acceptance. Cognitive defusion involves observing thoughts as words and images, and relating to them as 'just thoughts' rather than getting caught up their content or trying to suppress thoughts. Previous studies and reviews have examined the effectiveness of using brief and single session ACT, as well as interventions based on a single aspect of the hexaflex model of psychological flexibility. The aim of the present systematic review was to examine the existing evidence for the use of cognitive defusion as a standalone intervention to improve mental health and psychological wellbeing. A systematic search was conducted using PsycInfo, Medline, Emcare, CINAHL, Scopus and Web of Science databases. After screening 406 study abstracts and titles and reviewing 32 full-text articles, 12 papers met inclusion criteria and were included in the final review. The studies used various modes of intervention delivery and mental health outcome measures. The review indicates that cognitive defusion as a standalone intervention can be more effective than no-treatment controls in improving mental health outcomes. However, there is little evidence to suggest that cognitive defusion is superior in improving outcomes compared to other cognitive behavioural therapy techniques, such as cognitive restructuring. Limitations of the reviewed studies are explored, including a lack of clinically relevant samples and little evidence of significant changes to process measures of cognitive defusion and psychological flexibility.

Introduction

Acceptance Commitment Therapy (ACT) does not target the internal experiences directly, but aims to change the way the individual relates to these experiences through psychological flexibility (Zhang et al., 2018). Psychological flexibility has been described as being fully connected to the present moment and acting in accordance with one's values based on what the situation affords (Bond et al., 2006). ACT theory proposes that psychological flexibility is made up of six processes, as described in the 'hexaflex model' (see Figure 1). It suggests that acceptance, contact with the present moment, self as context, values, committed action and cognitive defusion all play important roles in psychological flexibility (Harris, 2019; McHugh, 2011). Various therapeutic techniques are used within ACT to develop skills in these processes and increase psychological flexibility.

Figure 1

A diagram of the ACT Hexaflex, taken from Chin and Hayes (2017).



Acceptance involves a willingness to open up to and make room for painful internal experiences, meaning that clients do not need to struggle in attempts to avoid them (O’Sullivan, 2013). Contact with the present moment refers to the ability to maintain non-judgemental awareness of ongoing internal and external stimuli; this allows the individual to accurately assess a situation and adapt behaviours as necessary, rather than ‘operating on auto-pilot’ or getting caught up in thoughts about the past or future (Segneri, 2021). Self as context work involves developing a stable perspective from which to notice psychological experiences (Segneri, 2021; Yu et al., 2017). Cognitive defusion, on which this systematic review will focus, helps the client to reduce the perceived intensity of power in thoughts by recognising them as *just thoughts*, rather than focusing on their content (O’Sullivan, 2013). Values work focuses on clarifying what is most important to an individual in their life, whilst committed action encourages engaging in effective action in line with their chosen values (Harris, 2019; Viskovich et al., 2021). The hexaflex model can be separated into three modules; the Open module relates to acceptance and cognitive defusion, the Aware module involves connecting with the present moment and self as context, whilst the Engaged module refers to the values and committed action aspects of ACT (Hemmings et al., 2021; Petersen et al., 2021).

There is a large and growing evidence-base demonstrating ACT is an effective therapeutic intervention for various mental health presentations (Dindo et al., 2017), including trauma (Orsillo & Batten, 2005; Twohig, 2009; Wharton et al., 2019), eating disorders (Fogelkvist et al., 2020; Parling et al., 2016), anxiety (Arch et al., 2012; Dalrymple & Herbert, 2007; Ritzert et al., 2016; Swain et al., 2013; Twohig & Levin, 2017), low mood and depression (Bai et al., 2020; Forman et al., 2007; Mahdavi et al., 2017; Walser et al., 2015). These studies have employed various research designs, including large-scale Randomised Controlled Trials (RCTs), individual case studies and reviews. Furthermore, studies have found ACT to be

effective within a range of populations, including healthcare workers (Barrett & Stewart, 2021; Garner & Golijani-Moghaddam, 2021; Prudenzi et al., 2021; Waters et al., 2018), older adults (Davison et al., 2017; Hadi Toroghi & Masoudi, 2019; Ross, 2018; Wetherell et al., 2011), children (Fang & Ding, 2020), adolescents (Halliburton & Cooper, 2015; Lappalainen et al., 2021; Livheim et al., 2015; Woidneck et al., 2014) and those with physical health conditions (A-tjak et al., 2015; Dindo et al., 2017; Trindade et al., 2021; Wetherell et al., 2011; Zhao et al., 2021). A review of ACT interventions for people with chronic pain found support for the cost-effectiveness within this population; however, the authors also acknowledged that further research would be required to evaluate the most efficient means of delivery (Feliu-Soler et al., 2018).

Theoretical Underpinnings of Acceptance Commitment Therapy

Traditional cognitive models have built upon mechanistic principles when understanding internal human experiences of thought and emotions. When applied to cognitive psychology, this suggests that the mind is composed of multiple parts that may develop faults or errors (Harris, 2019; Huys et al., 2016). For example, one mechanistic model hypothesised that habits of thought changed the sequence in which thoughts are retrieved during 'mind wandering' and consequently caused repetitive retrieval of negative memories that characterise rumination (Van Vugt et al., 2018). The aim of such therapeutic approaches would be to replace or repair components that prevent the wider system from functioning (Dodd & Lester, 2021; Michl et al., 2013; Van Vugt et al., 2012). When addressing mental health, this may mean that treatment focuses upon specified factors that appear important to achieving desired change or in alleviating symptoms; such as an individual's access to emotional support or expectations and credibility of the therapeutic intervention (Feinstein et

al., 2013; Fitzgerald & Gallus, 2020; Newman & Fisher, 2010). However, third wave models such as ACT consider psychological distress as a normal aspect of the human experience, rather than indicative of a pathological process; instead, deterioration in wellbeing is driven by attempts to avoid the negative thoughts, feelings or memories (Blackledge & Hayes, 2001).

ACT is grounded in the philosophy of functional contextualism, Applied Behavioural Analysis (ABA) and Relational Frame Theory (RFT) as a framework for understanding human experiences (Hayes, 2004; Vilaradaga et al., 2007). Functional contextualism suggests that we acquire knowledge about ourselves through what we are taught within our verbal and social world; thus, an individual's awareness of their own behaviour and self-knowledge has origins in their social experiences (Hayes & Gregg, 2001). ABA examines a person's behaviour from a functional perspective; this often focuses on the reasons an individual exhibited a specific behaviour, rather than interpreting the actions as symptoms of a general cause (Hoffmann et al., 2016).

According to RFT, the language we use to relate to our internal experiences and realities is fundamental to our psychological functioning (Barnes-Holmes & Roche, 2001). It is suggested that through verbal exploration and explanation, we construct an understanding of ourselves and the world around us based upon our descriptions of relationships between objects (Stynes et al., 2022). As a result, our beliefs about ourselves and others can be influenced by our ability to compare, distinguish, and categorise (Stynes et al., 2022). The more we learn, experience, and interact with our socio-verbal community, the larger and more complex our relational network becomes (McHugh et al., 2019). RFT proposes that human behaviour involves learned patterns of responding to stimuli, based on the relations between them; this is termed 'relational framing' (Boland et al., 2021).

RFT posits that stimuli can influence human behaviour based upon the arbitrarily derived descriptions and relations ascribed to them (Boland et al., 2021). Thus, our comprehension and reaction to an event is based upon the language we use to describe the experience and our relation to it, rather than the nature of the event on its own (Blackledge & Hayes, 2001). Environmental and linguistic terms can prompt the construction of relations; for example, terms such as “better than” or “bigger than” provides a context that can feed into our relational networks (Boland et al., 2021).

Previous literature has supported the clinical usefulness of RFT in formulating psychological experiences (Barnes-Holmes et al., 2020; Raaymakers et al., 2019) while acknowledging the difficulties of applying it within clinical practice (Enoch & Nicholson, 2020). The ACT model theorises that relational framing helps us to recognise and solve problems; however, this process can lead to distress where interpretations employ ‘problematic’ or ‘pathological’ terminologies (Harris, 2019; Hayes et al., 2004).

The term ‘experiential avoidance’ describes attempts to avoid unpleasant internal experiences; this may encompass cognitive strategies (such as thought suppression), engagement in harmful coping strategies (such as substance abuse) or attempts to evade external triggers (such as avoiding people, places, or situations). A fundamental principle of ACT is that the attempted suppression and experiential avoidance of unpleasant internal stimuli actually cause psychological distress (Harris, 2019; Hayes & Wilson, 1994). The ACT model predicts that such failed attempts to avoid unpleasant or negative thoughts, memories or emotions, rather than the content or nature of these internal experiences, impact on psychological wellbeing and overall functioning (Blackledge & Hayes, 2001; Hayes, 2004).

The aim of ACT is to support clients to recognise how the strategies used in attempts to avoid unpleasant internal stimuli can be counterproductive or harmful. Therapists use a range of exercises, including guided mindfulness meditation and metaphors, that help the client reduce their experiential avoidance of these negative internal experiences through the development or progression of psychological flexibility (McHugh, 2011). It has been acknowledged that psychological inflexibility and experiential avoidance are connected, although not related, concepts, as psychological flexibility describes a wider range of concepts (Angelakis & Gooding, 2021).

Psychological Flexibility

Psychological flexibility enables individuals to change their relationship with internal experiences and engage in desired behaviours, even in the presence of unpleasant thoughts or feelings (Pears & Sutton, 2021). The evidence base indicates that increased psychological flexibility can help improve mental health (Dionne et al., 2013), psychological wellbeing (Wersebe et al., 2018) and achieve desired behaviour change (Ciarrochi et al., 2010; Vasiliou et al., 2021). Kashdan and Rottenberg (2010) also suggest that processes of psychological flexibility are reduced or absent within the profiles of many mental health difficulties.

Previous studies have indicated that ACT effectively increases psychological flexibility within a variety of clinical and non-clinical populations (Ghorbani et al., 2021; Howell & Passmore, 2019; Stuart, 2019). Paliliunas et al. (2018) found that a six-week ACT-based intervention focused on the committed action and values aspects of ACT increased graduate students' scores on measures of psychological flexibility. Similarly, a systematic review and meta-analysis found that ACT produced a moderate effect on psychological flexibility among family caregivers (Han et al., 2020). Furthermore, research has presented psychological

flexibility as a mediating factor, affecting the relationship between other target variables. Lin et al. (2018) found that changes in psychological flexibility significantly mediated the effect of an online ACT-based intervention on all outcome measures in a sample of people with chronic pain. Finally, a paper by Prudenzi et al. (2021) found that an ACT-based intervention aimed at increasing psychological flexibility reduced psychological distress in NHS staff, and higher levels of psychological flexibility was associated with increased wellbeing and lower burnout during the covid-19 pandemic.

ACT Hexaflex Processes as Standalone Interventions

The ACT processes elucidated in the hexaflex model are interrelated, with work on one component complementing the development of other skills. For example, self as context skills are likely to assist in increasing contact with the present moment and cognitive defusion, due to the transferable skills of mindful observation (Godbee & Kangas, 2020). However, there is an increasing focus on the feasibility and effectiveness of using brief, targeted psychological interventions. A recent systematic review and meta-analysis indicated that ACT can be effective in people with chronic health conditions when delivered as a single session (Dochat et al., 2021). Similarly, one study found that a single, 60-minute session of ACT could prompt health behaviour changes within a sample of university students (Barreto et al., 2019).

The effectiveness of brief or single-session therapeutic interventions may be explained by studies that have illustrated the ‘dose-response curve’ of improvement during therapy. In this pattern of improvement, an individual makes rapid progress in the initial sessions, which plateaus in subsequent sessions (Baldwin et al., 2009; Harnett et al., 2010). This suggests that there may be limited benefit continuing for multiple sessions. The possibility that more brief

therapies could be equally effective is supported by a recent review demonstrating that ‘initial therapy contacts’, encompassing the first three or fewer hours of therapeutic contact, are the most critical stage of psychological intervention (Aafjes-van Doorn & Sweeney, 2019). Previous studies have found brief ACT-based interventions to be effective in addressing a number of difficulties, including chronic pain (Hadlandsmyth et al., 2019), panic disorder (Meuret et al., 2012) and depression (Kyllönen et al., 2018); however further investigation may be required to clarify the long-term benefits and drawback of this ACT format (Hahs et al., 2019). Godbee and Kangas (2020) reviewed the effectiveness of interventions that taught self as context in isolation and found that these interventions showed outcome improvements that were maintained at a later follow-up. Brief interventions that target identified needs through the selection and focus on specific ACT processes of the hexaflex may be an effective and economic alternative to full therapeutic protocols.

ACT research investigating interventions addressing specific sub processes of the hexaflex have evaluated the success of using only Open or Engaged modules of ACT to delivering full ACT or waiting list controls. One study (Levin et al., 2020) found that those who received the ACT intervention based only on the Engaged components of the Hexaflex demonstrated the same level of improvement as those who received the full ACT protocol. The Open condition also showed significant improvements when compared to the waiting list control, though not as much as the Engaged or full ACT conditions. Similarly, Petersen et al. (2021) found that participants in the Open, Engaged and full ACT conditions all showed improvements in mental health, psychosocial functioning and psychological flexibility; however, the Open intervention improved less on the psychosocial measures, when compared to full ACT.

Previous research using interventions with only the Open module of ACT have demonstrated improvements in mental health symptoms (Levin et al., 2020), symptom severity (Villatte et al., 2016), believability and emotional discomfort of self-critical thoughts (Wollach et al.,

2020). This evidence indicates that it may be effective and appropriate to consider the use of therapeutic interventions that focus on isolated hexaflex processes.

Research exploring individual hexaflex components also allows the evaluation of the theoretical model that underpins ACT-based interventions; thus ensuring robust ACT models of psychological experiences and the refinement of therapeutic ACT-based interventions in clinical practice (Levin et al., 2012; Murphy et al., 2009). One meta-analysis found significant positive effect sizes for all of the hexaflex processes in achieving desired outcomes (Levin et al., 2012). These effects were particularly pronounced when assessed outcomes were theoretically relevant to the psychological flexibility model; for example, believability of distressing thoughts rather than frequency of distressing thoughts. Therefore, while further research is required to ensure that ACT and the individual hexaflex processes operate according to the theoretical underpinnings of psychological flexibility, the existing literature provides some support for this model.

Cognitive Fusion and Defusion

ACT suggests that people can often become entangled in negative evaluations of psychological content; this leads to ‘fusion’ with thoughts that then have more influence over our behaviour (Harris, 2019; O’Sullivan, 2013). Recognising thoughts as simply words and images produced by the mind can enable the individual to defuse from their thoughts and enable engagement in valued actions (Harris, 2006; O’Sullivan, 2013; Segneri, 2021).

Techniques used to achieve cognitive defusion have been broadly organised into five categories, including language conventions, metaphors, changes to language parameters, distancing from internal events, and undermining verbal rules and narratives (Blackledge, 2015). Language conventions aim to help the individual distinguish between themselves and

their thoughts; for example, by verbally framing internal experiences as such: “I am having the thought that”, before reciting a distressing cognition or speaking about the individual using a third person narrative (Ruiz et al., 2021). Metaphors are used throughout ACT and many third wave CBT therapeutic approaches. In cognitive defusion, metaphors can allow the objectification of internal experiences, such as presenting thoughts as ‘passengers on a bus’ (Luoma & Hayes, 2009). One classic cognitive defusion technique that operates by changing language parameters is word repetition, often referred to as the “milk” exercise. The technique requires individuals to repeat a specified word for 30 seconds or more until the perceptual characteristics of the word become more salient than its initial functional meaning. Distancing exercises encourage the individual to observe their internal experiences from a non-judgmental stance, such as the ‘leaves on a stream exercise’. Finally, techniques focused on undermining verbal rules and narratives work by providing evidence against commonly held beliefs around the fusion of internal experiences and subsequent behaviour. For example, individuals can often overestimate the importance of thoughts in predicting behaviour; therefore, exercises may involve guiding individuals to think of not performing an action (e.g. “I will not pick up this pen”) and providing justifications (e.g. “the pen is dirty”), while simultaneously performing the action (Ruiz et al., 2021). Cognitive defusion interventions may, therefore, involve the exploration of each of these categories over several sessions or a brief introduction to a single exercise over the course of a few minutes.

There have been relatively few studies examining cognitive defusion as a standalone intervention for mental health. Previous research has looked at the effectiveness of providing the Open or Engaged modules of ACT, compared to a full-protocol ACT intervention. However, the interventions based on the Open module of ACT differ from the standalone cognitive defusion intervention, as this module also incorporates the hexaflex process of acceptance.

There have been no previous literature reviews focused solely on the use of cognitive defusion as a therapeutic intervention, in isolation from other hexaflex processes. A more detailed understanding of the effectiveness of cognitive defusion as a standalone intervention may provide useful insight to guide more effective and efficient therapeutic practice.

Systematic Review Aims and Objectives

The primary purpose of the current review is to examine, consolidate and evaluate existing research into the use of cognitive defusion as a standalone intervention.

The aims of the present systemic review are:

1. To consolidate existing empirical evidence regarding the use of cognitive defusion as a standalone intervention for the treatment of mental health difficulties and promotion of psychological wellbeing.
2. To assess the quality of these studies examining the use of standalone cognitive defusion interventions.
3. To determine the effectiveness of cognitive defusion as a standalone intervention in the treatment of mental health symptoms and promotion of psychological wellbeing.

Method

The systematic review was conducted in accordance with Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA). The 2020 updated PRISMA guidance (Page et al., 2021) informed the search, synthesis and presentation of this review. This review was also registered with PROSPERO.

Search Strategy

A systematic search was conducted using the PsycInfo, Medline, Emcare databases using Ovid. Searches were also conducted using CINAHL, Scopus and Web of Science. Any relevant grey literature was identified using Proquest Dissertations and Theses, Bielefeld Academic Search Engine and Google Scholar. Searches used the Boolean terms with the key words ‘cognitive defusion’, ‘cognitive fusion’, ‘thought defusion’, ‘thought fusion’, ‘experiential distance’, ‘intervention’, ‘therap*’ and ‘ACT based intervention’. Due to the diverse language used in relation to mental health and psychological wellbeing, searches were not narrowed using key words relating to these concepts; the gathered literature was refined to capture this aspect of the research question in the screening phase. The searches were conducted without publication year restrictions. Full search terms and operators can be found in Appendix B. Papers were not filtered by date of publication and the final search was completed on 29th April 2022.

After the search was completed, data from identified studies (including title and abstract) was then uploaded to Covidence systematic review online software for screening.

Inclusion and Exclusion Criteria

Inclusion criteria were: 1) papers were published in English, 2) papers were published in a peer review journal, published conference proceedings or dissertation available online, 3) study examined cognitive defusion as a standalone intervention, 4) study intervention and/or supplementary support designed by an ACT expert or ACT therapist, 5) study outcomes were validated measures relating to mental health symptoms or factors of psychological wellbeing, and 6) study participants with a mean age of more than 18 years with or without a reported mental health diagnosis.

Exclusion criteria were: 1) studies examining the impact of cognitive defusion on characteristics of the target thought only, 2) studies with a mean participant age of under 18 years, 3) studies that did not conduct statistical analysis of significance on outcome measures related to mental health or psychological wellbeing, and 4) studies in which the cognitive defusion intervention used other therapeutic approaches (including other ACT hexaflex components).

Outcome measures examining mental health symptoms and factors of psychological wellbeing were defined as a standardised and validated measure, completed by the individual, a professional or an informant. These measures would assess the intensity, severity or frequency or experiences associated with mental health or psychological wellbeing, such as depression, anxiety, self-esteem or trauma.

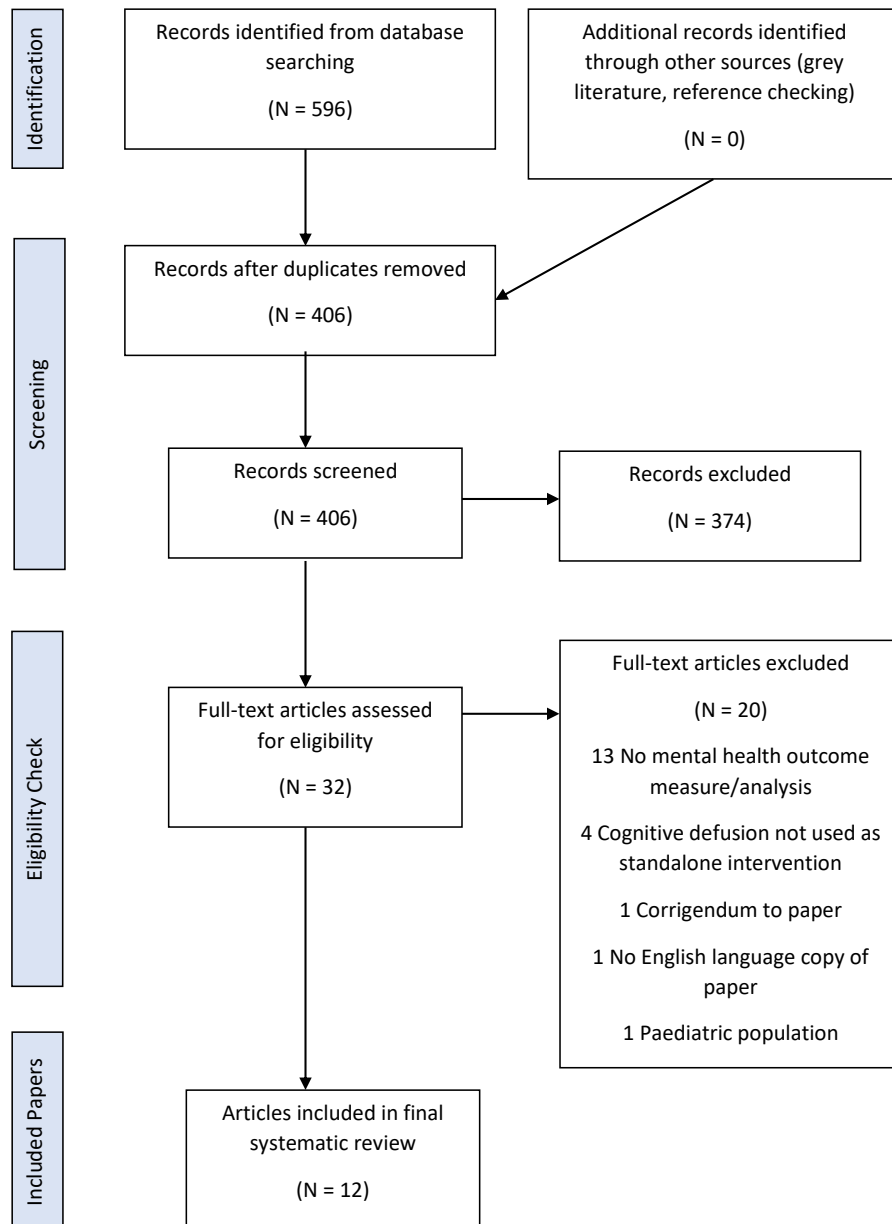
Article Selection

Duplicate articles were removed from the list of identified studies using Covidence prior to screening. The first stage of screening involved examining the title and abstract of identified

papers according to the established inclusion and exclusion criteria. In the second stage of screening, full-texts of the remaining articles were reviewed. A second reviewer also assessed a random selection of 8 papers (25% of total) for eligibility for inclusion during the full-text review. There was 100% agreement between reviewers on the inclusion or exclusion of these papers. Forward citations were then checked using the “cited by” function offered by Google Scholar and a grey literature search was conducted to identify any other eligible studies. The process of article selection is illustrated in Figure 2.

Figure 2.

PRISMA flowchart of the selection of articles for systematic review.



Data Extraction and Synthesis

Data extraction took place using Covidence systematic review online software. A data extraction template was developed to ensure that all relevant information was captured (see Appendix C for data extraction template).

A meta-analysis was not attempted for this review for two primary reasons. First, the included studies did not meet the required homogeneity for a meta-analysis, as the studies focused on different psychopathologies and some of the studies required participants to meet clinical cut-offs on a range of screening measures, whereas other studies did not have this requirement. Secondly, due to the number of different mental health outcome measures employed, encompassing a range of mental health symptoms, it would not be possible to conduct meaningful statistical analyses on the data.

Quality Assessment

The papers identified for inclusion in the review were assessed for quality using standardised critical appraisal tools from Joanna Briggs Institute (JBI). Eleven of the included studies were assessed using the JBI Critical Appraisal Checklist for Randomised Controlled Trials (Tufanaru et al., 2017), see Appendix D. Studies were categorised as RCT's according to the definition published by the National Institute of Clinical Excellence (NICE). The total score for this assessment could range from 0 to 13, with one point for each 'yes' answer and no points for a 'no', 'unclear' or 'not applicable' answer. This tool does not recommend standard cut-off scores for quality ratings, however, previous reviews within healthcare and ACT literature using the JBI critical appraisal tools have applied quality score parameters dependent on the number of questions included, author assessment of overall quality and the aims of the review (Esteves et al., 2022; Hudiyawati & Syafitry, 2021; Li et al., 2021). For

the purposes of this review, scores of 11, 12 or 13 were rated as ‘high’ quality, scores of 8, 9 or 10 were rated as ‘moderate’ quality and a rating score of lower than 8 was labelled as ‘low’ quality.

One paper, that detailed two experiments pertinent to this review, did not involve a randomised design and was assessed using the JBI Critical Appraisal Checklist for Quasi-Experimental Studies (Tufanaru et al., 2017), see Appendix E. The total score for this tool could range from 0 to 9, with one point for each ‘yes’ answer and no points for a ‘no’, ‘unclear’ or ‘not applicable’ answer. For the purposes of this review, scores of 8 or 9 were rated as ‘high’ quality, scores of 6 or 7 were considered ‘moderate’ quality and scores lower than 6 were rated as ‘low’ quality.

Studies rated as low quality remained within the review narrative synthesis, with noted cautions for the interpretation of study findings.

To ensure the assessments of study quality were robust, inter-rater reliability was calculated. A second reviewer independently applied the tool to a randomly selected subsample of three papers (25%). There was 100% agreement between raters on the assessment of papers as ‘high’, ‘moderate’ or ‘low’ quality. There were two differences in ratings of individual items of the JBI critical appraisal tool (i.e., “were outcome assessors blind to treatment assignment?”) noted during the double coding process. These differences did not affect the overall quality rating applied to papers and were resolved through verbal discussion until a consensus was reached.

Results

Overview of Study Characteristics

Table 1 provides an overview of the studies included in this systematic review. A total of 32 full-text articles were obtained and reviewed; 12 papers satisfied the inclusion and exclusion criteria and are summarised in this section. The final selection of studies included 8 published peer-reviewed journal articles and 4 dissertation papers.

Participants

In total, 938 participants were involved in the studies included in this review, however, 202 of these individuals (21.5%) were not included in final analyses due to attrition. The number of reported female participants in initial samples ($N = 465$) was more than double the number of male participants across the studies ($N = 221$). One study recruited two participants who identified as non-binary (Lavelle et al., 2021). Some studies reported information on gender distributions in initial samples, prior to attrition, and some reported gender in the final sample included in data analyses; however, one study did not report any gender distribution information (Way, 2013).

The majority of the studies involved recruitment of students through universities (9 of the 12 included papers). Demographic information, such as age, was reported differently across the studies. Eight studies presented a mean age; the average age of these 668 participants was 23.28 years. The study samples were recruited from the UK ($N = 2$), USA ($N = 6$), Canada ($N = 1$), Ireland ($N = 2$) and Spain ($N = 1$).

Six studies invited participants who subjectively reported or scored above pre-determined scores on measures of specific psychological difficulties. Populations sampled in the

reviewed studies included individuals who self-reported or were assessed at baseline as experiencing difficulties in body image (N = 1), claustrophobia (N = 2), low self-esteem (N = 1), worry (N = 1), self-criticism (N = 1) and mental health symptoms (N = 1). Two studies only involved participants without a current diagnosis of a mental health difficulty. The remaining studies did not screen participants' experience of mental health difficulties in any domain.

Table 1*Summary of included papers*

Author and Year	Country	Article Type	Study Design	Participants	Sample Size	Age (mean unless otherwise stated)	Gender
Brandrick et al., 2021	UK	Peer-reviewed published article	Randomised experimental study	Recruited from community (N=12) and university (N=51)	63	25.7	17 Male 46 Female
Cleary, 2021	Ireland	Dissertation	Two non-randomised experimental studies	University students without a clinical diagnosis of mental health difficulty	Experiment 1 - 55 Experiment 2 - 35	18-65 years old (range)	Experiment 1 – 18 Male, 37 Female Experiment 2 – 19 Male, 16 Female
Deacon et al., 2011	USA	Peer-reviewed published article	Randomised experimental study	University students scoring above mean for women with eating disorders on BSQ*	26	19.4	26 Female
Dublin, 2013	USA	Dissertation	Randomised experimental study	University students who scored above mean on CLQ* ² and reported SUDs of at least 50 out of 100 in behavioural task	45	18 – 27 years (range) 64.4% 18 years	7 Male 38 Female

Notes: * Body Shape Questionnaire (BSQ) *² Claustrophobia Questionnaire (CLQ)

Author and Year	Country	Article Type	Study Design	Participants	Sample Size	Age (mean unless otherwise stated)	Gender
Hinton & Gaynor, 2010	USA	Peer-reviewed published article	Randomised experimental study	University students exceeding one standard deviation above BSI* ³ or RSES* ⁴ mean	22	20.09	6 Male 16 Female
Larsson et al., 2016	UK	Peer-reviewed published article	Randomised experimental study	Adults recruited from community and university	83	26	16 Male 67 Female
Lavelle et al., 2021	Ireland	Peer-reviewed published article	Randomised experimental study	Adults without a current diagnosis of anxiety/depression or receiving psychological intervention	223 (72% attrition) 61 in final analyses	25.98	12 Male 49 Female
Levin et al., 2018	USA	Peer-reviewed published article	Randomised experimental study	Adults recruited from community and university with a score of 19 or higher on the Inadequate-Self subscale of the FSCRS* ⁵	87	22.76	60 Male 27 Female

Notes: *³ Brief Symptom Inventory (BSI) *⁴ Rosenberg Self-Esteem Scale (RSES) *⁵ Forms of Self-Criticism and Self-Reassurance Scale (FSCRS)

Author and Year	Country	Article Type	Study Design	Participants	Sample Size	Age (mean unless otherwise stated)	Gender
López de Uralde-Selva & Valero-Aguayo, 2021	Spain	Peer-reviewed published article	Randomised experimental study	No specific recruitment requirements reported	30	27	6 Male 24 Female
Monk et al., 2011	USA	Dissertation	Randomised experimental study	University students scoring within one standard deviation of the CLQ* ² mean for claustrophobic students	45	17-25 years (range) Mean age over 18 years	14 Male 31 Female
Watson, Burley & Purdon, 2009	Canada	Peer-reviewed published article	Randomised experimental study	University students	134 (25.37% attrition) 93 included in final analyses	19.33	46 Male 88 Female in initial sample
Way, 2013	USA	Dissertation	Non-randomised experimental studies	Adults who scored 3 or more on a minimum of 5 questions on the PSWQ* ⁶ and self-reported recurrent worry	90	Not reported	Not reported

Notes: *⁶ Penn State Worry Questionnaire

Cognitive Defusion Intervention Modalities

Reviewed studies provided cognitive defusion interventions using a variety of methodologies (see Table 2). Five studies focused on instructing participants in cognitive defusion in a face-to-face single session or focusing on a specific skill (such as verbal repetition). Of these, four studies used only the “milk” verbal repetition task as a cognitive defusion technique. Two additional studies used this design and also required participants to practice skills and complete ‘homework’ tasks for a pre-determined period following the single session. In two other studies cognitive defusion interventions were delivered using online chatbots or mobile phone apps. The chatbot intervention worked by providing a script to guide individuals through cognitive defusion techniques; this required participants to respond using text or selecting multiple choice options presented by the chatbot. The mobile app provided participants with skill coaching sessions for cognitive defusion exercises and techniques. The remaining three papers provided face-to-face cognitive defusion as a therapeutic intervention over multiple sessions and up to a six-week time period. Five of the studies assessed the effect of a cognitive defusion intervention conducted in a single session. Of the seven studies using interventions over longer time periods, six demonstrated significant improvements in outcome measures following cognitive defusion interventions.

Control and Comparison Interventions

Various control condition designs were evident in the studies. Most studies used waiting list (N = 4) or no instruction (N = 4) control conditions. Other papers also asked participants to complete a brief numerical task (N = 1), read neutral short stories (N = 1) and complete an imaginal exposure task as a control condition (N = 1).

Out of the twelve included studies, nine utilised both a control condition and a comparison intervention that did not use cognitive defusion. One study did not involve a control condition and two did not compare cognitive defusion to any other approaches. The majority of comparison interventions were based on cognitive restructuring (N = 7), a technique taken from Cognitive Behavioural Therapy (CBT). However, cognitive reframes (N = 1), extinction techniques (N = 1) and imaginal exposure (N = 1) were also used.

Post-Intervention and Follow-Up Measures

Five studies completed a post-intervention measure on the same day as the cognitive defusion intervention was conducted. Other single session intervention studies required participants to complete homework, with post-intervention outcome measures complete five to seven days later (N = 2). The remaining studies completed post-intervention outcome measures between five days and six weeks after starting the cognitive defusion intervention.

Although some of the included studies completed multiple baseline measures (i.e., baseline and pre-intervention), only one paper completed follow-up outcome measures to assess the longevity of change observed. Hinton and Gaynor (2010) conducted a one-month post-intervention follow-up which included completing Beck's Depression Inventory (BDI-II) and the Body Shape Questionnaire (BSI).

Process Measures and Manipulation Checks

Two studies conducted a manipulation check to ensure that concepts relating to cognitive defusion had been understood and retained by participants. Dublin (2013) found that 86.67% of responses in the manipulation check reflected learning of cognitive defusion principles,

and Monk et al., (2011) found that 80% of participants in the cognitive defusion condition correctly identified the strategy of the intervention.

Seven studies used psychometric process measures to examine whether cognitive defusion interventions led to changes in psychological flexibility and cognitive fusion. Five studies assessed changes in psychological flexibility using the Acceptance and Action Questionnaire (AAQ), with only Hinton and Gaynor (2010) observing a significant increase in AAQ scores following the cognitive defusion intervention, which was significantly different to the wait-list control. Three studies assessed cognitive fusion as a process measure using the Cognitive Fusion Questionnaire (CFQ; N = 2) and The Mini Cognitive Defusion and Slippage Scale (N = 1). Hinton and Gaynor (2010) found a significant improvement in the defusion subscale of the Mini Cognitive Defusion and Slippage Scale following cognitive defusion intervention; however, Lavelle et al. (2021) and Levin et al., (2018) did not find any significant changes in CFQ scores.

Three studies did not use process measures or assess participant understanding and retention of cognitive defusion principles (Brandrick et al., 2021; Deacon et al., 2011; López de Uralde-Selva & Valero-Aguayo, 2021).

Table 2*Summary of study designs*

Author and Year	Cognitive Defusion Intervention	Comparison Intervention	Control Condition	Primary Mental Health Outcome Measure	Secondary Mental Health Outcome Measure
Brandrick et al., 2021	Participants chose word to encompass their PSA*, completed 30-second word repetition of target word SS* ⁴	30-second exercise to cognitively reframe thought as positive and rational	30-second numerical task	Personal Report of Public Speaking Anxiety (PRPSA)	None
Cleary, 2021	Experiment 1 – 3.5-minutes of ‘Hands as Thoughts’ exercise SS* ⁴ Experiment 2 – 3.5-minutes of ‘Hands as Thoughts’ exercise, delivered by pre-recorded video SS* ⁴	None	Experiment 1 – participants read a compilation of short stories for 3.5 minutes Experiment 2 – as above but delivered via pre-recorded audio file.	State Trait Anxiety Inventory (STAI)	None
Deacon et al., 2011	CD * ² psychoeducation and introduction to word repetition exercise, instructed to practice technique daily for a week	CR* ³ psychoeducation and introduction to thought challenging, thought evaluation and generating alternatives	None	Body Shape Questionnaire (BSQ)	None

Notes: * Public Speaking Anxiety (PSA) *² Cognitive Defusion (CD) *³ Cognitive Restructuring (CR) *⁴ Single Session (SS)

Author and Year	Cognitive Defusion Intervention	Comparison Intervention	Control Condition	Primary Mental Health Outcome Measure	Secondary Mental Health Outcome Measure
Dublin, 2013	Three, 40-minute sessions over two weeks. Followed a CD protocol created by authors and delivered by graduate student	Three, 40-minute sessions over two weeks. Followed a CR protocol modelled on 'Mind over Mood' and delivered by different graduate students	Wait-list control	State-Trait Anxiety Inventory (STAI)	Claustrophobia Questionnaire (CLQ)
Hinton & Gaynor, 2010	Three, weekly, one-hour individual treatment sessions in addition to three assessment sessions	None	Wait-list control	Brief Symptom Inventory (BSI)	Becks Depression Inventory II (BDI-II)
Larsson et al., 2016	Introduced to three CD strategies - "I'm having the thought that", musical thoughts and funny voices. Used CD strategies over five-day period and completed online thought log.	Introduced to three cognitive restructuring strategies and unhelpful thinking styles. Advised to use CR strategies over a five day period and complete online thought log.	No instruction control; continued to complete online thought log	Becks Depression Inventory II (BDI-II)	State Trait Anxiety Inventory

Author and Year	Cognitive Defusion Intervention	Comparison Intervention	Control Condition	Primary Mental Health Outcome Measure	Secondary Mental Health Outcome Measure
Lavelle et al., 2021	Ten-minutes of daily conversations on author-programmed chatbot presented responsive text over five chronological days, audio and video recordings to introduce CD techniques (including vocalisations and visualisations). Accessed on Facebook instant messenger.	Ten-minutes of daily conversations on author-programmed chatbot presented responsive text, audio and video recordings, based on typical CR protocol. Accessed on Facebook instant messenger.	Inactive control	Positive and Negative Affect Scale	None
Levin et al., 2018	Twenty-minute online training session, followed by mobile app delivered skills coaching and CD exercises (including flexibility and interactive defusion skills) over two weeks	Twenty-minute online training, followed by mobile app delivered skills coaching and CR exercises (including gathering evidence and generating alternative thoughts) over two weeks	Two week wait-list control	Depression, Anxiety and Stress Scale-21	Forms of Self-Criticism and Self-Reassurance Scale
López de Uralde-Selva & Valero-Aguayo, 2021	“Milk” word repetition exercise SS* ⁴	Extinction exercise, in which ‘emotion word’ was repeated	No intervention control	Emotional Regulation Questionnaire	None

Author and Year	Cognitive Defusion Intervention	Comparison Intervention	Control Condition	Primary Mental Health Outcome Measure	Secondary Mental Health Outcome Measure
Monk et al., 2011	Pre-recorded 30-minute video session based on 'The Mindfulness and Acceptance Workbook' SS* ⁴	Pre-recorded 30-minute CR video session based on 'Mind over Mood'	Imaginal exposure control group. Given information on the effectiveness of exposure in reducing phobic avoidance	Claustrophobia Questionnaire (CLQ)	The Mood and Anxiety Symptom Questionnaire - Anxious Arousal scale
Watson, Burley & Purdon, 2009	Pre-recorded 5.5-minute video on CD and demonstrating the "milk" word-repetition technique SS* ⁴	Pre-recorded 4.25-minute video on exposure and demonstrating the imaginal exposure technique	Inactive control (sat quietly for three minutes)	Interpretation of Intrusions Inventory (III)	None
Way, 2013	Six-week program adapted from Forsyth & Eifert (2007), Hayes (2005) and McKay, Fanning and Zurita-Ona (2011b). Provided worksheets electronically and focused on developing CD skills.	Six-week program adapted from McKay et al. (2011a) self-help workbook. Provided with electronic worksheets and focused on CR.	Wait-list control	Penn State Worry Questionnaire	Depression Anxiety Stress Scale - 42

Quality Assessment and Critical Appraisal

A quality assessment of the included studies was conducted using the Joanna Briggs Institute (JBI) critical appraisal tools. Eleven studies were assessed using the JBI Checklist for Randomised Controlled Trials; one non-randomised study was assessed using the JBI Checklist for Quasi-Experimental Studies. Quality was rated as 'high', 'moderate' or 'low' according to the criteria set out in the method section.

The results of the quality assessment are illustrated in Table 3. Overall, the majority of included studies were assessed to be of high (N = 5) or moderate (N = 6) quality. One study was rated as low quality; this was primarily related to a lack of clear reporting of randomised processes, blinding and low statistical power (López de Uralde Selva & Valero Aguayo, 2021). The study rated as low quality was included in the review synthesis, as it was determined that, while the authors indicated several methodological considerations of risk of bias, the overall rating was primarily determined by the lack of specific description of how these biases were addressed. Therefore, it was felt that the findings of this study may still be of value within the context of the other reviewed papers, when interpreted with caution.

Three studies were also identified as having insufficient statistical power when completing time by condition interaction analyses based upon a significance level of $p < .05$ and a power level of 0.8 (Deacon et al., 2011; Lavelle et al., 2021; López de Uralde Selva & Valero Aguayo, 2021). This calculation was based upon effect sizes reported in the papers (where this was not reported a medium effect size was assumed) and calculated using the post-hoc feature of G*Power software by the author. However, two of these studies did have adequate statistical power to conduct the within-subjects analyses in the effect of time in the cognitive defusion intervention (Deacon et al., 2011; Lavelle et al., 2021). The study conducted by Lavelle et al. (2021) appeared to be affected by considerable participant attrition between pre

and post intervention assessment periods, which may have impacted on the achieved statistical power when examining interaction effects.

Table 3

Summary of quality assessment outcomes

Author and Year	Assessment Tool	Assessment Score	Overall Rating
Brandrick et al., 2021	JBI Checklist for RCT	9/13	Moderate
Cleary, 2021	JBI Checklist for Quasi-Experimental Studies	8/9	High
Deacon et al., 2011	JBI Checklist for RCT	8/13	Moderate
Dublin, 2013	JBI Checklist for RCT	8/13	Moderate
Hinton & Gaynor, 2010	JBI Checklist for RCT	11/13	High
Larsson et al., 2016	JBI Checklist for RCT	8/13	Moderate
Lavelle et al., 2021	JBI Checklist for RCT	10/13	Moderate
Levin et al., 2018	JBI Checklist for RCT	11/13	High
López de Uralde-Selva & Valero-Aguayo, 2021	JBI Checklist for RCT	5/13	Low
Monk et al., 2011	JBI Checklist for RCT	11/13	High
Watson, Burley & Purdon, 2009	JBI Checklist for RCT	11/13	High
Way, 2013	JBI Checklist for RCT	8/13	Moderate

Anxiety Outcomes

Half of the studies included in this review used a primary or secondary outcome measure assessing different types of anxiety (N = 6) and completed analyses examining the impact of cognitive defusion interventions on participants' scores. These measures include self-report questionnaires on public speaking anxiety (N = 1), state-anxiety (N = 3), anxious arousal (N = 1) and worry (N = 1). Two studies also conducted measures of claustrophobia (N = 2), which has been included within the anxiety outcomes due to the classification of specific phobias within 'Anxiety Disorders' of DSM-5 (American Psychiatric Association, 2013).

The outcomes of these studies are presented in Table 4. Five of the six studies observed statistically significant improvements in at least one measure of anxiety following a cognitive defusion intervention. However, only two studies demonstrated significant time x condition interactions, which would indicate one condition performing significantly better than the others. In these studies, it was found that participants in the cognitive defusion intervention condition saw a significantly greater improvement in scores on anxiety outcome measures compared to those in control conditions. However, the five studies that involved a comparison intervention condition (such as cognitive restructuring interventions) found that there was no significant difference between the cognitive defusion intervention conditions and comparison conditions.

Table 4*Outcomes of Studies Evaluating Cognitive Defusion Interventions on Measures of Anxiety*

Author and Year	Significant Time x Condition Interaction	Significant Effect of Time	Estimated Effect Size	CD Significantly Superior to Control	CD Significantly Superior to Comparison Intervention
Brandrick et al., 2021 <i>PRPSA</i>	No	Yes	$d = .51^{**}$	No † ²	No § ²
Cleary, 2021 <i>STAI</i>	Experiment 1 – Yes	Yes	$\text{Eta}^2 = .58^{***}$	Yes † ³	N/A
	Experiment 2 – No	Yes	$\text{Eta}^2 = .20^{***}$	No † ⁴	N/A
Dublin, 2013 <i>STAI</i> <i>CLQ</i>	Yes	Yes	Not reported	Yes †	No §
	No	No	N/A	No †	No §
Larsson et al., 2016 <i>STAI</i>	No	No	N/A	No †	No §
Monk et al., 2011 <i>CLQ</i> <i>MASQ-AA</i>	No	Yes	Not reported	No † ⁵	No §
	No	No	N/A	No † ⁵	No §
Way, 2013 <i>PSWQ</i>	No	Yes	Not reported	No †	No §

* Small effect size ** Medium effect size *** Large effect size

† Inactive control condition (waiting list, no instruction, etc.) †² 30 second numerical task †³ Read a compilation of short stories †⁴ Use typical cognitive strategies †⁵ Imaginal Exposure

§ Cognitive restructuring task/sessions §² Cognitive reframe §³ Extinction §⁴ Imaginal exposure

Depression and Mood Outcomes

Four of the included studies used outcome measures that assessed aspects of participant depression, emotional regulation or affect. This was assessed using the BDI-II (N = 2), PANAS (N = 1) and the ERQ (N = 1) as primary or secondary outcome measures of the cognitive defusion interventions. The results of these studies are summarised in Table 5.

The two studies that used the BDI-II as an outcome measure to evaluate the effectiveness of the cognitive defusion intervention reported significant effects of time in this condition (Hinton & Gaynor, 2010; Larsson et al., 2016). This indicates a significant improvement in participant scores on the BDI-II between pre and post treatment. Hinton and Gaynor (2010), which was rated as a high-quality study, also completed a one-month follow-up assessment and found that the large significant improvements observed between pre and post intervention were maintained with a small to moderate effect size between post-intervention and follow-up.

The other studies examining measures of emotion and mood did not report any significant changes in scores following the cognitive defusion intervention (Lavelle et al., 2021; López de Uralde Selva & Valero Aguayo, 2021). It is important to note that both studies were found to have inadequate power to detect a time by condition interaction with a medium effect size; however, Lavelle et al. (2021) did have sufficient power to establish an effect of time within the cognitive defusion intervention and this was not found. Furthermore, the study conducted by López de Uralde Selva and Valero Aguayo (2021) was assessed to be of low quality, according to the JBI quality appraisal tool. This was partially due to a lack of transparency around the methodological processes such as blinding and lack of assessment of baseline differences between participants.

Table 5*Outcomes of Studies Evaluating Cognitive Defusion Interventions on Depression and Mood Measures*

Author and Year	Significant Time x Condition Interaction	Significant Effect of Time	Estimated Effect Size	CD Significantly Superior to Control	CD Significantly Superior to Comparison Intervention
Hinton & Gaynor, 2010 <i>BDI-II</i>	Yes	Yes	$g = 1.04^{***}$	Yes †	N/A
Larsson et al., 2016 <i>BDI-II</i>	No	Yes	$d = 1.61^{***}$	Yes †	Yes §
Lavelle et al., 2021 <i>PANAS</i>	No	No	$np^2 = .009^*$	No †	No §
López de Uralde-Selva & Valero-Aguayo, 2021 (Assessed as low quality) <i>ERQ</i>	No	No	Not reported	No †	No § ³

* Small effect size ** Medium effect size *** Large effect size

† Inactive control condition (waiting list, no instruction, etc.) †² 30 second numerical task †³ Read a compilation of short stories †⁴ Use typical cognitive strategies †⁵ Imaginal Exposure

§ Cognitive restructuring task/sessions §² Cognitive reframe §³ Extinction §⁴ Imaginal exposure

General Mental Health and Symptoms Outcomes

The results of studies assessing the impact of cognitive defusion interventions on general mental health and symptoms are summarised in Table 6 (N = 5). Three of the studies in this group were rated as high quality and the remaining two were described as moderate quality.

Four studies within this group reported varied results across eight included measures. Of these, two studies were rated as high quality and two as moderate quality. In seven of the eight measures reported, significant improvements were observed in the cognitive defusion condition. Four measures showed significantly larger improvements in the cognitive defusion condition than other conditions. These improvements ranged from moderate to large effect sizes, where these were reported. However, improvements on only four of the measures were found to be significantly different to those detected in control groups. There were no significant differences between the changes observed in the cognitive defusion group and comparison intervention conditions.

Table 6*Outcomes of Studies Evaluating Cognitive Defusion Interventions on Measures of General Mental Health and Symptoms*

Author and Year	Significant Time x Condition Interaction	Significant Effect of Time	Estimated Effect Size	CD Significantly Superior to Control	CD Significantly Superior to Comparison Intervention
Deacon et al., 2011					
<i>BSQ</i>	No	Yes	$d = .49^*$	N/A	No §
Hinton & Gaynor, 2010					
<i>BSI</i>	Yes	Yes	$g = .70^{**}$	Yes †	N/A
Levin et al., 2018					
<i>DASS-21</i>	Yes	Yes	$d = .72^{**}$	Yes †	No §
<i>FSCSR – Hatred Self-Criticism</i>	Yes	Yes	$d = .93^{***}$	Yes †	No §
<i>FSCSR – Inadequate Self-Criticism</i>	No	No	Not reported	No †	No §
<i>FSCSR – Self-Reassurance</i>	Yes	Yes	$d = .61^{**}$	Yes †	No §
Watson, Burley & Purdon, 2009					
<i>III</i>	No	Yes	Not reported	No †	No § ⁴
Way, 2013					
<i>DASS-42</i>	No	Yes	Not reported	No †	No §

* Small effect size ** Medium effect size *** Large effect size

† Inactive control condition (waiting list, no instruction, etc.) †² 30 second numerical task †³ Read a compilation of short stories †⁴ Use typical cognitive strategies †⁵ Imaginal Exposure

§ Cognitive restructuring task/sessions §² Cognitive reframe §³ Extinction §⁴ Imaginal exposure

Discussion

Main Findings

Overall, the majority of studies included in this review demonstrated that cognitive defusion was effective in improving participant scores on measures of mental health or psychological wellbeing (N = 15 out of 21 study measures). Where reported, these significant improvements ranged between moderate and large effect sizes. Only one study completed a follow-up assessment beyond the end of the intervention, which found that small to moderate effects had been maintained one month after completion of the intervention (Hinton & Gaynor, 2010). However, this review did not consistently find significant improvements on outcome measures within cognitive defusion intervention conditions. Cognitive defusion outperformed its comparison intervention condition in only one study measure (out of a possible 17 study measures). Therefore, although most of the studies observed improved outcomes following a cognitive defusion intervention, these improvements were not often significantly different to comparison interventions such as cognitive restructuring.

These findings may be partially explained by some of the included study characteristics. First, the methods in which cognitive defusion interventions were administered and delivered were varied. Studies provided therapeutic interventions using traditional weekly sessions, pre-recorded videos, mobile apps, chatbots, single session interventions and single task experimental designs. Due to the number of different cognitive defusion intervention delivery methods, it is possible that differences in study results could be reflective of differences in participant engagement or suitability of a method for this intervention. It is notable that, of the eight studies that involved some of the cognitive intervention being delivered face-to-face by a researcher or therapist, all but one observed significant improvement in at least one relevant outcome measure. The exception was the study conducted by López de Uralde Selva

and Valero Aguayo (2021), which was rated as low quality due to the lack of reported methods and inadequate statistical power; this may have reduced the study's ability to detect any significant improvements in the cognitive defusion intervention.

Second, there was large variation in the outcome measures employed to assess the effectiveness of cognitive defusion interventions. The outcome measure used most frequently in the included studies was the State Trait Anxiety Inventory (STAI), however, other measures evaluated distress about body shape, intrusions, public speaking anxiety, claustrophobia, etc. ACT is a transdiagnostic therapeutic approach, however, as a single component of the wider ACT model, it is possible that cognitive defusion may produce greater effects when addressing specific processes. For example, this review found the largest effect sizes in studies using outcome measures assessing anxiety, depression and self-criticism. These difficulties often involve distressing and intrusive thoughts about oneself, others or the world. It is possible that cognitive defusion is particularly effective when targeting these difficulties, as it reduces the perceived intensity or distance to distressing thoughts. Furthermore, it is possible that the differences in findings of the studies assessing emotion and mood could be partially accounted for by the measures used. For example, although the BDI-II, PANAS and ERQ could be widely considered under the umbrella of 'emotion and mood', they assess very distinct constructs of psychological wellbeing. When considered using the psychological flexibility model, the circular and looping thoughts that characterise rumination in depression may be directly influenced by cognitive defusion techniques that prevent being 'hooked' into distressing thoughts. However, it is possible that affect and emotional regulation may be more strongly affected by other components of the hexaflex model, such as mindfulness or contact with the present moment (Kashdan & Rottenberg, 2010).

There were also considerable variations in the employment and findings of process measures or manipulation checks. Of the nine psychometric process measures used across the studies, only two used by Hinton and Gaynor (2010) demonstrated significant improvement following a cognitive defusion intervention. This may indicate that cognitive defusion interventions were not bringing about change via the conceptually proposed mechanisms. Five papers used the AAQ or AAQ-II to assess changes in psychological flexibility. However, this measure requires respondents to consider how their experiences impact on their general life, for example, one AAQ-II item is “Emotions cause problems in my life”. It may be that insufficient time had passed following the cognitive defusion intervention for changes to be detected on this measure. Alternatively, the intervention may have prompted increased awareness of psychological flexibility and cognitive fusion in participants, and caused non-significant changes to scores, as participants reflect more accurately on their experiences. Manipulation checks showed that 86.67% and 80% of participants were able to correctly identify cognitive defusion principles and strategies (Dublin, 2013; Monk, 2011); however, this does not demonstrate that participants applied these skills. This may suggest that unassessed factors, other than an individuals’ level of cognitive fusion, influenced the improvement observed in the included studies.

Integration with Existing Literature

Overall, the present review provides support for the use of the ACT and psychological flexibility model in the improvement of mental health domains within clinically relevant and general populations, as suggested by previous literature. Large effect sizes were observed in studies assessing the effect of cognitive defusion interventions on anxiety and low mood. Previous studies have demonstrated that ACT interventions are effective in the treatment of

individuals experiencing anxiety and low mood; however, this review may indicate that cognitive defusion, as a single component of the psychological flexibility model, can also be an effective therapeutic intervention (Bai et al., 2020; Twohig & Levin, 2017).

This review also included significant findings from a number of studies that used brief, single session and single technique cognitive interventions. This may provide further support for previous literature which suggests that brief psychological interventions can be effective in improving psychological wellbeing (Barreto et al., 2019; Dochat et al., 2021). Furthermore, a review completed by Godbee and Kangas (2020) found that a single component of the psychological flexibility hexaflex model could be utilised as an effective intervention for psychological wellbeing. The present review also provides some evidence for the use of single components of the hexaflex model as effective therapeutic interventions. However, further trials would be required to evaluate the use of cognitive defusion interventions within clinically relevant populations before more general conclusions could be drawn.

Strengths and Limitations of Included Studies

The quality assessment conducted for the purposes of this review indicated that the majority of the included studies were of a good quality. When applying the JBI critical appraisal tool, the paper by López de Uralde Selva and Valero Aguayo (2021) was evaluated to be low quality. When completing this quality assessment, it was apparent that, despite a low statistical power, the rating was most severely affected by missing information about the methodology and procedures.

Despite the generally good quality of the included studies, there were some limitations that should be considered when interpreting the findings of this systematic review. One limitation relates to the lack of demographic diversity within participants samples across the studies.

Not all studies reported the demographic characteristics of the samples. However, it is evident that there was a large over-representation of female participants. Similarly, a large portion of the total review sample were young adult university students. The location of recruitment (community, university, clinical settings, etc.) was reported for 515 of the total participants; of this number, 413 were identified as university students. Furthermore, whilst not all studies reported a mean age, the range of reported means was 19.33 to 27 years, thus representing a relatively young sample population. Finally, there was an inconsistent approach to existing mental health difficulties across the twelve studies and samples. Eight of the studies required participants to score above or below a screening measure cut-off to be eligible to complete the study. However, five studies excluded participants who had an existing mental health diagnosis or were receiving psychological/psychiatric treatment. In all, the inconsistent approach to recruitment and limited diversity in samples means that it is difficult to generalise the findings of the review or apply the results to therapeutic practice.

The wide variety of mental health outcome measures allowed the review to capture the impact of cognitive defusion intervention within a range of parameters and presentations. However, some of the measures assess short-term and transient aspects of wellbeing which may not be sufficiently reflective of overall mental health to make accurate or adequate conclusions on the impact of cognitive defusion interventions on these wider concepts of health and wellbeing. For example, the STAI-S measures anxiety in that moment; therefore, participants may achieve scores indicating high levels of anxiety on this measure without corresponding difficulties in daily functioning as a result of anxiety. Similarly, the PANAS assesses affect, which is transient and may change frequently without more generalised disorders of mood. For instance, a study participant may describe negative affect on assessment, however, this does not make them representative of an individual with persistent, clinically relevant low mood or depression.

All of the studies reported using cognitive defusion as a standalone intervention. However, ACT literature does acknowledge a large amount of overlap across the hexaflex processes presented in the psychological flexibility model (Godbee & Kangas, 2020; Rolffs et al., 2018). Therefore, it is possible that the cognitive defusion interventions unintentionally incorporated other hexaflex components. This may be particularly relevant to acceptance, which makes up the second process within the open module of ACT (Villatte et al., 2016). Some aspects of cognitive defusion, such as not fighting or trying to suppress distressing thoughts, may overlap with the concept of acceptance. Furthermore, few of the studies provided clear protocols to describe the content and delivery of the cognitive defusion intervention and none assessed the model fidelity of therapists or interventions. Assessments of fidelity would have increased the ability to generalise the findings of the studies, apply to clinical practice and ensure that research in this area is examining the same constructs (O'Neill et al., 2019). Therefore, future research in this area would benefit from the inclusion of clear intervention protocols and assessments of fidelity to increase the quality of this literature.

A final limitation of the reviewed studies is the lack of follow-up assessment subsequent to the post-intervention time point. Only one of the twelve studies conducted a one-month follow-up and observed that improvements post-intervention were maintained with small to moderate effect sizes. Without future research in this area providing further data, it will not be possible to comment on long-term effectiveness, participant knowledge and skill retention or the feasibility of conducting cognitive defusion interventions within clinical settings.

Strengths and Limitations of the Review Process

In order to best address the aims of the review, the inclusion and exclusion criteria for this paper ensured that only studies using validated and standardised measures of mental health or psychological wellbeing as evaluative outcome measures were included. However, 13 articles involving cognitive defusion interventions were excluded following a full-text review as they did not meet this criterion. Many of the excluded papers examined the characteristics of the thoughts, such as frequency of occurrence and believability, or aimed to reduce distress associated with negative beliefs or unhelpful thoughts (N = 9) (Barrera et al., 2016; Ely, 2011; Maisel, 2018; Maisel et al., 2019; Mandavia et al., 2015; Masuda et al., 2009; Masuda et al., 2010; Tyndall et al., 2017; Wollach et al., 2020). A future review may look to integrate theoretical literature on the impact of distressing thoughts on psychological wellbeing and quality of life with evidence for cognitive defusion interventions on thought characteristics.

Implications of Present Systematic Review

The present review suggests that cognitive defusion interventions may improve or protect mental health and psychological wellbeing, at least in the short term. Many of the included studies employed single session and even single task interventions, suggesting that future work in this area could focus on brief therapeutic interventions. However, the reviewed papers do not offer a widely representative sample that is generalisable to clinical mental health settings in the NHS or internationally. The existing body of evidence would benefit from further clinical trials, with samples taken from populations who are representative of people seeking professional help for mental health difficulties. This would aid the evaluation of cognitive defusion as a standalone intervention to improve mental health and psychological wellbeing.

Conclusions

The reviewed studies suggest that cognitive defusion as a standalone intervention can be as effective in improving mental health outcomes; however, there is a little evidence suggesting that cognitive defusion produces superior outcomes to other cognitive behavioural approaches and techniques. The findings of this review were consolidated from studies with predominantly good quality assessment ratings. However, further trials examining the efficacy and feasibility of standalone cognitive defusion interventions within clinical populations and settings are required to inform recommendations for people experiencing mental health difficulties.

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Paper 2

Examining the influence of psychological flexibility and athlete burnout on mental wellbeing in elite rugby athletes

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3 This paper is prepared in accordance with the author guidelines for the Journal of Clinical Sport Psychology (see Appendix F). APA 7th formatting has been used throughout, in line with both South Wales Doctoral Programme in Clinical Psychology and journal guidelines. For the purposes of thesis submission, the 8,000-word limit has been used to ensure all relevant information has been included for examination. This is in place of the 30-page limit, including references, set by the Journal of Clinical Sport Psychology for original research papers. Line numbering will also be used at time of submission for publication.

Abstract

Background: Psychological flexibility plays an important role in mental wellbeing and burnout in non-athlete populations. The present study aimed to elucidate how psychological flexibility and athlete burnout influence mental wellbeing in the elite rugby athlete population.

Method: 87 UK-based elite rugby athletes (81 male, 6 female) completed the CompACT, Athlete Burnout Questionnaire (ABQ) and Warwick-Edinburgh Mental Wellbeing Scale (WEMWBS) in a single time-point online questionnaire study.

Results: CompACT scores significantly predicted WEMWBS, with a large effect size ($R^2_{\text{adjusted}}=.382$). However, there was no significant association between overall ABQ scores and overall CompACT or WEMWBS scores. There were significant negative correlations between the emotional exhaustion subscale of the ABQ with CompACT total and CompACT openness to experience scores.

Conclusion: The findings are explored in the context of existing occupational and clinical literature. Overall, the findings indicate psychological flexibility has a large positive effect on mental wellbeing in elite rugby athletes.

Introduction

The relationship between occupational stressors and psychological wellbeing has been established in previous literature within specific job roles (Hasan et al., 2018; Purba & Demou, 2019), industries (Burman & Goswami, 2018; Collin et al., 2019; Daghigh Yazd et al., 2019) and organisations (Charman & Bennett, 2022). A number of occupational factors have been identified as predictors of individual psychological wellbeing and mental health, including emotional exhaustion (Jeon et al., 2018), stigma around help-seeking behaviours (Hom et al., 2020), work satisfaction and psychological capital (Hernández Varas et al., 2019). Furthermore, when attempting to improve outcomes associated with mental health and wellbeing, work-based psychological interventions for employees have established small to medium effect sizes (Phillips et al., 2019). For example, studies have reported improvements in aspects of mental health, wellbeing and occupational performance through the implementation of workplace mindfulness-based programmes (Lomas et al., 2019; Scheepers et al., 2020).

While engagement in sport at any level can provide health and wellbeing benefits (Griffin, et al., 2021), previous systematic reviews have indicated that individuals involved in elite or professional sport, as athletes or coaching staff, may be at an equivalent or increased risk of developing mental health difficulties compared to the general population (Castaldelli-Maia et al., 2019; Gouttebauge et al., 2019; Kegelaers et al., 2021). However, it is important to note that it is challenging to make direct comparisons to matched samples of the general population due to the sport-specific physical, psychological and organisational stressors that are rarely fully replicated outside this industry (Arnold & Fletcher, 2012). Entering elite level sport can mean exposing individuals to previously unexperienced stressors including media scrutiny, increased risk of injury and forced retirement (Oftadeh-Moghadam & Gorczynski, 2021). A scoping review by Küttel and Larsen (2020) identified a number of negative

influences of sport-specific stressors from studies in the area, such as competitive anxiety, fear of failure, concerns about deselection, and weight control on athlete mental health or wellbeing. The authors noted, however, that the influence and effect of some stressors differed according to athletes' career context. For example, it was suggested that athletes participating in individual sports in which a 'lean body-image' was considered to be necessary displayed higher levels of depressive symptoms. Similarly, some dynamics relating to team sports were acknowledged as significant stressors, including perceptions of support and stigma towards help seeking (Küttel & Larsen, 2020). While mental health difficulties can affect elite athletes due to factors unconnected to their elite sport participation, it is possible that sport-specific risk factors may compound generic influences to increase the risk of developing mental health difficulties (Reardon et al., 2019). This may be further exacerbated by the overlap between the typical peak competitive age range for elite athletes and peak time for the onset of many mental health difficulties (Küttel & Larsen, 2020).

Mental Health and Wellbeing in Elite Rugby Union Players

Rugby union is typically played in teams of 7 or 15, with people who play at grassroots, club and international levels around the world (Griffin, Perera, Murray, Hartley, et al., 2021).

Rugby union shares many characteristics with rugby league; however, key distinctions exist, as rugby league has fewer players, limited opportunities to use the ball before giving possession to the other team and different profiles of physical contact (King et al., 2022).

Typically, research within this population of elite rugby athletes has focused on concussion and physical injury, with limited literature on mental and psychological wellbeing (Gouttebauge et al., 2018).

Specific aspects of the nature and culture of participation in elite level rugby union may be helpful to consider in relation to mental health and wellbeing. Studies have indicated cultural norms within rugby union including hyper-masculinity (Parry et al., 2021) and excessive alcohol use (Grobler et al., 2022); these factors have been linked to lower psychological wellbeing, mental health stigma and reduced help-seeking behaviour in elite athlete and other populations (Åkesdotter et al., 2020; Castaldelli-Maia et al., 2019; Marsters & Tiatia-Seath, 2019; Schmits & Glowacz, 2022). A paper by Muir and Seitz (2004) suggested that societal perceptions of rugby hyper-masculinity can attract players who wish to emphasise these attributes in themselves. The drinking culture associated with rugby union has been long established and existed before the modern professional era (O’Callaghan, 2017). Previous literature suggests that rugby’s drinking culture is intertwined with the societal perceptions of the sport, including hegemonic masculinity, aggressive competitiveness and ‘toughness’ (Muir et al., 2020). However, when interpreting the role of masculinity in sport-related alcohol use, it is important to note that similarly problematic drinking culture can exist within women’s sport (Palmer & Toffoletti, 2019). A study conducted by Kahu-Kauika (2011) in New Zealand found that rugby athletes acknowledged how the normalisation of alcohol use within rugby culture can negatively affect performance and wellbeing, but perceived that drinking culture aided the development of meaningful connections and team unity, which was ultimately prioritised by players. Furthermore, studies have found that rugby cultural expectations of physical ‘toughness’ can demand high tolerance of pain and possibly reduce the likelihood of athletes seeking out help when needed for physical injury (Hokowhitu et al., 2008).

The value of masculinity and toughness within rugby culture can also influence elite rugby athletes’ attitudes towards mental health and psychological wellbeing. Stigma around the experience of mental health difficulties has been documented throughout the general

population and elite athlete populations (Bharadwaj et al., 2017; Gulliver et al., 2012). Previous studies have found that athletes viewed team mates more negatively for seeing a psychotherapist, compared to those who visited a sports psychologist (Van Raalte et al., 1992). Athletes may not seek help for mental health difficulties due to fears of appearing ‘weak’ or having a negative impact on their sport contracts and sponsorships (Castaldelli-Maia et al., 2019). This perceived stigma and low mental health literacy were found to be the most important barriers to help seeking behaviours in rugby league players (Kola-Palmer et al., 2019).

More recently, however, there has been an increasing awareness of elite athlete mental wellbeing and greater relevance of Clinical Psychology within this area of sport. The Olympic Committee Consensus Statement published in 2019 made a recommendation for future research to identify strategies that promote psychological wellbeing and prevent deterioration in mental health (Reardon et al., 2019). Furthermore, athlete mental wellbeing has become more prominent in social awareness, which may help in the reduction of mental health stigma. This has been aided by documentaries such as “Big Boys Don’t Cry” directed by Hughes (2021); this program followed elite rugby player Joe Marler as he explored his experience of mental health difficulties.

Previous research has suggested that certain occupational stressors may influence the wellbeing of elite rugby union athletes distinctly or more powerfully than within other sports (Musgrave, 2021). As a sport, rugby union involves intermittent physical contact with other players through collisions and tackles, thus increasing the risk of physical injury or concussion (Viviers et al., 2018). Kilic et al. (2019) found that players who experience a concussion are up to two times greater risk of developing symptoms of psychological distress, anxiety or depression over the course of a season, compared to those who had not been concussed. Elite rugby union athletes who had experienced ‘severe’ musculoskeletal

injuries were also more likely to develop symptoms of anxiety or depression in the following 12 months, when compared to uninjured counterparts (Kilic et al., 2019). This study may indicate that the severity of the injury acquired is important to the likelihood of it impacting mental wellbeing. The role of injury as a potential trigger for mental health symptoms has been illustrated in studies with non-rugby elite athlete populations (Putukian, 2016; Sheinbein, 2016). Athlete injury has been particularly associated with depressive symptoms (Gouttebarga et al., 2018; Leddy et al., 1994; Putukian, 2016; Souter et al., 2018). Brown et al. (2017) found that, when injury forced Irish, French and South African rugby players to retire, they were twice as likely to report symptoms of distress compared to those who had retired voluntarily.

A qualitative study conducted with young, male Pacific Island rugby union and rugby league players in New Zealand provided insight into factors that affect their psychological wellbeing. The participants in this study expressed the importance of family support, personal development, life balance and their own athletic performance as significant in their perceptions of mental health. Some participants explained that ratings of their rugby performance was synonymous with their psychological wellbeing (Marsters & Tiatia-Seath, 2019).

In all, there are few studies examining the factors affecting psychological wellbeing within the elite athlete population. The historic prioritisation of sport performance over athlete wellbeing in research literature has received criticism (Ringland, 2016). Previous research and reviews in this area have adopted the assumptions of the Mental Health Model (Morgan, 1985), which posits a direct association between athlete psychological wellbeing and sporting performance; in which increased mental health difficulties would predict poorer performance (White et al., 2021).

Occupational and Athlete Burnout

Maslach and Jackson (1984) described burnout as an increase in physical and emotional exhaustion alongside depersonalisation and feelings of reduced personal accomplishment (Lubbadeh, 2020). Depersonalisation refers to the distorted perceptions of oneself and the world; this is often characterised as a lack of empathy, cynicism or indifference (Prinz et al., 2012). This can cause negative personal outcomes in areas such as physical and mental health, as well as occupational outcomes due to reductions in performance, errors and absenteeism (Shoman et al., 2021).

In the previous literature, burnout has been examined as both an outcome variable and predictor of other psychological outcomes. Studies have suggested that clinically significant levels of occupational burnout are associated with an increased experience of mental health difficulties (Chen et al., 2022; Koutsimani et al., 2019), particularly Major Depressive Disorder (Stelnicki et al., 2021). One systematic review of prospective studies found that burnout significantly predicted depressive symptoms and use of antidepressants in the majority of studies. This prediction was particularly strong within the emotional exhaustion and depersonalisation elements of occupational burnout. The review also noted inconsistent findings on the nature of the relationship between occupational burnout and sleep difficulties; some studies showed burnout as a significant predictor of the onset of sleep difficulties, while others found no association with insomnia incidence or persistence (Salvagioni et al., 2017)

Other identified factors have been shown to impact the relationship between burnout and psychological wellbeing. For example, Pereira et al. (2021) found that higher levels of work-related quality of life reduced the negative impact of occupational burnout on mental health. Similarly, another study illustrated that occupational burnout and symptoms of anxiety were no longer significantly associated when intensive care unit physicians and nurses had higher

levels of social support (Zhang et al., 2020). In this sample, larger correlations between burnout and depression symptoms were connected to demographic variables, such as age and gender, as well as occupational factors, including longer work career length. The authors suggested that the repeated experience of negative work events over time was important in the development of occupational burnout of a severity that overlapped with symptoms of depression (Meier & Kim, 2022).

Athlete burnout describes the concept of occupational burnout specific to the experiences and demands of elite level sports-people. It encompasses the dimensions of emotional exhaustion, reduced sense of accomplishment and sport devaluation (Gustafsson et al., 2018). This concept of athlete burnout was developed by Raedeke and Smith (2001) and built upon the definition of occupational burnout set out by Maslach and Jackson (1984). This aimed to incorporate sport-specific factors, namely reduced sense of accomplishment within athletic skills or ability, and unmet expectations or goals within sport. Raedeke (1997) argued that depersonalisation aspect of occupational burnout was less important in the athlete burnout as the original description by Maslach and Jackson (1984) contextualised depersonalisation as changes in empathy towards patients within a healthcare setting.

While the majority of psychological research into the concept of athlete burnout has focused on performance-based outcomes, previous research has investigated both factors that affect the development of athlete burnout and how athlete burnout may impact upon other psychosocial factors. Studies have indicated that athlete burnout influences psychological wellbeing in a similar way to general occupational burnout. One review found a consistent association between athlete burnout and perceived stress across 48 studies (Lin et al., 2021). Significant associations have also been demonstrated between athlete burnout and symptoms of depression (Gerber et al., 2018; Kamimura et al., 2020), somatic anxiety and worry (Cho et al., 2019). When investigating vulnerabilities to the development of athlete burnout, one

study found that youth-elite athletes with high levels of performance-based self-esteem, but not high athletic-identity, may be at a greater risk of athlete burnout (Gustafsson et al., 2018).

Several factors have been identified as important in the protection of elite athletes from experiencing burnout. For example, trait optimism was found to be negatively related to the emotional exhaustion dimension of burnout within competitive Spanish kayak polo players (Angosto et al., 2021). Similarly, satisfaction of basic psychological needs (Vílchez Conesa et al., 2020), psychological resilience (Wagstaff et al., 2018), self-esteem (Markati et al., 2019) and mindfulness (Moen et al., 2015) have all been associated with lower levels of burnout in elite and non-elite athletes. Furthermore, a review paper concluded significant effects of social support and relatedness (defined as feelings of connectedness with other individuals involved in the sport) in reducing athlete burnout (Pacewicz et al., 2019).

Psychological Flexibility in Occupational and Athlete Mental Wellbeing

Psychological flexibility can be defined as “the tendency to respond to situations in ways that facilitate valued goal pursuit” (Doorley et al., 2020, p.2). Psychological flexibility encompasses six subprocesses; these are acceptance, contact with the present moment, self as context, values, cognitive defusion and committed action (Harris, 2019). Acceptance is a willingness to experience painful or distressing thoughts and emotions, so that we do not struggle to avoid or suppress them (O’Sullivan, 2013). Contact with the present moment enables an individual to accurately observe and adapt to the present, without getting caught up in thoughts of the past or future (Segneri, 2021). Self as context refers to one’s ability to notice internal experiences, such as thoughts and emotions, from a non-judgemental stance (Segneri, 2021; Yu et al., 2017). Cognitive defusion helps to move the focus on to the process of having a thought or emotion, rather than becoming hooked by their content (O’Sullivan,

2013). Values relate to the focus on clarifying what is most important to an individual in their life, whilst committed action encourages engaging in effective action in line with chosen values (Harris, 2019; Viskovich et al., 2021).

Previous literature has demonstrated that psychological flexibility can play a central role in the maintenance of good mental health within a wide range of populations and contexts (Kashdan & Rottenberg, 2010). This therefore indicates a consistent positive correlation between psychological flexibility and mental wellbeing in the general population.

In occupational settings, psychological flexibility has been found to be related to increased professional quality of life in healthcare workers (Garner & Golijani-Moghaddam, 2021), reduced stigma (Krafft et al., 2018) as well as individual and team-level wellbeing (Lamb, 2018). Furthermore, higher psychological flexibility has been associated with greater success in learning new occupational skills (Bond et al., 2006). Psychological flexibility was found to predict lower burnout in a sample of Spanish mental health workers (Ortiz-Fune et al., 2020). Puolakanaho et al. (2018) found that psychological flexibility related skills were significant predictors of occupational wellbeing and burnout, even when controlling for worksite factors. The authors concluded that it may, therefore, be possible to influence occupational wellbeing and burnout through the improvement of psychological flexibility skills. This notion is supported by outcomes of studies examining the effects of therapeutic interventions aimed at increasing psychological flexibility within occupational settings (Prudenzi et al., 2021; Rudaz et al., 2017; Towey-Swift et al., 2022).

Much of the literature examining psychological flexibility within the elite athlete population has focused on how it influences performance or improves factors of injury rehabilitation (DeGaetano, 2014; Shortway et al., 2018). However, a review of interventions designed to increase aspects of psychological flexibility processes within elite athletes indicated

preliminary support for its efficacy (Sappington & Longshore, 2015). Similarly, a systematic review and meta-analysis found that mindfulness, which overlaps core processes of psychological flexibility, was negatively associated with athlete burnout; however, the authors noted the small number of included studies and limited evidence of the effectiveness of mindfulness in preventing athlete burnout (Li et al., 2019). Chang et al. (2018) found that psychological flexibility moderated the association between athletic identity and athlete burnout; this meant that athletic identity was positively associated with emotional exhaustion in athletes with low levels of psychological flexibility, where the converse was found for those with high psychological flexibility.

Psychological flexibility is considered a skill that is amenable to change and can be advanced through psychological intervention or training (Lundgren et al., 2020). Much of the previous literature on psychological flexibility in elite athlete populations has focused on its association with sporting performance and physical injury recovery (Moore, 2009). However, as stated above, a shift in cultural expectations is increasingly requiring practitioners to offer holistic packages of care for elite athletes, that includes the protection and enhancement of mental wellbeing (Gardner & Moore, 2017; Reardon et al., 2019). Carrança et al. (2019) found that self-compassion increased and psychological inflexibility decreased in elite athletes following a mindfulness-based intervention. A systematic review conducted by Noetel et al. (2019) found large effect sizes for mindfulness and acceptance based interventions for improving competitive anxiety and present-moment awareness within athlete populations. White et al. (2021) recently published an ACT-based psychological intervention protocol for athletes, however, there is not currently any published research evaluating its effectiveness.

Impact of the Covid-19 Pandemic on Mental Wellbeing and Burnout

The association between psychological flexibility and mental wellbeing has also been assessed within the context of the covid-19 pandemic. Dawson and Golijani-Moghaddam (2020) completed a cross-sectional study, in which 555 UK residents were surveyed. The authors showed that psychological flexibility was positively associated with psychological wellbeing and negatively related to anxiety, depression and covid-19 related distress.

Similarly, higher psychological flexibility was correlated with lower reported symptoms of anxiety, depression and insomnia in a large-scale online questionnaire study conducted in Sweden during the covid-19 pandemic (McCracken et al., 2021).

A recent meta-analysis conducted by Johns et al. (2022) focused on the prevalence depression and anxiety in doctors during the Covid-19 pandemic. This estimated a prevalence rate of 20.5% for depression and 25.8% for anxiety in doctors across 30 studies. However, the authors noted that it was difficult to conclude whether this was significantly different to pre-pandemic levels.

Recent literature has also explored the impact of the covid-19 pandemic on athlete burnout and psychological wellbeing. One study conducted in the Republic of Ireland concluded that the suspension of organised sport did not significantly improve or worsen elite athletes experience of burnout (Woods et al., 2022). In addition, an Italian study found that elite and amateur athletes displayed significant increases in reported perceived stress, when pre- and post-pandemic levels were compared (Di Fronso et al., 2022). This may be important in understanding athlete burnout due to the established relationship between athlete perceptions of stress and burnout; particularly within the reduced sense of accomplishment aspect of burnout (Lin et al., 2021). Woodford and Bussey (2021) undertook a qualitative study which explored the perceived impact of covid-19 social distancing measures on athlete wellbeing.

They found that, despite the initial threat to athlete wellbeing following the suspension of organised sporting activities in the UK, the athletes involved in the research had adapted to develop numerous strategies to protect their wellbeing during national lockdowns.

The current research may indicate that the covid-19 pandemic had some effect on psychological and occupational aspects of wellbeing in elite athlete populations; however, more extensive research and reviews are required for this to be established.

Summary of Background Literature

Previous literature indicates that psychological flexibility and athlete burnout may be important factors in the protection and maintenance of mental wellbeing in elite athletes. However, there is limited research evidence clarifying how these factors may interrelate and exist within the context of the specific and unique stressors experienced by elite rugby athletes. Furthermore, while there is evidence suggesting that psychological flexibility plays an important role in the mental wellbeing and burnout in the general population, additional clarification within the elite rugby athlete population is required to ensure that therapeutic approaches are adequately tailored to meet their specific needs.

The Present Study

The present study aimed to clarify the role of psychological flexibility and athlete burnout on the mental wellbeing of elite rugby athletes. Specifically, the study intended to ascertain whether psychological flexibility and athlete burnout had an interaction effect on mental wellbeing, whereby higher psychological flexibility reduced the association between athlete burnout and mental wellbeing.

Hypotheses

1. Higher psychological flexibility will be significantly associated with lower athlete burnout and higher mental wellbeing.
2. Higher psychological flexibility will reduce the strength of association between athlete burnout and mental wellbeing.
3. Higher athlete burnout and lower psychological flexibility and mental wellbeing will be associated with reports of increased worry about Covid-19.
4. Participants who report a current injury will have lower mental wellbeing and higher athlete burnout.
5. Groups of participants who report longer times for expected recovery from physical injury will have lower scores on measures of psychological flexibility and mental wellbeing, and higher scores on measures of burnout.

Method

Participants

Participants were recruited through Rugby Unions and Rugby Players Associations in the UK and Ireland, with professional athletes based in Wales, Scotland and Ireland represented in the final data set. A further three Rugby Unions and Rugby Players Associations were invited to participate, but declined. Male or female professional rugby union players were eligible to take part if they were over the age of 18 years, were currently involved with a UK or Ireland rugby union, club or international team and understood written English well enough for the purposes of understanding the study information, raising any concerns and completing the study questionnaires. Past or retired rugby union players were not invited to take part, as it was considered that recollections may not provide an accurate measure of athlete burnout.

Information on which region, Rugby Union or Rugby Players Association each participant represented was not collected for the purposes of this study in order to maintain anonymity.

A total of 119 participants consented to complete the questionnaire. Data from 32 participants were excluded due to missing items on the CompACT, ABQ or WEMWBS, meaning that data from a total of 87 participants were included in the final analysis. Participants who omitted answering a demographic question, such as career length, but had completed every item of the CompACT, ABQ and WEMWBS were included in the analysis. Most participants identified as male (N=81 male; N=6 female). Around 75 % of participants reported that they were currently uninjured (N=65). Participants who reported a current injury stated expected recovery times ranging between “less than two weeks” (N=4) and “more than three months” (N=12). The length of time that participants had been involved in professional rugby ranged from less than 12 months up to 20 years, with a mean career length of 6.5 years.

Measures

Demographic Measures

Participants provided information on age, gender, length of career and injury status. It was anticipated that participants may have possibly felt concerned about identification if providing an exact age. Therefore, respondents selected their age from five choices; these were “18 – 25 years”, “26 - 30 years”, “31 - 35 years”, “36 – 40 years” and “40+ years”. Participants reported their gender by selecting “Male”, “Female”, “Other – please specify” or “prefer not to say”. Length of career was reported by participants stating the number of years they had been playing professional or elite rugby. Participants were also asked to indicate whether they were currently injured or recovering from any physical injuries by selecting a “Yes” or “No” option. If participants answered yes, indicating a current injury, they would be asked to report how long they expected their recovery to take by selecting “Less than two weeks”, “Two to four weeks”, “One to three months” or “More than three months”.

Office of National Statistics (ONS) Opinion and Lifestyle Survey (OPN) Covid-19 Related Worry; (Mayhew, et al., 2021)

Participants completed a single question from the ONS OPN survey on covid-19 related worry, see Appendix M. Participants were asked “How worried or unworried are you about the effect that the Coronavirus (COVID-19) outbreak is having on your life right now?”. Responses were set as “Very worried”, “Somewhat worried”, “Neither worried nor unworried”, “Somewhat unworried”, “Not at all worried”, “Don’t know” and “Prefer not to say”.

Comprehensive Assessment of Acceptance and Commitment Therapy Processes (CompACT); (Francis et al., 2016)

This study used the CompACT as a measure of psychological flexibility (see Appendix G). The 23-item measure provides an overall score and scores within the subscales of Openness to Experience (OE), Behavioural Awareness (BA) and Valued Action (VA). Participants were required to rate the degree to which they agreed with specified statements based on a 7-point Likert scale, which ranged from “0 – Strongly Disagree” to “7 – Strongly Agree”. Items include statements such as “I try to stay busy to keep thoughts or feelings from coming”, “I behave in line with my personal values” and “thoughts are just thoughts – they don’t control what I do”. Higher scores on the CompACT total and individual subscales indicate greater psychological flexibility.

Cronbach’s alpha was calculated for the CompACT total score and each of the subscales to assess internal consistency. The Cronbach’s alpha was .869 for the CompACT total score, .783 for the OE subscale, .759 for the BA subscale and .752 for the VA subscale. This suggests that the CompACT has good internal consistency in this study dataset.

Studies assessing the validity of the CompACT have suggested that it is suitable for use in clinical and research practice (Francis et al., 2016; Morris, 2019); therefore, this measure was selected for use in the present study.

Athlete Burnout Questionnaire (ABQ); (Raedeke & Smith, 2001)

Athlete burnout was assessed using the ABQ (see Appendix H). Participants read 15 statements about their experience in playing professional rugby and rated how often they felt this way on a 5-point rating scale ranging from “1 – Almost Never” to “5 – Almost Always”. Items included statements such as “I am accomplishing many worthwhile things in rugby”, “I

feel wiped out from rugby” and “I’m not into rugby like I used to be”. The questions are categorised into three subscales of Reduced Sense of Accomplishment (RA; characterised as feelings of loss of control over performance), Emotional/Physical Exhaustion (E; characterised as feelings of fatigue from sport participation and training) and Devaluation (D; characterised as a loss of interest in the sport and doubts over future participation). The scores for these individual dimensions are calculated as the mean score for ABQ items within each category. An overall score of global athlete burnout can be determined by calculating the mean across all 15 items of the ABQ.

The ABQ was developed as a measure of burnout specifically focused on the experiences of sportspeople, with some items initially derived from Maslach’s Burnout Inventory (MBI) which assessed occupational burnout (Maslach & Jackson, 1986). Previous research has reported adequate internal validity across the ABQ’s subscales (Curran et al., 2011; Kent et al., 2018). The ABQ has been used extensively as a measure of athlete burnout within sport psychology literature since its development (DeFreese & Smith, 2013; Gustafsson et al., 2011; Gustafsson et al., 2017; Mellano et al., 2022).

The internal consistency of the ABQ total score and subscales was calculated using Cronbach’s alpha. The Cronbach’s alpha was .913 for the ABQ total score, .830 for the RA subscale, .891 for the E subscale and .882 for the D subscale. This suggests that the ABQ total score and subscales demonstrated good internal consistency within this study data.

Warwick-Edinburgh Mental Well-being Scale (WEMWBS; Tennant et al., 2007)

Mental wellbeing was assessed using the WEMWBS (see Appendix I), a self-report measure that focused on positive mental health. The WEMWBS is a 14-item questionnaire for which participants read statements about feelings and thoughts, and rate how frequently they experienced these within the previous two weeks. Items include statements such as “I’ve been

feeling optimistic about the future” and “I’ve had energy to spare”. Higher scores on the WEMWBS indicate higher reported mental wellbeing. The authors of this tool indicated that 15% of the population could be expected to have a score of less than 42.3 in UK samples, and therefore, set a cut-off of 42 to indicate low mental wellbeing (Tennant et al., 2007).

A Cronbach’s alpha was calculated as .893 for the WEMWBS within this study data set, indicating good internal consistency. The WEMWBS has been validated for use within a range of populations, a variety of settings and has been found to be effective for clinical and research purposes (Clarke et al., 2011; Stewart-Brown, 2013; Taggart et al., 2013; Trousselard et al., 2016).

Procedure

Ethical approval for this project was granted by Cardiff University School of Psychology Research Ethics Committee (see Appendix J). Data was collected between September 2021 and March 2022. Specified leads within UK and Ireland Rugby Unions and Rugby Player Associations, who had consented to sharing the information with their members, circulated study information sheets (see Appendix K) and an anonymous link to the online questionnaire, hosted on Qualtrics. The questionnaire required participants to read and select an option to indicate that they met eligibility criteria and provide informed consent (see Appendix L), prior to continuing to the questionnaire.

Participants provided demographic information regarding age range, identified gender, career length, injury status and, where relevant, the expected length of time required for injury recovery. Participants also completed the single covid-19 related worry question adapted from the ONS OPN (Mayhew et al., 2021), see Appendix M; local and national covid-19 restrictions varied across the data collection period and geographic regions involved in the

study. The remainder of the questionnaire comprised of the CompACT, ABQ and WEMWBS. The median time taken to complete the questionnaire was 6.86 minutes.

Statistical Analyses

The data were first screened for missing and incomplete responses. Data from participants who did not complete more than two items from the demographic and covid-19 questions, and/or who did not complete every item from the CompACT, ABQ or WEMWBS were excluded from the analysis. The final data did not violate the assumptions of normality, linearity, homoscedasticity and the absence of multicollinearity. Descriptive statistics, including means and standard deviations were calculated for all of the included factors. A series of one-way ANOVAs were conducted to examine whether demographic variables such as injury status and worry about Covid-19 impacted on participant scores on the CompACT, ABQ or WEMWBS. Correlational analyses were then conducted to explore any significant associations between variables. Finally, a linear regression was conducted to examine the relationship between scores on the CompACT, ABQ and WEMWBS.

Results

Demographic Descriptive Statistics

The data from 87 participants were included in the final analysis. Due to the considerable difference in male and female representation in the sample, no analyses were undertaken to examine gender differences. Most participants were aged between 18 and 30 years old (83.9%). Seventy-eight participants provided information on the length of their elite athlete career, which ranged between less than 1 year and 20 years (Mean = 6.51, Mode = 2 years). Across all participants, 22 reported that they were currently injured; see Table 1 for expected length of recovery for these participants. There was a significant correlation between age and injury status, $r(85) = .219, p=.042$, in which higher age was associated with injury. Age and career length were also significantly associated, as would be expected, $r(76) = .902, p<.001$. There were no other significant correlations between demographic variables.

Table 1

Expected length of recovery for participants who reported a current physical injury

Expected Recovery Time	Number of Participants
Fewer than two weeks	4
Two to four weeks	4
One to three months	2
More than three months	12
Total	22

Covid-19 Related Worry Descriptive Statistics

The majority of participants reported that they were “not at all worried”, “somewhat unworried” or “neither worried nor unworried” about the covid-19 pandemic (N=66, 75.9%). One participant responded “don’t know” and one reported that they were “very worried”, with the remaining participants identifying as “somewhat worried” (N=19). There was a significant correlation between age and covid-19 related worry, $r(85) = .235, p=.029$, and career length and covid-19 related worry, $r(76) = .307, p=.006$.

Hypothesis 1: Higher Psychological Flexibility will be Significantly Associated with Lower Burnout and Higher Mental Wellbeing

Mean scores for the psychological measures are presented in Table 2. Across the sample, 13 participants (14.9%) scored below 42 on the WEMWBS, indicating the experience of clinically significant symptoms of mental health according to the WEMWBS guidance.

Table 2

Means and standard deviations for scores on psychological measures.

Measure	Mean Score	Standard Deviation	Range
Warwick Edinburgh Mental Wellbeing Scale (WEMWBS)	48.17	7.44	28 – 70
CompACT	82.61	17.40	44 – 118
Athlete Burnout Questionnaire (ABQ)	2.54	0.70	1.2 – 4.2

Significant correlations were found between participant scores on the WEMWBS and the total CompACT score. This indicates a significant association with a large effect size between psychological flexibility and mental wellbeing. The WEMWBS was also significantly associated with each of the subscales within the CompACT with medium to large effects sizes; openness to experience, behavioural awareness and valued action. There was no significant association found between scores on the WEMWBS and ABQ, or any of the ABQ's subscales ($p > .05$). A simple linear regression was conducted to ascertain if psychological flexibility, as measured by the CompACT, would predict mental wellbeing, as measured by the WEMWBS. CompACT scores predicted a significant amount of variance in WEMWBS scores, $F(1,85) = 54.239$, $p < .001$, $R^2 = .390$, $R^2_{\text{adjusted}} = .382$. Results of all bivariate correlations are presented in Table 3.

A confirmatory factor analysis and validation of the CompACT conducted by Bayliss (2018) suggested that scores of less than 85 indicate 'low psychological flexibility'. In total, 51 participants scored less than 85 on the CompACT (58.62%). However, no additional significant associations were found between measures or subscales when comparing those scoring less than and more than 85 on the CompACT ($p > .05$).

There were significant negative correlations between the CompACT total scores and CompACT Openness to Experience scores with ABQ Emotional Exhaustion scores. In analyses in which the ABQ Emotional Exhaustion was examined as a dependent variable, CompACT total and CompACT Openness to Experience scores were found to significantly predict ABQ Emotional Exhaustion scores ($F(1,85) = 5.237$, $p = .025$, $R^2 = .058$, $R^2_{\text{adjusted}} = .047$ and $F(1,85) = 8.306$, $p = .005$, $R^2 = .089$, $R^2_{\text{adjusted}} = .078$, respectively), with the Openness to Experience subscale providing a slightly stronger prediction.

Table 3*Correlations between measure total scores and subscales*

	1	2	3	4	5	6	7	8
1. WEMWBS								
2. CompACT - Total Score	.624**							
3. CompACT - Openness to Experience	.471**	.894**						
4. CompACT - Behavioural Awareness	.583**	.844**	.698**					
5. CompACT - Valued Action	.509**	.696**	.379**	.407**				
6. ABQ - Global Score	0.143	-0.180	-0.180	-0.142	-0.106			
7. ABQ - Reduced Sense of Accomplishment	0.111	-0.114	-0.110	-0.120	-0.047	.770**		
8. ABQ - Emotional Exhaustion	0.130	-.241*	-.298**	-0.153	-0.092	.841**	.483**	
9. ABQ - Sport Devaluation	0.115	-0.091	-0.042	-0.084	-0.115	.867**	.517**	.581**

All correlations are Pearson's r ; $N=87$; **Correlation is significant at .01 level (two-tailed); *Correlation is significant at .05 level (two-tailed)

A significant correlation between WEMWBS and total CompACT scores indicate a significant association with a large effect size between psychological flexibility and mental wellbeing. The WEMWBS was also significantly associated with each of the subscales within the CompACT with medium to large effects sizes. A simple linear regression was conducted to ascertain if psychological flexibility, as measured by the CompACT, would predict mental wellbeing, as measured by the WEMWBS. CompACT total scores predicted a significant amount of variance in WEMWBS scores, $F(1,85) = 54.239, p < .001, R^2 = .390, R^2_{\text{adjusted}} = .382$.

The results therefore partially support this hypothesis, as psychological flexibility was significantly associated with mental wellbeing, however, athlete burnout was not significantly related to either of the other concepts ($p > .05$).

Hypothesis 2: Higher psychological flexibility will reduce the strength of association between athlete burnout and mental wellbeing

A multiple regression was conducted to examine how much variance in WEMWBS scores was accounted for by CompACT and ABQ scores. While a significant amount of the variance in WEMWMS was accounted for by the CompACT and ABQ scores, $F(3,83) = 18.663, p < .001, R^2 = .403, R^2_{\text{adjusted}} = .381$, the ABQ did not significantly increase the amount of variance that was explained. An interaction effect between CompACT and ABQ scores on WEMWBS was found to be non-significant ($p > .05$).

The results therefore do not support this hypothesis, as burnout did not account for a significant proportion of the variance within scores of mental wellbeing. However, psychological flexibility was found to be a significant predictor of mental wellbeing in elite athletes, with a large effect size.

Hypothesis 3: Higher athlete burnout and lower psychological flexibility and mental wellbeing will be associated with reports of increased worry about Covid-19

There were no significant correlations between covid-19 related worry and scores on the CompACT, ABQ or WEMWBS. In subsequent analysis, participants were split into ‘high’ and ‘low’ worry, in which those reporting as “somewhat” or “very worried” were included within the ‘high worry’ group, and the remaining participants were in the ‘low worry’ group. Independent samples t-tests revealed no significant differences between scores on the CompACT, ABQ or WEMWBS according to groups ($p > .05$).

The results do not provide support for this hypothesis, as there is no evidence that worry about the covid-19 pandemic affected any of the psychological measures.

Hypothesis 4: Participants who Report a Current Injury will have Lower Mental Wellbeing and Higher Athlete Burnout

Participants were separated into groups based on those who reported a current physical injury (N = 22) and those who did not report a current physical injury (N = 65). Two independent samples t-tests were conducted to establish any significant differences between injured and non-injured participants in WEMWBS and ABQ scores. No significant difference was found in WEMWBS scores in injured (Mean=46.86, SD=8.31) and non-injured participants (Mean=48.62, SD=7.13; $t(85) = -.064, p = .949$). Similarly, there was no significant difference between ABQ score in injured (Mean=2.50, SD=.70) and non-injured (Mean=2.55, SD=.71) participants ($t(85) = .955, p = .342$).

This hypothesis was rejected, as there was no significant impact of injury status on WEMWBS or ABQ scores.

Hypothesis 5: Groups of participants who report longer times for expected recovery from physical injury will have lower scores on measures of psychological flexibility and mental wellbeing, and higher scores on measures of burnout.

Of the participants who reported a current injury (N = 22), respondents were grouped based on their reported expected recovery time between the options of “less than two weeks” (N = 4), “two to four weeks” (N = 4), “one to three months” (N = 2) and “more than three months” (N = 12). A one-way ANOVA was completed to assess whether length of expected injury recovery had a significant effect on WEMWBS, CompACT or ABQ scores. There were no significant differences observed in any of the psychological measures as a function of expected injury recovery (all $p > .05$).

As no significant differences or relationships were observed as a function of expected length of recovery from physical injury, this hypothesis was rejected.

Discussion

Findings

This study aimed to examine the roles of athlete burnout and psychological flexibility on the mental wellbeing of elite rugby athletes. A number of the results did not support the hypotheses of the study or the findings of previous literature within this subject area.

Firstly, as hypothesised, psychological flexibility was significantly associated with mental wellbeing, whereby higher psychological flexibility scores predicted better mental wellbeing.

This is consistent with previous literature in the general population and within occupational samples (Garner & Golijani-Moghaddam, 2021; Kashdan & Rottenberg, 2010). However, athlete burnout scores were not significantly associated with the total score measures of psychological flexibility or mental wellbeing. There were significant negative correlations observed between the Emotional Exhaustion subscale of the ABQ with the CompACT total score and CompACT Openness to Experience subscale; this may reflect emotional fatigue in attempting to suppress unwanted or distressing internal events. These findings may indicate that some elements of athlete burnout were associated with psychological flexibility in this sample; however, there did not appear to be any significant impact on mental wellbeing.

More than half of participant CompACT scores within the sample were considered to indicate 'low psychological flexibility', according to cut-off scores derived from Bayliss (2018), however, there was no difference in associations detected as a result of these groupings.

Although much of the previous literature exploring athlete burnout has considered its impact on performance, significant associations with measures of general wellbeing and mental health symptoms have been established with the ABQ (Cho et al., 2019; Gerber et al., 2018; Kamimura et al., 2020; Raedeke & Smith, 2009). The findings of this study do, however,

align with a study by Chang et al. (2018), who found that lower levels of psychological flexibility were associated with increased reports of emotional exhaustion in college athletes.

Secondly, whilst it was hypothesised that higher levels of psychological flexibility would reduce the association between burnout and mental wellbeing, no significant association was found and no interaction effects with psychological flexibility and burnout were present in the data. There may be several reasons for the lack of association between burnout and mental wellbeing seen in this study. Previous research examining mental wellbeing and burnout in other occupational populations have detected significant relationships; for example, Jenkins et al. (2021) demonstrated a moderate negative association in a sample of UK doctors. However, it is possible that the WEMWBS scale was not adequately comprehensive or specific to detect the association with burnout within this population of elite rugby athletes. Alternatively, as an athlete-specific measure of burnout was used, a measure of mental wellbeing adapted for specific use with sportspeople may have generated different results. However, the WEMWBS has been used in elite athlete research to assess mental health in the past (Walton et al., 2021).

Thirdly, it appeared that there were no significant relationships between scores on psychological measures and concerns about the covid-19 pandemic. It is possible that the association between experiences of the covid-19 pandemic and elite rugby athletes' mental wellbeing, psychological flexibility and burnout were over-estimated within this hypothesis. As the study was conducted over a six-month period, during which the UK underwent national lockdowns, 'firebreak' lockdowns and lifting of restrictions, participants' experiences of the pandemic may have been too inconsistent to detect meaningful associations.

Finally, no significant differences or associations were found as a function of elite rugby athletes' injury status or expected recovery time. This is contrary to some of the previous research around the experiences of injured elite athletes. For example, it has been suggested that exposure to sports-related injuries in athletes can increase their risk of post-traumatic stress disorders and use of substances, particularly within athletes who participate in team sports (Reardon et al., 2019). However, it has also been suggested that the individual's response to injury is predictive of any subsequent effects on mental health (Putukian, 2016).

Implications

The findings showed that psychological flexibility predicted mental wellbeing with a large and robust effect size, providing further support for this relationship within an elite rugby athlete sample. The findings may provide theoretical support for the use of psychological interventions that directly target psychological flexibility, including third-wave cognitive behavioural therapies such as Dialectical Behavioural Therapy (DBT), Acceptance Commitment Therapy (ACT) and mindfulness based cognitive therapies, in elite rugby athletes.

The lack of significant correlation between athlete burnout and mental wellbeing was not expected from the indications of previous literature in occupational and athletic populations. Studies examining burnout in elite athletes have found associations to perceived stress (Lin et al., 2021), depressive symptoms (Kamimura et al., 2020) and worry (Cho et al., 2019). This research has tended to investigate burnout in relation to measures of selected aspects or symptoms of mental health; in contrast, the present study used a measure of general mental wellbeing. It is possible that the concept of athlete burnout is more strongly related to certain symptoms or mental health difficulties, which may not have been sufficiently represented in

the WEMWBS. There is also little existing literature exploring experiences of burnout and mental wellbeing specifically in elite rugby athlete populations. Therefore, it may be that rugby union-specific factors, such as culture, affect the way in which burnout and mental wellbeing develop and present within this population. Further research examining burnout and mental wellbeing in this population may benefit from using a range of measures to assess various specific and general aspects of mental wellbeing.

This study illustrates a number of opportunities for Clinical Psychologists within this subject area. There is limited previous literature examining aspects of mental wellbeing within elite rugby athletes, despite evidence that they may undergo multiple, unique psychological stressors. Preventative and protective interventions aimed at increasing psychological flexibility have demonstrated effectiveness in improving mental wellbeing and generating a culture of psychological flexibility within organisations (Bond et al., 2006; Kashdan & Rottenberg, 2010). Despite not being evident in this paper's evaluation of athlete burnout, previous research in occupational settings have found associations between psychological flexibility and burnout; therefore, increases in psychological flexibility may provide a protective factor against general occupational burnout (Puolakanaho et al., 2018). Clinical Psychology may thereby be well placed to lead and empirically evaluate psychological flexibility-based approaches to protect, maintain and improve mental wellbeing within the elite rugby athlete population. This would be consistent with recommendations for future research and practice in elite-level sport (Reardon et al., 2019).

Overall, therefore, the findings of the present study indicate the importance of psychological flexibility within the mental wellbeing of elite athletes. Further research and intervention trials aiming to increase psychological flexibility within this population, and examine impacts on mental wellbeing and athlete burnout, would be required to provide more clarity on the nature of these relationships before meaningful implications could be generated.

Strengths and Limitations

One limitation of this study was related to difficulties in recruiting from a diverse range of elite rugby players across the UK and internationally. Three of the six UK, Ireland and International Rugby Unions and Rugby Player Associations approached agreed to circulate the study to their members. Although regional data was not collected in order to protect anonymity, fewer elite athletes than expected completed all of the study measures. This meant that, despite achieving the recommended power of 0.8 expected of psychological research, it was difficult to make meaningful comparisons across subgroups within the sample. For example, only 6 of the 87 respondents who completed all of the psychological measures were female elite rugby athletes. As a result, it was not possible to consider the differing presentations of effects of psychological flexibility on burnout and mental wellbeing. It is possible that this may have prevented the generation of useful information pertaining to female rugby athletes' experiences of these processes, as gender differences in the factors that mediate influences on athlete burnout have been illustrated in previous research (Amemiya & Sakairi, 2020). Similarly, further information around the nature of participant injuries was not gathered as a part of this project. Initially, this decision was taken to ensure participants confidence in the anonymity of the questionnaire. However, differentiating general musculoskeletal physical injury from head injury and concussion may have been important in understanding how these factors intertwined. Recent research by Gouttebauge and Kerkhoffs (2021) has indicated that repeated head trauma injuries are associated with symptoms of mental ill health in former elite athletes. This may be particularly relevant to the present findings due to a study by Faulkner et al. (2020) which identified psychological flexibility as a significant factor in recovery from mild Traumatic Brain Injury (mTBI) and the development of persistent post-concussion symptoms. As elite athletes who experience concussion are at a greater risk of developing symptoms of

psychological distress (Kilic et al., 2019), it is possible that future research would benefit from explicitly differentiating head injuries from other forms of sports-injury, and further examining the role of psychological flexibility within these distinct injuries.

Another possible criticism of the current study is the use of both measures of burnout and mental wellbeing within the study design. Due to the overlap in concepts describing burnout and mental health difficulties, such as depression, some authors have suggested that burnout should be considered an aspect of a depressive syndrome (Schonfeld & Bianchi, 2016).

However, the WEMWBS focuses on positive aspects of psychology, and therefore may not overlap with as many features of burnout, such as fatigue. Furthermore, studies have found measures of burnout and mental health difficulties to have good levels of discriminant validity (Salvagioni et al., 2017).

When integrating the findings of this study with existing literature, it may also be important to consider the specific cultural context of elite rugby union. For example, it is possible that the stigma around issues of mental health and wellbeing within elite rugby athletes, as presented in the introduction, was pervasive in participant responses in this study and impacted on study recruitment. While the online questionnaire was anonymous, the invitations to participate in the study were circulated via athletes' rugby unions and rugby players associations, which may have concerned some participants about the visibility of their responses. This is particularly relevant given the findings of Küttel and Larsen (2020), which identified fear of deselection as an important stressor. Data collection took place over the course of six months, in which time athletes were considered in selection for club and international teams. It is possible that this factor caused some elite athletes to avoid participation or influenced the responses of those who did take part. Alternatively, another psychological or occupational factor that is not captured within the bounds of this study may mediate the effect of athlete burnout on mental wellbeing. It is possible that a number of the

factors elucidated in this paper may have directly or indirectly affected participant reports and subsequent results, such as drinking culture, hypermasculinity and mental health literacy.

Overall, however, the present study provided a preliminary investigation into factors that affect the mental wellbeing of individuals, who may have been historically underrepresented within Clinical Psychology in preference for investigations of athletic performance.

Conclusions

The present study provided preliminary investigation into the influence of psychological flexibility on wellbeing and athlete burnout in elite rugby athletes. The findings demonstrated that psychological flexibility significantly predicts general mental wellbeing in this population. However, the overall measure of athlete burnout was not significantly associated with psychological flexibility or mental wellbeing. Psychological flexibility was found to significantly predict the emotional exhaustion subscale of athlete burnout; this predictive strength increased slightly when examining only the openness to experience subscale of psychological flexibility with emotional exhaustion. The findings provide initial support for the importance of psychological flexibility in the protection and maintenance of mental wellbeing in elite rugby athletes. However, further research is required to assess the theoretical and clinical implications.

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Appendices

Appendix A: Journal of Contextual Behavioural Sciences Guide for Authors

Types of article

All manuscripts must clearly and explicitly be of relevance to CBS. You may find the JCBS article "[Contextual Behavioral Science: creating a science more adequate to the challenge of the human condition](#)" helpful in assessing whether your manuscript is likely to be of interest to readers of this journal.

Articles should fall into one of six categories:

1. Empirical research (up to 6000 words)
2. Brief empirical reports (up to 3000 words)
3. Review articles (up to 10,000 words)
4. Conceptual articles (up to 6000 words)
5. Practical innovations (up to 6000 words)
6. Commentaries (up to 3000 words)
7. Registered reports (see instructions below)

Word limits exclude references, tables and figures but include the abstract

1. Empirical research. JCBS welcomes manuscripts across a breadth of domains from basic behavioral science to clinical trials. Potential methodologies include but are not limited to randomized controlled trials, single case experimental designs, cross-sectional and prospective cohort studies, mixed- methods designs, and laboratory-based studies. For randomized clinical trials, JCBS requires that submissions follow CONSORT guidelines (<http://www.consort-statement.org>). Papers reporting null findings are also welcome if their methodology is sound and their power sufficient.
2. Brief empirical reports. Manuscripts in this section may report preliminary, provocative or replicated results. Empirically sound methodology and adequate power remain important considerations.
3. Review articles. Manuscripts reviewing a wide range of topics are encouraged as long as their content is directly relevant to CBS. Systematic reviews and meta-analyses are particularly welcome. For meta-analyses and systematic reviews, JCBS requires submissions follow PRISMA guidelines (<http://www.prisma-statement.org/>).
4. Conceptual articles. Manuscripts in this section should address conceptual or theoretical issues relevant to CBS. This may include papers that discuss relevant philosophical assumptions and traditions, or conceptual papers which explore aspects of or inconsistencies in contextual behavioral theory and science.

5. Practical innovations. Manuscripts in this section share innovative and practically useful descriptions of applications of CBS to a given problem area based on real world implementation, with preliminary data supporting the innovation directly (preferred) or indirectly through relevant conceptual and empirical references. Submissions are evaluated based on the degree to which they 1) provide information that is directly useful to applied work, 2) provide innovative information (e.g., a novel protocol, population, issue), 3) are based on real world implementation/practice, and 4) are based on preliminary data reported in the manuscript, or a strong link to existing conceptual/empirical literature. Submissions that report empirical data should still primarily emphasize detailed descriptions of the intervention/training protocol and/or of the applied relevance of the findings (e.g., clarifying and problem solving how to address an applied challenge identified in the study).

6. Commentaries. In some circumstances, we will consider commentaries on other manuscripts that have been recently published in JCBS. Commentaries will be subjected to peer-review and will be held to the same standards of providing a notable contribution to our field to warrant publication. Authors will typically be informed when a commentary has been submitted on a manuscript they have published and will be given the opportunity to respond in print if the commentary is published. We encourage authors to contact the editor-in-chief prior to preparing a commentary to determine potential suitability for JCBS.

7. Registered reports. Registered Reports are a form of empirical article in which the methods and proposed analyses are pre-registered and reviewed by JCBS prior to research being conducted. This format is meant to encourage researchers to conduct research that is higher risk but addresses key issues or concerns of CBS in line with the Recommendations of the *ACBS Task Force Report on the Strategies and Tactics of CBS Research* (<https://www.sciencedirect.com/science/article/pii/S2212144721000302>). Further instructions on Registered Reports, including author guidelines and the submission process, can be downloaded here '[JCBS Author Guidelines for Registered Reports.](#)'

The Journal welcomes suggestions for Special Issues. Proposals for a themed Special Issue should be sent to the Editor-in-Chief, Michael Levin at Mike.Levin@usu.edu, and should include suggested Guest Editors, a proposed call-for-papers, 6-10 example authors and topics that would fit the special issue, a proposed timeline for submission, peer-reviewing, revision and publication. All manuscripts in a special issue will be subject to the normal process of peer-review.

Contact details for submission

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You can use this list to carry out a final check of your submission before you send it to the journal for review.

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- Include a statement on **both** ethical approval and informed consent for research involving human subjects
- All figures (include relevant captions)
- All tables (including titles, description, footnotes)
- Indicate clearly if color should be used for any figures in print

Highlights

Conflict of Interest: Authors who are on the Journal of Contextual Behavioral Science editorial board must include an editor statement acknowledging their role.

Response to Reviewers (without author details; for resubmissions)

Further considerations

- Manuscript has been 'spell checked' and 'grammar checked'
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- All references mentioned in the Reference List are cited in the text, and vice versa
- Permission has been obtained for use of copyrighted material from other sources (including

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- A competing interests statement is provided, even if the authors have no competing interests to declare
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Study and Analysis Registration

A study is considered pre-registered if study details are registered in a repository prior to when the study began. Some examples of repository sites include [ClinicalTrials.gov](https://clinicaltrials.gov) and [Open Science Framework](https://www.openscienceframework.org), but there are others. For instructions on how to mask your registration details for peer -review, see "Double Anonymized Review" under Preparation.

For all pre-registered studies, authors are required to provide information on where to access it (such as trial registration number) in the manuscript. **Pre-registration in a public trials registry is required for publication of randomized controlled trials (RCTs) in the Journal for Contextual Behavioral Science in accordance with International Committee of Medical Journal Editors recommendations:** <http://www.icmje.org/>. All RCTs that began data collection after April 2022 must have pre-registered their study. All RCTs submitted after April 2025 must have pre-registered their study irrespective of when data collection occurred. For submissions that did not pre-register their RCT after these deadlines and there is a compelling reason, authors can appeal for an exception to be made in the submission

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- **Present/permanent address.** If an author has moved since the work described in the article was done, or was visiting at the time, a 'Present address' (or 'Permanent address') may be indicated as a footnote to that author's name. The address at which the author actually did the work must be retained as the main, affiliation address. Superscript Arabic numerals are used for such footnotes.

Highlights

Highlights are mandatory for this journal as they help increase the discoverability of your article via search engines. They consist of a short collection of bullet points that capture the

novel results of your research as well as new methods that were used during the study (if any). Please have a look at the examples here: [example Highlights](#).

Highlights should be submitted in a separate editable file in the online submission system. Please use 'Highlights' in the file name and include 3 to 5 bullet points (maximum 85 characters, including spaces, per bullet point).

Abstract

A concise and factual abstract is required. The abstract should state briefly the purpose of the research, the principal results and major conclusions. An abstract is often presented separately from the article, so it must be able to stand alone. For this reason, References should be avoided, but if essential, then cite the author(s) and year(s). Also, non-standard or uncommon abbreviations should be avoided, but if essential they must be defined at their first mention in the abstract itself.

Keywords

Immediately after the abstract, provide a maximum of 6 keywords, using American spelling and avoiding general and plural terms and multiple concepts (avoid, for example, 'and', 'of'). Be sparing with abbreviations: only abbreviations firmly established in the field may be eligible. These keywords will be used for indexing purposes.

Research Data

This journal encourages, but does not require, you to share data that supports your research publication in an appropriate data repository, and enables you to interlink the data with your published articles. If you are sharing data, you are encouraged to cite the data in your manuscript and reference list. Please refer to the "References" section for more information about data citation.

For more information on depositing, sharing and using research data and other relevant research materials, visit the research data page.

Research data refers to the results of observations or experimentation that validate research findings. To facilitate reproducibility and data reuse, this journal encourages, but does not require, you to share your software, code, models, algorithms, protocols, methods and other useful materials related to the project whenever possible.

It is expected that all authors who publish in the Journal of Contextual Behavioral Science will share data upon reasonable request. Therefore, we ask authors who do not already have their data openly available to the public to include an author note indicating "Data is available upon reasonable request.". Authors can request to leave this note out if they can provide an adequately strong justification for not doing so in the cover letter.

Data linking

If you have made your research data available in a data repository, you can link your article directly to the dataset. Elsevier collaborates with a number of repositories to link articles on ScienceDirect with relevant repositories, giving readers access to underlying data that gives them a better understanding of the research described.

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For supported data repositories a repository banner will automatically appear next to your published article on ScienceDirect. Another data repository option is Open Science Framework (OSF). More information on how to share data through OSF is available. In addition, you can link to relevant data or entities through identifiers within the text of your manuscript, using the following format: Database: xxxx (e.g., TAIR: AT1G01020; CCDC: 734053; PDB: 1XFN).

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This journal follows reporting standards for key types of research, including clinical trials (CONSORT and its extensions) and meta-analyses (PRISMA) as outlined in the Equator website (<https://www.equator-network.org/reporting-guidelines/>). For randomized clinical trials, JCBS requires that submissions follow CONSORT guidelines (<http://www.consort-statement.org>). For meta-analyses and systematic reviews, JCBS requires submissions follow PRISMA guidelines (<http://www.prisma-statement.org/>). JCBS recommends that authors follow similar guidelines for other study designs such as observational studies (STROBE) and qualitative studies (SRQR), which are available at <https://www.equator-network.org/reporting-guidelines/>.

Math formulae

Please submit math equations as editable text and not as images. Present simple formulae in line with normal text where possible and use the solidus (/) instead of a horizontal line for small fractional terms, e.g., X/Y. In principle, variables are to be presented in italics. Powers of e are often more conveniently denoted by exp. Number consecutively any equations that have to be displayed separately from the text (if referred to explicitly in the text).

Footnotes

Footnotes should be used sparingly. Number them consecutively throughout the article. Many word processors can build footnotes into the text, and this feature may be used. Otherwise, please indicate the position of footnotes in the text and list the footnotes themselves separately at the end of the article. Do not include footnotes in the Reference list.

Artwork

Electronic artwork

General points

- Make sure you use uniform lettering and sizing of your original artwork.
- Embed the used fonts if the application provides that option.
- Aim to use the following fonts in your illustrations: Arial, Courier, Times New Roman, Symbol, or use fonts that look similar.
- Number the illustrations according to their sequence in the text.
- Use a logical naming convention for your artwork files.
- Provide captions to illustrations separately.
- Size the illustrations close to the desired dimensions of the published version.
- Submit each illustration as a separate file.
- Ensure that color images are accessible to all, including those with impaired color vision.

A detailed [guide on electronic artwork](#) is available.

You are urged to visit this site; some excerpts from the detailed information are given here.

Formats

If your electronic artwork is created in a Microsoft Office application (Word, PowerPoint, Excel) then please supply 'as is' in the native document format.

Regardless of the application used other than Microsoft Office, when your electronic artwork is finalized, please 'Save as' or convert the images to one of the following formats (note the resolution requirements for line drawings, halftones, and line/halftone combinations given below):

EPS (or PDF): Vector drawings, embed all used fonts.

TIFF (or JPEG): Color or grayscale photographs (halftones), keep to a minimum of 300 dpi.

TIFF (or JPEG): Bitmapped (pure black & white pixels) line drawings, keep to a minimum of 1000 dpi.

TIFF (or JPEG): Combinations bitmapped line/half-tone (color or grayscale), keep to a minimum of 500 dpi.

Please do not:

- Supply files that are optimized for screen use (e.g., GIF, BMP, PICT, WPG); these typically have a low number of pixels and limited set of colors;
- Supply files that are too low in resolution;
- Submit graphics that are disproportionately large for the content.

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Ensure that each illustration has a caption. Supply captions separately, not attached to the figure. A caption should comprise a brief title (**not** on the figure itself) and a description of the illustration. Keep text in the illustrations themselves to a minimum but explain all symbols and abbreviations used.

Tables

Please submit tables as editable text and not as images. In accordance with APA style, tables should be placed on separate page(s) at the end of the manuscript. Number tables consecutively in accordance with their appearance in the text and place any table notes below the table body. Be sparing in the use of tables and ensure that the data presented in them do not duplicate results described elsewhere in the article. Please avoid using vertical rules and shading in table cells.

References

Citation in text

Please ensure that every reference cited in the text is also present in the reference list (and vice versa). Any references cited in the abstract must be given in full. Unpublished results and personal communications are not recommended in the reference list. If these references are included in the reference list they should follow the standard reference style of the journal and should include a substitution of the publication date with either 'Unpublished results' or 'Personal communication'. Citation of a reference as 'in press' implies that the item has been accepted for publication.

Web references

As a minimum, the full URL should be given and the date when the reference was last accessed. Any further information, if known (DOI, author names, dates, reference to a source publication, etc.), should also be given. Web references can be listed separately (e.g., after the reference list) under a different heading if desired, or can be included in the reference list.

Data references

This journal encourages you to cite underlying or relevant datasets in your manuscript by citing them in your text and including a data reference in your Reference List. Data references should include the following elements: author name(s), dataset title, data repository, version (where available), year, and global persistent identifier. Add [dataset] immediately before the reference so we can properly identify it as a data reference. The [dataset] identifier will not appear in your published article.

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List: references should be arranged first alphabetically and then further sorted chronologically if necessary. More than one reference from the same author(s) in the same year must be identified by the letters 'a', 'b', 'c', etc., placed after the year of publication.

Examples:

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<https://doi.org/10.1016/j.sc.2010.00372>.

Reference to a journal publication with an article number:

Van der Geer, J., Hanraads, J. A. J., & Lupton, R. A. (2018). The art of writing a scientific article. *Heliyon*, *19*, Article e00205. <https://doi.org/10.1016/j.heliyon.2018.e00205>.

Reference to a book:

Strunk, W., Jr., & White, E. B. (2000). *The elements of style* (4th ed.). Longman (Chapter 4).

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Reference to a website:

Powertech Systems. (2015). *Lithium-ion vs lead-acid cost analysis*. Retrieved from <http://www.powertechsystems.eu/home/tech-corner/lithium-ion-vs-lead-acid-cost-analysis/>. Accessed January 6, 2016

Reference to a dataset:

[dataset] Oguro, M., Imahiro, S., Saito, S., & Nakashizuka, T. (2015). *Mortality data for Japanese oak wilt disease and surrounding forest compositions*. Mendeley Data, v1.

<https://doi.org/10.17632/xwj98nb39r.1>.

Reference to a conference paper or poster presentation:

Engle, E.K., Cash, T.F., & Jarry, J.L. (2009, November). *The Body Image Behaviours Inventory-3: Development and validation of the Body Image Compulsive Actions and Body Image Avoidance Scales*. Poster session presentation at the meeting of the Association for Behavioural and Cognitive Therapies, New York, NY.

Reference to software:

Coon, E., Berndt, M., Jan, A., Svyatsky, D., Atchley, A., Kikinzon, E., Harp, D., Manzini, G., Shelef, E., Lipnikov, K., Garimella, R., Xu, C., Moulton, D., Karra, S., Painter, S., Jafarov, E., & Molins, S. (2020, March 25). *Advanced Terrestrial Simulator (ATS) v0.88 (Version 0.88)*. Zenodo. <https://doi.org/10.5281/zenodo.3727209>.

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List: references should be arranged first alphabetically and then further sorted chronologically if necessary. More than one reference from the same author(s) in the same year must be identified by the letters 'a', 'b', 'c', etc., placed after the year of publication.

Examples:

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<https://doi.org/10.1016/j.Sc.2010.00372>.

Reference to a journal publication with an article number:

Van der Geer, J., Hanraads, J. A. J., & Lupton, R. A. (2018). The art of writing a scientific article. *Heliyon*, *19*, e00205. <https://doi.org/10.1016/j.heliyon.2018.e00205>.

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Reference to a chapter in an edited book:

Mettam, G. R., & Adams, L. B. (2009). How to prepare an electronic version of your article. In B. S. Jones, & R. Z. Smith (Eds.), *Introduction to the electronic age* (pp. 281–304). New York: E-Publishing Inc.

Reference to a website:

Cancer Research UK. Cancer statistics reports for the UK. (2003).
<http://www.cancerresearchuk.org/aboutcancer/statistics/cancerstatsreport/> Accessed 13 March 2003.

Reference to a dataset:

[dataset] Oguro, M., Imahiro, S., Saito, S., Nakashizuka, T. (2015). *Mortality data for Japanese oak wilt disease and surrounding forest compositions*. Mendeley Data, v1.
<https://doi.org/10.17632/xwj98nb39r.1>.

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Engle, E.K., Cash, T.F., & Jarry, J.L. (2009, November). The Body Image Behaviours Inventory-3: Development and validation of the Body Image Compulsive Actions and Body Image Avoidance Scales. Poster session presentation at the meeting of the Association for Behavioural and Cognitive Therapies, New York, NY.

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Appendix B: Systematic Review Search Terms and Operators

1	cognitive defusion
2	cognitive fusion
3	thought defusion
4	thought fusion.mp
5	intervention
6	therap*
7	experiential distance
8	ACT based intervention
13	5 or 6 or 8
14	1 or 2 or 3 or 4 or 7
21	13 and 14
22	remove duplicates from 21

Appendix C: Data Extraction Template

Study ID

Title

Title of paper / abstract / report that data are extracted from

Lead author contact details

Country in which the study conducted

- United States
- UK
- Canada
- Australia
- Other

Notes

Characteristics of included studies

Methods

Aim of study

Study design

- Randomised controlled trial
- Non-randomised experimental study
- Cohort study
- Cross sectional study
- Case control study
- Systematic review
- Qualitative research
- Prevalence study
- Case series
- Case report
- Diagnostic test accuracy study
- Clinical prediction rule
- Economic evaluation
- Text and opinion
- Other

Primary Outcome Measure of Interest

Secondary Outcome Measure of Interest

Format of Cognitive Defusion Intervention

Format of Comparison Intervention

Control conditions

Delivery method

- Face-to-face
- Online videoconferencing
- Mobile app
- Pre-recorded video
- Scripted guidance

Intervention setting

Participants

Recruitment Method

Participants contacted using

- Phone
- Email
- Social media
- University research participation schemes
- Other

Inclusion criteria

MH inclusion criteria:

- Existing MH diagnosis
- Scored above cut-off on MH measure
- Self-reported MH symptoms
- None required for participation

Exclusion criteria**Participants recruited from**

- Community
- University
- Clinical setting
- Clinical waiting lists
- Other

Participant Recruitment Data

	Number of Participants
Community	
University	
Clinical Setting	
Clinical Waiting List	
Other	

Total number of participants**Results****Baseline Population Characteristics**

	Number of Participants
Total N	
Male	
Female	
Mean Age	

Baseline Population Characteristics by Condition

	Cognitive Defusion Intervention	Intervention 2	Control
N			
Gender Percentage			
Mean Age			

Participant attrition**Outcome table Cognitive Defusion Condition**

	Pre-intervention	Post-Intervention	Follow-Up
Mean			
SD			

Outcome Table Comparison Condition

	Pre-Intervention	Post-Intervention	Follow-up
Mean			
SD			

Outcome Table Control Condition

	Pre-Intervention	Post-Intervention	Follow-up
Mean			
SD			

Outcome table (change data)

	Cognitive Defusion Intervention	Comparison Intervention	Control Condition
Mean			
SD			
Effect Size			

Significant main effects (primary outcome measure)

- Effect of condition on outcome measure
- Effect of time on outcome measure

Cognitive Defusion Intervention Data

	Effect of Time on Outcome Measure
F value	
p value	
Effect size	

Evaluation of cognitive defusion in improving MH outcome

- CD more effective than comparison and control
- CD more effective than control, no difference to comparison
- CD more effective than comparison, no difference to control
- Comparison and/or control more effective than CD
- No difference between conditions

Conclusions**Key findings**

Appendix D - JBI Critical Appraisal Checklist for Randomised Controlled Trials

INTRODUCTION

JBI is an international research organisation based in the Faculty of Health and Medical Sciences at the University of Adelaide, South Australia. JBI develops and delivers unique evidence-based information, software, education and training designed to improve healthcare practice and health outcomes. With over 70 Collaborating Entities, servicing over 90 countries, JBI is a recognised global leader in evidence-based healthcare.

JBI Systematic Reviews

The core of evidence synthesis is the systematic review of literature of a particular intervention, condition or issue. The systematic review is essentially an analysis of the available literature (that is, evidence) and a judgment of the effectiveness or otherwise of a practice, involving a series of complex steps. JBI takes a particular view on what counts as evidence and the methods utilised to synthesise those different types of evidence. In line with this broader view of evidence, JBI has developed theories, methodologies and rigorous processes for the critical appraisal and synthesis of these diverse forms of evidence in order to aid in clinical decision-making in healthcare. There now exists JBI guidance for conducting reviews of effectiveness research, qualitative research, prevalence/incidence, etiology/risk, economic evaluations, text/opinion, diagnostic test accuracy, mixed-methods, umbrella reviews and scoping reviews. Further information regarding JBI systematic reviews can be found in the [JBI Evidence Synthesis Manual](#).

JBI Critical Appraisal Tools

All systematic reviews incorporate a process of critique or appraisal of the research evidence. The purpose of this appraisal is to assess the methodological quality of a study and to determine the extent to which a study has addressed the possibility of bias in its design, conduct and analysis. All papers selected for inclusion in the systematic review (that is – those that meet the inclusion criteria described in the protocol) need to be subjected to rigorous appraisal by two critical appraisers. The results of this appraisal can then be used to inform synthesis and interpretation of the results of the study. JBI Critical appraisal tools have been developed by the JBI and collaborators and approved by the JBI Scientific Committee following extensive peer review. Although designed for use in systematic reviews, JBI critical appraisal tools can also be used when creating Critically Appraised Topics (CAT), in journal clubs and as an educational tool.

JBI CRITICAL APPRAISAL CHECKLIST FOR RANDOMIZED CONTROLLED TRIALS

Reviewer _____

Date _____

Author _____ Year _____ Record Number _____

	Yes	No	Unclear	NA
1. Was true randomization used for assignment of participants to treatment groups?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
2. Was allocation to treatment groups concealed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
3. Were treatment groups similar at the baseline?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4. Were participants blind to treatment assignment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5. Were those delivering treatment blind to treatment assignment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6. Were outcomes assessors blind to treatment assignment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
7. Were treatment groups treated identically other than the intervention of interest?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
8. Was follow up complete and if not, were differences between groups in terms of their follow up adequately described and analyzed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
9. Were participants analyzed in the groups to which they were randomized?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
10. Were outcomes measured in the same way for treatment groups?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
11. Were outcomes measured in a reliable way?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
12. Was appropriate statistical analysis used?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
13. Was the trial design appropriate, and any deviations from the standard RCT design (individual randomization, parallel groups) accounted for in the conduct and analysis of the trial?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Overall appraisal: Include Exclude Seek further info

Comments (Including reason for exclusion)

EXPLANATION FOR THE CRITICAL APPRAISAL TOOL FOR RCTS WITH INDIVIDUAL PARTICIPANTS IN PARALLEL GROUPS

How to cite: Tufanaru C, Munn Z, Aromataris E, Campbell J, Hopp L. Chapter 3: Systematic reviews of effectiveness. In: Aromataris E, Munn Z (Editors). *JBIManual for Evidence Synthesis*. JBI, 2020. Available from <https://synthesismanual.jbi.global>

Answers: Yes, No, Unclear or Not/Applicable

Critical Appraisal Tool for RCTs (individual participants in parallel groups)

1. Was true randomization used for assignment of participants to treatment groups?

The differences between participants included in compared groups constitutes a threat to the internal validity of a study exploring causal relationships. If participants are not allocated to treatment and control groups by random assignment there is a risk that the allocation is influenced by the known characteristics of the participants and these differences between the groups may distort the comparability of the groups. A true random assignment of participants to the groups means that a procedure is used that allocates the participants to groups purely based on chance, not influenced by the known characteristics of the participants. Check the details about the randomization procedure used for allocation of the participants to study groups. Was a true chance (random) procedure used? For example, was a list of random numbers used? Was a computer-generated list of random numbers used?

2. Was allocation to groups concealed?

If those allocating participants to the compared groups are aware of which group is next in the allocation process, that is, treatment or control, there is a risk that they may deliberately and purposefully intervene in the allocation of patients by preferentially allocating patients to the treatment group or to the control group and therefore this may distort the implementation of allocation process indicated by the randomization and therefore the results of the study may be distorted. Concealment of allocation (allocation concealment) refers to procedures that prevent those allocating patients from knowing before allocation which treatment or control is next in the allocation process. Check the details about the procedure used for allocation concealment. Was an appropriate allocation concealment procedure used? For example, was central randomization used? Were sequentially numbered, opaque and sealed envelopes used? Were coded drug packs used?

3. Were treatment groups similar at the baseline?

The differences between participants included in compared groups constitute a threat to the internal validity of a study exploring causal relationships. If there are differences between participants included in compared groups there is a risk of selection bias. If there

are differences between participants included in the compared groups maybe the ‘effect’ cannot

be attributed to the potential ‘cause’ (the examined intervention or treatment), as maybe it is plausible that the ‘effect’ may be explained by the differences between participants, that is, by selection bias. Check the characteristics reported for participants. Are the participants from the compared groups similar with regards to the characteristics that may explain the effect even in the absence of the ‘cause’, for example, age, severity of the disease, stage of the disease, co-existing conditions and so on? Check the proportions of participants with specific relevant characteristics in the compared groups. Check the means of relevant measurements in the compared groups (pain scores; anxiety scores; etc.). *[Note: Do NOT only consider the P-value for the statistical testing of the differences between groups with regards to the baseline characteristics.]*

4. Were participants blind to treatment assignment?

If participants are aware of their allocation to the treatment group or to the control group there is the risk that they may behave differently and respond or react differently to the intervention of interest or to the control intervention respectively compared to the situations when they are not aware of treatment allocation and therefore the results of the study may be distorted. Blinding of participants is used in order to minimize this risk. Blinding of the participants refers to procedures that prevent participants from knowing which group they are allocated. If blinding of participants is used, participants are not aware if they are in the group receiving the treatment of interest or if they are in any other group receiving the control interventions. Check the details reported in the article about the blinding of participants with regards to treatment assignment. Was an appropriate blinding procedure used? For example, were identical capsules or syringes used? Were identical devices used? Be aware of different terms used, blinding is sometimes also called masking.

5. Were those delivering treatment blind to treatment assignment?

If those delivering treatment are aware of participants’ allocation to the treatment group or to the control group there is the risk that they may behave differently with the participants from the treatment group and the participants from the control group, or that they may treat them differently, compared to the situations when they are not aware of treatment allocation and this may influence the implementation of the compared treatments and the results of the study may be distorted. Blinding of those delivering treatment is used in order to minimize this risk. Blinding of those delivering treatment refers to procedures that prevent those delivering treatment from knowing which group they are treating, that is those delivering treatment are not aware if they are treating the group receiving the treatment of interest or if they are treating any other group receiving the control interventions. Check the details reported in the article about the blinding of those delivering treatment with regards to treatment assignment. Is there any information in the article about those delivering the treatment? Were those delivering the treatment unaware of the assignments of participants to the compared groups?

6. Were outcomes assessors blind to treatment assignment?

If those assessing the outcomes are aware of participants' allocation to the treatment group or to the control group there is the risk that they may behave differently with the participants from the treatment group and the participants from the control group compared to the situations when they are not aware of treatment allocation and therefore there is the risk that the measurement of the outcomes may be distorted and the results of the study may be distorted. Blinding of outcomes assessors is used in order to minimize this risk. Check the details reported in the article about the blinding of outcomes assessors with regards to treatment assignment. Is there any information in the article about outcomes assessors? Were those assessing the treatment's effects on outcomes unaware of the assignments of participants to the compared groups?

7. Were treatment groups treated identically other than the intervention of interest?

In order to attribute the 'effect' to the 'cause' (the treatment or intervention of interest), assuming that there is no selection bias, there should be no other difference between the groups in terms of treatment or care received, other than the manipulated 'cause' (the treatment or intervention controlled by the researchers). If there are other exposures or treatments occurring at the same time with the 'cause' (the treatment or intervention of interest), other than the 'cause', then potentially the 'effect' cannot be attributed to the examined 'cause' (the investigated treatment), as it is plausible that the 'effect' may be explained by other exposures or treatments occurring at the same time with the 'cause' (the treatment of interest). Check the reported exposures or interventions received by the compared groups. Are there other exposures or treatments occurring at the same time with the 'cause'? Is it plausible that the 'effect' may be explained by other exposures or treatments occurring at the same time with the 'cause'? Is it clear that there is no other difference between the groups in terms of treatment or care received, other than the treatment or intervention of interest?

8. Was follow up complete and if not, were differences between groups in terms of their follow up adequately described and analyzed?

For this question, follow up refers to the time period from the moment of random allocation (random assignment or randomization) to compared groups to the end time of the trial. This critical appraisal question asks if there is complete knowledge (measurements, observations etc.) for the entire duration of the trial as previously defined (that is, from the moment of random allocation to the end time of the trial), for all randomly allocated participants. If there is incomplete follow up, that is incomplete knowledge about all randomly allocated participants, this is known in the methodological literature as the post-assignment attrition. As RCTs are not perfect, there is almost always post-assignment attrition, and the focus of this question is on the appropriate exploration of post-assignment attrition (description of loss to follow up, description of the reasons for loss to follow up, the estimation of the impact of loss

to follow up on the effects etc.). If there are differences with regards to the loss to follow up between the compared groups in an RCT, these differences represent a threat to the internal validity of a randomized experimental study exploring causal effects, as these differences may provide a plausible alternative explanation for the observed ‘effect’ even in the absence of the ‘cause’ (the treatment or intervention of interest). When appraising an RCT, check if there were differences with regards to the loss to follow up between the compared groups. If follow up was incomplete (that is, there is incomplete information on all participants), examine the reported details about the strategies used in order to address incomplete follow up, such as descriptions of loss to follow up (absolute numbers; proportions; reasons for loss to follow up) and impact analyses (the analyses of the impact of loss to follow up on results). Was there a description of the incomplete follow up (number of participants and the specific reasons for loss to follow up)? It is important to note that with regards to loss to follow up, it is not enough to know the number of participants and the proportions of participants with incomplete data; the reasons for loss to follow up are essential in the analysis of risk of bias; even if the numbers and proportions of participants with incomplete data are similar or identical in compared groups, if the patterns of reasons for loss to follow up are different (for example, side effects caused by the intervention of interest, lost contact etc.), these may impose a risk of bias if not appropriately explored and considered in the analysis. If there are differences between groups with regards to the loss to follow up (numbers/proportions and reasons), was there an analysis of patterns of loss to follow up? If there are differences between the groups with regards to the loss to follow up, was there an analysis of the impact of the loss to follow up on the results? [Note: Question 8 is NOT about intention-to-treat (ITT) analysis; question 9 is about ITT analysis.]

9. Were participants analyzed in the groups to which they were randomized?

This question is about the intention-to-treat (ITT) analysis. There are different statistical analysis strategies available for the analysis of data from randomized controlled trials, such as intention-to-treat analysis (known also as intent to treat; abbreviated, ITT), per-protocol analysis, and as-treated analysis. In the ITT analysis the participants are analyzed in the groups to which they were randomized, regardless of whether they actually participated or not in those groups for the entire duration of the trial, received the experimental intervention or control intervention as planned or whether they were compliant or not with the planned experimental intervention or control intervention. The ITT analysis compares the outcomes for participants from the initial groups created by the initial random allocation of participants to those groups. Check if ITT was reported; check the details of the ITT. Were participants analyzed in the groups to which they were initially randomized, regardless of whether they actually participated in those groups, and regardless of whether they actually received the planned interventions? *[Note: The ITT analysis is a type of statistical analysis recommended in the Consolidated Standards of Reporting Trials (CONSORT) statement on best practices in trials reporting, and it is considered a marker of good methodological quality of the analysis of results of a randomized trial. The ITT is estimating the effect of offering the intervention, that is, the effect of instructing the*

participants to use or take the intervention; the ITT it is not estimating the effect of actually receiving the intervention of interest.]

10. Were outcomes measured in the same way for treatment groups?

If the outcome (the ‘effect’) is not measured in the same way in the compared groups there is a threat to the internal validity of a study exploring a causal relationship as the differences in outcome measurements may be confused with an effect of the treatment (the ‘cause’). Check if the outcomes were measured in the same way. Same instrument or scale used? Same measurement timing? Same measurement procedures and instructions?

11. Were outcomes measured in a reliable way?

Unreliability of outcome measurements is one threat that weakens the validity of inferences about the statistical relationship between the ‘cause’ and the ‘effect’ estimated in a study exploring causal effects. Unreliability of outcome measurements is one of the different plausible explanations for errors of statistical inference with regards to the existence and the magnitude of the effect determined by the treatment (‘cause’). Check the details about the reliability of measurement such as the number of raters, training of raters, the intra-rater reliability, and the inter-raters reliability within the study (not as reported in external sources). This question is about the reliability of the measurement performed in the study, it is not about the validity of the measurement instruments/scales used in the study. *[Note: Two other important threats that weaken the validity of inferences about the statistical relationship between the ‘cause’ and the ‘effect’ are low statistical power and the violation of the assumptions of statistical tests. These other two threats are explored within Question 12).]*

12. Was appropriate statistical analysis used?

Inappropriate statistical analysis may cause errors of statistical inference with regards to the existence and the magnitude of the effect determined by the treatment (‘cause’). Low statistical power and the violation of the assumptions of statistical tests are two important threats that weaken the validity of inferences about the statistical relationship between the ‘cause’ and the ‘effect’. Check the following aspects: if the assumptions of statistical tests were respected; if appropriate statistical power analysis was performed; if appropriate effect sizes were used; if appropriate statistical procedures or methods were used given the number and type of dependent and independent variables, the number of study groups, the nature of the relationship between the groups (independent or dependent groups), and the objectives of statistical analysis (association between variables; prediction; survival analysis etc.).

13. Was the trial design appropriate for the topic, and any deviations from the standard RCT design accounted for in the conduct and analysis?

Certain RCT designs, such as the crossover RCT, should only be conducted when appropriate. Alternative designs may also present additional risks of bias if not accounted for in the design and analysis.

Crossover trials should only be conducted in people with a chronic, stable condition, where the intervention produces a short term effect (i.e. relief in symptoms). Crossover trials should ensure there is an appropriate period of washout between treatments.

Cluster RCTs randomize groups of individuals, forming 'clusters.' When we are assessing outcomes on an individual level in cluster trials, there are unit-of-analysis issues, as individuals within a cluster are correlated. This should be taken into account by the study authors when conducting analysis, and ideally authors will report the intra-cluster correlation coefficient.

Stepped-wedge RCTs may be appropriate when it is expected the intervention will do more good than harm, or due to logistical, practical or financial considerations in the roll out of a new treatment/intervention. Data analysis in these trials should be conducted appropriately, taking into account the effects of time.

Appendix E - JBI Critical Appraisal Checklist for Quasi-Experimental Studies

INTRODUCTION

JBI is an international research organisation based in the Faculty of Health and Medical Sciences at the University of Adelaide, South Australia. JBI develops and delivers unique evidence-based information, software, education and training designed to improve healthcare practice and health outcomes. With over 70 Collaborating Entities, servicing over 90 countries, JBI is a recognised global leader in evidence-based healthcare.

JBI Systematic Reviews

The core of evidence synthesis is the systematic review of literature of a particular intervention, condition or issue. The systematic review is essentially an analysis of the available literature (that is, evidence) and a judgment of the effectiveness or otherwise of a practice, involving a series of complex steps. JBI takes a particular view on what counts as evidence and the methods utilised to synthesise those different types of evidence. In line with this broader view of evidence, JBI has developed theories, methodologies and rigorous processes for the critical appraisal and synthesis of these diverse forms of evidence in order to aid in clinical decision-making in healthcare. There now exists JBI guidance for conducting reviews of effectiveness research, qualitative research, prevalence/incidence, etiology/risk, economic evaluations, text/opinion, diagnostic test accuracy, mixed-methods, umbrella reviews and scoping reviews. Further information regarding JBI systematic reviews can be found in the [JBI Evidence Synthesis Manual](#).

JBI Critical Appraisal Tools

All systematic reviews incorporate a process of critique or appraisal of the research evidence. The purpose of this appraisal is to assess the methodological quality of a study and to determine the extent to which a study has addressed the possibility of bias in its design, conduct and analysis. All papers selected for inclusion in the systematic review (that is – those that meet the inclusion criteria described in the protocol) need to be subjected to rigorous appraisal by two critical appraisers. The results of this appraisal can then be used to inform synthesis and interpretation of the results of the study. JBI Critical appraisal tools have been developed by the JBI and collaborators and approved by the JBI Scientific Committee following extensive peer review. Although designed for use in systematic reviews, JBI critical appraisal tools can also be used when creating Critically Appraised Topics (CAT), in journal clubs and as an educational tool.

JBI CRITICAL APPRAISAL CHECKLIST FOR QUASI-EXPERIMENTAL STUDIES

Reviewer _____

Date _____

Author _____ Year _____ Record Number _____

	Yes	No	Unclear	Not applicable
1. Is it clear in the study what is the 'cause' and what is the 'effect' (i.e. there is no confusion about which variable comes first)?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
2. Were the participants included in any comparisons similar?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
3. Were the participants included in any comparisons receiving similar treatment/care, other than the exposure or intervention of interest?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4. Was there a control group?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5. Were there multiple measurements of the outcome both pre and post the intervention/exposure?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6. Was follow up complete and if not, were differences between groups in terms of their follow up adequately described and analyzed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
7. Were the outcomes of participants included in any comparisons measured in the same way?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
8. Were outcomes measured in a reliable way?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
9. Was appropriate statistical analysis used?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Overall appraisal: Include Exclude Seek further info

Comments (Including reason for exclusion)

EXPLANATION FOR THE CRITICAL APPRAISAL TOOL FOR QUASI-EXPERIMENTAL STUDIES

How to cite: Tufanaru C, Munn Z, Aromataris E, Campbell J, Hopp L. Chapter 3: Systematic reviews of effectiveness. In: Aromataris E, Munn Z (Editors). JBI Manual for Evidence Synthesis. JBI, 2020. Available from <https://synthesismanual.jbi.global>

Critical Appraisal Tool for Quasi-Experimental Studies (Experimental Studies without random allocation)

Answers: Yes, No, Unclear or Not/Applicable

1. Is it clear in the study what is the ‘cause’ and what is the ‘effect’ (i.e. there is no confusion about which variable comes first)?

Ambiguity with regards to the temporal relationship of variables constitutes a threat to the internal validity of a study exploring causal relationships. The ‘cause’ (the independent variable, that is, the treatment or intervention of interest) should occur in time before the explored ‘effect’ (the dependent variable, which is the effect or outcome of interest). Check if it is clear which variable is manipulated as a potential cause. Check if it is clear which variable is measured as the effect of the potential cause. Is it clear that the ‘cause’ was manipulated before the occurrence of the ‘effect’?

2. Were the participants included in any comparisons similar?

The differences between participants included in compared groups constitute a threat to the internal validity of a study exploring causal relationships. If there are differences between participants included in compared groups there is a risk of selection bias. If there are differences between participants included in the compared groups maybe the ‘effect’ cannot be attributed to the potential ‘cause’, as maybe it is plausible that the ‘effect’ may be explained by the differences between participants, that is, by selection bias. Check the characteristics reported for participants. Are the participants from the compared groups similar with regards to the characteristics that may explain the effect even in the absence of the ‘cause’, for example, age, severity of the disease, stage of the disease, co-existing conditions and so on? [NOTE: In one single group pre-test/post-test studies where the patients are the same (the same one group) in any pre-post comparisons, the answer to this question should be ‘yes.’]

3. Were the participants included in any comparisons receiving similar treatment/care, other than the exposure or intervention of interest?

In order to attribute the ‘effect’ to the ‘cause’ (the exposure or intervention of interest), assuming that there is no selection bias, there should be no other difference between the groups in terms of treatments or care received, other than the manipulated ‘cause’ (the intervention of interest). If there are other exposures or treatments occurring in the same time with the ‘cause’, other than the intervention of interest, then potentially the ‘effect’ cannot be attributed to the intervention of interest, as it is plausible that the ‘effect’ may be explained by other exposures or treatments, other than the intervention of interest, occurring in the same time with the intervention of interest. Check the reported exposures or interventions received by the compared groups. Are there other exposures or treatments occurring in the same time with the intervention of interest? Is it plausible that the ‘effect’ may be explained by other exposures or treatments occurring in the same time with the intervention of interest?

4. Was there a control group?

Control groups offer the conditions to explore what would have happened with groups exposed to other different treatments, other than to the potential ‘cause’ (the intervention of interest). The

comparison of the treated group (the group exposed to the examined ‘cause’, that is, the group receiving the intervention of interest) with such other groups strengthens the examination of the causal plausibility. The validity of causal inferences is strengthened in studies with at least one independent control group compared to studies without an independent control group. Check if there are independent, separate groups, used as control groups in the study. *[Note: The control group should be an independent, separate control group, not the pre-test group in a single group pre-test post-test design.]*

5. Were there multiple measurements of the outcome both pre and post the intervention/exposure?

In order to show that there is a change in the outcome (the ‘effect’) as a result of the intervention/treatment (the ‘cause’) it is necessary to compare the results of measurement before and after the intervention/treatment. If there is no measurement before the treatment and only measurement after the treatment is available it is not known if there is a change after the treatment compared to before the treatment. If multiple measurements are collected before the intervention/treatment is implemented then it is possible to explore the plausibility of alternative explanations other than the proposed ‘cause’ (the intervention of interest) for the observed ‘effect’, such as the naturally occurring changes in the absence of the ‘cause’, and changes of high (or low) scores towards less extreme values even in the absence of the ‘cause’ (sometimes called regression to the mean). If multiple measurements are collected after the intervention/treatment is implemented it is possible to explore the changes of the ‘effect’ in time in each group and to compare these changes across the groups. Check if measurements were collected before the intervention of interest was implemented. Were there multiple pre-test measurements? Check if measurements were collected after the intervention of interest was implemented. Were there multiple post-test measurements?

6. Was follow up complete and if not, were differences between groups in terms of their follow up adequately described and analyzed?

If there are differences with regards to the loss to follow up between the compared groups these differences represent a threat to the internal validity of a study exploring causal effects as these differences may provide a plausible alternative explanation for the observed ‘effect’ even in the absence of the ‘cause’ (the treatment or exposure of interest). Check if there were differences with regards to the loss to follow up between the compared groups. If follow up was incomplete (that is, there is incomplete information on all participants), examine the reported details about the strategies used in order to address incomplete follow up, such as descriptions of loss to follow up (absolute numbers; proportions; reasons for loss to follow up; patterns of loss to follow up) and impact analyses (the analyses of the impact of loss to follow up on results). Was there a description of the incomplete follow up (number of participants and the specific reasons for loss to follow up)? If there are differences between groups with regards to the loss to follow up, was there an analysis of patterns of loss to follow up? If there are differences between the groups with regards to the loss to follow up, was there an analysis of the impact of the loss to follow up on the results?

7. Were the outcomes of participants included in any comparisons measured in the same way?

If the outcome (the ‘effect’) is not measured in the same way in the compared groups there is a threat to the internal validity of a study exploring a causal relationship as the differences in outcome measurements may be confused with an effect of the treatment or intervention of interest (the ‘cause’). Check if the outcomes were measured in the same way. Same instrument or scale used? Same measurement timing? Same measurement procedures and instructions?

8. Were outcomes measured in a reliable way?

Unreliability of outcome measurements is one threat that weakens the validity of inferences about the statistical relationship between the ‘cause’ and the ‘effect’ estimated in a study exploring causal effects. Unreliability of outcome measurements is one of different plausible explanations for errors of

statistical inference with regards to the existence and the magnitude of the effect determined by the treatment

(‘cause’). Check the details about the reliability of measurement such as the number of raters, training of raters, the intra-rater reliability, and the inter-raters reliability within the study (not to external sources). This question is about the reliability of the measurement performed in the study, it is not about the validity of the measurement instruments/scales used in the study. *[Note: Two other important threats that weaken the validity of inferences about the statistical relationship between the ‘cause’ and the ‘effect’ are low statistical power and the violation of the assumptions of statistical tests. These other threats are not explored within Question 8, these are explored within Question 9.]*

9. Was appropriate statistical analysis used?

Inappropriate statistical analysis may cause errors of statistical inference with regards to the existence and the magnitude of the effect determined by the treatment (‘cause’). Low statistical power and the violation of the assumptions of statistical tests are two important threats that weakens the validity of inferences about the statistical relationship between the ‘cause’ and the ‘effect’. Check the following aspects: if the assumptions of statistical tests were respected; if appropriate statistical power analysis was performed; if appropriate effect sizes were used; if appropriate statistical procedures or methods were used given the number and type of dependent and independent variables, the number of study groups, the nature of the relationship between the groups (independent or dependent groups), and the objectives of statistical analysis (association between variables; prediction; survival analysis etc.).

Appendix F: Journal of Clinical Sport Psychology Author Guidelines

Prior to submission, please carefully read and follow the submission guidelines detailed below. Authors must submit their manuscripts through the journal's ScholarOne online submission system. To submit, click the button below:

Authorship Guidelines

The Journals Division at Human Kinetics adheres to the criteria for authorship as outlined by the International Committee of Medical Journal Editors*:

Each author should have participated sufficiently in the work to take public responsibility for the content. Authorship credit should be based only on substantial contributions to:

- a. Substantial contributions to the conception or design of the work; or the acquisition, analysis, or interpretation of data for the work; AND
- b. Drafting the work or revising it critically for important intellectual content; AND
- c. Final approval of the version to be published; AND
- d. Agreement to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

Conditions a, b, c, and d must all be met. Individuals who do not meet the above criteria may be listed in the acknowledgments section of the manuscript. *<http://www.icmje.org/recommendations/browse/roles-and-responsibilities/defining-the-role-of-authors-and-contributors.html>

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Manuscript Guidelines

The *Journal of Clinical Sport Psychology (JCSP)* is a scholarly, peer-reviewed journal that provides practical and clinically relevant recommendations to mental health providers and practitioners in sport and exercise psychology, stimulates provocative discussions, promotes best practices and intervention strategies, and disseminates applied research findings that clearly show clinical relevance. The journal covers a broad range of topics. Some examples of relevant topics include psychology of injury, eating disorders, exercise and mental health, and substance use disorders. This journal highlights the clinical application of research findings within direct psychological service delivery spanning a wide range of clients and settings. *JCSP* recognizes the importance of diverse methodologies, varied treatment approaches, and the importance of having authors represent a wide range of perspectives around sport and exercise psychology. *JCSP* is international in scope and submissions from around the world and with diverse populations are encouraged.

Writing Style

All manuscript submissions should be written in accessible language, free of scientific jargon, and easily understood by a wide variety of clinicians and applied sport psychology practitioners. To align with the mission of *JCSP*, submissions should strive to demonstrate clinical relevance. Authors should follow the latest version of APA style, and manuscripts should be void of typos and grammatical errors. Line numbering and 12-point font should be used for the double-spaced document.

Types of Papers

- **Original research papers** follow traditional format and section headings to demonstrate literature background, methods, results, and discussion. All methodologies are welcomed (e.g., quantitative, qualitative, and mixed methods). To follow ethical research procedures, prior institutional review board approval is expected. Applied and clinical relevance of the project should be clearly demonstrated throughout. Authors are strongly encouraged to include a clinical implications section in their manuscript to provide practical implications for clinicians or practitioners working in this field. Including abstract (150 word maximum), tables, and references these papers should not exceed 30 pages.
- **Practice papers** are applied pieces that may, for example, detail “best practice” prevention, intervention, or treatment approaches to addressing problems within sport, exercise, and performance contexts. Another option is a “lessons learned” paper that describes challenges and recommendations gleaned from implementing a program or intervention with athletes, exercisers, or performers. Papers may also use published research findings or theoretical frameworks to inform practice recommendations to clinicians, practitioners, and others working in the field. These papers should be 20 pages or less, including the abstract (150 word maximum), tables, and references.
- **Commentaries** are brief papers that describe hot topics in the field or argue for a particular viewpoint, therapeutic approach or strategies for navigating challenges that may arise in research and practice for sport and exercise psychology. Although these may be viewed as “opinion papers”, authors should tie ideas to the literature and employ a professional tone to ensure a compelling scholarly argument. These papers require abstracts (150 word maximum) and should not exceed 10 pages.
- **Case illustration and therapeutic approach papers** articulate a specific and in depth case to make a problem “come to life” and illustrate challenges and intervention strategies in a clinical or applied setting. If based on a real case, all identifying characteristics should be altered to protect the identity of the client(s). These papers must include a case narrative, case interpretation, and a clinical implications section with a recommended therapeutic approach supported by research. These papers require abstracts (150 word maximum) and should not exceed 7 pages.
- **Around the World papers** allow for more inclusive publishing of works by authors outside of North America who give voice to under-represented countries. While these submissions can follow any of the formats accepted by *JCSP* (original research, practice paper, commentary, or case illustration), all papers should clearly list the country in the title. A clinical implications section that elaborates on the role of sport and exercise psychology within the country being studied should be included as well as the implications of the work to influence the prominence of clinical sport psychology within the region. Editors may identify certain papers as eligible for this designation. Papers that appear in the “Around the World” section (which started in March 2021 issue of *JCSP*) are open access to allow for increased visibility and potential impact.

Questions about the journal or manuscript submission should be directed to the Editor-in-Chief of the journal, Dr. Justine Reel, reelj@uncw.edu.

Artwork

In figures, use black and white only, no shading or color. Resolution of digital images should be 300 dpi at full size for photos and 600 dpi for line art; color images cannot be accepted. Figures or photos should be in .jpeg or .tif files. Format tables using the “Table” function of your word processing program rather than aligning columns in text with tabs and spaces or using text boxes.

Review

The review process is blind, with manuscripts read by external reviewers. There are no page charges to contributors. Authors of manuscripts accepted for publication must transfer copyright to Human Kinetics, Inc.

Please note that papers decisioned as reject and resubmit during 2017 should no longer consider resubmitting as of August 2018. Please also be aware that while authors are welcome to submit their work that has been previously reviewed, all manuscripts will be treated as new submissions and need to adhere to current author guidelines.

Desk Rejection Policy

Before full review, submissions are examined at the editorial level. If the Editor and an Editorial Board Member believe the submission has extensive flaws or is inconsistent with the mission and focus of the journal, the manuscript may receive a desk reject decision.

Appendix G: Comprehensive Assessment of Acceptance and Commitment Therapy

Processes (CompACT)

Please rate the following 23 statements using the scale below:

	0	1	2	3	4	5	6
	Strongly disagree	Moderately disagree	Slightly disagree	Neither agree nor disagree	Slightly agree	Moderately agree	Strongly agree
1. I can identify the things that really matter to me in life and pursue them	0	1	2	3	4	5	6
2. One of my big goals is to be free from painful emotions	0	1	2	3	4	5	6
3. I rush through meaningful activities without being really attentive to them	0	1	2	3	4	5	6
4. I try to stay busy to keep thoughts or feelings from coming	0	1	2	3	4	5	6
5. I act in ways that are consistent with how I wish to live my life	0	1	2	3	4	5	6
6. I get so caught up in my thoughts that I am unable to do the things that I most want to do	0	1	2	3	4	5	6
7. I make choices based on what is important to me, even if it is stressful	0	1	2	3	4	5	6
8. I tell myself that I shouldn't have certain thoughts	0	1	2	3	4	5	6
9. I find it difficult to stay focused on what's happening in the present	0	1	2	3	4	5	6
10. I behave in line with my personal values	0	1	2	3	4	5	6
11. I go out of my way to avoid situations that might bring difficult thoughts, feelings, or sensations	0	1	2	3	4	5	6
12. Even when doing the things that matter to me, I find myself doing them without paying attention	0	1	2	3	4	5	6
13. I am willing to fully experience whatever thoughts, feelings and sensations come up for me, without trying to change or defend against them	0	1	2	3	4	5	6
14. I undertake things that are meaningful to me, even when I find it hard to do so	0	1	2	3	4	5	6
15. I work hard to keep out upsetting feelings	0	1	2	3	4	5	6
16. I do jobs or tasks automatically, without being aware of what I'm doing	0	1	2	3	4	5	6
17. I am able to follow my long terms plans including times when progress is slow	0	1	2	3	4	5	6
18. Even when something is important to me, I'll rarely do it if there is a chance it will upset me	0	1	2	3	4	5	6
19. It seems I am "running on automatic" without much awareness of what I'm doing	0	1	2	3	4	5	6
20. Thoughts are just thoughts – they don't control what I do	0	1	2	3	4	5	6
21. My values are really reflected in my behaviour	0	1	2	3	4	5	6
22. I can take thoughts and feelings as they come, without attempting to control or avoid them	0	1	2	3	4	5	6
23. I can keep going with something when it's important to me	0	1	2	3	4	5	6

Appendix H: Athlete Burnout Questionnaire (ABQ)

Please read the following statements and rate how often you feel this way.

1. I am accomplishing many worthwhile things in rugby
2. I feel so tired from training that I have trouble finding energy to do other things
3. The effort I spend in rugby would better spent doing other things
4. I feel overly tired from my rugby participation
5. I am not achieving much in rugby
6. I don't care as much about my rugby performance as I used to
7. I am not performing up to my ability in rugby
8. I feel "wiped out" from rugby
9. I am not into rugby like I used to be
10. I feel physically worn out from rugby
11. I feel less concerned about being successful in rugby than I used to
12. I am exhausted by the mental and physical demands of rugby
13. It seems that no matter what I do, I don't perform as well as I should
14. I feel successful in rugby
15. I have negative feelings towards rugby

Response set is a 5-point Likert scale of (1) "almost never", (2) "rarely", (3) "sometimes", (4) "frequently", (5) "almost always".

Appendix I: Warwick-Edinburgh Mental Well-Being Scale (WEMWBS)

The Warwick–Edinburgh Mental Well-being Scale (WEMWBS)

Below are some statements about feelings and thoughts.

Please tick the box that best describes your experience of each over the last 2 weeks

STATEMENTS	None of the time	Rarely	Some of the time	Often	All of the time
I've been feeling optimistic about the future	1	2	3	4	5
I've been feeling useful	1	2	3	4	5
I've been feeling relaxed	1	2	3	4	5
I've been feeling interested in other people	1	2	3	4	5
I've had energy to spare	1	2	3	4	5
I've been dealing with problems well	1	2	3	4	5
I've been thinking clearly	1	2	3	4	5
I've been feeling good about myself	1	2	3	4	5
I've been feeling close to other people	1	2	3	4	5
I've been feeling confident	1	2	3	4	5
I've been able to make up my own mind about things	1	2	3	4	5
I've been feeling loved	1	2	3	4	5
I've been interested in new things	1	2	3	4	5
I've been feeling cheerful	1	2	3	4	5

Warwick–Edinburgh Mental Well-being Scale (WEMWBS)

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Appendix J: Cardiff University School of Psychology Research Ethics Committee

Approval

Ethics Feedback - EC.21.03.09.6306R

psychethics <psychethics@cardiff.ac.uk>

Mon 29/03/2021 13:13

To: Sophie McFarland <McFarlandS@cardiff.ac.uk>

Cc: Victoria Samuel <SamuelV3@cardiff.ac.uk>; James Stroud <StroudJ@cardiff.ac.uk>

Dear Sophie,

The Ethics Committee has considered your PG project proposal: *Examining the factors that influence psychological wellbeing in professional rugby athletes* (EC.21.03.09.6306R).

The project has been approved.

Please note that if any changes are made to the above project then you must notify the Ethics Committee.

Best wishes,

Lucy Scarisbrick-Wright

On behalf of Adam Hammond



This mailbox is monitored by members of the Education Team



Adam Hammond



Lucy Scarisbrick-Wright

School of Psychology Research Ethics Committee

Cardiff University
Tower Building
70 Park Place
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Please note that I do not expect a response to this email outside of your normal working hours

Nid wyf yn disgwyl ymateb i'r ebost hwn y tu allan i'ch oriau gwaith arferol

Appendix K: Study Information Sheet



School of Psychology
Ysgol Seicoleg

School of Psychology, Cardiff University

Examining the factors that influence psychological wellbeing in professional rugby athletes

Study Information Sheet

Invitation

You are being invited to take part in this research project. Before you decide to do so, it is important you understand why the research is being done and what it will involve. Please take time to read the following information carefully and discuss it with others if you wish. Ask us if there is anything that is not clear or if you would like more information and take time to decide whether or not you wish to take part. Thank you for reading this.

What is the purpose of the project?

This study aims to look at some of the factors that can have an impact on the psychological wellbeing of professional rugby athletes. It is hoped that this project will provide a better understanding of how we can tailor support to enhance the wellbeing professional rugby union players.

Why have I been invited to participate?

You have been invited to take part in this study because you are a professional rugby union player. Therefore, your particular experiences will be important in understanding how different factors impact on psychological wellbeing within this population.

Do I have to take part?

No. Participation in this study is completely voluntary, so you only take part if you want to. Even if you begin the study and change your mind, you can stop at any time without giving a reason. If you don't take part, or decide to stop, it will not have any impact on your training, career or access to services.

What will happen?

If you decide to participate, the whole study will take place online. First, you will need to read through a consent form, confirming that you are happy to continue by ticking to agree with six statements about the project. Second, you will be asked some questions about your demographic information (age, gender, length of career, etc), and about your experience of the covid-19 pandemic. Finally, you will be asked to answer three questionnaires about

psychological wellbeing, burnout and a skill called psychological flexibility (which relates to our ability to stay connected to the present moment and adapt our behaviour).

Will my answers be kept anonymous?

Yes. All of the answers you give will be held totally anonymously, so that it is impossible to trace this information back to you individually. The data you provide will only be seen by the research team, and we would not be able to identify your data personally. The data will be stored on the Cardiff university system and accessed using an encrypted laptop. The findings of the study, using any answers provided by you and other professional rugby athletes, will be written up and may be published; however, individual data will not be reported and the number of participants from each region or rugby union will not be stated.

What will happen at the end of the study?

The answers you and other professional rugby athletes provide will be analysed to look for the ways different factors affect psychological wellbeing. These will then be written up and submitted as part of *Sophie McFarland's* training in Clinical Psychology. They may also be published in a research article and presented to other professionals who work in psychological wellbeing in professional athletes. *Sophie McFarland* and other members of the research team will also offer a feedback letter to all regions and rugby unions who take part in the study, detailing the findings of the project.

Who is organising and funding the research?

This project is organised and funded by the School of Psychology at Cardiff University.

Who has said that the study is OK to go ahead?

The research study has been reviewed and approved by the School of Psychology Research Ethics Committee at Cardiff University. If you have any questions or concerns, you can contact the researcher, *Sophie McFarland*, research supervisors, *Dr James Stroud* and *Dr Victoria Samuel*, or the Ethics Committee directly using the contact details at the end of this information sheet.

If you would like more information about the project, please feel free to contact us:

Primary Researcher:	Research Supervisor:	Research Supervisor:	Ethics Committee:
Sophie McFarland Trainee Psychologist	Dr James Stroud Clinical Psychologist	Dr Victoria Samuel Clinical Psychologist	Secretary to the Ethics Committee
School of Psychology Cardiff University Tower Building 70 Park Place Cardiff	School of Psychology Cardiff University Tower Building 70 Park Place Cardiff	School of Psychology Cardiff University Tower Building 70 Park Place Cardiff	School of Psychology Cardiff University Tower Building 70 Park Place Cardiff

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McFarlandS@cardiff.ac.uk

StroudJ@cardiff.ac.uk

SamuelV3@cardiff.ac.uk

PsychEthics@cardiff.ac.uk

029 2087 4007

029 2087 6804

029 2087 6804

029 2087 0360

Thank you for taking the time to consider this invitation.

Privacy Notice:

The information provided will be held in compliance with UK GDPR regulations. Cardiff University is the data controller and James Merrifield is the data protection officer (inforequest@cardiff.ac.uk). This information is being collected by *Sophie McFarland*. This information will be held securely and separately from the research information you provide. Only the researcher will have access to this form and it will be destroyed after 7 years. The lawful basis for processing this information is public interest.

Appendix L: Study Consent Form

School of Psychology, Cardiff University

Examining the factors that influence psychological wellbeing in elite rugby athletes

Consent Form - Anonymous data

Please tick to confirm your agreement with each statement:

- I understand that my participation in this project will involve completing demographic questions and three questionnaires around my psychological wellbeing and feelings of burnout, which will take approximately 15 minutes of my time.
- I understand that participation in this study is entirely voluntary and that I can withdraw from the study at any time without giving a reason, and without this impacting on my career.
- I understand that I am free to ask any questions at any time. I am free to withdraw or discuss my concerns with the researcher *Sophie McFarland* (McFarlands@cardiff.ac.uk) or the supervisors *Dr James Stroud* (StroudJ2@cardiff.ac.uk), *Dr Victoria Samuel* (SamuelV3@cardiff.ac.uk) or *Dr Dale Thomas* (DaleThomas@wru.wales).
- I understand that at the end of the study I will be provided with additional information and feedback about the purpose of the study.
- I understand that the research information provided by me will be held totally anonymously, so that it is impossible to trace this information back to me individually. I understand that the information I provide may be retained indefinitely.
- I understand that the findings of this project may also be published in a research article and presented to other professionals who work in related fields.
- I consent to participate in the above study conducted by *Sophie McFarland* School of Psychology, Cardiff University with the supervision of *Dr James Stroud*, *Dr Victoria Samuel* and *Dr Dale Thomas*.

Privacy Notice:

The information provided on the consent form will be held in compliance with GDPR regulations. Cardiff University is the data controller and Matt Cooper is the data protection officer (inforequest@cardiff.ac.uk). This information is being collected *Sophie McFarland*. This information will be held securely and separately from the research information you provide. Only the researcher will have access to this form and it will be destroyed after 7 years. The lawful basis for processing this information is public interest.

Appendix M: Office of National Statistics (ONS) Opinion and Lifestyle Survey (OPN)

Question of Covid-19 Related Worry

How worried or unworried are you about the effect that the Coronavirus (COVID-19) outbreak is having on your life right now?

1. Very worried
2. Somewhat worried
3. Neither worried nor unworried
4. Somewhat unworried
5. Not at all worried
6. Don't know
7. Prefer not to say